Regional Oral History Office
The Bancroft Library

University of California
Berkeley, California

University History Oral History Series

Kenneth Sanborn Pitzer

CHEMIST AND ADMINISTRATOR AT UC BERKELEY, RICE UNIVERSITY, STANFORD UNIVERSITY, AND THE ATOMIC ENERGY COMMISSION, 1935-1997

With Introductions by
Robert Curl
and
Marilyn Chapin Massey

Includes an Interview with Jean Mosher Pitzer

Interviews Conducted by
Sally Smith Hughes
and
Germaine LaBerge
in 1996, 1997, and 1998

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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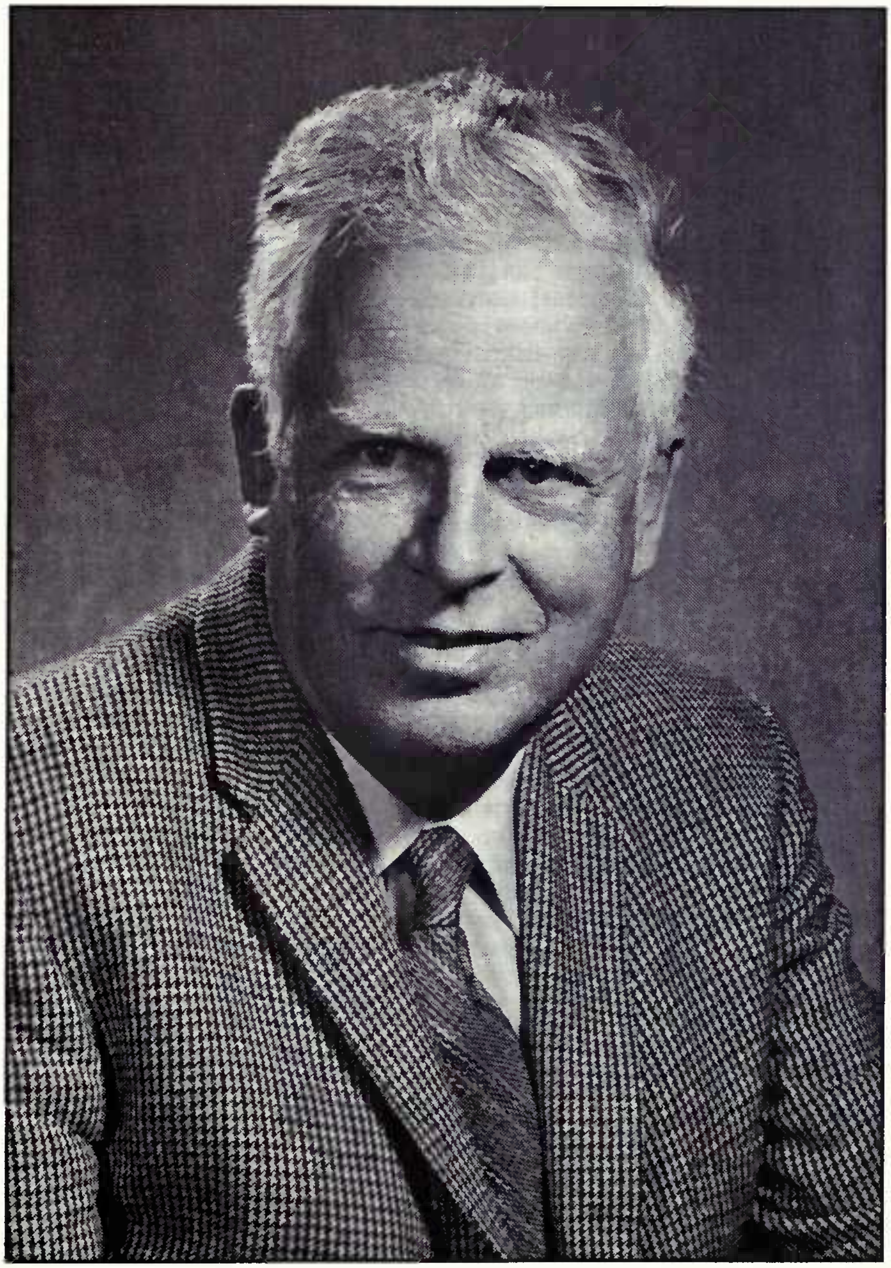

Kenneth Pitzer, 1970s.

San Francisco Chronicle

December 29, 1997

Kenneth S. Pitzer

Kenneth S. Pitzer, a highly respected chemist and the former president of Stanford and Rice universities, died Friday in a Berkeley hospital of heart failure after an illness. He was 83.

Mr. Pitzer was known both for his illustrious research career and for his tenure as a university administrator. A former dean of the College of Chemistry at the University of California at Berkeley, he retired in 1984.

"He was a world-class physical chemist who did exceptional work on the theory of predicting the thermodynamic properties of molecules," said Alexis T. Bell, dean of Berkeley's college of chemistry. "He was particularly known for his work in strong saline solutions, which has tremendous fundamental (research) importance, and is also essential in predicting the behavior of materials used in industrial processes."

Mr. Pitzer published widely on both chemistry and physics, and served as the technical director of the Maryland Research Laboratory during World War II. He was director of research for the Atomic Energy Commission from 1949 to 1951, and was the commission's chairman from 1960 to 1962. He was a member of the National Aeronautics and Space Administration's Science and Technology Advisory Committee from 1964 to 1965.

Mr. Pitzer began his administrative career at Rice, where he served as president from 1961 to 1969. He left Rice to accept the Stanford presidency.

His tenure at Stanford was short and tumultuous, culminating in his resignation after 10 months. Although he was a critic of the Vietnam War, Mr. Pitzer and his administrative policies were roundly criticized by students opposed to the war. Police were called to the campus in response to rioting at least 13 times during his administration. Citing his weariness with "matters of a purely administrative or even a police nature," he returned to an academic life at Berkeley.

Mr. Pitzer is survived by his widow, Jean M. Pitzer, of Kensington; three children, Ann E. Pitzer of San Diego, Russell M. Pitzer of Columbus, Ohio, and John S. Pitzer of McLean, Va.; and five grandchildren.

A memorial service is pending. Donations may be sent to the College of Chemistry, Latimer Hall, UC Berkeley, Berkeley, Calif. 94720, where they will be used to endow a scholarship.

The Sunday Times
2/1/98

Kenneth Pitzer, 83, knew the formula for achievement

By Kate Darby Rauch
TIMES STAFF WRITER

BERKELEY — The simplicity of an orange tree, the complexity of a molecule, the riddle of turning pieces of wood into a boat at sail. These were a few of the many fascinations of Kenneth Pitzer.

Pitzer — a renowned physical chemist, an enthusiastic grower of fruit trees, a keen boat builder and sailor — died of heart failure Dec. 26 in Berkeley. He was 83.

Pitzer's career as a scientist typified an academic life bursting at the seams, with strong ties to UC-Berkeley. Born and raised in Pomona, he came to Berkeley in 1935 to pursue his Ph.D. in chemistry and was hired after graduation as a professor.

He taught at UC-Berkeley for many years, developing a fast reputation as a scholar and as a congenial mentor for graduate students. Eclectic on and off campus, Pitzer had a variety of specialties within physical chemistry, but was well-known for his examination of the thermodynamics of molecules, or the way they spin and bounce and dance.

"He was really a paragon. Everyone admired him and recognized he was one of the best scientists and scientific leaders," said John Prausnitz, a colleague of Pitzer's and professor of chemical engineering at UC-Berkeley.

Pitzer served as dean of UC-Berkeley's School of Chemistry from 1951 to 1960, then became president of Rice University in Houston for eight years. After Rice, he spent a brief period as president of Stanford University, from 1968 to 1970. He resigned because he was uncomfortable overseeing the campus during a time of student antiwar demonstrations. Openly against the Vietnam War, Pitzer didn't enjoy playing head cop, said his wife, Jean Pitzer, of Kensington.

After Stanford, Pitzer returned to Berkeley and his familiar UC chemistry department. He was an active professor and professor emeritus until his death.

"He was invited back to Berkeley even though at his age, it was thought he wouldn't be productive," said Leo Brewer, Pitzer's friend and colleague and a professor emeritus of chemistry at UC-Berkeley. "He was very productive."

In between his academic appointments, Pitzer worked for the U.S. government, including as director of research for the Atomic Energy Commission from 1949 to 1951. Though involved in discussions about developing the hydrogen bomb, Pitzer wasn't particularly wrapped up in the Washington politics of the time, Jean Pitzer said. His main focus was in converting government laboratories for peacetime pursuits, she said.

Career was never work for Pitzer, it was passion, Jean Pitzer said. "He loved his work. He was over (on campus) all day, every day. He was always excited about his field," she said.

Jean Pitzer met her husband in elementary school in Pomona. She didn't know then, she said, that he would become her life companion, but after dating during their senior year in high school the lasting nature of the friendship became clear. Kenneth Pitzer went to Cal Tech for his undergraduate studies, and Jean went to Pomona College. They married after they graduated.

Even in elementary school, Jean Pitzer said, her husband showed signs of becoming a scientist, with an early flare for math. Pitzer's mother was a high school math teacher, his grandfather had a degree in mathematics and his father was an accomplished mathematician — though by career a lawyer. Lawyer and farmer, that is.

Pitzer's father, Russell — a founder and benefactor of Pitzer College, one of the Claremont Colleges — owned orange groves near Pomona. And just as Pitzer was influenced by the mathematicians in his family, he also was influenced by orange trees. Pitzer's father spent time in the groves, often taking his son along.

"(Kenneth) knew all about orchards and how to fix all the machinery," said Russell Pitzer, one of his two sons and a professor of chemistry at Ohio State University.

It was wisdom that lasted a lifetime. Pitzer's family owns a country house at Clearlake; this became his own growing ground.

"Orange, grapefruit, persimmon, apricot, walnut, peaches, apples," said Jean Pitzer. "He did all the work. He pruned them and cultivated them. He was very active."

But the Clearlake retreat was more to Pitzer than an outlet for his green thumb. It was also an outlet for his fascination with sailing and for building the craft he sailed. An accomplished boat builder, Pitzer crafted several sailboats. For awhile he kept a boat at the Richmond Yacht Harbor, later using Clearlake as his nautical home base.

Whether hoisting a jib or snipping a bud, Pitzer simply enjoyed being outdoors, family members said. As well as trees and water, he was drawn to mountains, being an avid hiker and camper. The Pitzer family often headed out with camping gear, driving back roads, meandering.

"He liked to explore, to see new areas," said son Russell.

Family members and friends describe Pitzer as the quintessential well-rounded individual, not stuck in his science, his orchards, his boats.

Pitzer is survived by his wife, Jean Pitzer; three children, Russell, Ann Pitzer of San Diego and John Pitzer of McLean, Va.; and five grandchildren. Memorial donations can be sent to the Kenneth S. Pitzer Fund, College of Chemistry, Latimer Hall, University of California, Berkeley, CA 94720.

Cataloguing information

PITZER, Kenneth Sanborn (1914-1997) Chemist, college president

<u>Chemist and Administrator at UC Berkeley, Rice University, Stanford University, and the Atomic Energy Commission, 1935-1997</u>, 1999, xiii, 544 pp.

Childhood and education in Pomona, B.S. from Caltech; College of Chemistry, UC Berkeley, 1935-1997: graduate student years, G.N. Lewis, William Giauque, Wendell Latimer; postwar expansion and reorganization of the college; Director of Research, Atomic Energy Commission, 1949-1951; research: internal rotation in ethane, ring molecules, corresponding states, relativistic effects on molecular properties, spin species conversion in methane, other condensed state research, ion interaction equations for aqueous electrolytes, Pitzer equations for concentrated solutions; discusses teaching and scientific collaborations, committee work, the research process; presidency of Rice University, 1961-1968, Stanford University, 1968-1970. Includes an interview with JEAN MOSHER PITZER on faculty wives, College of Chemistry, years at Stanford, and includes interviews conducted at Rice University on Rice presidency.

Introductions by Robert Curl, Professor of Chemistry, Rice University, and Marilyn Chapin Massey, President, Pitzer College.

Interviewed 1996-1997 by Sally Smith Hughes and Germaine LaBerge, Regional Oral History Office, The Bancroft Library, University of California, Berkeley.

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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 of 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 Morrough 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 Robert Curl

A person is measured by the impact that they have upon others. By that standard, Kenneth Pitzer was a giant. I first heard of him in a natural products course that I took as a senior from Dick Turner. I admired Dick Turner quite a lot and when Dick talked enthusiastically in this course about Pitzer's discovery of barriers to internal rotations and the profound importance of this discovery in organic chemistry, I decided I wanted to go to graduate school at Berkeley and study with this great chemist. It was a decision that I not only never regretted but every day I thank my lucky stars that I trusted Dick. I certainly didn't know enough myself to make such a decision rationally and it was a very fortunate choice.

After I arrived at Berkeley and joined Ken's research group, Ken proved to be an ideal mentor, at least for me. He believed that each student should develop at his own pace and in his own way, but he was always accessible, always cordial, always helpful when I'd visit him in his office. I knew that he must have been very busy as Dean of the College of Chemistry but he was always relaxed. We interacted a great deal in his direction of my thesis research. I must confess that all the new ideas were his. He was marvelously generous in giving credit.

What I learned from Ken is that if you want to be a successful scientist, you must strive to look at things in a different way, strive to learn, strive to come up with new ideas. Anyone who knew Ken would also know that all this striving must be done in a perfectly relaxed manner. That is one lesson that I didn't learn even though I often wished I could. The most important things I learned from Ken are that a good scientist is honest and generous. God, it was fun to work with him. It was really great when he later came to Rice and I got a chance to work with him again.

Ken had a kind nature. I remember my first year at Berkeley he invited me to Thanksgiving dinner where I met Ann and John--I think Russ was at Caltech that Thanksgiving. I believe that he guessed correctly that a Texas boy 2,000 miles from home might be a little lonely at Thanksgiving.

I'll always remember the vital role that Ken played in guiding the start of my scientific career, but there's nothing unique about my case. Ken had dozens of graduate students and post-doctorals over the years and I bet they all feel the same way.

Ken had a natural talent for leadership. He could see further and more clearly into how events would develop in the future than anyone I ever knew. More importantly he knew the clear shining light of quality.

And he had a vision for leading an institution to achieve the highest quality.

As a Rice chauvinist, I think his leadership of Rice was his finest hour. It's the only case I can speak of first-hand. When Ken and Jean came to Rice in 1961, Rice was a good regional university emphasizing science and engineering. It attracted good students from the state of Texas through its reputation for rigor and its free tuition. Ken had a vision of Rice as a national university where the best minds--students and faculty--would discover and propagate knowledge. In the history of Rice, his was a pivotal presidency. He deserves much of the credit for elevating Rice from a nice regional school to a national university. He was instrumental for removing racial restrictions, he vastly improved the humanities and the social sciences, and expanded and improved the graduate program. The most important thing he gave Rice was a vision of itself as a great university. Ken's vision lives on at Rice in the hearts and minds of hundreds of dedicated people. Many of them have never met him.

There are many varied aspects of Ken's career. After he graduated from Caltech about the time that he and Jean were married, they moved to Berkeley where he became a graduate student. After completing his Ph.D., he stayed on as a member of the faculty. Then during World War II, the Pitzers moved to Maryland where he became director of the Maryland Research Lab. They moved back to Berkeley, then to Washington again when Ken became the director of the Atomic Energy Commission. After Washington, there were several years at Berkeley. They moved to Rice in 1961 for seven years, and then on to Stanford, and finally back to Berkeley. This makes me think about how the U.S.A. gives the impression of rootlessness: people move hither and thither across the country throughout their lives pursuing various career goals. Ken and Jean Pitzer were always willing to pursue a goal wherever it took them--whether it was to serve the country near Washington, or to develop a university like Rice or Stanford. They were happy to move when there was an important reason to move.

But looking more deeply, one realizes that ever since Ken chose Berkeley as a place to start his academic career, Ken and Jean's roots really remained here. They kept their home in Berkeley and their retreat home in Clear Lake no matter where they moved. When Ken came to Rice he resigned his professorship at Berkeley because it was the the right thing to do, the choice that Ken would always make, but even so, I think he always thought of Berkeley as home.

The lives of Ken and Jean Pitzer remind us that it's possible to reach out, to grow, to explore, to lead without forgetting who we are or where our roots are. Sixty-two years of a happy marriage and three adult autonomous children who appear to be very happy are accomplishments with a value equal to the important advances in human knowledge or the development of a great university. A few rare, gifted, hard-working, lucky individuals have and do all these things. Kenneth Pitzer was such a person.

Robert F. Curl
Professor of Chemistry
Rice University

January 25, 1998
Houston, Texas

INTRODUCTION by Marilyn Chapin Massey

I write this introduction to the oral history of Kenneth S. Pitzer on behalf of Pitzer College, its trustees, faculty, staff, and students. We are all privileged to have had our lives enriched by the heritage of Ken and the Pitzer family.

Created by the generosity of his father, Russell K. Pitzer, our college holds as its founding purpose to excel in the social sciences. Because of the circumstances of the college's birth in the 1960s, this purpose was infused with social concern, a commitment to social justice. And we are privileged to carry the Pitzer family's name as part of our own histories: there are now nearly 6,000 Pitzer College graduates and currently 800 students. Ken Pitzer's integrity lives on in southern California and around the world, wherever Pitzer students and graduates live out the college mission: *Provida Futuri*, to provide a better future, to make a better world.

When I last saw Ken, I asked him what exactly he was doing when Pitzer was being founded in the early 1960s. He told me that he had been asked to become president of Rice University, which was racially segregated at the time. As a reflection of his deep integrity, he agreed to accept the position only on the condition that Rice become racially integrated. Though this was a bold stand for the period, the university agreed. I became fascinated with this important story and with its meaning to Pitzer students past and present. Ken graciously allowed us to write about this shared moment in history.

Pitzer College was born in that same era when Ken fought the legal battles to eliminate segregation from Rice. What a gift to this college community to have been founded by a family whose members themselves understand--and, more important, live out--the balance of brilliance and social concern.

Having won the battle to integrate Rice University, Ken gave the 1966 graduation address at Pitzer College for the three graduates of this barely breathing institution. The title of the address was "Orthodoxy and Dissent." He said:

> The role of a college is complex; it is not only a place of learning, but it is also a place of living... On its academic side, the college must pass to the next generation the intellectual heritage of [humankind]; this is the orthodoxy of my title. But a college must also prepare students to contribute to progress in the future... to do this, one must encourage students to question ideas which are commonly

> accepted today. This is the dissent in my title.
>
> It is not just any orthodoxy and any dissent we seek. It is easy to disagree with the present way of doing almost anything--what we seek is responsible dissent--it is the duty of truly educated [persons]... to compare the actual events they observe with the currently accepted theories. And when they find that the real world does not behave in the manner predicted by the theory, [they] should draw this to the attention of others...

Today, in 1998, Pitzer College has as its most cherished and difficult educational objective to educate for social responsibility. Decades ago, Ken himself stood as a living exemplar of that goal. How is it that something so immeasurably complex becomes so simply evident? Only through a rare brilliance and clarity of purpose like Ken's. As a scholar, I envy Ken's students. I can imagine how wonderfully their lights have dawned.

And yet, in a sense, I have been his student. Early in my tenure as president, Ken and his wife, Jean, came to visit the college and stayed at the president's house. I was facing an extraordinarily difficult situation at the college. I was so green, I thought I needed to show this founding family that all was well. But as I sat at the breakfast table with Ken, who was then a life trustee, I found myself relaxing in the presence of his kindness and wisdom. I eventually shared my troubles.

Ken listened hard and restated my problem insightfully and far more succinctly. Then he smiled at me and added: "You know what to do, and you will do it well." And, of course, I did know. But that smile, the simplicity of the words, transformed me. As a wind to a sail, or a hand on the shoulder, Ken's smile gently sustained me as I went on to do one of the hardest things I had ever done, and did it well. And in many other difficult moments since then, I have kept that smile with me, and I will continue to do so.

That the source of that strength was Ken's own integrity would have been enough to guide me. But more than that, Ken was a Pitzer, and his life inseparable from the Pitzer legacy in Pomona and Claremont. In all my interactions with other Claremont Colleges, I encounter that legacy. In Pitzer Hall at Claremont McKenna College, that college's first academic building, funded by Ken's father as a founding trustee in 1949. At Sanborn Hall at Pitzer, named for Ken's mother. At Scott Hall, named for his stepmother. In the Flora S. Sanborn Professorship at Pitzer and the Russell K. Pitzer Professorship at Claremont McKenna.

As Ken himself said in 1960, "For me, there are in Claremont innumerable personal ties with a father, a mother, and a wife--and a relationship for which the dictionary provides no standard term, the relationship to Pitzer College, which is the gift of my father arising from a lifelong interest in education and a truly generous nature."

That statement bears repeating: There is no standard term for the relationship of the Pitzer family to Pitzer College.

One last narrative illustrates why. Last year, the Claremont Colleges founded a graduate institute of bioengineering, the first institution born in Claremont since Pitzer. Of the six Claremonts, only Pitzer College raised important questions about the founding of that institute. In most basic terms, we asked what it would take for it to be academically excellent and ethical. In this, we were supported and led by the Pitzer family: Ken and his son Russ.

This issue was a burning one when Ken was chosen two years ago to be honored by all trustees of the Claremont Colleges to receive the Robert J. Bernard Award for Outstanding Service to the Colleges. At the awards ceremony, the featured talk concerned the pending creation of the graduate institute of bioengineering. In his acceptance speech, in front of the trustees of the Claremonts, Ken spoke his mind about excellence and ethics in bioengineering.

His speech was a moment of truth telling, one that was courageous, timely, gracious, and effective. The students and faculty at Pitzer, concerned about ethical issues, discovered that its founders stood with them on principle.

And indeed we continue to stand in loyal opposition, signaling that we will settle for nothing less than the blend of the excellent and the ethical, and that we will work positively to achieve it. Ken and I both smiled after his talk, and I caught the twinkle in his eye. He and I received the praise of others, trustees of the other colleges among them, for Pitzer's insight and courage to raise the hard questions.

It was another lesson by Ken and his son Russ to us at Pitzer College on the importance of acting responsibly and collaboratively for principles: a lesson in social responsibility, taught thirty years after Ken first embodied that term for Pitzer.

I thank Ken Pitzer and the Pitzer family for their generosity and for the lessons taught so magnificently to me and to this generation.

Marilyn Chapin Massey
President, Pitzer College

Claremont, California
May 21, 1998

INTERVIEW HISTORY--Kenneth Pitzer

Kenneth Pitzer was a distinguished man by any measure. He has been described by his Berkeley colleagues as "one of the greatest physical chemists of this century." He arrived at Berkeley for graduate work in 1935, a time when the College of Chemistry was in productive ferment. He and his peers--Glenn Seaborg, Harold Urey, and Melvin Calvin among them--perceived no limits to what they might accomplish. Although the depression continued, the new field of quantum mechanics opened vistas for basic theoretical treatment of many chemical problems. Pitzer immediately set about teaching himself quantum mechanics. Within two years he had earned a Ph.D. under Wendell M. Latimer and was soon launched on path-breaking work on internal rotation in ethane. He went on from this stellar beginning to make contributions in many key areas of chemistry--chemical thermodymanics, quantum theory of atomic and molecular structure, and statistical theory of liquids, solids, and solutions. He describes in detail in the oral history the key aspects of this research.

Dr. Pitzer was also a fine administrator. During World War II he served as technical director of the Maryland Research Laboratory, which designed and tested devices for behind-the-lines warfare. From 1949 to 1951 he was the first director of research at the Atomic Energy Commission, a predecessor of the Department of Energy. He returned to Berkeley to become dean of the College of Chemistry (1950-1961) where his calm and diplomatic administrative style was widely appreciated, particularly during the tumultuous period when the Department of Chemical Engineering was being created. Dr. Pitzer is also remembered for his presidency of Rice University (1961-1968), where he changed the racial restriction for entrance, and of Stanford (1968-1970) during the height of campus turmoil over the Vietnam War. He was invited back to UC Berkeley as a full professor. In fact, Dr. Pitzer was one of the only university presidents from the Vietnam War period who returned to "productive academic life," as Mrs. Pitzer mentions in her interview.

Dr. Pitzer was also distinguished in manner and appearance, even in the casual shirt and slacks which he routinely wore for work in his functional office and laboratory in the basement of Hildebrand Hall on the Berkeley campus. All the interviews were recorded there. Soft-spoken and gracious, he looked younger than his eighty-three years, despite a fringe of snow-white hair, and retained the quick mind for which he has always been noted.

The Oral History Process

Sally Hughes, science historian, conducted eleven interviews with Dr. Pitzer on his science; Germaine LaBerge, university historian, conducted three interviews on his childhood, family background and university governance. Two more sessions were planned to cover his presidency of Stanford and outside activities, before Dr. Pitzer's untimely death in December 1997. Jean Pitzer, his wife and confidante of sixty-two years, graciously stepped in and filled in the gaps, offering the perspective of the supportive and knowledgeable spouse and partner. The transcripts are included in this volume. Dr. Pitzer had reviewed and carefully edited his interviews; Mrs Pitzer, in like manner, her own.

As background for the science interviews, Hughes talked, at Dr. Pitzer's suggestion, with Bradley Moore and Herbert Strauss, both colleagues in the College of Chemistry. She is grateful to both men for their patience in explaining and interpreting the significance of Dr. Pitzer's research for a neophyte in the physical sciences.

Dr. Pitzer came prepared for every session with copious notes on the topic to be discussed. In the course of the interviews, he frequently consulted a bound volume of his selected papers[1] or a book from the shelves lining his office. His methodical and comprehensive approach took much of the burden from the science interviewer. He talked slowly and deliberately, sometimes interspersing a chuckle. We agreed at our first meeting not to repeat the history of his early career which had already been well covered in previous oral histories.

The reader will find an older interview by Robert Seidel for The Bancroft Library at the beginning of the volume, and in the Appendix one conducted by Professor Harold Hyman of Rice University on Dr. Pitzer's accomplishments there as president, and one conducted by Dr. Louis J. Marchiafava and Dr. John Bowles for the Rice University Oral History Project. Thanks very much to Rice University for permission to include these fine interviews. Also included in the Appendix is an interview conducted with Dr. Pitzer in the mid-1970s by David Ridgway of the Lawrence Hall of Science in Berkeley.

We are grateful to Dr. Pitzer's colleagues, Robert F. Curl, Jr., Rice University professor and Nobel Laureate, and Marilyn Chapin Massey, president of Pitzer College, whose introductions to this volume describe Kenneth Pitzer's many contributions to scientific research and science policy, and governance at the federal and university levels.

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[1] Molecular Structure and Statistical Thermodynamics: Selected Papers of Kenneth S. Pitzer, Kenneth S. Pitzer, editor. World Scientific Series in 20th Century Chemistry, Vol. 1, World Scientific, 1993.

This history, we hope, reflects the life, myriad achievements, and personality of this eminent scientist, administrator, and citizen. Researchers will also want to consult the extensive collection of Kenneth and Jean Pitzer's papers deposited in The Bancroft Library. We are grateful to have captured most of his story before his sudden death on December 26, 1997, just short of his eighty-fourth birthday.

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 Willa K. Baum, Division Head, and the administrative direction of Charles B. Faulhaber, James D. Hart Director of The Bancroft Library, University of California Berkeley.

Sally Smith Hughes
Germaine LaBerge

January 1999
Regional Oral History Office
The Bancroft Library
University of California, Berkeley

Regional Oral History Office
Room 486 The Bancroft Library

University of California
Berkeley, California 94720

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Kenneth Sanborn Pitzer

Date of birth Jan. 6, 1914 Birthplace Pomona, CA

Father's full name Russell Kelly Pitzer

Occupation Lawyer - Farmer - Businessman Birthplace Mills County, Iowa

Mother's full name Flora (Anna) Sanborn (Pitzer)

Occupation Teacher - Housewife - Mother Birthplace Windsor, Missouri

Your spouse Jean (Elizabeth) Mosher (Pitzer)

Occupation Housewife - Mother - archeologist Birthplace Pomona, CA

Your children Ann Elizabeth Pitzer, Russell Mosher Pitzer, John Sanborn Pitzer.

Where did you grow up? Pomona, CA

Present community Berkeley (Kensington), CA

Education B.S. 1935 Calif. Institute of Technology
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Other interests or activities Sail boats including designing and building; sailing including via charters in many locations; mountain hiking and other travel in many areas.

Organizations in which you are active National Academy of Sciences; Pitzer College (Trustee); American Chemical Society (Trustee),

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

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Date of birth Sept 2, 1914 Birthplace Pomona Calif

Father's full name John Alba Mosher

Occupation teacher, principal Birthplace Kansas (Lyon Co?)

Mother's full name Merle Amelia Zander Mosher

Occupation teacher Birthplace Neilsville, Wisc

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Organizations in which you are active

I EARLY YEARS

[Date of Interview: April 11, 1985]

Undergraduate Years at Caltech[1]

Seidel: Today is Thursday, April 11, 1985, and we are in the office of Professor Kenneth S. Pitzer in the basement of Hildebrand Hall at the University of California, Berkeley, to talk about his career in the Chemistry Department and elsewhere with reference to the development of science and technology and with some particular reference to its influence on industry.

I'd like to begin, Professor Pitzer, with your undergraduate career at the California Institute of Technology. You've said in *Current Biography Yearbook* at one point that you were greatly influenced by A. A. Noyes, who was head of the Chemistry Department. I wonder if you could be a little more precise about the nature of that influence.

Pitzer: Well, A. A. Noyes was one of the leading chemists of his day, but more than that, he was one of the fathers of Caltech in its modern form. The real initiator in the modern form was first the astronomer George Ellery Hale, and the two key people that he persuaded to come were A. A. Noyes and Robert Millikan. Millikan was basically the outside man at Caltech, and Noyes was the inside man. Neither of them took a very high-sounding title, i.e., Millikan was chairman of the executive committee or something like that, and Noyes was a member of it. Millikan ran the Physics Department, and Noyes ran the Chemistry Department. Between them they ran the rest of the institute.

This was way at the end of Noyes's career and his administrative responsibilities were dropping off, and he decided to take a real interest in undergraduates, including even freshmen, with the idea of encouraging them to go into research at an early stage. I had a very interesting experience in this

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[1]Robert Seidel, a science historian at The Bancroft Library, began an oral history with Dr. Pitzer in 1985, which was not completed at that time. The project was reopened in 1996.

connection. It started in the final quarter of my freshman year, and then continued more actively through a good part of the summer between my freshman and sophomore years. And then it sort of tapered off into the next year.

Noyes had a quite elaborate house, much too big for him--he was a bachelor--and his household consisted of one German housekeeper who had been with him for practically his lifetime. It was gorgeously situated right on the entrance channel to the Newport-Balboa Harbor. He had earlier persuaded the institute to buy an old commercial building or warehouse right next to his property and set up a very small, mainly marine, biological laboratory. But Noyes retained one room for a chemical laboratory.

He invited a very small group of people to come and carry on relatively simple research during the summer. By simple I mean it did not require elaborate equipment because there was neither space nor facilities for elaborate equipment. The research we did then concerned the higher oxidation states of silver. They were produced relatively simply with an ozone generator, and we were measuring the states produced, the kinetics, that is, the rates of both production and decomposition. The higher oxidation states of silver are all unstable in an aqueous environment: they oxidize water to oxygen, but slowly. Actually, I suspect the work that two or three of us did in that period is essentially still the authoritative work on higher oxidation states of silver in simple aqueous solutions.

Seidel: This was with Noyes and [James L.] Hoard?

Pitzer: Yes. The academic rules that he again no doubt influenced were that I got credit for the sophomore chemistry course for this summer and went on then to take what amounted to one of the junior chemistry courses in the sophomore year. That left time for quite a little research with Don Yost and Linus Pauling in my senior year at Caltech, and those were very interesting experiences, too.

Seidel: Before we get to that I want to ask a few more questions about these first experiments with Noyes. I've seen a letter Noyes wrote to Millikan of 1927 in which he quite explicitly makes the point that he has a few years left of life and that he could do one of two things. One would be to continue in the administration, active in the executive council; he had done a lot of fund-raising at Caltech in those years. In fact, he told the story that he was time after time on the verge of a breakthrough and would be interrupted by Millikan with another idea for fund-raising. [laughter] This was an interesting example of the consequences of his decision. You said there were a very small number of people the year that you left, but I take it this [research program] went on each summer?

Pitzer: It went on for several years. I can't really say how many. When I say it was a small group, I think he had maybe three or four freshmen or sophomores, and three or four graduate students, such as Hoard in this case. Ernest Swift, who was a professor of analytical chemistry, had a summer home inland, not right out on the bluff like Noyes had but close enough that he carried on some activity. Now when I say that Swift owned a house, I'm not sure whether he owned it or not. Some of our work was essentially analytical.

Seidel: It sounds like this was sort of an advanced quantitative analysis exercise.

Pitzer: That's what we got credit for: quantitative analysis, the sophomore course.

Seidel: Now, did you stay out there during the whole summer?

Pitzer: I had the odd situation that my father [Russell K. Pitzer] had just bought a lot and built over on Lido Isle a small summer weekend place. Although the family wasn't there except now and then, I stayed the whole summer over there and commuted three or four miles by automobile. I've forgotten; I don't know if there were some dormitory facilities or just what the other people did. I'm sure the older ones like Hoard had rented accommodations at some distance away, but there may have been a small dormitory facility for the other freshmen.

Seidel: Even though it was the Depression, your family was in relatively comfortable circumstances, I take it.

Pitzer: Yes.

Seidel: The people who might have been able to take advantage of this [opportunity] would be people more in your situation than some other?

Pitzer: I think Noyes did not invite many people, but he was quite well off. Having no children, he was quite prepared to spend what money he had. I think he simply made it feasible out of his own personal resources for maybe two other people that needed some help. In our case, I think he furnished free lunches or something like that, and I didn't need anything further.

Seidel: Now, was this in any way financed by the Carnegie monies that he got in this period?

Pitzer: It could be. If so, I wasn't aware of it; at least I don't remember it.

Seidel: I tracked those [monies] through the twenties, but I haven't tracked them through the thirties. You had a prize scholarship there, according to one of your short biographies. Can you remember which scholarship that was?

Pitzer: I don't remember that it had any further name. They had a system at the time that freshmen with top records were awarded prize scholarships. It automatically included one-half or one-third tuition.

Seidel: But it wasn't a Blacker or--

Pitzer: I don't recall it having been, no. I lived in Blacker House, but that was quite a separate decision.

Seidel: They had also given money for physics fellowships, I know.

Pitzer: I don't believe it had any individual name, and, as I say, it carried partial tuition automatically. Then if you filed financial need, you could get larger financial aid, which I didn't do. I just took whatever came automatically.

Seidel: This was based on your performance as a freshman?

Pitzer: It was initially based on entrance credentials and then carried on another year, maybe [based on] performance as a freshman. I forget now just how long it carried. There were entrance exams, as I recall.

Seidel: This was before the SAT's [Standard Aptitude Tests]?

Pitzer: Well, [they] didn't start until later. I'm sure it wasn't just based on a high school record because high school records aren't an adequate basis for that sort of thing. I think Caltech gave its own entrance exam, but they may have waived it for people at a distance with good records. We're having our fiftieth reunion this June [1985]. I haven't thought about these things for a long time. I've thought more about them in the last six months than for thirty-five years.

Seidel: You apparently started in engineering there. Were you a member of the engineering honor fraternity?

Pitzer: Your basic facts are correct, but the interpretations are somewhat wrong. The only real undergraduate scholarship honor society was Tau Beta Pi. There was no Phi Beta Kappa, and Sigma Psi, which did admit undergraduates just prior to graduation, was almost purely a graduate student-faculty organization, as it is most places. Under those circumstances, Tau Beta Pi elected the science majors, at least those that had any engineering-related courses. I'm not sure whether they elected pure mathematicians--[tape interruption] but they elected me and they elected, I recall, Bill McLean, who was a physics major who later invented the Sidewinder Missile and all sorts of other things.

Seidel: He was at the Naval Weapons Center at China Lake?

Pitzer: Yes, he was technical director at China Lake for a while. That's when he did the Sidewinder Missile. We were very close friends. I know he was elected at the same time.

Seidel: So it was like the school honor society?

Pitzer: It became at Caltech sort of the school honor society, whereas here at Berkeley Tau Beta Pi is purely an engineering affair. Non-engineers, if they're in basic science or humanities, are eligible for Phi Beta Kappa here.

Seidel: The reason I asked is there are some people like Pauling, for example, who, when they started their careers in the university, were going into engineering simply because they didn't know there were such things as chemists and physicists.

Pitzer: Well, I have even to this day a considerable affinity for engineering-type subjects, and at the time there was no chemical engineering program. Pre-chemical engineering was just an option within the chemistry program. Bill Lacey was the professor of chemical engineering at the time. I took some of his courses, not all of them, but some. It was only at the master's level that there was an official chemical engineering degree and official chemical engineering program.

Seidel: That's rooted, I guess, in Millikan's philosophy, because he proposed or promoted the program that engineering should grow out of the basic sciences. And therefore you first mastered the basic sciences, and then you became an engineer.

Pitzer: There were undergraduate, four-year engineering degrees in electrical and mechanical. Students took a heavy dose of physics in the beginning, but still they were labeled [engineering], whereas chemical engineering--and also aeronautical--appeared only at the master's level.

Seidel: I gather, though, you'd become interested in chemistry, and nothing really changed your mind.

Pitzer: I had become reasonably strongly interested in chemistry, but this first-year interaction strongly reinforced it. If it had been negative, I could easily have been turned into something else.

In addition to Noyes, I should also mention E. B. Wilson, Jr., Bright Wilson, who was later professor at Harvard. He was my freshman lab section instructor, and he was very good.

Seidel: He was the son of the Wilson from MIT-Harvard, or was there any relationship?

Pitzer: No, his father was a lawyer or something like that. There was an E. B. Wilson also in the Cambridge area. An older man.

Seidel: Yes, secretary of the National Academy of Sciences.

Pitzer: I knew him through the academy, as a matter of fact. A very interesting old man. But there's no relationship there. It's very confusing, [laughter] but if they're related, it's four generations back and a remote relationship. He [E. B. Wilson, Jr.] was very good. Arnold Beckman was involved in freshman courses then, too. He gave some of the lectures.

Seidel: I guess he left there in '38, '39?

Pitzer: Sort of a transition during World War II. He got into the instrument business during World War II and never came back, but you'd have to go into the formal records to know when he finally resigned.

Work with Pauling

Seidel: Now we move on a little bit to the later work with Pauling, which was on the crystal structure of tetramminocadmium perrhenate. I hope I got all those words right.

Pitzer: Yes.

Seidel: Now, there were several interesting things about this. One, it seems to be your first work in crystal structure.

Pitzer: In fact, my only work in crystal structure, really.

Seidel: The second thing was that it was published, unlike all your other works I've seen, in a German journal rather than an American one.

Pitzer: No, I've got others in German journals, but not many.

Seidel: Third, Pauling was funded by the Rockefeller Foundation in this project for a number of years in the thirties. Can you give me some of your recollections of that research program and how it fit into the general scheme of things in the college's early Chemistry Department?

Pitzer: As I said before, having got into an accelerated schedule, I had quite a little time, even in my junior year, but certainly a lot of time senior year for research activities. Part of this was with Don Yost. It did not, however, lead to any formal publications, as I recall.

But I was also attending some of Pauling's graduate lectures, and along the way it was proposed that I do some fairly simple crystal structure problem. Rhenium hadn't been discovered long before and became available in a significant quantity only at that time. I think it was Yost actually who got a few grams of rhenium. Pauling, I judge, noticed in the literature that this tetramminocadmium rhenate had cubic symmetry. Now cubic crystals are relatively simple to solve. So, it went through his head, I'm sure, "I'd like to give Pitzer some simple problems that he can work out in a month or two, and Yost has just got some rhenium, and here's a cubic crystal." So he proposed that with some guidance from Yost I prepare the crystal and then do the x-ray diffraction on it.

And the paper was relatively simple to write up. I'm sure he chose the journal. As I recall at that time *Acta Crystalographica* hadn't been formed yet, and *Zeitschrift fur Kristallographie* was <u>the</u> international journal of x-ray crystallography, and accepted papers in English as well as German.

Seidel: Probably Pauling knew the editor.

Pitzer: <u>Sure</u>, Pauling knew the editor. [laughter] It was a good, cozy arrangement. But there was nothing controversial about it. It was rather a nice little structure, and it was an experience very valuable to me ever since. Having done that much serious work on crystals, I'm now even to this day quite at home with the languages of crystal symmetries and all that infrastructure of theory which most chemists don't have and which puts them off. I've said many times that this whole modern era of solid-state physics could well have been essentially done by chemists as a part of chemistry, except most chemists were put off by the basic infrastructure theory of crystals and didn't want to get into it. Therefore the physicists who felt more at home with that much additional mathematical theory took the whole thing up. But the basic sort of rationale--thinking essentially of similarities and differences between compounds of different elements but of the same formula--is really of a chemical nature.

Seidel: Do you think the mathematics was the chief barrier here?

Pitzer: It's not an insurmountable barrier.

Seidel: You cross it all the time now.

Pitzer: They cross it. But even today not many chemists cross it. When I taught the general physical chemistry course, I always put in two or three weeks on this sort of thing, but I have to do it in the optional time that is allowed for other things. Even today it's not a required part of the chemist's preparation.

Seidel: When I took P[hysical] Chem here, I was struck by the fact that it was taught simply as the application of quantum mechanics to chemical structures, dealing with approximations one makes in the Schroedinger equation and that sort of thing. So basically physics is taught under the guise of physical chemistry.

Pitzer: Well, that's really what physical chemistry is. It's physics focused on chemical problems.

Seidel: I'd taken an earlier course elsewhere in which thermodynamics was the basis of physical chemistry. It began with classical thermodynamics, and only toward the end did we get quantum mechanical interpretations.

Pitzer: Well, no, this is our sequence now: there's a sophomore course, Chemistry 14, which is really the beginning of physical chemistry, and that's thermodynamics. Then the second term is quantum mechanics, and the third term is a whole array of applications, including kinetics, some statistics and so on, and I always shoehorn about two weeks of crystal work in there. But most of my colleagues don't do it. So even today most of the chemists, unless they get attracted into solid-state work as a graduate student, essentially never learn. They can read about it in a casual way, but they're never at home, feeling self-confident about going ahead and doing something with it.

Seidel: Since Pauling led you into this, I wonder to what extent you feel you were or have become privy to his other motivations. He went to Germany in the twenties as a Guggenheim fellow, learned quantum mechanics at various places, and then came back. The Rockefeller Foundation, Warren Weaver in particular, was responsible for getting him started on applying the techniques of physics and chemistry to biological problems and related problems. So chemical bond work moved in the direction of biology ever so slightly in the 1930s. Of course, the Rockefeller people take a great deal of credit for having opened the new area of research here. I wonder to what extent Pauling was following that kind of lead or to what extent he was following his own nose.

Pitzer: Pauling's a pretty strong character. He's also a social creature. He interacts with other people in a very charming, friendly way. But I would think it's about 90 percent Pauling, and that if he hadn't found money from Rockefeller, he'd have found it somewhere else in all probability. He might have been frustrated, but he probably wouldn't have been. Of course, Pauling's own Ph.D. thesis was on x-ray diffraction with Roscoe Dickinson, who was still around Caltech when I was there. And I think Pauling may have come with a little mineralogy from Oregon State. I'm not sure. He not only knew solid-state crystal theory backwards and forwards, but he could rattle off the mineralogical names of all sorts of silicates and so on.

Seidel: Of course, he's got two Nobel Prizes for completely different things. I was just interested in to what extent his establishment there was separate from, say, Noyes's work and the other work being done at the department. Did he have his own group?

Pitzer: He had his own group, but this was a reasonably cohesive department, even so.

Seidel: You didn't notice anything particular about the way the group operated that would have differentiated it from any other group there?

Pitzer: I guess that Pauling's was the biggest and most active professor's group, but it was still a research group of one of the faculty. The fact that Yost, [Howard] Lucas, or some of the other people had only half as many--mostly students, some postdocs--as Pauling did, was more a quantitative difference than a qualitative difference.

Seidel: Could his group be better funded than the others?

Pitzer: In later years, and this begins about the time I left, but in the later thirties, of course, Pauling got those early IBM machines into the crystal structure reduction process, the punched cards and card sorters, and he began to set up more of a semi-research institute.

Seidel: When you left in '35 you were still doing--

Pitzer: We were still doing crystal structures with hand-cranked calculators and slide rules.

Seidel: Those great big old forty-column hand-cranked calculators?

Pitzer: No, you didn't need that much. Just the regular size would do. Eleven columns or so were enough. It was still <u>very</u> small-scale science. You had to have, of course, the x-ray sources and photographic film development, guided visual estimates of intensities. The technology has gone through three or four generations since.

Seidel: This must have made some contribution to your later studies of Baeyer's Strain Theory--this understanding that there is some analogy between crystalline structure and organic structure?

Pitzer: Yes, and as I say, this has been an influence all the time. I feel at home in going into the crystal structure literature and finding things that are relevant to something I'm working on, even though I don't go and determine another structure now. Sometimes I encourage somebody else to do it.

Seidel: Since another thing you would get into was the analysis of pucker strains, was there a carry-over, an analogy?

Pitzer: There's some, but not very much. The carry-over there was essentially just quantum mechanics.

Seidel: So you feel you got the best grounding in quantum mechanics in this particular aspect by working with Pauling?

Pitzer: With Pauling and listening to some of his lectures, and knowing Wilson and Pauling and getting that Pauling-Wilson book when it was hot off the press, I developed a reasonable confidence in my self to use quantum mechanics about '35 and '36, when I was first here.

Seidel: At Caltech did you also take courses from any of the theorists who were there, William Houston, for example?

Pitzer: Yes, yes. I should mention that. I took Houston's introduction to mathematical physics, and this was very valuable.

Seidel: Willie Fowler tells me the only real theoretical physicist at Caltech in the 1930s was Robert Oppenheimer.

Pitzer: But that isn't what was relevant to a young physical chemist.

Seidel: That's just a preface to the question: did you do any work with him there at all?

Pitzer: No, you see, Oppie was only there in the spring. The Berkeley calendar then, as now, finished in May. He would leave here a few weeks before the end of the Berkeley calendar and be there for most of the spring term in Pasadena. I knew who he was, but that's all I knew at the time. I think Willie's a bit nostalgic about that.

Seidel: One of his avid followers.

Pitzer: Houston was a remarkable expositor of reasonably well-established mathematical physics of that time. I'm sure that course was designed with a strong Millikan influence, as we were saying before, of providing stronger theoretical foundations for engineers and others who used physics. And I think it was really designed, more than anything else, for, say, electrical and aeronautical engineers to give them another strong year of physics after the usual freshman-sophomore sequence. They used it as the junior year in the physics undergraduate major, and it was a large class, actually. I guess I took it as a senior; I don't think I took it as a junior. At least half the class were new graduate students in electrical and aeronautical engineering and other fields.

Seidel: So it really wasn't a quantum mechanics course.

Pitzer: No, but it had some quantum mechanics in it.

Seidel: So the Harry Bateman influence was there?

Pitzer: Yes.

Seidel: I mean, one of the things they were concerned about making possible through this course was the application to questions like hydrodynamics and aerodynamics.

Pitzer: Yes, but it didn't go very far into any of these applications. It was pretty much the bare-bones fundamental physics presented in a very clear and concise fashion. For example, as compared to the physics program here, in one quarter you got really pretty well grounded in advanced methods in mechanics, whereas it's a year course here. And then next you get electricity and magnetism; you got one quarter there, in which you were reasonably capable of handling fairly advanced methods.

Then there were some statistical mechanics and some quantum mechanics and so on. I don't regard that as a major introduction to quantum mechanics. I would say that Pauling had more to do with introducing quantum mechanics to me. And I still picked up some here. In my first year here, I took Brode and Jenkins--first-graduate-year physics focused on quantum mechanics and spectroscopy, you might call it. In other words, it wasn't highly abstruse quantum mechanics; it was quantum mechanics applied to atomic and molecular problems.

Decision to Come to Berkeley

Seidel: Let's talk a little bit more about that transition before we put you all the way in the classroom here. Why had you chosen to come here? Obviously there was G. N. Lewis and Latimer and [William] Giauque and all these people to work with.

Pitzer: There was really a very close and friendly relationship between the two departments. There was very high mutual respect between Berkeley chemistry and Caltech chemistry at those times.

Seidel: I know you said to Ridgway[1] that there was much visiting back and forth, but I was wondering if you could give some concrete idea. Would you have regular symposia or colloquia?

Pitzer: Pauling was in the habit of spending a week here almost every year. [William C.] Bray wrote the *Rare Element Chemical Analysis* jointly with A. A. Noyes. That was back in the twenties. I think that was done mostly in Pasadena, but there must have been a good deal of going back and forth. Both departments essentially grew out of MIT. Noyes and Lewis were both out of MIT, and [Richard C.] Tolman had been here a while and then there. Bray came out of MIT.

Seidel: Well, almost every physical chemist came out of MIT in that generation.

Pitzer: So they had all manner of similarity of background. I indicated in rather positive terms that I wanted to go somewhere else [not remain at Caltech]--that I thought it was better to get broader experience and I wanted to go somewhere else. Then I sought their advice--Pauling, Yost. Yost was an undergraduate here, by the way, and worked quite closely with Bray in some research even as an undergraduate here.

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[1]An interview with Kenneth Pitzer by David Ridgway, reprinted from Journal of Chemical Education, Vol. 52, p. 219, April 1975, is in the Appendix.

Yost had a very close tie with Bray. I remember years later his father appeared here and wanted to talk to G. N. Lewis. I was the one who happened to be standing around and guessed that this was Father Yost. [laughter] And so I made the arrangements to get him in to see Lewis.

There were a lot of ties, and the net result was that I investigated Harvard and Princeton and Berkeley. Those were the only three I really investigated. Harvard didn't seem to be very cordial. I was going to be married, and Princeton was highly prejudicial against married graduate students; that came through clearly. Everybody at Caltech was enthusiastic about Berkeley, so I just chose it without any further thought, really.

Seidel: Now, at Harvard you would have been going to work with [James B.] Conant, would you?

Pitzer: They didn't have anywhere near as close ties. It wasn't anywhere near as apparent who I would be working with. Harvard's Harvard, you know. You don't ignore Harvard.

Seidel: Who was at Princeton that you knew?

Pitzer: Of course, Henry Eyring was there and was getting into these things. I would say Eyring was the principal positive attraction. Hugh Taylor, of course, ran the situation at Princeton. It was Hugh Taylor who was highly prejudiced against any married graduate students.

Seidel: I wrote a dissertation on the development of physics research in California. There is a strong community between Caltech and Berkeley from the twenties onward. Of course Pauling's story was one of Lewis and Noyes coming to some disagreement about Pauling. Pauling in his interview with the American Institute of Physics found out later that Noyes had pulled a fast one on him in moving to Caltech.

Pitzer: You know, we could expand that a little further. Pauling applied to come here as a graduate student. Lewis was slow about giving him an offer. One result of that a few years later was that Lewis delegated the selection of graduate students to Latimer so they wouldn't miss any [laughter] prizes like that--by his not wanting to give it such active attention.

Seidel: When did Pauling do his undergraduate work?

Pitzer: In '22 or something like that. I think the story is genuine. I think I heard it from both of them. I know that he applied here and that he might well have come if a prompt and enthusiastic offer had been made.

Seidel: Yes, I think he does refer to the fact in his interview with the AIP that he had applied to both schools, and he got a quick response from Noyes at Caltech and therefore went there. At this point he was still aiming at engineering, of course, so for that reason maybe Caltech looked a little more attractive. He didn't become a chemist, I'm quite convinced, until late in his education.

Pitzer: Yes.

Wendell Latimer

Seidel: That leads me into the next question. Your Ph.D. committee was Latimer, [Ermon] Eastman, [Paul] Kirk, [David] Greenberg, and [Joel] Hildebrand. I was curious about Latimer because he appears, as you just indicated, to have been on the fast track to replace Lewis as chair. I know he had a very ambitious program in physical chemistry here. Yet he was not from this tradition, the MIT tradition certainly. I think he'd come from Kansas.

Pitzer: He'd come from Kansas, yes. And he got his degree here. So he's in the next generation within the tradition, though.

Seidel: What kind of person was Latimer? Could you sketch his character and his kind of work?

Pitzer: As Latimer himself said, he was a person who was very much interested in people and affairs generally. I'm sure he would have been a success as a banker or a lawyer or a business executive or in almost any field of endeavor which involved the combination of sharp, critical thinking, high intelligence, and good human relations. The net result was that in departmental affairs he was always interested in not just the science but also the human relations, the organizational aspects that would be necessary or desirable in order to facilitate it.

Seidel: Of course, G. N. Lewis--

Pitzer: Lewis' personal predilection was quite the contrary. In other words, he was a friendly enough individual, although a bit reserved, but his real interest was in the science of the question. All these human affairs and organizational matters were a nuisance that had to be taken care of in order to make the whole operation go, but he didn't want to spend any more time on them than he had to.

The net result was that when he found that Latimer rather enjoyed this sort of thing, he would delegate more and more in an informal way. Latimer never had any official title in this regard as far as I know, until later, of course, when he was dean. He was doing some things in a very informal fashion. Mrs. Mabel Kittredge Wilson had long been a secretary or administrative aide and she handled an awful lot of this organizational matter. I'm sure she had Lewis' instructions: "If you want to act on anything of this sort, go get Latimer's opinion, and then do whatever Latimer tells you to do." And that's what she did.

Now Latimer had quite an active program at the time I came--heat capacity measurements in relation to the Third Law of Thermodynamics and in terms of chemical thermodynamics of various inorganic species--ions, solids, or the like--with an underlying quantum-statistical mechanical guiding theory. These systems were too complicated to treat in any rigorous, detailed fashion, but you still had this underlying theory to guide your empiricism in the area. This seemed interesting to me, so I chose to do that.

Latimer was very much inclined to give his students general guidance and support but a minimum of supervision. That was fine with me. I was perfectly willing to make my own decisions. I was physically located right next to Giauque's laboratory, and Giauque's office was right in the lab, essentially. So in many respects I got as much advice from Giauque as I did from Latimer by just knowing that he had the answer and he was right there. And I was brash enough to go in.

Seidel: I gather Giauque was a very shy man. The reason I say that is that there were famous Lewis research conferences that went on and on. [Giauque] said, "I went to one and I was so scared I never went back again."

Pitzer: He got over that pretty fast. [laughter] He was, I would say, reserved, rather than shy. In other words, he wasn't just comfortable meeting lots of people the way Latimer was. But at least once one convinced him by some example that you were worth talking to, he was really, I found, very easy to deal with. Both Lewis and Giauque were people who enjoyed a sort of scientific jousting in a friendly way. And if you folded your tent, as it were, and withdrew, they weren't much interested in you thereafter. Whereas if you stood up, maintained your side of the argument with good foundation, you became a respected member of the community and somebody that they welcomed.

Seidel: I gather that mode of interaction had been quite common at Caltech--I mean that in the Chemistry Department there was this free and open interchange between faculty and students. Did you find much the same atmosphere here?

Pitzer: Yes, much the same. Oh, yes.

Seidel: So there weren't a lot of people folding their tents and withdrawing.

Pitzer: No, no, although there were some cases of that sort. But I'm thinking of this not so much with respect to one's own research director, where there was a sense of responsibility and willingness to be more sympathetic. I was Latimer's student. There was no problem there anyway. But I was able to develop that sort of relationship with Lewis and Giauque relatively early on, whereas most people who were not their students I don't think ever did, at least at the graduate student level.

Seidel: Now your relation to Lewis was not in the sense of doing research with him.

Pitzer: No, this was really mainly after I was on the faculty.

Seidel: He was then doing color theory?

Pitzer: Yes, and the triplet state. One of my arguments with him was that we ought to have a quantum mechanics course in chemistry, which after two or three years we finally got.

Seidel: Why did he resist that?

Pitzer: Well, he wanted to keep it as an undergraduate course. He believed almost as a religion that there ought not to be any formal classroom courses labeled graduate courses. I think he was just trying to keep the amount of time that graduate students spent in formal instruction to a minimum. And if they had to take courses that were labeled undergraduate honors courses, then there wouldn't be many of them that they hadn't already taken. They would free themselves from the guidance of a formal course and start really working as independent scientists sooner. It was a good basic philosophy. If it had been carried to the extreme, it would have been overdone. But these pseudo-graduate courses existed, including the one in quantum mechanics that Bill

[Willard] Libby and I started the first year, and then I taught for several years thereafter. Many, many years thereafter. It was molecular quantum mechanics, if you wish, in chemistry.

Seidel: That was the basis of your book,[1] wasn't it?

Pitzer: That was the basis of the book, yes.

Seidel: To what extent would the chemistry student have in that time gone to the Physics Department and taken courses in quantum mechanics?

Pitzer: Oh, one did. And this was part of Lewis' plan, I think--by essentially forcing the physical chemistry student to go get some quantum mechanics in physics, he would get the physicist's general point of view on things. In addition, it would broaden his education, and then the chemical aspects could be brought out in seminars here. I argued, particularly after Pauling's book[2] came out--of course the Eyring, Walter, and Kimball book[3] came along not too long thereafter--that there was enough development and application of quantum mechanics of major interest to chemistry that wasn't being presented in physics courses, or at least wasn't being presented efficiently to a chemist in physics courses, that we ought to have our own. Lewis agreed quite gracefully eventually, but he resisted it for a while.

Seidel: It's been said of this period in the University of California that one could not distinguish chemistry and physics except that one went on in Gilman [Hall] and one went on in LeConte [Hall]. I think that's one of [Raymond] Birge's exaggerations.

Pitzer: Yes, I've probably said things like that too. [laughter]

Seidel: I'm impressed with the fact that there did continue to be distinctions between the departments, and that there were arrangements like [Glenn T.] Seaborg's where he worked closely with [Ernest] Lawrence, but these were not as natural as one might think--they required some arranging. And there still seems to have been throughout the period of the thirties a clear distinction between them.

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[1]Kenneth Pitzer, Quantum Chemistry (New York: Prentice-Hall, 1953).

[2]Linus Pauling and E. Bright Wilson, Introduction to Quantum Mechanics with Applications to Chemistry (New York: McGraw-Hill, 1935).

[3]Henry Eyring, John Walter, and George E. Kimball, Quantum Chemistry (New York: J. Wiley & Sons, Inc., 1944).

Pitzer: There was a reasonably clear distinction, but there was no problem in arranging joint activities. I've recalled one thing because they're getting together this *Festschrift* for Luis Alvarez. Luis and I did a neutron-scattering experiment on ortho- and parahydrogen in about '38. This was very easily done, that is, I prepared liquid and gaseous parahydrogen. We were scattering off of gaseous hydrogen and we needed para- as well as normal, which is three-fourths ortho-. And then we needed liquid hydrogen as coolant to get the temperature down. We used liquid air in those days rather than nitrogen as a subsidiary coolant. I had a scattering chamber made in the chemistry shop out of general chemistry funds. Of course, the neutrons came from cyclotrons. Luis put all the counting apparatus together. The whole thing was done with no formal arrangements at all.

Seidel: So there continued to be good and close connections between the departments.

Pitzer: Of course, the Seaborg aspect of that connection tended to be the more dominant--Libby to some degree, too, and Sam Ruben until his death.

Seidel: One of the connections in your Ph.D. committee would be Greenberg, who was working in the Rad[iation] Lab. How did he get on your committee?

Pitzer: I don't have any idea. I don't remember him, really. He was just an "outside" member.

Problem of Internal Rotation

Seidel: To get to your own work in this period, you said in the interview with Ridgway that the problem of internal rotation about the single bond in ethane was a great puzzle at the time you entered graduate school.

Pitzer: There was a controversy in the literature. That statement in the Ridgway interview may have overstated it a little bit, but it was a significant controversy. Chemists through the years had said there was free rotation about single bonds without knowing just how free "free" needed to be. There were several papers calculating thermodynamic properties for hydrocarbons on the assumption of completely free rotation that led to disagreements with entropies measured by low-temperature heat capacities and the Third Law [of Thermodynamics].

Seidel: Is this a Latimer experimental observation you're talking about?

Pitzer: It's not Latimer at all. I just happened to work for Latimer as far as this was concerned. He didn't know anything about this problem. It did have a locus in Berkeley in the Giauque lab, but Giauque himself wasn't interested in it either, or not much.

Seidel: So how did you become aware of this?

Pitzer: Well, I'll tell you in a minute. Let me fill in the general background, and then I'll fill in the local background. In the published literature, there were these papers by physical chemists--statistical mechanicians--using free rotation as part of their assumption and getting answers that were in disagreement with the experiment. For ethane itself there was, I think, one calculation in the literature--interestingly by Edward Teller, still in Europe at the time--saying that a barrier of the order of three kilocalories per mole would bring the ethane information into agreement. But he didn't have the low-temperature Third Law entropy of ethane; he had some other data, primarily that for the reaction hydrogen plus ethylene to form ethane.

Now we come to the local scene. A man by the name of [Ralph] Witt had come very early in the thirties, maybe '29 or '30, as a National Research Council fellow, postdoctoral from Hopkins, I believe, to work with Giauque. Giauque never did understand why he insisted on measuring ethane. But he did. Giauque had no objection to his measuring ethane; he'd come with his own funds and so forth, and it was within the capacity and facilities of the lab. Giauque assigned a new graduate student by the name of J. D. Kemp, who just needed to learn the ins and outs of the lab, and they measured the low-temperature heat capacity of ethane. Then Witt left and became a chemical engineer elsewhere--I think it was back at Hopkins, but I'm not sure. Kemp measured something else for his thesis; it was nitrogen oxides as I recall.

But he had this investment in these measurements on ethane which Witt didn't seem to show much interest in. Then I arrived on the scene claiming to know something about quantum mechanics. [laughter] So Kemp interested me in the problem, and I got into the literature and saw that the conflict was not just for ethane. It was for two or three other light hydrocarbons, and it became enormous for tetramethylmethane, where you've got four such rotations in a still fairly small molecule. I looked up the spectroscopy and the internal rotation quantum mechanics, which had been worked out reasonably well by a spectroscopist-physicist named [Harald] Nielson at Ohio State.

I said, "I don't see anything too difficult about this problem. Why don't we go ahead and solve the statistical mechanics for the restricted rotation?" Which we guessed Teller had done but he didn't give any detail. He just made sort of a statement that these data could be resolved. But he didn't have the data that were really critical to the problem. And so we went ahead and did it. I did most of the statistical mechanics, but Kemp knew what was going on.

We first published a "Letter" in the *Journal of Chemical Physics*[1] on the result. Later, there were two full papers: one was a Witt-Kemp paper on the experimental Third Law measurements,[2] and then Kemp and I wrote a paper on fitting the data with a potential barrier of around three kilocalories.[3] We brought into the picture the gaseous heat capacity data which Teller had also talked about, but there were uncertainties of spectral assignments and other things there. So that was nowhere near as clear-cut or definitive as these new Third Law entropy measurements that had come out of the lab here. So with that start, I thought, Well, why don't I redo these other statistical thermodynamic calculations that someone else had made on the free rotation assumption and put in a potential barrier of the order of three kilocalories and see if we can't fit the rest of these data? Which proved to be possible.

Seidel: So you became a true believer at this point.

Pitzer: I was a true believer earlier. I began to convince some other people to get into this whole area of statistical thermodynamics, which involves spectroscopy, particularly for the larger molecules. You've got to get the conventional vibrational frequencies in addition to these internal rotation modes. In

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[1]The Journal of Chemical Physics, Vol. 4, No. 11, 749, November 1936.

[2]Witt and Kemp, Journal of the American Chemical Society, 59, 273 (1937).

[3]J. D. Kemp and Kenneth S. Pitzer, "The Entropy of Ethane and the Third Law of Thermodynamics. Hindred Rotation of Methyl Groups," Journal of the American Chemical Society, 59, 276 (1937).

ethane for the entropy, the internal rotation was the only really important variable left, but for the heavier molecules or even for the gas-heat capacity of ethane, you had other vibrational modes where the spectroscopy was usually incomplete. You had to do some work there, too, or at least make some reasonable assumptions.

Chemistry and Physics

Seidel: To look just a bit more at the prewar period, you became an instructor in '37 and an assistant professor in '39.

Pitzer: Yes, with a little encouragement from a Caltech competitive offer. [laughter]

Seidel: Obviously you had impressed the faculty. Can you tell me how you were appointed and whether there were other roads not taken? Caltech was a goad to Lewis and all the people here.

Pitzer: Although it was never a big deal. I assume it probably accelerated things by about one year.

Seidel: You mean to assistant professor?

Pitzer: It may not have accelerated it at all because instructorships still existed then, but they were pretty short-term. As we were coming out of the Depression, it was not a promotion that caused any upheavals in the general structure.

Seidel: Would you [consider] that you were proprietor of your own group and separate from the Latimer group?

Pitzer: Yes. While Latimer and I were still cooperating on some remaining questions, the percentage of my attention on essentially Latimer-centered ideas was down at the 10 percent level within a reasonable time. The hydrocarbon and other statistical thermodynamic work, including the paper on the basic molecular expansion of corresponding states and several other papers on basic statistical thermodynamic questions, really had nothing to do with the Latimer program as such.

Seidel: We really ought to mention Richard Tolman somewhere in here since you were doing the old system of thermodynamics and had been at Caltech.

Pitzer: We never really got closely acquainted. I knew him, but I never actually attended any extensive series of lectures or full course of his. So in a sense I knew more of him than I knew him. I read a good deal of Tolman's statistical mechanics, but actually other people's statistical mechanics seemed to be more closely applicable to what I wanted to do.

Seidel: I gather your work involved more approximations than some physicists would be happy with. You said to Ridgway that a good deal of the art of this period was making those approximations in such a way that you could defend them, and yet at the same time they were possible.

Pitzer: [Peter] Debye, I suppose, was the great artist in this regard--several of his earlier papers of the teens or the twenties. But in terms of statistical mechanics there were other books that taught you the approximations and even got the fundamental equations closer to the problems you wanted to solve. Tolman was getting more and more interested in cosmology and things of this sort. [Ralph] Fowler, for example, at Cambridge, England, and then particularly the Fowler-Guggenheim book[1] which came along later, but still early enough to be useful for some of the things I was doing.

Seidel: On the previous tape you were speaking about the influence of [Gerhard] Herzberg. Where was he?

Pitzer: He came from Germany to Saskatchewan and then went to the National Research Council in Canada in Ottawa. He's been there ever since. Herzberg is the number-one molecular spectroscopist in the world. I think there's no question that he's excellent. His books are very readable and very sound. Although he usually stops with molecules somewhat simpler than I was working with, he comes closer to the range that I was interested in than anybody in physics here, so that I found Herzberg very valuable and have since gotten acquainted with him. He's a wonderful person. It's great to see him still going strong.

Seidel: This may seem somewhat simplistic, but it occurs to me from what you said that you were getting a lot of your physics from books.

Pitzer: Yes.

Seidel: As opposed from your chemistry which you were probably getting in articles. Do you think this is the standard mode for chemists?

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[1]Ralph Fowler and E. A. Guggenheim, *Statistical Thermodynamics*, Cambridge University Press, 1949.

Pitzer: I don't mean I didn't read articles in physics. But you get what you can in a more organized, more comprehensive way first. For the most part, I could work from the advanced books in physics, and then I would go from there into my own research or into the physical chemical literature. That doesn't mean there weren't some articles published in, say, *Physical Review* that were really in the same territory; I'd make full use of them. But one tended rather to go to the *Journal of Chemical Physics* or *Zeitschrift fuer Physikalische Chemie* or Faraday Society *Transactions*, one of the physical chemical journals for the next stage.

Seidel: Do you think of yourself as a theoretical chemist?

Pitzer: Not particularly. Most of us of that generation were not pure theorists. I thought of myself as probably making more theoretical contributions and fewer experimental contributions than most, but I've never been a pure theorist. I've always had some experimental program going.

Seidel: The reason I ask is twofold. First, at Caltech in the twenties, chairs of theoretical chemistry were created for Tolman and other people. And G. N. Lewis and Tolman and to some extent I guess Noyes were considered in the twenties to have been theoretical chemists. This is before the term fell out of favor--when the theoretical physicists came to be the theorists.

Pitzer: Noyes certainly never thought of himself as a theoretical chemist. He used theory, but he always had primarily an experimental program. Certainly Lewis never thought of himself that way. There's Tolman, yes. Tolman had sort of a special professorship with all sorts of titles, but that was a very unique situation. There may have been a few people doing purely theoretical work in chemistry in the thirties and early forties, but better theory was being done by people who also did experiments--Pauling, for example, and in the older generation, Lewis and so on.

Seidel: The concept of the theoretical chemist is one which emerges now and again.

Pitzer: It's emerged now, i.e., there are people here in this department that very openly have no pretenses of doing any experiments. I think I contributed some to their success in guiding them as young people, not to do experiments, but to maintain contact with experiments, so that you're doing theory on something that is really of interest to the main body of chemistry, which is experimental.

A man like Fritz Schaefer, for example, on the faculty here. When I came back first to the department in '71, I urged him very strongly, "Now, you're doing beautiful quantum mechanical calculations, but be sure you do a reasonable number of them on molecules where somebody is interested in the answer besides a few spectroscopists or other theorists." And he's done that; he's done it beautifully. I think the breadth of his reputation in chemistry as a whole depends greatly on that.

Seidel: I suppose physicists would ask you if there is such a thing as theoretical chemistry, or aren't you really talking about physics when you talk about theoretical chemistry? In your work, of course, you came along at a time when you could develop theories of sufficient generality that were unique to the interests of chemists. Here was something clearly not physics. The physicists weren't interested in this theory, but clearly it was theoretical. Do you think that's still true?

Pitzer: I think basically it's still true. Just as physical chemistry is essentially physics but applied to problems that are of particular chemical interest and of limited interest to many physicists, likewise I think theoretical chemistry is certainly physics applied to problems of particular chemical interest that therefore have not drawn very heavy attention from physicists. The only difference between theoretical chemistry and physical chemistry, as I see it, is that physical chemistry is primarily an experimental field. In the early years there was a lot of dual theory and experiment, still true to a considerable degree, whereas the theoretical chemist just doesn't do any experiments.

You can make further distinctions. You can find areas of theory applied to obviously completely chemical topics, and you can find other theories much like what we've been talking about in terms of statistical thermodynamics and spectroscopy of rather large molecules. The latter are extensions of what physicists are commonly interested in but involve somewhat more complex molecules or molecules of interest because of their chemical importance, rather than because they're illustrations of phenomena. As a physicist tends to emphasize new phenomena rather than the intercomparison of similar structures or similarities for different elements or different compounds, so a physicist might be interested in internal rotation. Once he found two or three examples, he'd probably quit, whereas a chemist, having learned how to handle the internal rotation problem, sees a multitude of other cases of chemical interest where you can use it. I carried through the first study and happened to be in chemistry. The first study could easily have been done in physics as an initial example of internal rotation. Indeed, research on internal rotation was done by [Harold] Neilson who was in physics. Teller was in physics. They'd already looked at the internal rotation, but they hadn't carried it as far as I did.

Seidel: I suppose one could say that physicists are interested in the simplicity of the world and chemists are interested in the complex view.

Pitzer: That's a good restatement of what I was saying.

World War II

Seidel: We talked of World War II coming along. I don't know what precisely you did during the war here in terms of Latimer's high-temperature chemistry group.

Pitzer: I just knew about that, but I was with Latimer again. He had a program on gas-flow properties. It was essentially micrometeorology--smoke-flow patterns and gas flows which could be toxic poison gases, or, as it were, crowd control gases--

Seidel: What did this relate to? Would it relate to the military?

Pitzer: Yes, it was military.

Seidel: So they were thinking it might come back and they needed to get some research started.

Pitzer: Yes, I think we had to do it. In other words, chemical warfare/gas warfare was a substantial piece of World War I. Lewis and Hildebrand were over there. One had to be prepared to deal with it again.

Seidel: Was this an OSRD [Office of Scientific Research and Development] contract?

Pitzer: Yes. I've forgotten the exact organizational pattern, but Latimer was on one of the central committees for this. Yost was involved with it at Caltech, too.

Seidel: This was straight through the College of Chemistry, not through the Rad Lab?

Pitzer: Yes, this was straight through the College of Chemistry. I got involved with this in terms of micrometeorology. We arranged to get a field up in the Yolo Bypass, just west of Sacramento where there is a long causeway. The owner was an old Spanish Mexican descendant. We got in contact with the UC Davis people to arrange this. One of the Davis agricultural faculty arranged this with the Pena family. We took over this field in the spring.

That area floods during the winter if we've had lots of rain. And then it dries up, and unless they do something more intensive with it, they just run cattle on it, and the cattle eat the grass. So we arranged to get this area after the water receded. The owner agreed to keep his cattle somewhere else to give us some time to do some gas-flow experiments. Actually, I eventually did some theory of the fluid dynamics of gas-cloud movement, too. There's one minor publication that eventually came out of this. It's rather interesting.

The conditions were extreme in late May or June up there. With the grass cover after a very hot and sunny day, you get what is called a temperature inversion, thermal inversion. As long as the sun is up, there's turbulence. After the sun goes down in the early evening, the solid surface of the earth cools by radiation through a relatively dry atmosphere, so you get an inversion condition with low temperature below.

But it turns out we got more extreme conditions there than you get out in the desert, because of the grass cover, which has low heat capacity and yet very good infrared radiation emissivity. So we would get conditions in which you would get twenty degrees' difference between the grass surface and six feet up in the air. If you get sufficiently strong inversion, the net result is essentially laminar flow in the atmosphere--you get a gentle breeze. That means that the velocity at two meters is twice the velocity at one meter. And we essentially got that. This is highly atypical. In other words, I don't think I'll ever put this in the military manual, because the chances are slight that anybody ever finds it in real conditions. Dr. William Gwinn participated, and we had a crew of mostly undergraduate and graduate students up there, and we took a series of measurements. Then that project moved on into forest conditions; Dr. Gwinn stayed with it. Later they went down to an island off Panama for jungle conditions.

I was invited to go east, outside of Washington, to help run a laboratory to serve Division 19 of NDRC [National Defense Research Council], the final division to be set up. It was to serve the OSS, the Office of Strategic Services, in a sense the predecessor of the CIA [Central Intelligence Agency]. This was a laboratory concerned with devices primarily for guerrilla warfare but also for intelligence. I would say about 80 percent of our work was for behind-the-lines-operations activity and only about 20 percent for intelligence. We worked on time-delay devices, devices the operator could put in the oil spout of a truck to prevent the truck from operating. Almost everything had a time delay to it, so that the operator could get away. How to wreck a train, and all sorts of nasty things like that.

This was largely in cooperation with the British, because in the European theater the British had all the operations. They welcomed better devices and further supply. We had operations in the Far East in connection with the Chinese, so it was not entirely for the British.

Seidel: This was NDRC, i.e., the Vannevar Bush-[James B.] Conant committee?

Pitzer: Yes.

They recruited Thorfin Hogness, who was a Berkeley Ph.D. from the early twenties and then on the Chicago faculty, to be the technical director of this operation. They got a big engineering firm, Ford, Bacon, and Davis, to take a contract and provide management, accounting, engineering services for the operation. And Hogness recruited me. He was quite a good friend of Latimer's, and I suspect there was a Latimer connection there, although I'm not absolutely sure of that. I probably knew at the time. In any case it is plausible. There also could have been other connections. I was sufficiently known by then. It was not unreasonable.

We moved east in '43 and were there for not more than a month when the Chicago group in the Manhattan [Engineer] District demanded that Hogness come back and help with the chemistry side of the Manhattan Project with Arthur Compton and so on. So I continued on as acting director, and later the "acting" was removed. In other words, they did not recruit any more senior person to run the relatively small lab there.

Seidel: Where was it located?

Pitzer: In the Congressional Country Club.

Seidel: Really?

Pitzer: The OSS had taken over the Congressional Country Club and then given us about half of the building for labs and offices and then a small portion of the grounds for our exclusive test area. We could have a bigger test area by arrangement with the OSS folks. Eventually we leased a twenty-acre patch of woods across the street for an additional test area.

Our explosives got a little big for the country club site. My office was the bedroom of the presidential suite. The only president to use it had been Herbert Hoover. He liked golf and apparently actually used that presidential suite. Roosevelt, of course, with his physical infirmity never used it. So it was an interesting, essentially engineering period in many respects.

Seidel: Did anything of great significance come out of this work?

Pitzer: Yes and no. By far the biggest thing of military significance, of course, was that the entire railway system of France was tied up after D-Day. The Germans, insofar as they could resupply their retreating forces, had to do so by highway transport. There are some interesting--now open, published books--accounts of how those railways were tied up. I don't say that this wouldn't have happened to a considerable degree without our work, but our time-delay devices and our various types of railroad intercepting devices were all used by British agents. So there was some operational significance there. The number of other things that really got into use was I think rather marginal. We had some other pretty good gadgets, but this was late enough in the war that I doubt their operational significance was really terribly great.

Seidel: There were about ten scientific staff there?

Pitzer: Yes, that's about right. Probably a little on the high side. Interestingly enough, there was a chap, Chinese [Lu, Jiaxi], who had been in the West for his Ph.D., and then at Caltech on a postdoctoral, when essentially everyone else went off on war-related projects. I heard about him, and I hired him. For the American operations, knowing something about China would be useful, because that's where they were going to be applied. He is now the president of the Chinese Academy of Sciences. [laughter] I see him every once in a while. I have an invitation to come over and see him in September. I'll probably get there. [I did visit China in 1984 at the invitation of President Lu, Jiaxi.]

Seidel: Do you know if this ever got written up in any of the official histories?

Pitzer: I doubt if it got much attention.

Seidel: Because of the intelligence.

Pitzer: Probably the CIA would prefer it not get much attention.

Seidel: Was Wallace Brode involved with any of this work?

Pitzer: Not significantly. The Division 19 head was Harris Chadwell, who was a Harvard organic chemistry Ph.D. and a close associate of Conant's. In my opinion, a perfectly honorable but not a terribly bright man. When the CIA later decided to go into operations as well as intelligence, they pulled Chadwell in. I guess Brode was probably brought in still later.

Seidel: Yes. Right after the war he was at Naval Weapons Center.

Pitzer: The CIA didn't do this. It was several years after the war before they did this. They were purely intelligence. Initially they decided that this type of operations was not on their schedule, but several years later they took it up.

Seidel: I'm glad you told that story. I was getting very concerned because I'd fancied myself knowledgeable about OSRD. Maryland Research Laboratory--I couldn't find it in Baxter's *History* or in any of the official histories. And then I thought, Well, maybe he took a long sabbatical. [laughter]

Pitzer: No, as a matter of fact, I don't know if the reports have been declassified.

Seidel: Do you have a clearance? Maybe I should put the pause button on.

[tape interruption]

Seidel: I gather this added to your repertoire some of the elements that may be called upon in working in both government and sensitive positions, that is, you were cleared and met some of the people who were going to be active in the postwar period.

Pitzer: Of course, as we all knew, in order to be really effective you had to have informal communication channels as well as formal ones. I'd been far enough into this gas and smoke-flow business that I knew people like W. Albert Noyes, Jr.--no relation to A. A.

Seidel: From Illinois?

Pitzer: Well, his father was from Illinois. He was at Rochester; eventually went to Texas. W. A. Noyes, Sr., was W. A. Jr.'s father.

Seidel: I see.

Pitzer: That's the family connection. But the younger one I knew very well. I think he was head of Division 8 [of OSRD] or whatever division had the gas business that Latimer was involved with. So I had contacts there.

I'll tell you, if you don't mind, one more story about that period. Stan Lovell was the chief science technician for OSS itself. He was a New Englander. He got the idea that the army's hand grenades were too heavy, and that with good explosives and fragmentation design, a lighter hand grenade would be much better. And while the OSS could only use a few thousand of them, once he got it developed, why maybe the army would take a few million. He wanted it patterned on the baseball because that's what American boys knew how to throw. [laughter]

So he asked us to develop a baseball hand grenade. And we did. We knew that the baseball was too light. So we ran tests. Got some young fellows out to see how much heavier it could be and still be thrown almost as far and almost as accurately as a baseball. We almost got a step function there: we could make it twice as heavy as a baseball, but still much lighter than the army's hand grenade.

Then we designed the fuse and safety mechanism and got Eastman Kodak to make them. I made a number of overnight trips on the rather interesting rail connection between Washington and Rochester. Eventually, army ordnance said, "If it wasn't invented here, we don't want it." Some little accident would occur. Somebody would fail to replace the safety on something, and it would blow up accidentally. That sort of thing always happens. If the agency doesn't want the product anyway, that's an excuse for not going any further with it.

Seidel: Usually in those OSRD-army relations you had to have somebody who was willing to push and try hard, an idea sponsor to take over and push into the services, and you didn't have that.

Pitzer: So OSS accepted the pilot production from Kodak, and no doubt the Chinese threw them at a few Japanese or something like that.

Atomic Energy Commission

Seidel: Back in '49 you were asked to become director of the Division of Research at the Atomic Energy Commission. Was [James] Fisk your immediate predecessor?

Pitzer: Yes, although Ralph Johnson was interim director in between. In other words, Fisk was the first director briefly and had been gone a short period of time. He may have still been there when they approached me, but I didn't actually take over directly from him.

Seidel: Apparently Lawrence was involved in some way. I've seen a letter from two fellows who came out to recruit you.

Pitzer: Carroll Wilson was general manager and the vice chairman of the commission. Sumner Pike, who was from Maine--they came. They arrived the day after the '48 election. And if you remember, [Harry] Truman's reelection in '48 was a surprise. Sumner Pike was a Republican and was so shocked by this that we sat and talked half the morning about this election. Finally I said, "Gentlemen, did you really just come to talk to me about the election?" [laughter] Whereupon Pike shut up and Carroll Wilson got down to business of offering me the position of director of research, with the commissioner's endorsement.

Ernest O. Lawrence and the AEC

Seidel: So you don't know anything about any negotiations with Lawrence?

Pitzer: Lawrence was involved. Albert Noyes, Jr., was also consulted, I'm sure.

Seidel: Had Lawrence approached you?

Pitzer: Yes, Lawrence had approached me.

Seidel: So you knew you were going to get this offer.

Pitzer: Yes, I knew what was up.

Seidel: One of the problems Lawrence had in early 1948 was getting the bevatron here. There was a series of negotiations in which Fisk was involved, and Fisk was opposed to the idea of giving these big accelerators to the university instead of to the [national] laboratories. He felt everyone should work with the equipment they already had. So my assumption from that is that relationships between Lawrence and Fisk could have been better. Possibly Lawrence wanted someone he knew better to be in that position.

Pitzer: I have no reason to doubt that Lawrence would have pushed in that direction; that he would have anywhere near a controlling voice is, I think, quite doubtful. I should also add that, although I'd been very friendly with Lawrence through the years, I'd never been really close to Lawrence. When I came back from this Maryland Research Lab activity in late '44, Ernest wanted me to help untangle the chemical problems he was having with the electromagnetic separation process on U_{235} and talked about those who were already working on it. I said, "I don't see that I can add enough to their progress. I've already broken up my scientific life with two different war research projects, the gas-flow thing and then the one in Washington. The nuclear project is succeeding and you've got good talent on this." So I declined. He was disappointed, but obviously didn't hold it against me because he was certainly encouraging four years later in this.

I suspect he would have thought of me as somebody that he might well be able to encourage others to support in this regard. In other words, someone like [Glenn] Seaborg or [Luis] Alvarez would have been too close to him, would have been too much a Lawrence protege, whereas I was really independent from Lawrence and yet certainly understanding of his point of view and sympathetic to him.

Carroll Wilson was quite a strong person, who had a strong delegation of authority from the commission to recruit on a national basis people of top talent. They felt that this is a new field to which young people adapt more readily than older people. So they, in general, recruited relatively young people who had by that time established real attainments but were younger than you might have ordinarily recruited for that level of position in some established line of activity. I know they consulted W. A. Noyes, Jr., and, I'm sure, a number of other people with no Berkeley connections or at least no significant Berkeley connections in making the choice.

Seidel: One speculation in the press at the time was the fact that really the problems that the AEC was beginning to face were now chemical, rather than strictly physical. Therefore a chemist was appropriate for this position. Was there discussion of this issue when you were recruited?

Pitzer: Yes, it was understood that a lot of their problems related to materials and chemistry and so on. The people already in the leading positions in the labs were almost all physicists, and some broadening of this sort was good strategy.

Seidel: Why was it attractive to you?

Pitzer: It was clearly a position where one would have a real impact on the future of government science, national and international affairs, and so on--writing on practically a clean slate. In other words, you would not have to be fighting entrenched interests and long-established habits in order to change anything or get anything new done.

Seidel: Did you find that to be true?

Pitzer: To a considerable extent, yes.

A Question of Morale

Seidel: In what is usually called at that point the heritage of the AEC were the labs which were very powerful and continue to be powerful in the DOE [Department of Energy] in terms of policy. The other legacy is the government scientific establishment, the stodgy civil-servant-dominated enterprise.

Now one of the things that you must have done almost immediately was to prepare and testify before the Joint Committee because everyone in the AEC was preparing to testify in the mismanagement hearings.

Pitzer: That was a little while yet.

Seidel: Yes, summer of '49. But in your testimony there, you remarked that at the end of the war the transfer of the program from the Manhattan Engineer District to the AEC caused "instability and loss of morale, through no fault of anyone. Although I was not immediately associated with the Atomic Energy Program, like most American scientists I was well aware of these difficulties."

One of the sources of the instability was simply that people were not content to continue working for the government once the emergency was over. They knew there was going to be some sort of atomic energy commission, but nobody knew whether it would be dominated by the military or not.

Pitzer: Of course, by the time I was involved, the AEC had been established, but the roles and operations of the major labs were still very much in flux. To give you one example: the General Advisory Committee made a recommendation that all work on civilian nuclear power be centered at the Argonne [National] Laboratory near Chicago. If Oak Ridge [Tennessee] hadn't been badly enough upset anyway, they were shattered by this, because they actually had a better array of skills for the full range of problems that needed to be dealt with than Argonne had. In the actual physics of reactor design, Argonne may well have been a little better. But in terms of the chemical and materials engineering and all the rest of the tasks that had to be done, Oak Ridge was probably stronger.

Of course, in the end both were brought together on various projects on a team project basis; Argonne was strengthened in some respects, and Oak Ridge was given the central role in others. But this had to be worked out. Oak Ridge had to be encouraged that they would have a useful role with self-respect and understanding in the longer run. But they also had to be encouraged very strongly to put their own house in order and to insist that, if people were really dissatisfied they leave, and if they wanted to stay, they find the appropriate niche for the future. This included some relatively free basic research, but it couldn't be all that. And to some extent the same was true at Argonne, although there they had a stronger point of immediate departure so there was less problem of morale.

Seidel: What about Berkeley?

Pitzer: Berkeley was not really a problem in this regard, because Berkeley didn't claim to be a nuclear-reactor-type organization. It was basically a nuclear-physics, fundamental-science laboratory. By the time I got there, the idea that the bevatron would be built had been accepted. So I was not put into any severe conflict-of-interest situation. I always had to be careful about signing off on documents that sent so many dollars to Berkeley. I'd have my deputy take it directly to the general manager in order to avoid any conflict of interest on that.

But that was always feasible because there wasn't a major issue. There was only a minor issue as to whether the number of dollars should be 5 percent larger or smaller. That's the sort of thing the general manager could use his own judgment on. But there was a lot of flexibility in Washington. The AEC was not under civil service at that time, you know, and there was in the headquarters area by and large a young, relatively new and able group of people who were really very pleasant to work with. A lot of exciting questions came up.

Seidel: In relation to the loss of morale and the problems that had occurred, you said that 5,500 of the wartime scientists out of 7,100 had left after the war. Five hundred later returned, which meant that you had to recruit the balance. You said, "Although the commission had retained a valuable core of wartime scientific and technical personnel, it had been necessary to do large-scale recruiting to staff laboratories." Perhaps you want to discuss this laboratory by laboratory. Do you think that the people recruited during the postwar period were qualitatively equivalent to the people that had been lost? Or do you think there had been a decline in the quality of the labs?

Pitzer: I think there was some decline. After all, during the war you could get absolutely top people to stop what they were doing and go do something that appeared to be terribly important to the war effort. You simply don't have that level of drawing power for recruitment in peacetime, even in a relatively exciting new technological area like atomic energy. But the people who were recruited were good people, and this was basically what I would say to the Joint Committee--that these were good people. Now, for the most part, I didn't recruit many of them myself; I tried to establish the climate so that the laboratory directors and division heads in the laboratories could recruit good people.

Seidel: I gather, and this you may know from your experience in Berkeley, that the two draws in the postwar labs were: one, you could work with reactors, and this was really the only place that you could work with reactors; two, you could work with the largest accelerators that existed. That is to say, ONR [Office of Naval Research] had very vigorous accelerator programs for the university. But if you wanted to work with machines at the frontiers, the only places you could find them were at Berkeley and Brookhaven [National Laboratory, Upton, New York]. So I had taken those to be the two central elements of the draw to recruit people to the laboratory at that period. Was that the perception at the time?

Pitzer: Yes, I think that is correct. For Brookhaven and Berkeley your statement really covered the subject. In other words, those were <u>the</u> most advanced accelerators, and if you were interested in nuclear physics, there is where you could do the most advanced work in nuclear physics. For the reactors, it might be basic science. Suppose you wanted to do neutron diffraction, you did it at Oak Ridge or maybe Argonne or maybe later at Brookhaven, or else you didn't do it. But that was only a part of the picture for a place like Oak Ridge, because some of the neutron diffraction might be done by others who might come in and visit while part of it was done by the Oak Ridge staff itself.

But the reactor side was incipiently a major civilian industrial technological development. If there is going to be a peaceful nuclear power industry in the world, the way to get into that is to get involved with the reactors that existed or with the design of prototype civilian power reactors. And the place you could do that was at Argonne or Oak Ridge. That was attractive to engineering-oriented people, many of whom may have been trained as physicists or chemists or chemical engineers, but became the nuclear engineering community of the future. That was an attraction.

In the small-scale science that physical chemistry knows particularly well, there was quite an attraction in places like Brookhaven or Oak Ridge or even Berkeley, for that matter. There was a climate of sophistication in instrumentation; even though it didn't involve the bevatron or the reactor, you had sophistication in instruments that would allow you to do the best mass spectroscopy or the early work on colliding molecular beams or other areas of frontier physical investigative techniques in the chemical world. I know people who went to Oak Ridge or Brookhaven for that. They didn't really work with the reactor, but they took advantage of the instrumentation skills and sophistication and support to do pioneering work.

Seidel: I notice that you include Berkeley in this latter category of labs interesting to work in. That brought two things to mind. About 1947 [Robert] Thornton was complaining that the detector side had been particularly undeveloped here at Berkeley, especially for the physicists, and that was something they had to work on. The other is [Albert] Ghiorso's work on the channel analyzers. That was beginning to flower here after the war; there was an active effort in the Seaborg group to develop detectors. They had to have great advances on instrumentation. Your association would have been more with the Latimer work on high-temperature chemistry in the postwar period. I'm not quite sure to what extent you were involved with general chemistry.

Pitzer: Only in a limited way. Of course Leo Brewer was really the key figure there. We come back to that when we start IMRD [Inorganic Materials Research Division]. As far as I was concerned, both before and after the AEC period, I had my own research group focused initially around the ring molecule and other internal rotation problems. Later I generalized from that, but this was not particularly related to the AEC.

Seidel: In your testimony you talked about the general situation of the laboratories. On a policy level, you were the director of research of the AEC in 1949--before MTA [Materials Testing Accelerator]. What were the problems facing the Atomic Energy Commission at that time?

Pitzer: It was understood right with my appointment that the AEC should have a substantial array of smaller research projects in universities, generally on subjects that had reasonable relevance to the basic AEC objectives but independent of the major labs. I was in favor of that and saw this as one of my major purposes--to establish such a program with good traditions. It was understood that the AEC wanted it and would give high priority to the funding.

Seidel: The JCAE [Joint Committee on Atomic Energy] minutes of this period reflect a very strong concern with just that issue from the earliest date on. Why had this been so difficult to get started?

Pitzer: You would have to ask Jim Fisk, and you're going to have a hard time because he's no longer alive.

Seidel: A lot of the money also seemed to be going to the building of the labs. There were only two labs built, really: Oak Ridge and Berkeley. Argonne had to be built; Brookhaven had to be built. So you had a real problem.

Pitzer: I think it's all quite understandable. The AEC is totally new. Jim Fisk is their, as it were, number-two scientist, with Bob Bacher on the commission as number-one scientist. This is a highly scientific and technological enterprise. Decisions have to be made about weapons, about U_{235} from plutonium production, about just keeping the existing labs in some reasonable semblance of order, and about Brookhaven getting started. Fisk is essentially scientific staff to the general manager, and that was occupying most of his time. I think I'm fair to Jim. I don't think he regarded this independent, separate, university research program as terribly urgent or terribly important. In other words, I don't think it was very high on his priority scale. He brought in an applied mathematician, [H. M. MacNeille], and gave him a trivial amount of money and said, "Go ahead and get started with it." I think he let three contracts: in other words, it went nowhere for all practical purposes. So that was really the major pre-agreed-upon project.

It was understood that there was lots to be done with the major labs, that the role of scientific staff to the general manager still existed, but it was understood that there would be substantial amounts of money over a period of years for an outside program on the same order of magnitude as that for the major labs. We had to develop policies and central office staff and contracting mechanisms and so on. It was understood that the actual contracting and immediate monitoring would be done through the regional area offices, but that the scientific selection and monitoring would be done out of Washington with such assistance as one might obtain from either AEC area offices or major labs.

But I felt that it had to be kept quite independent of the major labs. Therefore budgetarially I split the Division of Research budget essentially right at the very highest level. In other words, there was so much for the major labs and so much for offsite contracts for a given year. The major labs had a much bigger amount, but not by orders of magnitude, just on the larger side. That was allocated among laboratories. Of course, the physical research component at Argonne and Oak Ridge was a relatively small part of the total lab budget there, whereas it was virtually the whole budget at Berkeley or Brookhaven. There were biology and medicine at those places, but they were specialized and small. Then there was the offsite component, which was broken down into physics, chemistry, and materials. I think physics carried with it a little mathematics. There was a three- or fourfold division there.

On the physics side initially there was Ralph Johnson, who had been interim director, and then Paul McDaniel was a physicist already on the staff. I let them take the physics immediately. Spofford English, who was a Ph.D. from here and whom I'd known as a graduate student, was already there. He took the chemistry. We handled the material science mainly by using Chipman from MIT as a very active consultant, with a relatively junior man by the name of Dave Lilly in the office in Washington (and riding the "Federal," an overnight train back and forth to Boston pretty frequently) to carry on the material science program. Fred Seitz was another consultant for materials.

Solid state physics was, as it still is, on the materials side of things rather than with nuclear physics. Very soon it became apparent to me that I had to do something further on the physics side because Ralph Johnson had left. I got advice from a number of people, and it ended up that I recruited Joseph Platt from Rochester to come in as physics branch head. He was a great strength.

Seidel: Was he one of [Lee] DuBridge's people?

Pitzer: He had been with DuBridge at the MIT lab, and at Rochester, too, but of course by this time DuBridge was at Caltech, wasn't he?

Seidel: Yes.

Pitzer: I got strong recommendations for Joe Platt from Wheeler Loomis at Illinois and probably from Fred Seitz. I talked to various GAC members, too. And he was really very good. Then I used Paul McDaniel as an immediate deputy. I brought in John Thomas, who is now the head research man for Chevron, to be number two for the chemistry branch with Spoff English and to be sort of a special projects deputy. When there was some special project we needed to handle, John Thomas was very good. So it was a relatively small staff. MacNeille, the mathematician, was around for a while. I don't think we brought in any other mathematicians after he left.

<u>Office of Naval Research</u>

Pitzer: We can turn to the ONR cyclotron program, which required a good deal of negotiating because I felt the AEC should not indefinitely fund a program on nuclear physics administered by the navy. I had a high respect for ONR. I had actually had an ONR project myself previously during the '44-'48 period.

Seidel: Who was head of the physics division at ONR then? It wasn't Isaacson was it?

Pitzer: No. The fellow who headed the cyclotron program--what was his name? He was not the head of the physics division, but he was quite a strong character who had built up the cyclotron program. Alan Waterman was still at ONR, and Manny Piore was at ONR. I got very well acquainted with them, and many of the higher-level negotiations were with Manny Piore and with Waterman. My view was that this was a good program. I thought it had been somewhat overdone with more machines of almost exactly the same type than would have been best. But that had been done. There was no point in undoing it.

Seidel: What was the justification from the point of view of the AEC? Just for training people in cyclotrons?

Pitzer: Training people in nuclear physics. Yes.

Seidel: There was no thought that you were going to get any breakthroughs here?

Pitzer: It's hard to say.

Seidel: The explanation for the bigger machines is always that the more you learn about nuclear forces, the better off you are.

Pitzer: Yes, yes.

Seidel: But with the replication of more and more smaller machines it's harder--

Pitzer: Well, that was my feeling. The 184-inch plus maybe two or three machines under ONR sponsorship or AEC sponsorship would have made sense. To have seven or eight of them, whatever the number was, seemed to have over-duplicated essentially the same generation of machines. There was no point to canceling ones that were already well under way. But we eventually negotiated a scheme whereby the AEC contribution through the navy gradually went down. The AEC took over one or two of the machines. I think we took over

Carnegie Tech. Joe Platt essentially did all this. I handled it at a higher level. But I was much more active in promoting university projects in things like high-temperature chemistry or radiation chemistry at other sites or broadening the materials science program beyond the fairly narrow sort of radiation-damage focus that it had at the time I arrived.

Seidel: That was at Oak Ridge and at Ames [Laboratory of the Atomic Energy Commission, Iowa]?

Pitzer: No, I mean at the offsite areas. I don't mean that we didn't do those things at Oak Ridge and Ames and places like that. We did. But these were areas that were small enough in scale that they could be done in universities, and the atomic energy program in various universities would build a fabric of relationship and an infrastructure of talent and so forth valuable for the AEC.

Seidel: Those words may come back to haunt you. I'm interested in feedback from materials science. One of the criticisms you hear is, why did you think you could do this thing in small universities?

Pitzer: I think my position is consistent on that. You can.

Seidel: I have difficulty getting a grip on the materials science work during this period. I've read through the GAC minutes from '48 through about '70, and it's something that recurs. It always seems to be a weak stepsister to the main programs in physics and chemistry and even biomed which has its own establishment by the time you get there.

Pitzer: Yes. Shields Warren was, although only halftime, a division director on the same organization level. He had a staff there in Washington, and he testified to the congressional committees just as I did and so on. The money for biology and medicine here and in Brookhaven and all the rest of the places came through his budget.

Seidel: I'm interested in a number of things about materials science. One, the principal rationale is reactors require the mass of materials in materials. Also, one of the things you face at the university is the university is basically divided into disciplines. Materials science is multidisciplinary in its nature. To get two people to work together from two different departments is not always as easy as it was between physics and chemistry at Berkeley. Another argument at that point might have been, why can't the labs do this? After all, they're the ones building the reactors and needing the materials. They can put together the engineers and scientists from different disciplines on project-oriented teams. How are we going to get any help from the universities? Did you hear that argument then?

Pitzer: I think I got that argument, but I think the counterargument or the other side of it is perfectly clearcut. That is, you've got to get students into these areas. Students are in universities. Insofar as it's feasible to do meaningful, substantial research in a given area of knowledge--disciplinary or multidisciplinary or interdisciplinary--you ought to be doing it in universities to get students into those specializations. Then if the more complex experiments need to be done at Argonne or Oak Ridge, a certain number of those students will go to Argonne or Oak Ridge and they'll already be materials scientists, if we're talking about materials science, with a reasonably broad and strong background.

Otherwise, if we take your scenario, it means that Oak Ridge and Argonne have to persuade physicists or chemists or metallurgists with essentially no experience in this interdisciplinary or more modern phase to come and learn the other aspects at the major lab. That is a perfectly open process, but it is not one that is very easy. It is much easier to get the student during his student status and to interest him in the new field then.

Seidel: So it began as a training aspect?

Pitzer: The smaller offsite government contracts and grants, it seems to me, all have a training aspect to them. There may be a few subjects where the training aspects may be trivial, but by and large across the areas of chemistry and physics with the NSF [National Science Foundation] contracts, the smaller AEC contracts, NIH grants and so on, it's always a combination of research and training. It seemed to me that AEC ought to have a part of this. At that time the NSF didn't exist, so that there was extra reason for the AEC to be operating at the level of smaller projects and universities. Of course, some of that activity was taken over by NSF later. But NSF came along soon enough so that was not a major matter to transfer; rather, they settled on their own activities.

Seidel: In a sense, what you have there is an argument for using existing institutional structures like the universities in coordination as far as possible with the laboratories. It seems to me that there is that tension there, which is never really resolved, when the lab director says, "Why are you getting universities to do what I can do?" Particularly Ernest Lawrence could say, "If you want a university, I've got one here in Berkeley connected to me, and I can do anything you want at the Rad Lab and have students to train." Why should you give this money to Texas A & M which doesn't have this capability?

Pitzer: Actually I had very little trouble with that argument. The labs, of course, would be glad to have more money, but there was a strong commitment backed by the commission. I made it abundantly clear that we were going to have the offsite program and that we were telling Congress that we were going to put X million dollars into the offsite program and Y million dollars--larger than X, considerably larger than X--in the major labs. That was not a differentially moveable boundary during the year. Therefore it was their job to do their thing, as it were, in the most efficient manner with their good judgment as to what was needed and that they should cooperate with these people in universities that were receiving AEC support independently with Washington authorization. That was tapping and training an array of people that they would never get in contact with otherwise. And that same argument would apply to Ernest's argument: not everybody is going to come to Berkeley, even to Berkeley as a university. If it's a nationally important program, it should have a presence in Illinois and Wisconsin and Harvard and MIT and so on.

Seidel: It seems to me the staff of AEC became much more powerful later on. The bureaucracy grew and the control of the work done in the laboratories increased. I know that your relations with the committee were not always serene, but my feeling from reading the GAC minutes is they felt very strongly in the same way, based on the fact that we have to be supportive of university research from the AEC in a strong way, not only because it is important for the AEC but because it's important for science. And there is no National Science Foundation, and therefore at least in this matter you were closely in accord with them on the level of university support.

Pitzer: We had no argument about that.

Seidel: And I gather that you plus the GAC are capable of outweighing the labs plus in-house people who oppose this sort of extramural funding. I also gather from what you say that there wasn't that much open conflict in the matter. But there seems to be an underlying tension to all this.

Pitzer: There is always tension, and my strategy was to keep that tension focused at a very clean high-level decision where if the attitude at the commission level and the Congress was different, then of course you move the boundary. But I didn't want Spoff English, the head of my chemistry section, to be buffeted between the major labs and offsite proposals as to how much of his money went one way or the other. He couldn't shift it. He could recommend something maybe, but it was understood that he could advise me as to whether Oak Ridge was doing a good job and maintain contact with chemists at Oak Ridge. He could recommend allocations of offsite grants at Rochester and Madison and so on, but he couldn't transfer money and put it over on the other side. And therefore nobody pushed him to. It was known that he couldn't do it, so nobody pushed him.

Seidel: But in those cases where there is discretion on the part of staff members in the Division of Research, there was considerable political pressure from the labs to get big pieces.

Pitzer: I think there have been times when there has been a lot of pressure, particularly when budgets were not maintaining cost-of-living increases anyway. In the time I was there, we were always getting at least modest increases even in terms of cost of living. The labs were having their own troubles in terms of maintaining staffs of good quality people.

Seidel: There is a policy by the AEC, at least in the fifties, that the national labs, including Berkeley which never called itself a national lab but is in some sense, should always have unique facilities, meaning accelerators and to some extent reactors. Now that changes with the Second Atomic Energy Commission Act. But certainly with accelerators throughout the fifties one reads in the AEC and GAC deliberations a commitment to the principle that we don't build an accelerator at Madison because that would make Argonne a weak sister and would take away from the AEC labs one element of their uniqueness. Now <u>that</u> is the kind of conflict I was thinking of within the Division of Research.

Pitzer: We had very little of that. Because the two big machines at Brookhaven and Berkeley were already agreed to in principle and were being built. The reactor at Brookhaven went way over budget, but obviously had to be finished.

Seidel: Who was the contractor for that?

Pitzer: Oh, I forget. I took the position that until the bevatron and cosmatron at Brookhaven were finished and beginning to operate that there was no reason to think about a new machine at that level. ONR, as we have already said, in my opinion had overbuilt synchrocyclotrons and we shouldn't have any more of them. My stance was, if you've got a really brilliant new idea of something quite distinct in its nature, bring it around and we'll look at it and maybe we'll build it. One thing that I recall that came up was the first controlled thermonuclear stellerator (or whatever it was called) that Lyman Spitzer from Princeton proposed. And that program did get started.

Seidel: That was right at the end of your tenure.

Pitzer: Yes, right at the end. We did start that.

Seidel: But you've forgotten another major accelerator.

Pitzer: Yes, the MTA project, it's too late to start that now. That's at least a two-hour story.

II COLLEGE OF CHEMISTRY, UNIVERSITY OF CALIFORNIA, BERKELEY

[Interview 1: May 22, 1996] ##[1]

Graduate Student, University of California, Berkeley, 1935-1937

Arrival

Hughes: Dr. Pitzer, we're going to take up the story in the year 1935, when you arrived as a graduate student at the University of California at Berkeley. Please describe the atmosphere in the department at that time.

Pitzer: Surely, I'll be glad to comment about that, and a little about my personal situation and so on. I had just been married [July 7, 1935], and after a honeymoon in the mountains in southern California, we packed up our very few belongings and drove here. We had visited in the preceding late spring, probably about Memorial Day, spring vacation, so that we knew the general situation. Also an aunt of mine, Amy Allen, was the wife of a professor of Greek, James Allen, so we had a personal basis of welcome too.

We arrived considerably before classes started. I was to be a teaching assistant, but I was interested in getting started in my own research as quickly as possible and convenient. As the [Robert] Seidel interview will have recorded, my schedule at Caltech had allowed me to engage in considerable research during my senior year there, so that I was better prepared than many new graduate students as far as getting started in professional work.

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

I found a very cordial welcome here from various faculty and students already on the scene. Remember that this was the Depression. Everyone was barely having enough money to get along with. I was fortunate family-wise that I was not on the absolute minimum, but still, one was being careful to spend only the necessary money.

The arrangements were really very informal. The department was small, relatively very cohesive, as was Caltech's. In that comment, I mean that there were no serious or major subdivisions within the department. Everyone on the faculty was sort of on an equal basis with everybody else, without regard to the subdivision of chemistry that they were in. Physical chemistry had the greatest attention and percentage of the population, but it didn't in any way dominate organic or inorganic areas.

Hughes: Were those other areas as strong?

Pitzer: Well, there was very strong work in inorganic and organic, but in the borderline areas with physical chemistry rather than way off at the far corner of those fields. In particular, Professors Gerald Branch and T. D. Stewart were in that intermediate organic area, and William Bray was an inorganic chemist of very high distinction, but again on the physical chemistry-inorganic interface. I talked to various members of the faculty, and I was already fairly familiar with some of their publications, so there was no very long getting-acquainted period.

William Giauque and Wendell Latimer

Hughes: Did you come with a research problem in mind?

Pitzer: Not with an immediate research problem in mind. I had done a short crystal structure problem with [Linus] Pauling, but x-ray diffraction crystal structure was not an activity at Berkeley--at least it was not an activity in Berkeley chemistry. It was being done in Berkeley geology, actually, but I wasn't going to go further with that. I had some background in relation to what you might call chemical equilibrium and chemical thermodynamics related to chemical processes from Noyes and other people at Caltech, so I was quite receptive to that.

I was well aware of both Professor [William] Giauque and Professor [Wendell] Latimer's work, which are quite different in detail but are in that general field. And this was the field in which Berkeley was really in a great leadership role. They had the equipment to obtain very low temperatures, which was not commonplace at that time, and experience in making measurements at sufficiently low temperatures so that the extrapolation of the absolute zero temperature was possible, and that is an important factor in chemical thermodynamics.

Hughes: Was there anybody else working in low-temperature thermodynamics in the country?

Pitzer: Oh, yes, I'm sure there was, but not many, and not necessarily in universities. I think what was then the [U.S.] Bureau of Standards--it's been now renamed--had a program. Otherwise, the major activity in that area was actually in Holland, although it was also in England and probably in Germany. Leiden in Holland was very prominent in low-temperature research, and I suspect Berkeley would have come next, at least in the side connected to physical chemistry. There are other sides of low-temperature research.

I don't really very clearly remember what proposal Professor Giauque suggested to me. I must have talked to him, and he must have suggested something, but I sensed that, while he was very friendly and students who were already working with him were quite satisfied, that he had a much closer supervision relationship with his students than Latimer did. And since I was rather self-confident already, the idea of a research director who would welcome my own ideas and support them, and give me freedom and initiative, was very appealing.

Hughes: So was it that rather than research interests that drew you to Latimer?

Pitzer: No, no, it was the combination of the two. In other words, if Latimer's research interests hadn't appealed to me, I obviously wouldn't have joined him. He was very friendly, clearly somebody who was supportive of his students. He had several students, and they seemed to be very happy with their relationship with him too. Physically, his low-temperature research students were right next door to Professor Giauque's laboratory, and Latimer's office was upstairs and down the hall. Not very far away, but not immediately there.

I soon ascertained that Latimer was much more involved with departmental and university affairs, as compared to Professor Giauque, and in fact, it became apparent that Latimer was almost an unofficial vice chairman of the department to [Gilbert N.] Lewis.

Gilbert N. Lewis

Hughes: Do you want to say a bit about Lewis?

Pitzer: Yes, I'd be glad to. I was of course very well aware of Lewis before I came. He had been interested in these same areas of research but many years earlier. His own personal interests were now in somewhat different fields, which were interesting to me but didn't particularly appeal.

Lewis took very few graduate students personally. He had one university-supported postdoctoral research associate, and frequently someone else would win a national fellowship or something like that. He had maybe two or three total, but they were mostly postdoctoral people or young faculty members visiting on sabbatical or people like that. So the question of working with him directly at the beginning graduate-student level was really not particularly on the table.

Lewis, in a reserved manner, was friendly. He knew of me at least very indirectly, because my aunt and professor of Greek uncle were personally acquainted with the Lewises. Not close friends, but they knew one another, so that I was introduced there. This was a fairly small community, as compared to what it is now. People knew one another across department lines. Particularly in the thirties, there had been very few new people coming in at the faculty level, so practically everyone knew one another very well.

Hughes: What about links with the faculty at Caltech?

Pitzer: Well, they were quite close. Bray and [A. A.] Noyes had co-authored a book in the twenties (A. A. Noyes and W. C. Bray, <u>A System of Qualitative Analysis for the Rare Elements</u>. New York: Macmillan, 1927). Pauling had visited and continued to visit [Berkeley], usually in the early fall, just as [Robert] Oppenheimer in physics [at Berkeley] visited Pasadena in the late spring. This was because of the calendars.

The Berkeley calendar at that time was, as it is now, something that started really in the late summer, broke its first semester at Christmas, and finished in the middle of May. Whereas Caltech was a quarter system starting in late September, with a third of the year before Christmas, and then two-thirds of the calendar after Christmas, extending into June. So there was an opportunity for Berkeley people to visit Caltech toward the end of the academic year, after Berkeley had closed, and vice versa for Caltech people to come to Berkeley for two or three weeks, and still be back in time for their academic year. And Pauling did that for at least a few years.

Yost, whom I had worked with at Caltech, actually got his bachelor's degree in Berkeley with quite close contact with Professor Bray, and also knowing Latimer well. I could probably think of some more cross-connections, but those are the most important ones.

As I said, Lewis was rather reserved, and therefore, I didn't get anywhere near as well acquainted with him until after I was a beginning young member of the faculty and so on. It was not that there was any difficulty; it was just his personality. A Lewis story is that he was an inveterate cigar smoker. [laughs] You virtually never saw him without a cigar, either in his hand or his mouth or lying on an ashtray right next to him.

It turned out that very early in his career, he had gone on some type of appointment in the Philippines, and whether he had this cigar-smoking habit before, I don't know, but he came back from the Philippines with this involvement with Philippine cigars, which are very inexpensive. The story is that his favorite brand, that cost one cent per cigar, stopped sales in the U.S. He raised such an objection that they agreed to reactivate their sales here, provided he guarantee the consumption of so much, [laughter] and much to their surprise, he agreed to buy that much. Whether he smoked it all or not, we don't know. But in the current nonsmoking atmosphere of the world, he would have been quite an anomaly.

Lewis took a great scientific interest in other fields, and it became very apparent in the weekly research conference that we had that he presided over that he had very incisive understanding and comments about most anything that anyone was talking about that would come before a chemistry research conference. Not that some others didn't also, but he was remarkable in the respect that he had thought about these areas many times earlier in his career, and then he just had a very bright mind.

Hughes: Were those seminars conducive to discussion at all levels? I mean by that, you as a new graduate student were welcome to chip in?

Pitzer: Well, yes and no. There was no question but that status was considered. The faculty sat around the table, and the graduate students and maybe junior faculty or postdoctoral visitors were in the rows a little ways back. But it was a small room. Most of the discussion was from around the faculty table, but other people said things from time to time. It was relatively democratic, I would say.

There was one memorable little exchange. I've forgotten who said it, but some student or junior faculty commented in a rather critical tone about something that had been said, and Lewis said, "Well, that was an impertinent comment, but it was pertinent." [laughter] And invited the person then to expand on it.

Hughes: I understand that Lewis invited debate, that science was what he was interested in, and he was pleased when people stood their ground and tried to make a point.

Pitzer: Indeed. I very soon sensed that he really enjoyed sort of jousting about the scientific questions. If you proposed something to him and he gave it a discouraging, negative type of reply, if you just withdrew, well, he wasn't much interested in you thereafter. But if you brought in counter-arguments and pursued your point, well, then his interest would perk up, not only with respect to that conversation but indefinitely in the future. He respected you, provided your arguments had a basis.

Comparisons with Caltech

Hughes: Had you learned that openness at Caltech?

Pitzer: Yes. The general atmosphere was similar. But of course, I was an undergraduate at Caltech. I remember one time I was invited to come to what was the equivalent seminar or research conference there, which I did not attend regularly. I came in, and I sat down in a chair, and they said, "Oh, no, that's Professor So-and-so's chair." [laughter] The geometry was not so obvious as it was here. That is, here there were chairs around the table that you could suspect were for the senior people, so one wasn't inclined to [sit there]. At Caltech, the chairs were not so distinguished. Somebody said, "Well, why don't you sit over there? That won't offend anybody." But on the whole, the atmosphere was really very similar.

The real difference was that the undergraduate population here was so much larger than it was at Caltech, and that meant the undergraduate teaching obligations at Berkeley were very much greater than at Caltech. There was an obligation to teach large numbers of students that were interested only in a little chemistry, not that there wasn't some of that at Caltech, but that the numbers were so much smaller that it changed the character of the undergraduate teaching.

But that was just a quantitative difference. The way things were taught even at the freshman level was essentially the same. At Caltech, there were two or three lectures a week to the whole group. Here, that had to be given maybe in two sections and in a much larger auditorium. The lectures that [Joel Henry] Hildebrand gave here were really very similar to those--it's interesting who it was, it was Arnold Beckman at Caltech for my freshman year. You recognize the name in terms of his instrument corporation [Beckman Instruments, Inc.] later?

Hughes: I do.

Pitzer: And then there was a graduate student teaching assistant, or possibly a faculty member, supervising a discussion portion with a small group of students--twenty, twenty-five, something like that--and then the graduate student teaching assistant supervised the actual laboratory time. In some respects, I think that [course] was better organized here than it was at Caltech, because here, these twenty-five students were in a separate room, but it was arranged so that the discussion could occur there too, so you could move directly from the discussion to the lab work. There, the lab space was less well arranged from that point of view, but it didn't cause any trouble, because the total numbers were smaller. It was easy to accommodate it.

Hughes: What about course content?

Pitzer: Again, very similar. Oh, that brings up another interesting Lewis point.

Courses

Pitzer: Lewis wanted no labeled graduate courses in chemistry. Now, it soon became apparent that a couple of courses that were labeled senior-level honors courses were primarily for graduate students, but he was anxious to avoid having an extended array of formal classroom courses for the graduate students. He wanted to get them into research promptly, and then insofar it was some physics or some mathematics that they were going to learn, that they'd go to [the department of] physics and go to mathematics to take the course, rather than having a pseudo-mathematics course taught in chemistry. I thought in later years he overdid that, and persuaded him that--

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Pitzer: --quantum mechanics designed for chemists was so important and so really substantially different than anything physics presented that we ought to present it in chemistry. He agreed to that, provided again it was labeled as a senior-level honors course. Then after he retired as chairman and dean, we renumbered that course and one or two others to the graduate level, still open to undergraduates if they wanted to attend and were prepared.

Hughes: Do you remember the year you first began to teach that course?

Pitzer: I don't remember it right off hand.

Hughes: Prewar?

Pitzer: Well, pre-U.S. in World War II [1942]. World War II would have been on, because Bill Libby and I were involved, as I recall, the very first year we gave it. As World War II came on, he went first on a sabbatical leave to Princeton, but then promptly ended up at Columbia University with the Manhattan District uranium isotope separation project, so that it must have been, I suppose, about '39.

Quantum Mechanics

Hughes: Now, were you the only one in the department at that stage who had a real grasp of quantum mechanics?

Pitzer: You mean among the students?

Hughes: And the faculty.

Pitzer: Oh, no. But not many were at home with it. Giauque certainly was at home with quantum mechanics; there was no question about that. He had the amount of quantum mechanics necessary in relation to the Third Law of Thermodynamics. It was generally understood that Berkeley had had a large role in that, including Lewis, way back in the teens and the early twenties and so on.

But the quantum mechanics that I used, say, in connection with internal rotation [about the single bond in ethane], in terms of actually interpreting spectroscopic data and then calculating microscopic, thermodynamic, or other problems, was not widely used in chemistry in Berkeley, except by Giauque. Now, Giauque had played a major role in that area himself, so that he was fully in command of that.

Hughes: Quantum mechanics was not used in the department because people didn't have strength in that area, or because the problems that they were interested in didn't particularly demand a quantum mechanical approach?

Pitzer: Some of both. [laughs] The first good book on quantum theory for chemists was the book of Pauling and Wilson, which was published in '35, if I remember rightly. I got a copy sort of hot off the press. I had been exposed to it at Caltech in my senior year there, so that I could read the book easily. I don't mean that I'd had the full course there, but I had been exposed to it fairly substantially.

Hughes: Don't I remember that you never took a course in quantum mechanics from Pauling?

Pitzer: The plan at Caltech was that Pauling gave, I think, three separate courses, one after another, year by year. He gave a quantum mechanics course, but he wasn't giving it in the year that I was a senior and had time to take it. He was giving a crystal structure course which I did audit. I don't think I took it for credit, but I certainly audited it.

My introduction to quantum mechanics at Caltech was at least as much in the course that was given in physics by William Houston, which was intended mainly for new graduate students, but also for senior undergraduate physicists, but at the graduate level serving a number of disciplines--chemistry, physical chemistry, and aeronautical engineering and the more theoretical sides of metallurgy, and so on. About a third of that course was quantum mechanics, and while it didn't go into the molecular side of it, which one needs for most chemistry purposes, it was a good introduction, which made it possible for me to take up the Pauling-Wilson book relatively easily.

Hughes: Was that an unusual intellectual endeavor to have as an undergraduate? Would you have found that opportunity at other departments of chemistry at that time?

Pitzer: I would suspect not at many, but probably at a few, including here. It wouldn't have been as easy here, because although Giauque could have taught it, [laughs] he didn't. Any course in physics that would have been appropriate wasn't as focused toward the chemistry applications. Well, of course, I listened to the lectures in the course that [Robert] Brode and [Francis] Jenkins gave the first year I was a graduate student, '35-'36. Some of this was by now repetitious, but it was well done and still valuable, and it added another dimension, another aspect of things that I hadn't had before.

Research Atmosphere and Facilities

Hughes: Well, in your biography of Giauque, you spoke of "the excellent research atmosphere in the College of Chemistry."[1] Have you described it adequately?

Pitzer: Well, I guess we've covered it pretty well. The openness and receptivity to communication throughout, and from graduate students right on up, it was really very conducive. But I suppose most of all, it's just that the faculty were active in research and interested in one another's research, and active and well acquainted with what was going on in the rest of the country and in the rest of the world--mainly, of course, western Europe, Great Britain--so that you knew you were in the forefront community for research and you were welcome to become a contributing member as soon as possible.

Hughes: Well, you pointed out that this was the Depression. Was funding ever a limitation?

Pitzer: Well, [laughs] yes, in a sense. But the things that we were doing in those days weren't that expensive. We had a good mechanical shop, funded just by university funds, so that mechanical things could be obtained. And even simpler, the carpentry or anything like that could be obtained. There was a good glassblower. In those days, electronics was in its infancy, and one, by and large, didn't use elaborate electronics in instrumentation. One needed to understand electricity in its more elementary sense [because] lots of our measurements were electrical. You introduced energy electrically, and then you measured temperatures with resistance thermometers, or thermocouples with electrical detecting instruments.

Giauque's laboratory, which in effect the Latimer students in that low-temperature research specialty also used, had what we called galvanometers that got sensitivity by reflecting light practically all the way across the room to a scale, so that a very small twist of a mirror detected a very small voltage difference or whatever it was. Now, one would never do it that way today.

The underlying electrical theory of these measurement devices was all relatively straightforward, so that if you had even good sophomore-level physics, let alone, say, the undergraduate level

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[1] K.S. Pitzer and D.A. Shirley. William Francis Giauque, 1895-1982. *Biographical Memoirs* [National Academy of Sciences], 1996, 3-21.

physics that I'd had at Caltech, you had the physics you needed for that.

College, not Department, of Chemistry

Hughes: Why is it the College of Chemistry?

Pitzer: Oh, that's an interesting story. Let's have a word about that.

This goes way back to the very beginnings of the University of California. There was an initial organization into a small number of colleges--Agriculture, Letters or something like that, and Engineering--and for some reason, Chemistry was chosen as one of those initial units. It was a mix of basic chemistry and applied chemistry in terms of service to the state, a little bit like the College of Agriculture had Agricultural Extension and farm advisors. Chemistry never did anything of that diffuse type, but if you go into the early history, you find that the person that, for example, checked water purity in Chico would have some question and would write a letter and would get a reply from somebody here at Berkeley.

That went on but became a less important feature through the years, and virtually everything else in the university was reorganized in some fashion, reassembled. There was still a College of Agriculture and there was still a College of Engineering, but there were various subdivisions created and so on. Chemistry didn't seem to need any additional subdivisions for many years, but there was no reason for it to be subsumed into a more general College of Letters and Science. It seemed to be doing very well the way it was, and I'm sure Lewis said, "If it ain't broke, don't fix it," and Hildebrand said, "It's working beautifully now, don't disturb it," and so it wasn't disturbed.

Chemical Engineering

Pitzer: It proved convenient, because chemical engineering had never been developed here. Lewis wasn't interested in it. He didn't oppose its existence; he just didn't want to be doing it. He arranged to have some courses taught that would give some chemical engineering-type application to students, and he brought with him from MIT Merle Randall essentially to do that. I don't know how far we went into it in the academy [National Academy of Sciences] biography, but Giauque was a product of this. He took a number of engineering courses--electrical engineering, mechanical engineering, and so on--and was really seriously thinking of becoming, as it were, a kind of chemical engineer. But he got so interested in his basic science, and Lewis encouraged him to stay and continue and expand his basic science, that he didn't do it; [he decided] that he'd just stay in basic science. But he was a good engineer.

Now, after Lewis' retirement, it was quite clear that chemical engineering ought to be developed here at Berkeley. I was one who said this. Latimer was the person who actually could push it. He became dean after Lewis, you know, and so we started immediately after World War II to set up chemical engineering, and it was easily done under the College of Chemistry. When I was dean [1951-1960], I actually established the two separate departments, to give chemical engineering its own focus and a chairman who could be the representative at national chemical engineering meetings and so on.

There was a little competition with the College of Engineering, which they lost and we won, [laughs] reasonably friendly, you understand. We won it by getting better people. The national standing of our faculty was so much better that the other program just sort of tapered off. We were rated number three in the country in chemical engineering in the last report by the National Research Council.

Hughes: So engineering students came to the College of Chemistry for chemical engineering?

Pitzer: Well, other engineering students don't take much chemical engineering. A few do; there are a few civil engineering areas where there is enough of a chemical aspect that they come. They take some chemistry, but in metallurgy, for example, which is a somewhat similar field, [the School of Engineering] will teach its own metallurgy. I don't say they never take a chemical engineering course, but they don't take many. It's more or less a separate field.

Hughes: Has Chemistry's status as a college given it an advantage that a department wouldn't have?

Pitzer: Oh, yes. It means the dean of the college was essentially one level higher in the university administrative structure than the chairman of the physics department. Now, in recent years, [the College of] Letters and Science has had divisional deans, and one of those divisional deans was a physicist about 80 percent of the time, so physics was not unrepresented at that level. Over the long period of time, it's helped Chemistry; the College of Chemistry has prospered. One of the things that Hildebrand used to say, and I've said many times, is that it's very important that we continue to manage our own affairs so that we don't cause any problems elsewhere on the campus. Then we can always use that argument, as I've said before, "If it ain't broke, don't fix it."

There is no other campus in the University of California that has this pattern. Illinois has this pattern, not quite the same, but pretty close. In fact, the chemistry unit at Urbana-Champaign at the University of Illinois includes chemistry and chemical engineering, and I think there it may include metallurgy, possibly even biochemistry, I'm not sure. It has a third division. And there are a few other places where chemical engineering is essentially hooked onto chemistry rather than to other engineering fields.

Academic versus Industrial Careers

Hughes: Was there any division along the lines of the ultimate destiny of students in chemical engineering? I'm thinking of the academic/industry dichotomy.

Pitzer: Well, at Caltech, chemical engineering is very close to chemistry. That was a similarity.

Now, for students with a bachelor's degree in chemistry and no further work, they are only marginally professional in the eyes of the industrial world. They are members of the American Chemical Society, all right, but that is not really primarily what I'd call a tight professional society. They frequently use their chemical background in supervising operations and sales, advertising, finance, and so forth, rather than doing chemistry per se. If you want to be a professional chemist per se, you essentially need a Ph.D. There are certain specialties where a master's degree may get you in.

In contrast, in chemical engineering, particularly if you've got a master's degree, you have full professional status in the industrial world. The net result is, if people are not going on for an advanced degree but are graduating in chemistry, it's important and valuable for them to take a few essentially introductory chemical engineering courses so they'll be able to talk to colleagues in later employment and so on.

Well, that was what was done here earlier, with just the courses that Randall gave, and what was done at Caltech and other places, even where the full-fledged chemical engineering program may not have been yet developed.

Hughes: Was there any sort of pecking order in this system?

Pitzer: Well, I'm not sure what you mean.

Hughes: In some areas of science, there was some stigma attached to going into the applied aspects of the field. There was more prestige associated with pure research, the academic approach.

Pitzer: Okay, I see what you're after there. No, in the eyes of some people, this is true. That is, there are people in pure sciences, I think in physics more so than in chemistry, that have this view that it's a lower-grade sort of business to get off in the applied area. It's an important balancing aspect to give full recognition to the importance of industrial application by having a chemical engineering department with leading figures that have high prestige nationally and so on.

Then people that have a background more or less as I do, not pretending to be a chemical engineer but taking an interest in basic science that is more or less directly applicable and valuable, are more at home and there is probably more encouragement to taking that point of view.

Hughes: I should think that that sentiment would be encouraged by the fact that right here in the same plant, so to speak, there was a chemical engineering program.

Pitzer: Well, that's what I was trying to say. I think you said it better than I did. [laughter]

Theory and Technology

Hughes: In William Jolly's history,[1] one of his viewpoints was that college has stressed the coupling of the state-of-the-art experimental techniques with new theoretical methods. Would you use that as a distinguishing characteristic?

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[1] William L. Jolly. A short history of the College of Chemistry at Berkeley. *The Hexagon*, spring 1988, 3-13.

Pitzer: Oh, I think that's very much the case. And there's no better example than Giauque in the early days in that regard, in that he was at the very forefront of knowledge, say, of quantum mechanics and spectroscopy of chemical interest, and in developing low-temperature calometric instrumental methods and experimental methods. I think one could cite others that were similarly at the forefront on both sides, but maybe in less widely recognized examples.

Social Networks in Science

Hughes: Historians and others are very interested in the social networks that are so important to science; hence my question. You have mentioned the seminars. But what other official and non-official occasions were there for scientific exchange other than the established publications and meetings? Were you, for example, on the telephone or writing letters to people in your field all along?

Pitzer: Oh, yes, sure. As you got acquainted with people that were doing work at the forefront in areas that interacted with yours, why, surely. You frequently had gotten acquainted with them at a national meeting. In those days, an American Chemical Society meeting wasn't so enormous but what you could get acquainted with people fairly well. Now you do that much more in a more specialized meeting of some sort.

One used the telephone some; it was more expensive than it is now, but not unduly so. Mail service was about as good then as it is now, and one used ordinary mail. We didn't have e-mail then. But certainly in my own work, for many years I was acquainted with people at various other places that were doing related things. I would get in touch with them directly, or write them, and so on.

A Job Offer at Harvard

Pitzer: [George] Kistiakowsky at Harvard had things going that were quite closely related to mine. He essentially invited me to go there as what they called a junior fellow at the time. I guess I had already become an instructor here, but just a year after my Ph.D. I decided, no, I'd stay here. That would have been, in a way, a more prestigious position that wouldn't have involved any teaching obligations, but I had various reasons for preferring to stay here. But I was already in communication with him. A junior faculty member at Harvard who did go that route had been my freshman section instructor at Caltech, E. B. Wilson, Jr. Obviously, I knew him well. We communicated easily.

Hughes: That was a job offer? That wasn't just to come for a year or a short period of time?

Pitzer: Well, it was a job offer, but it had maybe a three-year total period on it. As we gradually came out of the Depression, it was quite clear that if you were well enough regarded, you'd be promoted here on your merit right on up to a full professorship.

Harvard, I think I knew it even then, and I certainly learned it very soon thereafter if not, has a different pattern. They have junior appointments, including assistant professorships. Of course, it's only assistant professorships now, plus these junior fellowships. But when they come to an end, if the university and the department decide there is no tenured position in your field, you're at a dead end. You just have to go somewhere else. And they've lost many very able people that way. Sometimes, they find special intermediate positions that keep people around for another two or three years until the general position and budget pattern allow an appointment to be made.

By this time, I was married and had at least one child and maybe another one on the way, and we knew the situation here. [Harvard] just seemed not unattractive, but less attractive. So I never considered it seriously. But I'm sure I had scientific correspondence with Harvard people right along through those years.

III RESEARCH

[Interview 2: May 29, 1996] ##

Internal Rotation in Ethane

Assumptions in the 1930s

Hughes: Dr. Pitzer, you have discussed internal rotation about the single bond in ethane in the Seidel interview and elsewhere, but there's more detail that I'd like to extract from you.[1] I understand that there was a controversy in the mid-thirties about internal rotation, and I wonder what that controversy was about.

Pitzer: Well, the prevailing assumption in the mid-thirties was that the potential barrier to internal rotation in a situation like ethane was so small, it could be ignored for statistical thermodynamic purposes at high enough temperatures that the substance was in the vapor phase. This had some theoretical basis, in terms of approximate quantum mechanical calculations, and if I remember rightly, there was a paper by Henry Eyring on it, but I'm sure there were others also.[2]

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[1] Aside from the Seidel interview, internal rotation is discussed in: Kenneth S. Pitzer. Of physical chemistry and other activities. *Annual Review of Physical Chemistry* 1987, 38: 1-25; and, Interview with Kenneth Pitzer by David Ridgway. *Journal of Chemical Education* 1975, 52: 219-223. (Hereafter, Ridgway interview.) For key papers in Pitzer's research as a whole, arranged by research area and usually with a topical introduction, see: Kenneth S. Pitzer, ed. *Molecular Structure and Statistical Thermodynamics: Selected Papers of Kenneth S. Pitzer.* Singapore: World Scientific Publishing Co., 1993. (Hereafter, collected papers.)

[2] H. Eyring, *J. Am. Chem. Soc.*, 54, 3191 (1932). M. L. Eidinoff and J. G. Aston, *J. Chem. Phys.*, 3, 379 (1935). L. S. Kassel, *J. Chem. Phys.*, 4, 276, 435 (1936).

So when I came into the situation in 1936, there was a published literature on the statistical thermodynamics side, all assuming the zero potential barrier for internal rotation, not believing it was absolutely zero, but that it was so small that it could be ignored.

Calculation Disagreements and Ambiguities

Pitzer: There had already been noticed for other, more complex molecules, particularly tetramethylmethane of pentane, that there was a conflict between the experimentally measured entropy based on the Third Law [of Thermodynamics] and that calculated on this basis of zero internal rotation barrier. But there were complications in that calculation concerning low vibration frequencies that were important.

For ethane, these complications disappeared in that the molecule just wouldn't have any other vibration frequencies low enough to contribute for that molecule. So the answer that [R.K.] Witt and [J.D.] Kemp had for ethane, which Kemp and I were interpreting, was an unambiguous conclusion in favor of a barrier of about three kilocalories per mole, which was much higher than had been estimated.

I think in the Seidel interview, I commented about a paper of Edward Teller's, published just before our work, in which he considers data for ethane. He discusses other, more ambiguous information concerning the equilibrium reaction between hydrogen plus ethylene to form ethane, where there appeared to be a disagreement with the free rotation calculations in the same phenomenon. He remarks that a barrier of about three kilocalories would remove it, but he doesn't claim that the case is established.

Hughes: Because his data is ambivalent?

Pitzer: Yes. Well, he was using other data from the literature [and] the data just weren't that accurate. That was the problem. And again, there is this complication of higher frequency vibrations, bending vibrations of hydrogen atoms with respect to the carbon structure, that have enough effect on other properties, but they have virtually no effect on the entropy at the boiling point of ethane. Therefore, you could readjust those to any reasonable value and it didn't change our conclusion about the barrier.

The question of the quantum mechanical explanation, if you wish, of the 3,000-calorie barrier remained a puzzle in the quantum mechanical literature, and, very interestingly, when my son, Russell, went as a starting graduate student to Harvard in 1959, Professor [William] Lipscomb suggested this problem to him.

Hughes: Somewhat because he was your son?

Pitzer: I think so. [laughter] And in effect, he solved it. See, by that time, it was feasible to include in the calculation terms which had been neglected previously. And with the advance of the electronic computer, it became feasible to include these terms, and they were surprisingly big enough to yield pretty good agreement. So his thesis was published on that [problem], and there were one or two follow-on papers related to it.

Hughes: By him?

Pitzer: With his name and Lipscomb's, and I think one with his name alone. So that's an interesting story.

Hughes: I wonder how many sons and fathers have worked on the same dissertation project.

Pitzer: Well, this wasn't exactly the same, but it was closely related. I don't know. I would suspect not many.

Well, your next question.

Hughes: In the Seidel interview, you stated that the assumption of totally free rotation led to disagreements with the entropy values. Were the entropy values well established in the literature?

Pitzer: No. That is, as of 1935 or 1936, I think the only really well established entropy value that was pertinent to this was that for tetramethylmethane from a man by the name of J. G. Aston and collaborators at Penn State. There were more approximate values that were based on heat capacity measurements going down only to liquid air temperatures, say, 80, 90 Kelvin, which involved a large extrapolation on down to the absolute zero to get the entropy value. The uncertainties on those were just too big to hence say anything about this.

I think I'm correct in saying that there was just that one value. But as I said earlier, for the tetramethylmethane, there were very low frequency bending vibrations of the C-C [carbon-carbon] around the central carbon atom that left the calculation somewhat ambiguous. Thus, the ethane value for the entropy was the first case that could be really unambiguously interpreted, and that's what we did.

Witt and Kemp

Hughes: This problem arose afresh upon your arrival at UC Berkeley. It was not something that you had been puzzling about at Caltech.

Pitzer: Oh, no. What I said in the Seidel interview was that J. D. Kemp, who had finished his Ph.D. with Giauque on a different problem, had been asked to assist this postdoctoral visitor who came on a fellowship and wanted to measure ethane, and had done so.

Hughes: That was Witt?

Pitzer: Yes. Witt had left and apparently lost interest in it. He was a chemical engineer and was off on something else. To this day, no one seems to know why he chose ethane when he took this up. Kemp was perceptive enough to recognize that there was a controversy about this, and that he couldn't just publish an interpretation of ethane without either learning a lot more about the quantum mechanics of internal rotation, or else getting somebody else to.

Pitzer's Contribution

Hughes: Would Kemp have been able to handle the quantum mechanical aspects?

Pitzer: I can't answer that. He didn't feel comfortable with it, let's put it that way. It would have probably taken a very extended period of study, which he had no desire to get into. I came from the beginning with a better understanding of quantum mechanics than most physical chemistry graduate students had at the time.

Hughes: Graduate students, or anybody in the field?

Pitzer: Well, there was understanding of quantum mechanics, mostly by physicists with respect to this sort of matter. Of course, physical chemistry overlaps into physics in the appropriate areas. There was--I think I mentioned this--the Pauling and Wilson quantum mechanics book, which was the first such book written for a physical chemistry audience. I was aware of it hot off the press and got a copy, and that augmented my understanding of and my capacity to read the physics literature.

A physicist by the name of [Harold] Neilson, I think at Ohio State--I could be wrong about his location--had published with respect to the energy level diagram possibly related to spectroscopy, not with respect to the statistical mechanics and the calculation of an entropy. So we in effect did the calculation of the entropy using just the published solutions for the energy levels. This is relatively straightforward statistical mechanics, but it still had to be done. And it is not terribly simple, because the pattern of energy levels is complicated enough that there is no simple closed mathematical expression. You just have to add up on a computer, in effect, the contribution of each energy level. So it was not a trivial task, but it didn't really take very long to do it once one had the sufficient background.

Hughes: But you didn't have computers at that time.

Pitzer: Oh, yes. One had a calculating machine and turned a crank and pushed buttons.

Hughes: [laughter] Oh, that kind.

So you were quite aware at the time that there were limits to the values you were obtaining? They were the best that you could do at the time?

Pitzer: Oh, you mean in terms of mathematical accuracy?

Hughes: Yes.

Pitzer: Well, that's one of the things I wanted to comment further about. The first calculations that we did, and that I did later on other molecules in addition to ethane[1], were adequate for the purpose, but were inconvenient and involved rather clumsy mathematical methods. So I suggested to my graduate student, William Gwinn, that we see if we couldn't do a better job of this, a more elegant treatment of it, and indeed we did. The first paper of the series is in the *Selected Papers* volume.[2]

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[1]J. D. Kemp and K. S. Pitzer. *Selected Papers*, pp. 11-14 and 15-21.

[2] K. S. Pitzer and W. D. Gwinn, "Energy Levels and Thermodynamic Functions for Molecules with Internal Rotation. I. Rigid Frame with Attached Tops," Journal of Chemical Physics, 1942, 10: 428-440.

We presented tables of entropy values and heat capacity and enthalpy values and so forth contributed by an internal rotation mode in the molecule, in terms of the potential barrier and the temperature and the reduced moment of inertia for fairly simple cases in the first paper and then for more complicated cases in the three additional papers that spread out over quite a number of years [1942-1959].[1]

[Our work] didn't change the picture; it just reduced the uncertainty to a negligible level, let's say, and provided a convenient basis. Particularly the tables in the first paper have been republished in various books through the years where internal rotation was of some importance.

In that first paper, there is also a short section entitled "A useful approximation," in which we point out that if the difference between the quantum mechanical calculation and a classical mechanical calculation is not too large, the difference concerns what's known as the zero point energy, in other words, the energy of the very lowest quantum state, which is not at the bottom of the potential curve, and then of the very next lowest, the second lowest quantum state, which is a finite height above the first one.

And so we suggested an approximation in which a quantum calculation for these first two energy levels is compared with a classical calculation for that particular pattern, and that difference be subtracted from the classical calculation for the exact curve of up to as high a level as desired. Well, this has been used and cited by various people doing different things, not internal rotation things, but cases where a classical mechanical, statistical mechanical, calculation was almost good enough but not quite. It's been interesting to see it pop out again fifty years after it was originally published.

Hughes: You said in your 1987 account in the *Annual Review of Physical Chemistry* that the result for ethane was received with surprise but little controversy. Now, why the surprise?

Pitzer: Well, because the existing quantum mechanics at that time had indicated a very low potential barrier, which could be approximated to zero. The firm evidence against it experimentally was nonexistent until the ethane case. There were the indications, but sort of within the range of experimental uncertainty, that Teller had noticed in the past and had pointed out. Well, people had been saying, "Well, we'll wait and see.

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[1] *Selected Papers*, see p. [46] for references and summary.

We'll tend to believe the free rotation approximation until there's a more convincing case than that." Then the ethane case was just, as it were, two orders of magnitude more convincing and essentially inescapable.

Hughes: You chose to work on ethane because it's the simplest molecule that will demonstrate this problem of internal rotation barriers?

Pitzer: [laughs] Oh, no. It's merely the accident that Kemp had the data and essentially didn't know what to do with it. He recognized that there was a controversy here, that it would be of interest, but he didn't feel capable of handling it. As soon as he pointed it out to me, I could recognize that it was of interest too, and was able to bring sufficient ability and knowledge of bond theory and spectroscopic physics-type knowledge to go ahead and make the necessary calculation.

Facility in Quantum Mechanics

Hughes: And from what you were saying before, I gather that you were not unique in coming to the problem with quantum mechanical knowledge.

Pitzer: Oh, no, other people could have done it.

Hughes: But, in this department, you had the finest grasp of the field. Is that a correct assumption?

Pitzer: Among the students then, certainly yes. It's a curious point that although I was a Latimer student, [this work] was being done essentially in Giauque's laboratory. Giauque was probably the one person among the physical chemistry faculty that really had that command on the quantum mechanical side if he wanted to use it. I assume that Kemp must have tried to interest him in it, and he didn't take it up, didn't become interested in it. We went ahead and did it.

Hughes: Did he show particular interest when you did do it?

Pitzer: Yes, and a few years later, he had one of his graduate students do--what was it? [propylene] It was probably a thesis problem for one of his students, and the internal rotation of the methyl group and one end of the propylene molecule was, I suppose, one of the primary points of interest in the problem. Oh, yes, he became interested in it then. [laughs]

Hughes: Well, I'm trying to get at the pervasiveness of comfort with quantum mechanical techniques in physical chemistry in the thirties. Would it be true to say that quantum mechanics was not an approach that everybody felt comfortable with in physical chemistry at that time?

Pitzer: That's right. It was just coming in, and the Pauling and Wilson book had a great deal to do with making people comfortable with it. Henry Eyring was certainly comfortable with it, with different sorts of applications than Giauque. Pauling probably among the more senior people--and he wasn't very old then--was most comfortable with it, but Pauling had not interested himself in the particular applications to statistical thermodynamic problems.

But I'm sure my comfort with quantum mechanics depended more on knowing that this outstanding physical chemist named Linus Pauling was comfortable with it, found it useful, was using it for other things. After all, Wilson, the co-author of that book, had been my freshman teaching assistant instructor when I was a freshman at Caltech. So I had personal relations with both of those authors.

Choice of a Scientific Problem

Hughes: How much does technical skill with a particular approach determine the choice of a scientific problem?

Pitzer: You may recognize problems, but if you don't see any approach in which you have some confidence that you can accomplish something, you don't spend much more time doing anything about it; you pick a different problem. Now, you may put it on a list to think about again some time a few years hence when it may be more feasible or you may be in a better position to do something about it, if somebody else hasn't done something in the meantime. That's definitely true.

Long-chain Hydrocarbon Molecules

Pitzer: There is one other paper during that internal rotation period that I want to say a few words about. It's the one on long-chain hydrocarbon molecules.[1]

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Pitzer: I proposed a method of solving initially at the classical mechanical level by starting with the atom at one end of the chain, and then integrating successively from that atom to the second atom, and then to the third atom, and then to the fourth atom, using coordinates relative to the preceding part of the chain. Insofar as I know, it was novel at the time. Now, I used this going down a hydrocarbon chain, carbon atom by carbon atom, and then treated the hydrogens as the second stage of the calculation. Then I used that approximation that I mentioned before of correcting for quantum mechanical effects on the chain units.

This method again has come up as a method of solving other chain-like molecule problems. I came back to it within the past year on polymers of sodium chloride in vapor,[2] where you go from a sodium ion, to a chloride ion, to a sodium ion, to a chloride ion, down a linear chain. The question is, can you calculate the statistical entropy and so on for a long chain without undue complication? And in essence, you can by this method, if you've got enough information about the first four ions, that is, the dimer, with two sodiums and two chlorides. Then you can predict the rest of a longer chain, and that was sufficient to solve a longstanding problem about sodium chloride vapor at very high temperatures, 1700 Kelvin.

Hughes: You referred in one of these accounts to a novel method, without further explanation. Is that what you were thinking of?

Pitzer: There are two novel methods that I have mentioned here. This last one about chain molecules is rather special--not too many examples.

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[1] K.S. Pitzer. The vibrational frequencies and thermodynamic functions of long chain hydrocarbons. *Journal of Chemical Physics* 1940, 8:711-720; *Selected Papers*, pp. 22-31.

[2] K.S. Pitzer. Sodium chloride vapor at very high temperatures; linear polymers are important. *Chemical Physics* 1996, 104: 6724-6729.

Quantum Mechanical Corrections

Pitzer: The other was a more general method of making quantum mechanical corrections to a problem that was readily solvable in terms of classical mechanics, but the answer wasn't quite good enough, but the quantum corrections could be made terribly simply. And that's in the first Gwinn paper,[1] and it's been used widely.

Hughes: Why was your first publication, in 1936, on internal rotation in the form of a letter[2] rather than a full paper?

Pitzer: Because it was a subject of active interest at the time.

Hughes: You wanted to get it out quickly.

Pitzer: [laughing] And we wanted to get it out before somebody else did. Actually, nothing else came along in the interim [before the paper was published in 1937[3]], but that's what a letter is for, to get it out promptly.

Other Researchers

Hughes: While you were doing this work, were you in touch with other people?

Pitzer: Not particularly. There weren't many people to be in touch with. Well, I was in no particular position to be in touch with them either, because I was--

Hughes: You were very junior at this point.

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[1] K.S. Pitzer. Thermodynamic functions for molecules with internal rotation. *Journal of Chemical Physics* 1942, 10: 428-440. (With W.D. Gwinn.)

[2] J.D. Kemp and K.S. Pitzer. Hindered rotation of the methyl groups in ethane. *Journal of Chemical Physics*, 1936, 4: 749. *Selected Papers*, p. 6.

[3] J.D. Kemp and K.S. Pitzer. The entropy of ethane and the Third Law of Thermodynamics. Hindered rotation of methyl groups. *Selected Papers*, pp. 7-10.

Pitzer: Edward Teller wasn't much older, but he was somewhat older. I don't know whether he'd come to this [country]. His paper was published when he was in London, but he's Hungarian and was wandering around western Europe, avoiding Hitler. Not that Hitler was in Hungary yet, but he was making German locations unattractive, and Teller was in London at the time. He came to this country very soon thereafter.

Hughes: What about Kistiakowsky?

Pitzer: He was interested in the subject.

Hughes: At that time?

Pitzer: At that time, he was interested in the subject, because he was one who made measurements on that ethylene-plus-hydrogen-equals-ethane equilibrium that Teller had pointed out could be reconciled with a higher barrier. But he hadn't suggested that explanation, and I don't know that he felt he was in any position to do anything more with it right then. But very soon thereafter, E. Bright Wilson was at Harvard as a junior fellow and then was promptly on the faculty and was of course in contact with Kistiakowsky.

Hughes: And this is the Wilson of Wilson and Pauling?

Pitzer: Yes. So Wilson and Kistiakowsky were active in this field almost immediately thereafter, and I had quite a little communication with them, including an invitation from Kistiakowsky to be a junior fellow at Harvard, which I turned down.

Hughes: Yes, you discussed that on tape last time.

Utility of Research Results

Hughes: You said in your interview with Ridgway, "We went out of our way to present the results [on internal rotation] in a form that would be convenient for other people to use."[1]

Pitzer: Yes, and that particularly relates to that first paper with Gwinn on tables of internal rotation contributions in terms of the potential barrier in the temperature and so on, so that the individual doesn't have to make a detailed calculation himself; he can just interpolate. It takes a two-way interpolation. You

[1] Ridgway interview, p. 220.

can't do a single interpolation with this problem; you have to have two variables and a rectangular diagram of numbers, and you interpolate both horizontally and vertically.

Hughes: I see. Is the usefulness of your findings an important aspect to you?

Pitzer: Yes. I wouldn't say it's a world-shattering aspect. [laughs] Since it was mine, I'm interested in it. But it is widely used.

Hughes: I meant in general. I imagine that for some people, coming to the solution of a problem is enough in itself, and that that solution may not be put into a form that is widely useable. Do you take that extra step to make your finding as widely accessible as possible?

Pitzer: Yes, I did then, and I have done so ever since. In other words, I think of science as somewhat of a social community enterprise, and that it's important in terms of the advance of science that, if you have some contribution to make, you describe it in a fashion and make it relatively understandable and applicable to other people's problems. I think most people do this. Most people in chemistry do this, anyway.

Chemistry is more social in this regard than the extreme case that the Unabomber illustrates. Mathematicians are more isolationists. If they can impress a few of their most immediate colleagues, they don't care much about the larger community, so I'm told. Don't take this too seriously.

Hughes: Do you think this orientation towards practical application is somewhat because the real world out there is very close in chemistry?

Pitzer: That's right. Most chemists take an interest in applications of their work even if they don't do the applying themselves. They're much more in contact with applications. In part, the applications appear in the form of chemical engineering, but a lot of them do not. A lot of them appear in more or less direct applications of chemistry.

Most applications in physics, of course, occur through engineering, and engineering is frequently applying parts of physics that were discovered centuries earlier. But engineers also apply very recent discoveries. That is, computer science is [taught] along with electrical engineering in most schools, and most of electrical engineering is now microelectronics, not the electrical end of a steam turbine generator. The one part of physics that tends to go more or less like chemistry, I would say, is solid state physics, where the physicists do get off into applications. It's much more like chemistry in this regard.

Hughes: To a degree, then, does potential application determine your choice of a scientific problem?

Sodium Chloride at High Temperatures

Pitzer: Oh, yes. It does with me, for example. Well, I was mentioning this very recent paper on sodium chloride at very high temperatures, as a vapor. In terms of the science of it, any alkali halide, in other words, lithium, sodium, potassium, rubidium, cesium, fluoride, chloride, bromide, iodide--what's that, twenty different molecules?--would all have essentially the same quality and character and the same possibly interesting features.

But in the real world, sodium chloride is so much more abundant that it is much more likely to be of practical applied interest. My choice in this last paper of sodium chloride rather than one of these other alkali halides was based in part on this fact that it is likely to be of more practical interest. You've got to start with one if you're going to get into detail, and I'm leaning on published literature, both recent and back a good many years, in terms of the experimental basis, and there's more thorough experimental coverage of sodium chloride, too.

Now, whether I ever get around to doing some other alkali halides with respect to this same quantity or not is rather doubtful. The novelty has pretty well worn off, and if anyone is enough interested in one of these others, it will be fairly straightforward to apply the same methods to it.

Hughes: I understand your choice of sodium chloride within the halides, but to choose the problem at all, was that determined by its application?

Pitzer: Not primarily. That is, I don't know of any practical appearance of sodium chloride at those very high temperatures. The practical aspect is indirect there. There is a two-volume set of thermodynamic tables that goes under the name JANAF--Joint Army Navy Air Force Tables. The Defense Department financed these, a collection from the literature, an organization of convenient formulations, and among other things they chose sodium chloride as being of sufficient interest to include. They include a few other alkali halides; I think maybe only potassium chloride.

Some years ago, I used their treatment to extrapolate upward in temperature and predict the properties of sodium chloride vapor-liquid equilibrium, and where a critical point would occur when the liquid expands and the vapor becomes denser and they become the same. I predicted [a critical point] about 3800 or 3900 Kelvin.

Some calculations were made with a pretty good model for sodium chloride by a French pair, V. Guissani and B. Guillot. They came out lower, around 3200. So I thought I'd better take another look at that situation, and in fact they were right. For the JANAF tables, the authors had in a perfectly plausible manner assumed that the dimeric sodium chloride species--two sodiums, two chlorides; they're ions--are in essentially a square geometry with attraction across each edge and repulsions across the diagonals. They ignored the possibility of a linear geometry, and at larger species, trimers and so on. That was a good approximation up to about 1300 Kelvin, but not for these other, looser, floppier, higher-entropy species which become important at higher temperatures. When I correct for that, I agree pretty well with the Frenchmen; around 3200 is about right.

Now, why do it for sodium chloride? Well, because the Frenchmen did it for sodium chloride, because JANAF had tables for sodium chloride, because some people would be interested in sodium chloride. You might as well start with the species with the most known about it. The same story can be told for others that I haven't gotten around to doing and probably won't.

Participant in Latimer's Research Program

Hughes: Well, should we move to ring molecules?

Pitzer: No, there's one other topic I'd like to take up. I was a student of Latimer's, and his personal interests were in the entropies of aqueous ions for use in aqueous inorganic chemistry, shall we say. I want to acknowledge that I was a part of that program, too. In all, there were eleven papers in 1937-38, maybe into '39, that concerned this work. I was a co-author with Latimer and other Latimer students on more than half of these direct investigations of particular substances, getting information about particular ions.

The most important co-author was Wendell Smith, with I think five or six of the papers. There were three names on them [Latimer, Pitzer, Smith]. So a very appreciable amount of my time was spent, actually, on Latimer's particular interest. His interest wasn't short-changed at all. There were two papers that were a little different than that general pattern but still fitted in, and those two are in the *Selected Papers*, but only those two.

Heats of Ionization

Pitzer: One concerned the heats of ionization of weak acids and bases,[1] in which I set out to put together a simple calorimeter myself and measured heats in mixing of either the acid with sodium hydroxide or something equivalent to that. I came up with not only precise numerical data for quite a number of examples, but then generalized to the change of entropy and of heat capacity with ionization, which in turn determines the change of the ionization constant with the temperature. Out of that, I proposed a generalized approximation where, with much less detailed information, people could use the properties that were more widely measured at room temperature to make good estimates both at lower temperatures and at higher temperatures. This pattern that I came up with was presented as a major improvement over one which was in common use and had come out of Harned's laboratory at Yale.

Free Energy of Hydration

Pitzer: The second paper I want to mention involves Latimer and his student, [Cyril M.] Slansky, and myself.[2] [looks at *Selected Papers*] This is '39, so I was on the faculty by then. It concerns the free energy of hydration of a gaseous ion or a gaseous pair of ions. It uses a very simple equation of Max Born, which is in the physics literature widely familiar to many people, which gives that quantity as a function of the radius of the ion. It's a simple calculation. You treat the water as a dielectric and the ion as a rigid sphere of a certain radius. For real water

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[1] K.S. Pitzer. The heats of ionization of water, ammonium hydroxide, carbonic, phosphoric, and sulfuric acids, *Journal of the American Chemical Society*, 1937, 59, 2365. *Selected Papers*, pp.477-484.

[2] W.M. Latimer, K.S. Pitzer, and C.M. Slansky. The free energy of hydration of gaseous ions, and the absolute potential of the normal calomel electrode, *Journal of Chemical Physics*, 1939, 7: 108. *Selected Papers*, pp. 485-489.

and for real ions, it's an approximation, primarily because the dielectric constant of the water doesn't remain constant up to the surface of the ion.

But it's also a matter of what's the effective ion radius, and if you use crystallographic radii, which have now been pretty well standardized by Pauling, you don't get the right answer. What we showed was that if you added an increment to the radius of the ion, and the same increment for all positive ions, and a different increment but the same for all negative ions, you fitted the data pretty well. The physical picture was that the dielectric constant arose from the orientation of the water molecule in its dipole, or its positive hydrogen atom parts of the water, and that will orient with a hydrogen next to a negative ion. On the other hand, for a positive ion, the electron pair on the other side of the water molecule will be next to the positive ion. But we evaluated those increments and got really quite good results. This [paper] again keeps coming up in the literature.

Within the past ten years, two men, Alexander Rashin and Barry Honig, who are interested really more in biological systems, have refined this treatment a little bit. I'm not sure whether they really improved it or not. As I say, this paper keeps getting cited. It captured the essence of the problem in a very simple way, and to do much more about it is terribly complicated. So it's been fun to watch it and to see that it's still useful.

I'm sure Slansky was just being carried along in doing the details. This was classical physics, and Latimer was fully in command of the classical physics of it and familiar with ion radii and so on. So it undoubtedly came out of a conversation with Latimer, Wouldn't it be interesting to see if something like this could be worked out?

Latimer as a Research Director

Hughes: Would it have been politically inexpedient if you had pursued only the internal rotation problem which was not directly in Latimer's sphere?

Pitzer: Well, that's a good hypothetical question.

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Pitzer: If I had had a research director who had much more strict ideas about what he wanted his students to do, it could have become a very uncomfortable situation, and I don't know what I would have done then. Fortunately, I had a research director who was very flexible, who was interested in seeing his student succeed more than he was worried about how much credit he got and under his own name. We would have been perfectly happy if Latimer had wanted to put his name on the Kemp and Pitzer paper. We even offered it, but he always said no to anything like that. He said, "If I haven't really contributed, leave my name off."

Hughes: But that would be in keeping with protocol in the field, that because it was happening in his laboratory, he could have been an author, regardless of his input?

Pitzer: Yes. Even if his input was negligible, I don't think it's very appropriate, but it happens. Some of the controversies you read about in the literature I think happen because somebody insisted on having their name on a paper even though they didn't know too much about it, and then it turned out there was something wrong with it. They're embarrassed then.

But Latimer was very generous about that sort of thing, more so than some other people even within the department, and the department here was, I'm sure, quite flexible and generous about that sort of thing. I don't recall Giauque having many students who published things separately except for this Kemp case. But he was, I'm sure, also quite generous about that if a student had gone off on some other activity that he thought was significant and important and valuable but that he had very little to do with.

Hughes: Well, I suspect there was another element and that is Latimer's assessment of Kenneth Pitzer. If he had not felt you capable of pursuing this problem, he would have reined you in somehow.

Pitzer: That's exactly right; he thought that I had a high probability of coming up with the right answer.

Hughes: And having the skills to get there as well.

Pitzer: Right.

More on Quantum Mechanics

Hughes: Did Latimer recognize a young man with a facility in quantum mechanics, and why not let him loose on this problem?

Pitzer: Well, I think that's correct. He recognized that quantum mechanics was going to become much more important to chemistry in the near future, and no reason why he shouldn't encourage somebody that seemed capable of moving the process along.

Hughes: Did people see quantum mechanics as opening up an exciting new vista? Was it that dramatic?

Pitzer: I think so. Various people saw it more clearly or sooner than others, but by the mid-thirties, quantum mechanics itself had become well enough established that it was recognized as important. He made the statement that quantum mechanics contains something less than all of physics, and <u>all</u> of chemistry. [laughter] [Paul A.] Dirac? Yes, I'm sure it was Dirac. I probably haven't got the quotation quite right, but it's something like that. People realized that if one became capable enough with quantum mechanics, that one could solve a great deal of chemistry. Of course, there were sort of waves of this, really as computational capacities improved. The basic possibility was there, but certainly with the calculational capacity of the mid-thirties, you couldn't go very far, but you could go further than you could have earlier.

Electronic Computation in Chemistry

Pitzer: But there was no significant advance in calculation until after the thirties; I guess that's fair enough to say. It was quantum mechanics itself that improved in its connection to things of chemical interest. It was only later with electronic computation that the field really opened up much more widely.

Hughes: Was that predictable in the 1930s?

Pitzer: Only in the most general way, that once there was an interest in this sort of thing--and not just for science necessarily; maybe for financial and business matters, improved typewriters maybe, and so on--technology would improve; it would make calculation much better.

Just after World War II, of course, there were the vacuum tube computers, the Eniac at the University of Pennsylvania. The trouble there was that they were unreliable. Tubes burned out or didn't function adequately, and you had to have all manner of checks. Even with all the checking within the computer, the only safe thing to do was to do the calculation all over again a month later and see if you got the same answer. It was only with the solid state electronics plus improved error-checking circuitry that computations became so reliable that one seldom even thinks about being given a mistaken result. You might program it wrong, but the idea that the computer gets it wrong has almost disappeared.

More On Internal Rotation in Ethane

[Interview 3: June 5, 1996] ##

Hughes: Dr. Pitzer, I believe you wanted to add a bit of clarification to our discussion of internal rotation last time.

Pitzer: Yes, in particular I wanted to make a little clearer the quantum theory calculations related to internal rotation, and why some of them became feasible to do accurately only many years later than the time we were first working on this in the mid-1930s.

For a molecule such as ethane, with thirty particles all together, counting electrons and nuclei and in three-dimensional space, ninety coordinates, it's impossible to do a truly rigorous quantum mechanical calculation. The general pattern of approximation involves one stage in which the heavy particles are fixed, and one attempts to solve the electronic motion--[interruption] One assumes fixed positions for the nuclei and attempts to solve the electronic motion, and as a result of many such calculations for different locations of the nuclei, one gets the energy as a function of the nuclear coordinates.

Then the second stage in the calculation is to get the actual energy levels for the molecule, including nuclear motion subject to this potential energy calculated from the electronic side.

Well, in 1936, the electronic calculation simply had not been done with sufficient accuracy to get the potential barrier at all. In effect, the potential barrier had been approximated out, and the net result was that the estimated value of the potential barrier was essentially zero. At that time, it was feasible to calculate the final energy levels on the basis of an assumed potential. That's what we did with respect to ethane and found the potential barrier of about three kilocalories.

With advance of electronic computers, it became feasible to do the problem of electron motion reasonably satisfactorily. The electron calculation was feasible in the early 1960s, and that's what Bill Lipscomb suggested as a problem for my son when he was a graduate student at Harvard.

Ring Molecules

Relationship to Research on Ethane

Hughes: Well, let's advance to ring molecules. In the Ridgway interview, you said that after the work on internal rotation, "...I turned to the more general question of unusual motions in organic molecules, particularly ring molecules."[1] Could you explain why this was what I'm assuming to be a natural transition?

Pitzer: Yes. If one can calculate the energies involved in internal rotation, which is a matter of change of angle of the groups at opposite ends of a chemical bond, and can calculate all the other energy terms in connection with the geometry of a molecule, which had been pretty well established earlier, then one can, in principle, calculate the entire energy pattern for a molecule as a function of its geometry. For the rotation about a double bond, it had been known for some time that there was a severe restriction and the vibration frequency had been observed spectroscopically. For single bonds, which as I said before had been assumed to have no potential, we obtained for ethane, and in the few years thereafter for other open-chain molecules, the potential barriers which were really quite similar for single bonds from one case to another.

In a ring molecule, if there were only single bonds going around the ring, then the geometry of the ring, including the substituent atoms outside the ring attached to the ring atoms, involves, in pieces, the same geometry as that of ethane or slightly larger molecules such as propane. So one can transfer the knowledge of ethane to calculating the energy for the ring. This was clear enough in principle in the late 1930s, but I didn't get around to doing much about it until later. One recalls that World War II was on, and I was distracted very strongly by other obligations at that time.

I received the American Chemical Society award in pure chemistry in 1943, and I did mention this matter with respect to ring molecules in my award address, and then in 1945 published a

[1] Ridgway interview, p. 220.

short note in *Science* magazine for cyclopentane and cyclohexane,[1] and I think maybe one other molecule, but not anything about the substituted cyclohexanes.

Related Research by Others

Pitzer: In the meantime, O. Hassel in Oslo, Norway, had measured the substituted cyclohexane molecules by electron diffraction and had published in a rather obscure Norwegian journal some very interesting conclusions about the structures of the disubstituted cyclohexanes. He was aware of our work on the potential barrier in ethane and simple open-chain hydrocarbons, and interpreted very correctly the results for the substituted cyclohexanes. His first publication was in 1943 in this relatively obscure journal, and I was only aware of it some few years later. This, of course, was the basis for his eventual Nobel Prize.

Before going further, I need to say a word about bond angle strain, or as it's sometimes called, Baeyer strain, which says that if all the bonds around a carbon atom are single bonds, the bond angles will tend to be the tetrahedral angle. If you run tetrahedral angles around a ring, it comes out about right for a five-membered ring, which would be cyclopentane. But, if you look at the torsional orientation about those ring bonds in cyclopentane, they have the hydrogens all lined up, which we had every reason to believe was not the potential minimum in ethane but was rather the top of the potential barrier in ethane. So there would be a torsional strain energy in planar cyclopentane the equivalent to five times the ethane barrier, and that conclusion was stated in my 1945 *Science* paper.

Pseudorotation

Pitzer: Actual cyclopentane is not planar, and it has a very interesting property which I called pseudorotation in the later, more detailed paper.[2] That implies that there is no single preferred location

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[1] K.S. Pitzer. Strain energies of cyclic hydrocarbons. *Selected Papers*, pp. 70-72.

[2] J.E. Kilpatrick, K.S. Pitzer, and R. Spitzer. The thermodynamics and molecular structure of cyclopentane. *Selected Papers*, pp. 74-79.

of the nonplanarity of the puckering around the five-membered ring, but instead it can rotate around the ring with essentially no change in potential energy. This is a very novel motion which has appeared for a few other molecules through the years, but I think this is the first example.

We worked this out in considerable detail, and actually in the later paper, refined the numerical calculations and I think corrected an error that was significant numerically but not in terms of the general picture.

The cyclohexanes are actually of more general interest. There, if the molecule were planar, the bond angle would be 120 degrees, which is too large. The tetrahedral angle was a little less than 110. Therefore, it had been understood certainly since the 1920s that cyclohexane would be nonplanar, would be puckered, and that there were two plausible geometries, which had been named in English the "chair" and the "boat". The chair had three carbon atoms [above] and three below, and if you looked at it the right way, it did look like a chair with one atom down in the front and one atom up which would be the back of the chair, with respect to the other four. And the boat has the two odd atoms both up or both down for a capsized boat.

These, in the absence of torsional strain or internal rotation potential, would have the same energy, and that had been the accepted picture.

Hughes: When was that data worked out? Prior to your entry into the field?

Pitzer: The Baeyer strain, that is, the bond angle strain, had been worked out in the 1920s, in large measure by Baeyer in Germany, but it was in all the organic textbooks by the 1930s.

Hughes: Did your colleagues accept the concept of pseudorotation?

Pitzer: Well, it was accepted I think with very little controversy. And as I say, a few similar cases have come up for which the term pseudorotation has been adopted, even though they are not exactly the same as the cyclopentane case. The five substituted molecules, like phosphorous with five chlorines, nominally has a structure with three of them in a plane and one chlorine above and one below. But it turns out with very little extra energy that molecule can rearrange itself so that a different atom is in this polar or axial position. Now, this is a more complex thing, but the term pseudorotation has been used for that peculiar type of rearrangement. In that case, it is not completely free. There is a potential barrier between the different positions, but it's pretty low.

Hughes: You don't mind the term being used that way, even though you didn't conceive it for that instance?

Pitzer: No, I don't mind, as long as they call it type II or something like that. [laughter]

Hughes: So type II tells you that there is a potential barrier?

Pitzer: No, a substituted cyclopentane will have a potential barrier. The PCl_5 or "type II" is just geometrically different--similar in a very broad sense, but not in a more detailed sense.

Determining the Structural Pattern

Pitzer: Now, if you put two substituents, two methyl groups or two chlorine atoms, on the cyclohexane molecule in a chair configuration, it turns out that one of these substituents on a given carbon atom is--well, if one defines an approximate plane for the six-carbon atoms that are actually puckered up and down from the plane, then one of these substituent atoms is more or less in that plane, but not exactly. The other atom is perpendicular to the plane, with that substituent for three of the carbon atoms of the cyclohexane ring up, and the other three down. In other words, one set of three is above the plane and the other set of three below the plane.

Well, these conclusions essentially followed from the idea that there was a substantial ethane-like potential barrier for the six-ring bonds in the cyclohexane. In the boat geometry, two of those bonds are at their maximum in energy for an ethane-like potential, and thus one would expect the boat form not to be present in any appreciable amount, because for the chair form, all of those bonds are in their staggered or potential minimum orientation. So actual cyclohexane molecules are going to be predominantly in the chair geometry and their substituents will be located as I outlined a minute ago.

Hassel in his electron diffraction work verified this or found this structural pattern; if you wish, verified the predictions which I had not explicitly made but could have made, and others could have made after our work of 1936 and '37. Our more detailed calculations verified this and worked out in some detail the more precise energies that might be expected for these various substituted cyclohexanes.

Labeling the Substituents

Pitzer: There were two or three interesting sidelines that arose. One, it turned out that the labeling of some disubstituted cyclohexanes as accepted in the literature were wrong. They had been labeled as if the cyclohexane ring was flat, planar, which it isn't. Therefore, cis and trans disubstitutions, in other words, the two substituents, were both above the plane, or one above and one below for trans, [and] the labeling would follow the cis and trans similarities for as detailed, similar measured properties.

It turns out that the important question for these properties that were the basis of labeling the substituents were whether the substituted atom, say a methyl group, was in an equatorial geometry or in the polar or axial geometry. That was one rather interesting development, and there is a brief publication about that.[1]

Terminology

Pitzer: Long after the structural facts had been accepted, a question arose as to what the terminology should be. There is a brief paper in 1954 on that subject which has four authors.[2] Hassel had used some Greek names and Greek letters for the geometry of the substituent that is more or less in the plane, or more or less along the axis perpendicular to the plane. I had preferred "polar" for the ones along the axis and "equatorial" for the ones more or less in the plane. But other people didn't like my choice of polar, because polar is also used to mean electrical polarity, with positive and negative partial charges.

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[1]K. S. Pitzer and C. W. Beckett. Tautomerism in Cyclohexane Derivatives; Reassignment of Configuration of the 1,3-Dimethylcyclohexanes. *Selected Papers*, p. 73.

[2] D.H.R. Barton, O. Hassel, K.S. Pitzer, and V. Prelog. Nomenclature of cyclohexane bonds. *Selected Papers*, p.88.

At an international chemical meeting in Stockholm in 1954, Professor Vladimir Prelog, who had not been involved initially in the work on these molecules but was very much aware of it, was at the meeting. He was a professor at Zurich in Switzerland. We happened to be staying in the same hotel and riding a bus to the meeting site, and in the course of those bus rides, he suggested that he take the lead in putting together a publication which would recommend my term, equatorial, rather than Hassel's Greek term, but would recommend the word "axial" instead of "polar" for the other position. He thought everybody would be happy with that, and I certainly was happy with it. I realized that it was a better choice.

So Prelog apparently obtained Hassel's agreement, and Barton in England had been interested in the same topic and disliked my polar term, but was quite happy with equatorial. Barton eventually shared the Nobel Prize with Hassel. So in due time, in *Science* and in at least one German journal, this brief recommendation about nomenclature was published.

Decision to Leave the Ring Molecule Problem

Pitzer: This whole question of calculating the energies of ring molecules, including more complex structures, was one that developed quite rapidly in the 1950s, and Barton was very active in these more complex structures. I was not enough of an organic chemist to feel anywhere near as much at home with that work as he did, and Prelog did, and others. So I essentially watched that with interest but didn't participate in it further.

Hughes: I gathered from your explanation of the move from internal rotation to the consideration of ring molecules that the same sort of approach in a more fragmented way could be applied to ring molecules. So what was your hesitation about going further with ring molecules and organic molecules in general?

Pitzer: Well, I think it was the type of information that was available. That is, for ethane and for the simple open-chain molecules, we had essentially physical chemical data--low temperature heat capacities leading to entropy values, measured heat capacities, spectra in sufficient detail to determine all of the vibration frequencies, or at least all those of low enough frequency to be important thermodynamically. For hydrocarbons, electron diffraction was not useful. In those days, electron diffraction didn't pick up the hydrogen locations. But once one went to substituting chlorines for hydrogens, then electron diffraction was useful in determining structures.

This sort of information was also becoming available with Hassel's work, for example, for the cyclohexane molecules with as many as two substituents, and various people were making the more detailed thermodynamic measurements, and we did some of that ourselves. For the more complex condensed ring molecules and so on, this type of physical information was not available or was too complex. The overall situation was too complex to interpret it. Barton and Prelog were interpreting the sort of measurements that organic chemists make in a manner that was undoubtedly correct most of the time, but it involved different types of reasoning as well as different types of measurements that other people were just more expert at than I was. And I found other interesting things to do, so I left it to them.

Hughes: Hassel and Barton are organic chemists?

Pitzer: Barton and Prelog. No, Hassel was just as much a physical chemist as I was. And he didn't pursue it into more complex structures.

Other Aspects of Ring Molecules

Pitzer: I should add a few more words about other aspects of ring molecules. Cyclobutane is a case like cyclopentane, except that you would expect it to be strongly planar, on the basis of the 90-degree bond angles being much less than tetrahedral. On the other hand, it is true that the hydrogen atoms are lined up in the opposed geometry in the planar configuration just like cyclopentane, so there would be four times the ethane barrier in the first approximation as the strain energy there.

One of my very first graduate students, William Gwinn, who was on the faculty by that time at Berkeley, took up the question of was cyclobutane really planar or not and its whole array of properties. He started this work roughly in the '49-'51 period when I was with the AEC [Atomic Energy Commission], but I was aware of it. It was completed after I was back at Berkeley, so they were kind enough to put me on as the co-author, along with [G.W.] Rathjens and [N.K.] Freeman.[1]

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[1] G.W. Rathjens, Jr., N.K. Freeman, W.H. Gwinn, and K.S. Pitzer. Infrared absorption spectra, structure and thermodynamic properties of cyclobutane. *Selected Papers*, pp.89-97.

It turned out that cyclobutane is not planar. As the paper shows, there is a lot of detailed vibrational spectra, and when the strain energies are calculated out, with considerable uncertainty, it's consistent with the idea that the cyclobutane molecule is not flat.

As I mentioned earlier, in 1959 we did refine the cyclopentane calculations a bit, but that didn't really change things in any very significant manner.

Structure of the Cyclopentane Molecule

Hughes: Did the earlier work change things in a significant manner for cyclopentane?

Pitzer: Oh, yes. In other words, it had been thought to be planar, and it's puckered--but in this very interesting way that the pattern around the ring that goes out of the plane is not localized. There are two somewhat simple types of puckering that you can suggest for cyclopentane. One, put four atoms in the plane, and the fifth one either above or below. Or, you can define the plane with three atoms, and then the other two adjacent to one another you can twist, with one above the plane and the other below the plane.

It turns out that the release of internal rotational strain and the addition of bond angle strain for the two is exactly the same, to within a relatively high accuracy. You can define a more general type of puckering of cyclopentane in which the departure from some sort of an average plane rotates around the ring. It turns out that this is essentially a free rotation, not of a physical particle, but of a geometrical anomaly, if you wish. And that's what I called a pseudorotation. Its quantum mechanics and statistical mechanics are essentially that of a rotation. There is a pseudo-moment of inertia or mass factor in the potential function for the amplitude of the puckering, and no potential energy associated with the location. So there is an effective, as I say, moment of inertia or mass for this otherwise free rotational motion.

Using Others' Data

Hughes: I'm wondering how much of this data you had to create, you had to innovate, and how much of it existed prior to your entry into the field.

Pitzer: Well, for the most part, our own measurements were thermodynamic, measurements of the heat capacity in the condensed phase from roughly 15 degrees Kelvin up to whatever temperature the material melted, say, and then its heat of fusion, and then the heat capacity of the liquid, and then the heat of vaporization. Sometimes there were solid transitions that had thermal effects too. We also measured heat capacities in the gas phase. If I remember rightly, Gwinn's thesis involved in part building a calorimeter and measuring gas heat capacities. He did a lot of theoretical calculations too.

We did not ordinarily make spectroscopic measurements, either infrared spectra or Raman spectra, but we made full use of them and regarded ourselves as capable of judging the correctness of the interpretation that the original author had given for the measurements, or of resolving differences between different interpretations or different sets of measurements that might be in the literature, and we were in communication with people that were currently making those spectroscopic measurements.

Hughes: So you decided that there was no point in recreating the data?

Pitzer: Yes. That is, I had no opposition to making such measurements, but there's a limit to how much you can do. Likewise, if electron diffraction would clarify something, other people were measuring electron diffractions so we didn't get into that ourselves.

Hughes: So you did what needed to be done.

Pitzer: Yes. And we selected problems where, with our measurements plus what was already available, one could do a quite complete description of the structure and properties of the molecule. One of the things one can do is calculate properties at higher temperatures or other conditions where there hasn't been any measurements. And as I say, I was in communication with other people, so that if we were interested in and had a relatively good information basis but something was missing, I could frequently persuade somebody else to measure it fairly promptly.

Quantum Mechanical Calculations

Hughes: From what I gathered from your discussion today about internal rotation, there had been a problem with the approximations which did not pick up the potential barrier. So in that particular case, it was not a question of coming up with more accurate data; it was a question of missing important information. Now, was that a danger here too, that it wasn't just a matter of having a more refined picture of what was going on, you could actually be missing phenomena?

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Pitzer: The quantum mechanical calculations for the first stage, starting with electronic motion, which were not yet feasible for ethane, although people thought they might have drawn some conclusions for them, were just completely impossible for even slightly more complicated molecules. In other words, for things like cyclopentane or cyclohexane, it was completely out of the question in those years to do those electronic calculations. Even today, you'd need a supercomputer, and it would strain it to get meaningful accuracy.

So once one had gotten away from the simplest structures that had a certain characteristic situation, one was dealing totally with an experimental database, and an interpretation of it in terms of the potential for motion of nuclei of the atoms as a whole inferred from either the experiments on that molecule or from simpler molecules, and that's what we've been talking about. In other words, if one assumed that if the atomic situation was about the same as in ethane or propane, where you put in one extra carbon atom and extra hydrogens, but along either of the single bonds, it's mostly hydrogens, and one found experimentally that the potential barrier was about the same as it was in ethane. One solved the quantum mechanical problem for the heavy atom motions for these more complex molecules, but that was mainly an insistence in interpreting the spectrum.

Then for thermodynamic purposes, if you had the complete set of energy levels experimentally from spectral measurements, or inferred from heat capacity and entropy measurements, you had essentially a complete picture for statistical thermodynamic purposes.

Corresponding States

Background

Hughes: Do you want to talk about corresponding states?

Pitzer: Well, the first paper on corresponding states, so far as I was concerned personally, goes back to '39.[1] The concept is more than 100 years old now. It goes well back into the 19th century.

Hughes: Explain what it is, please.

Pitzer: It concerns the properties of a fluid that has both gaseous and liquid states. The properties of such fluids are such that if you heat them, under some confinement, eventually the difference between the vapor and liquid states disappears at a critical point. Where if you heated it along a path where both liquid and vapor are present, the vapor gets more dense and the liquid expands and gets less dense, and eventually at the critical point, they have the same density, and there's no difference any more.

If you compare the properties of fluids on what we call a reduced basis, in other words, at temperatures the same ratio to the critical temperature and at pressures or densities at the same ratio as the critical density or pressure, the idea of corresponding states is that the properties would all be the same. And of course, qualitatively, they are the same, and semi-quantitatively, they are, but the accuracy to which they're the same is not very high. These are properties that can be measured rather easily with rather high accuracy, and so it was very soon recognized that the corresponding states concept was not an accurate general principle.

Nothing much changed between about 1890 and around 1930, in this regard. But with the development of quantum theory and more detailed structural information, quantum theory for intermolecular forces--in other words, forces between molecules--it became feasible to examine that situation further. That 1939 paper was based on the idea that by now one could say from this structural background that the inert gas elements, at least the heavier ones, say argon, krypton, xenon, maybe neon, and probably methane, which is not strictly spherical but where the average external interaction is essentially spherical, should show "corresponding

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[1] K.S. Pitzer. Corresponding states for perfect liquids. *Journal of Chemical Physics* 1939, 7: 583-590.

states" behavior. Methane, for example, begins to rotate in the crystal way down at around 14 degrees Kelvin, and by the time you're talking about 200 or 300 degrees Kelvin, any nonspherical aspect of methane is pretty trivial.

I set helium aside and maybe neon, because even for translational motion, there are quantum effects. For helium, this is a major effect. The properties of liquid helium have peculiarities because of quantum mechanical effects. For argon, these have become completely negligible, and the translational motion is a classical matter with some interacting potential when the atoms get too close together. For neon, there is some quantum mechanical correction needed, and so it was best to leave neon out, but for the additional reason there was less known about it anyway. [laughs]

Substances Following Corresponding States

Pitzer: So with this much background--which was not background at that time; it was put together from recent knowledge--I decided that there was enough information on--at least, as I recall, it was those three. Let's see here. [refers to paper] Oh, xenon, not krypton. That was just a matter of was there available information? Argon, xenon, and methane ought to show corresponding states of behavior, and so I got the data out. Oh, I did include krypton. It's not on one graph, but it's in the table. Neon is in the table too.

The data were consistent with corresponding states of behavior, with a small departure for neon, so it was best to make it a special case. This was true for a number of properties: the reduced densities, entropies of vaporization, the whole series of properties. And I did discuss this question of quantum mechanical correction effects and showed that they were appreciable for neon but not large, were large for either hydrogen or helium, and were pretty much negligible for methane and anything heavier.

Well, this was a significant advance. An Englishman, E. A. Guggenheim, did much the same thing at about the same time. I think my work preceded his by a little, but I could even be wrong about that. But that was a beginning. You knew what substances were supposed to follow corresponding states, but that wasn't much of what you were interested in. You were interested in a lot more substances that didn't quite fit.

One or more chemical engineers actually suggested that a third parameter, a third coordinate, for a family treatment of fluid properties could be based on the properties at critical point. It could be the ratio, the PV [pressure volume] over T [temperature] ratio, for example, which would be the same for any group that was following corresponding states. And indeed, that pressure times volume divided by temperature ratio does decrease as you go from argon even to nitrogen, and certainly to carbon dioxide or water or anything else.

The trouble with that is that the critical volume is not accurately measurable. [laughs] The critical pressure and critical temperature are measurable quite accurately, but the volume is the thing which has just become the same from the liquid side to the vapor side, and it has zero slope at that point, and measuring something that has zero slope is very difficult.

Paired Theoretical and Empirical Papers

Pitzer: That's about the situation at the time I proposed the acentric factor approach in about the mid-fifties. And as I've done in some other cases, the initial publication involves two papers.[1] There is one in which I examine pertinent theory and theoretical calculations that seemed to illuminate the subject, and then in the second paper, I adopt some definite but empirical definitions, and then treat the data for a large number of systems. In the first paper, I make use of a number of concepts about what are the essential differences in molecular structure, interactions of molecules, that would affect this pressure-volume-temperature relationship. I describe how it would affect the intermolecular potential, in other words, the energy, depending on how far molecules are apart, and how it would be affected if it isn't a simple distance question but also if the molecules are nonspherical, their angular orientation.

The number of possible causes for departure from corresponding states are multiple. In other words, it could be nonspherical shape, or it could be a sort of globular shape in which the attracting centers are out on the periphery rather than

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[1] K.S. Pitzer. The volumetric and thermodynamic properties of fluids. I. Theoretical basis and virial coefficients, and II. Compressibility factor, vapor pressure and entropy of vaporization. *Journal of the American Chemical Society* 1955, 77: 3427-3433; and 3433-3440. *Selected Papers*, pp. 296-302 and 303-310.

in the center of the molecule. Or it could be electrical polarity, or it could be any combination of those, or maybe something else. Hydrogen bonding such as occurs in water and so on is an extreme polarity type.

It's hard to do the calculations even now, but they're quite feasible now, but at that time, it was impractical to do detailed calculations for the entire pressure-volume-temperature behavior for a more complex type of molecule. What was feasible was to calculate what's known as the second virial coefficient, which was the first order departure from the ideal gas law, which is caused by intermolecular interactions.

I found in the literature very interesting calculations of a Japanese, [looks through papers] T. Kihara. These calculations had just been published in the preceding two or three years. He assumed molecules of different shapes, but with a simple potential applying to the shortest distance between these shapes rather than to the center of the molecules. And then you could have linear molecules, or you could have spherical molecules of some size, or you could have other shapes--squares, rectangles, whatever you wished. And he worked out a number of these as far as the second virial coefficient was concerned.

I found that the effect on the second virial coefficient for these different shapes was essentially the same, or let's put it this way: they fell on a single line going away from the spherical simple molecule origin. If it was length, why, you had one departure; if you had a different shape, the amount of deviation from a point would be different; but they would all fall in a single family. By that time, there was also some information on electrically polar molecules, even without regard to other change in shape, and although that didn't follow on exactly the same family, it was pretty close.

So arguing then just from the second virial coefficient behavior, it seemed as if the deviation from corresponding states could fall into a single family with a single third coordinate or third parameter. That's the first and theoretical paper.

For the second paper, I enlisted several students, including Robert Curl, to do the numerical work with data from the literature about properties of various substances of potential interest. And the question was, What should be chosen as the measure of departure from corresponding states? It seemed to me that the vapor pressure properties were much more accurately measurable than the PV/T at the critical point, which had been used in this respect. The vapor pressure is one of the most easily and most accurately measurable quantities. So I proposed this definition involving the logarithm of the vapor pressure at a given ratio of temperature to the critical temperature, and as a departure from that quantity for the simple argon-methane pattern. So this was defined as the third parameter for fluid properties.

Actually, as I recall, it was some time before I decided on what to call this parameter. Somebody suggested this acentric factor name, which I adopted and has been widely used in the profession since. It was defined in terms of the standard distance from the critical point, but actually, the vapor pressure property all the way from the critical point down in temperature is very relatively simple and clear-cut, and a measurement anywhere along that range is adequate to determine the acentric factor.

Then we went over the various types of experimental data. The second paper involved, in addition to the vapor pressures, the compressibility factor--in other words, pressure times volume divided by temperature. The result was a whole series of tables for the compressibility factor for the simple fluid, the argon-methane fluid, and another table of the departure or change in that factor per unit of acentric factor.

This was worked out in considerable detail and with some special considerations in the region which is close to the two-phase region, with both liquid and vapor present, as well as the properties with two phases actually present, in addition to the vapor pressure, and the entropy of vaporization, and so on.

In a third paper with Robert Curl[1] we gave further consideration to the second virial coefficient with an empirical equation which was to be more useful than the Kihara formulation, although the territory was somewhat the same. And then in a fourth paper[2] the enthalpy and entropy properties were dealt with. There was enough experimental information for a number of substances so that good tables and graphs could be worked out.

So that this became a body of simple equations and rather extensive tables to give the properties of almost any fluid, gas or vapor in a reasonably accurate manner.

Hughes: So you were systemizing a field?

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[1]*Selected Papers*, pp. 311-312.

[2]*Selected Papers*, pp. 313-322.

Pitzer: Yes. In other words, instead of having separate tables of detailed properties of maybe fifty different substances, in addition to the critical temperature and critical pressure, one had this third property, the acentric factor, and this set of tables, and you could calculate more or less whatever you wanted to know.

There are at least two additional things I should say about this. [refers to his *Selected Papers*]

L. Riedel's Work on a Third Parameter

Pitzer: A man in Germany, L. Riedel [spells], published essentially simultaneously, 1954 to '55, '56, a series of papers on essentially the same idea of a third parameter or property that would systematize fluid properties. Interestingly enough, there was practically no duplication between the two.

Hughes: Which was fortuitous.

Pitzer: At the early stages, at least. Instead of the vapor pressure on a reduced basis at a substantial distance away from the critical point, which I chose for the acentric factor, he chose the slope of the vapor pressure curve <u>at</u> the critical point. Well, that's a lot harder to measure. It's not only harder to measure, but in terms of hundreds of different fluids, it's not in the books already, whereas the vapor pressure down near the ordinary boiling point, the one atmosphere boiling point, is in the books already.

Hughes: So you deliberately chose that point.

Pitzer: I had deliberately chosen it that way so that it would be as useful as possible. So the net result was that my recommendation and term and so on was the one that was adopted by the community generally. But his contributions, as I say, were all positive and in the same general direction.

The key thing that made it so widely available and useful was that the third property could be determined, literally if you only had the boiling point temperature, provided you also had the critical temperature. And the critical temperature is less available than the boiling point temperature, but having those two was enough to get the essential factor.

Hughes: Why do you suppose that Riedel had used a point that was more difficult, and also ultimately less useful?

Pitzer: Well, the only thing I can say to that is, since the critical point is the unique point in fluid properties, there was naturally a tendency to look at things close to the critical point as you started to generalize. You see, previous to either Riedel or myself, there was a chemical engineer, I think in Wisconsin--I've forgotten his name--and a few others, who used the compressibility factor, the PV over RT factor of the critical point, as the third parameter. Well, now, that is no harder to measure, but it's hard to get it accurate.

Riedel's was an advance in that the slope of the saturated vapor line close to the critical point was at least something that could be measured much more accurately, if you had measurements in that region. And, of course, having located the critical point, you presumably had some measurements in that region. But again, it was a less convenient measurement than the one that I found and used and proposed.

Hughes: And you chose to measure where you did precisely for those two reasons?

Pitzer: I was working essentially with existing data. We didn't make many measurements ourselves.

Hughes: Yes, but you, like Riedel, could have chosen to measure at the critical point. So my question is, you did not because it would be less useful?

Pitzer: That's right. My first paper was exploring the theoretical side, and I didn't adopt a working measurement there, a working definition. It was only in the second paper where we were actually dealing with a lot of examples that we adopted the acentric factor definition.

The Acentric Factor and Chemical Engineering

Pitzer: As I said, this has been widely adopted by mainly the chemical engineering community, and they invited me to review this. It was published in '77, under the title "Origin of the Acentric Factor."[1] I guess the meeting might have been the year before. This was the opening talk at a special meeting for the chemical industry on phase equilibrium and fluid properties.

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[1] *Selected Papers*, pp. 278-287.

If anyone wants an easier, more general discussion of that subject, I put that paper at the beginning of this section [on "Extended Corresponding States and the Acentric Factor" in *Selected Papers*]. It's historically out of order, but I thought it was useful. [moves to bookshelves] But you take a book like this one, *Properties of Gases and Liquids* [by Robert C. Reid], the chemical engineers' book on the subject. It's a very widely used, convenient book for the properties of liquids and gases for the chemical engineering community, and here right at the beginning--this is right out of our initial publication--the tables.

Hughes: Verbatim.

Pitzer: Yes. Maybe not, but the acentric factor is in right at the beginning. Long tables here of the pure component parameters for large numbers of fluids. The critical temperature is the first property given, and the acentric factor is the second property of the table. And then more specialized properties come thereafter.

Detailed Numerical Equations

Hughes: Well, anything more on corresponding states?

Pitzer: Yes, I could add a little more.

As the computer developed, quantitative detailed numerical equations with many terms and many parameters, which are required to quantitatively give the fluid properties within approximately experimental accuracy, had now become reasonably convenient. Those equations that many people developed frequently, more often than not involve the acentric factor along with the temperature and either the pressure or the density as variables, so that they applied not just to one fluid but to a whole array of fluids. If you want the maximum accuracy, of course, you have an equation for each individual fluid. And one of the most important fluids, water, [laughs], steam, doesn't fall within the acentric factor family. It's too-polar. The water-water interaction is too dominated by hydrogen bonding to fall within the acentric factor family. But so many things do.

We have had some role in developing equations of this moderately complicated type, but explicitly including the acentric factors as one of the characteristics to be used. But many other people have been involved in that at least to an equal degree, if not more, so that I don't know that it's very useful to try to discuss it further.

IV POSTWAR EXPANSION OF THE DEPARTMENT OF CHEMISTRY

Wendell Latimer's Efforts

Hughes: There's a subject which I think we should interject here, and that is the attempts to expand the Chemistry Department after World War II. I came upon a letter that Latimer wrote in 1945 to Robert Gordon Sproul, who of course was president of the university at that point, asking him to support departmental expansion.[1] He placed it in the light of the development, which I assume was somewhat accelerated by the war effort, of the chemical industry and atomic energy. His argument was more or less along the lines, "If we are going to continue to supply brainpower to these efforts, then we've got to have a larger department in general, and higher salaries, and we've also got to expand organic chemistry."

I just wondered if you had any insight into the changes in the department at that period.

Pitzer: Oh, yes. I was in close communication with Latimer about that sort of thing. Of course, he was the leader in this. You have described his position quite accurately. He felt that the department, without criticizing it with respect to the past, from the point of view of the future, it had been too narrowly a physical chemistry department, per Lewis's choice, with adequate teaching in other areas of chemistry, but not very much research, doctorate-level activity, and so on. Latimer thought that for the future, it ought to be much more comprehensive in covering all important sides of chemistry.

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[1] Wendell Latimer to Robert G. Sproul, September 14, 1945. (Bancroft Library, University Archives, CU-5, 1945, 400-Chemistry)

Organic Chemistry

Pitzer: One that he noticed first and pushed in this letter was organic chemistry, which led to the appointments of James Cason, who was about my age or a little older, and then a year or so later, after he got this authorization I presume from Sproul, of William Dauben and [Henry] Rapoport who are both still here, retired but active. And additional people. They came in soon after the war. They had done some postdoctoral research, I guess, during the war period.

Chemical Engineering

Pitzer: The next letter that Latimer wrote I presume said essentially the same thing with respect to applied chemistry and chemical engineering. That was implemented a little later, and Philip Schutz was the first person brought in, and Charles Wilkie at a junior level soon thereafter. Schutz was taken ill very soon after he came, and died, and had to be replaced at a somewhat senior level. I was fairly heavily involved in that recruitment of Ted Vermeulen who became our senior chemical engineer.

Pitzer's Efforts in Organizing the Departments of Chemistry and Chemical Engineering

Pitzer: When I came back from the AEC and was then dean of the College [of Chemistry], I think one or two other appointments had been made and I thought that chemical engineering was sufficiently distinct a profession that it ought to be recognized as a department. So I reorganized the college in terms of two academic departments, chemistry and chemical engineering, but with the dean still supervising a lot of joint services in support--machine shops, electronic shops, and all that sort of thing. The dean essentially represented the college to the by now chancellor, originally the president directly, but each department chairman was in a position to deal with colleagues elsewhere in his profession on the basis of appropriate stature.

There was an interesting contest with the College of Engineering over the chemical engineering side. Morrough O'Brien was then dean of engineering, and the College of Engineering had not done anything with chemical engineering prior to World War II. In chemistry, we had taught a couple of courses that were an appropriate part of chemical engineering but didn't constitute an adequate program in the eyes of the chemical engineering profession generally, although a lot of graduates of Berkeley became chemical engineers on that basis.

Hughes: Were those courses strictly in chemical engineering?

Pitzer: Well, they were in what you could call applied chemistry. They would be an appropriate part of a chemical engineering program, but didn't constitute a full chemical engineering program, I would say. Now, the student of those years could get the other parts over in the College of Engineering reasonably satisfactorily, and did, but it was not really a good arrangement.

The College of Engineering also made two or three appointments. They were unable to get the words "chemical engineering," so they called the courses "process engineering," which was more or less a synonymous term, but not the most generally recognized one.

Hughes: Why were they stopped from using the term?

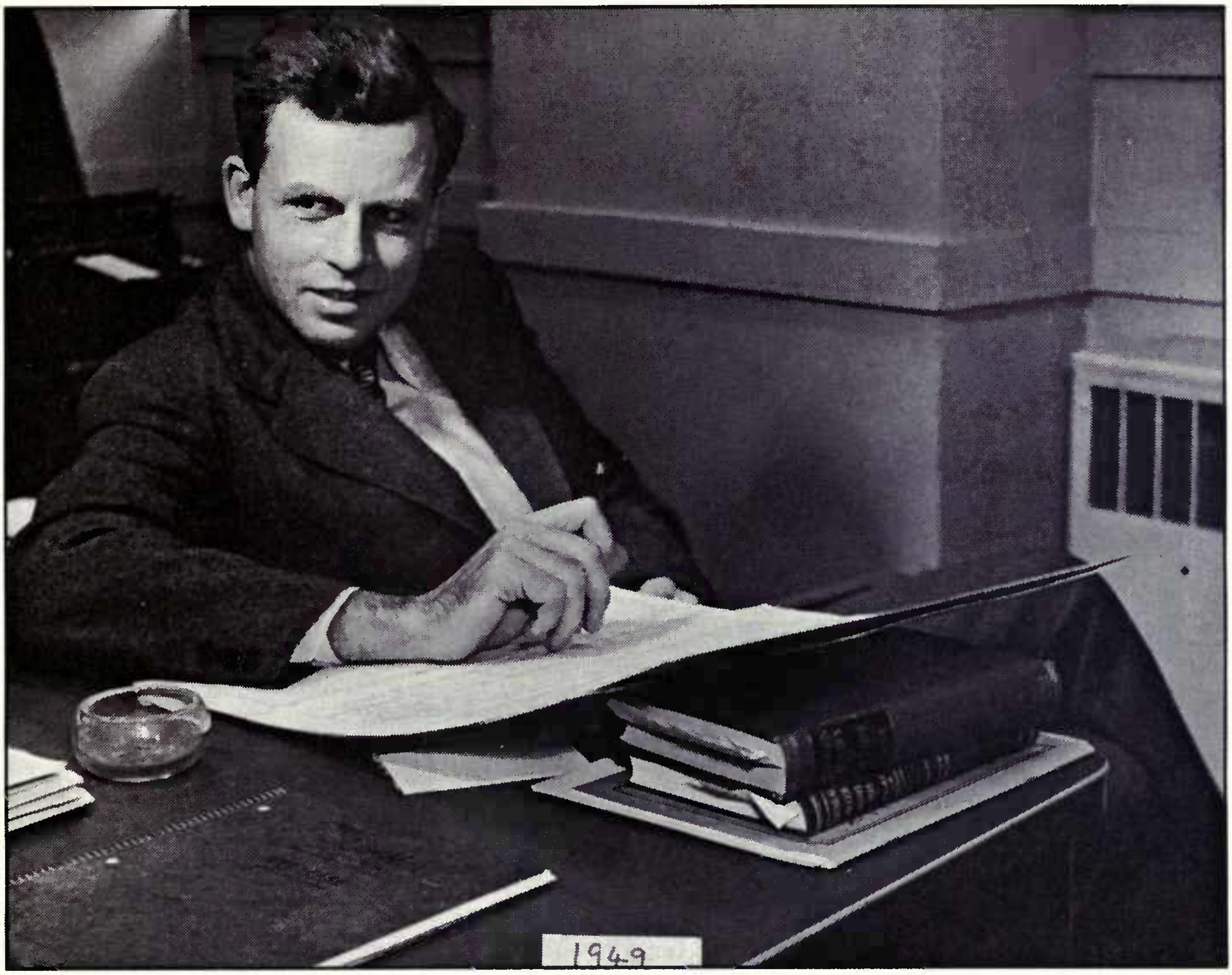
Pitzer: Well, because we had gotten Professor Schutz appointed as a professor of chemical engineering, and the academic authorities thought that if an appointment were made in the College of Engineering, it ought to use a different term, I guess. I'm not privy to all the arguments that went on. At the time I became dean in the fall of '51, these appointments had been made.

In other words, the early steps in this process, I was aware of, but not necessarily in full detail, so far as communications to President Sproul were concerned. But we were given good backing in terms of some additional appointments. In fact, I had no complaint about backing for appointments in chemical engineering. I suppose about '55 or thereabouts, I recommended the separate department structure. I don't recall that I had any particular opposition [from others] to that.

By that time, our chemical engineers had made quite a name for themselves nationally, and the people that were appointed under the term "process engineering" had essentially done their job locally but didn't have a national reputation. So they were allowed to serve out their careers in the Department of Mechanical Engineering. The Department of Mechanical Engineering has various sub-groups in it, so that they were not completely out of place there. But that's all developed very successfully, and in the last national survey, our chemical engineering was number three in the country.

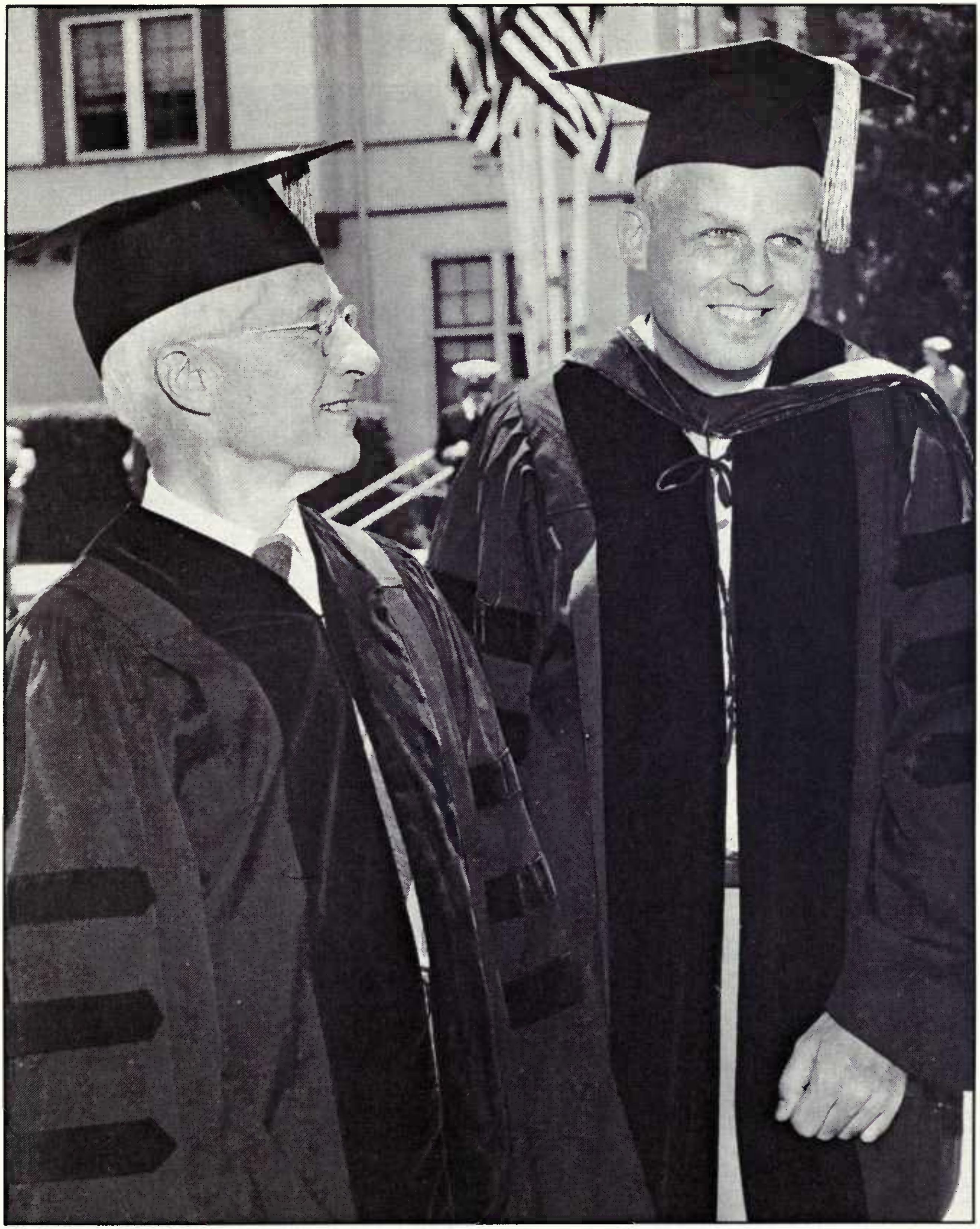
Left: Kenneth Pitzer and Grandma Sanborn, June 1922. Kenneth nearly eight and one half years old; Grandma S. nearly eighty-four years young. With boat Kenneth built.

Right: Kenneth Pitzer and Jean Mosher, Easter, 1935.



Kenneth Pitzer in his office at the College of Chemistry, 1949.

Pitzer family, Christmas 1949, Pomona, California. Top row: father Russell Pitzer, Kenneth Pitzer. Seated on couch: Ann, Jean, and Ina Pitzer (stepmother). On floor: Russell and John Pitzer.



Kenneth Pitzer with Joel Hildrebrand at the Greek Theatre, University of California Commencement Day, June 18, 1954. Pitzer is wearing G.N. Lewis's academic gown.

Photograph by Chris Kjobech

Visit of President John F. Kennedy to Rice University, 1962. (Far left: Kenneth Pitzer.)

Photograph by Ray Covey

V DIRECTOR OF RESEARCH, ATOMIC ENERGY COMMISSION, 1949-1951

[Interview 4: June 12, 1996] ##

Materials Testing Accelerator

Hughes: I believe you wish to continue your account of the years you were at the AEC that you began a number of years ago with Bob Seidel.

Pitzer: That's right. Right at the end of the Seidel interview, it refers to the MTA, and I said it would take too long to tell that story at that time. MTA stands for Materials Testing Accelerator. This is an interesting side story, with respect to the main trend of Atomic Energy Commission affairs.

To understand this particular series of events, one has to look into the supply of raw materials, primarily, of course, uranium-containing ores. As of 1949 or 1950, most had come from the Belgian Congo, where there was a rich ore that could be mined relatively inexpensively but was limited in amount. The AEC as well as Canadian-British authorities had encouraged exploration in both the U.S. and Canada, as according to the various agencies, and had offered a somewhat higher price than they were paying for the Belgian Congo product. More ore was being found, and some was being mined.

Nevertheless, [from] the consideration of the requirements from the Department of Defense and various other aspects, the prediction was of an expected shortage of uranium raw material. This was not in my department at the AEC; there was a raw materials division. But I was involved at least with the general manager, Carroll Wilson, and maybe at times with the commission. I was present when this was being discussed, certainly with the general manager, and I suspect also with the commission. I advocated offering a higher price, at least temporarily, as an incentive for further exploration for uranium in the U.S., and, for that matter, also in probably Canada, because that would be available to us under favorable terms in any international situation.

The raw materials division was reluctant to raise the price, and the general manager and the commission were reluctant to order the raw materials division to raise the price offering.

Hughes: Do you know the thinking behind the reluctance?

Pitzer: Well, the raw materials division, these were traditional mining people, and the idea of paying a very much higher price in order to get supplies of an ore was just not within their experience with respect to copper or iron or other commercial ore situations.

This assumption of a shortage in natural uranium became fairly well known in the inner circles around the atomic energy community, including the national labs and so on. Ernest Lawrence proposed producing plutonium, which is, of course, the end product from the nuclear weapon point of view, also producing tritium, which was at least potentially important from weapons point of view. He proposed producing this by bombarding with neutrons uranium in a high current accelerator, this high current accelerator to be the MTA then.

Well, from the point of view of the AEC organization, I was in the position as director of research to support Lawrence's MTA project. I could handle that in the Washington office, and I did. I don't know that I ever said to Lawrence that I hoped or expected it would never be implemented because the raw materials situation would change, but at least personally that was my feeling about the situation. However, the leverage that I had was to support the MTA project, which might well have other byproduct advantages, even though it never became a production mechanism for raw materials. Well, that's what happened [laughs]--in time.

I was actually with Lawrence one time when we drove around this area, just looking for federally owned land that might be an appropriate site for this MTA. Among other things, we looked with favor on the Livermore site, and that's how the Livermore site got transferred to the AEC, for the MTA.

I was also party to enlisting the support, the actual participation, of what's now Chevron but was then Standard Oil of California to provide what you might call engineering technical support and to be possibly the operating contractor for the MTA, if it went into commercial operation. I can remember going with Lawrence, probably with the local AEC manager, Harold Fidler, I believe was his name, to visit the president of Standard Oil, because I was aware of people down in the Standard Oil research and development organization that seemed to have the qualifications that would be appropriate and might well have the interest.

In that connection, I had John Thomas with me on leave from research at Standard Oil of California, so that I had that further contact with that organization.

Hughes: Was he the reason that you knew people at Standard Oil?

Pitzer: Well, not entirely. Many of those people had come through the University of California Berkeley, either in chemistry or in engineering, and were a fairly close group personally. While I knew John Thomas better than any other one person, I knew more or less directly quite a number of the others, so that it wasn't just through John Thomas.

Hughes: Had you taught any of them?

Pitzer: Oh, yes, I had undoubtedly taught some of them.

Well, as I said, with this actually going on, at least in the development stage, the raw materials people decided to counter, if you wish, by putting out some greater incentives for exploration, which were mostly in that Utah-western Colorado area where a lot of uranium was discovered. According to the historical record, the MTA project was finally canceled in August, 1952, and in fact, no very large amount of money was ever spent on it. Long before it was finally canceled, the rate of expenditure on the development dropped off. Of course, I was gone by the summer of 1951, so the last year was after I was away.

Hughes: So you didn't have any role in the decision to stop it?

Pitzer: No, I didn't. I didn't need to. [laughs]

Acquisition of the Lawrence Livermore Laboratory Site

Pitzer: A secondary result of the MTA project was the AEC acquisition of the Livermore site and Lawrence's interest in it for what you might call practical AEC purposes. And thus, when the question of a second weapons laboratory developed a little later, in which Lawrence was very much interested, there he had that Livermore site already in AEC hands. To have set up totally de novo a second weapons lab would have been a long and complicated process, compared to just taking over this MTA site which was no longer needed for the MTA, but was in the AEC hands and under Lawrence's influence. [telephone interruption]

Hughes: Say a little more about what was involved in acquiring the Livermore site.

Pitzer: Well, it was already owned by the federal government for some other purpose. When Lawrence and I looked around, with I think Harold Fidler, the local AEC manager, who was a very helpful and very cooperative and very wise person, it was clear to us that acquisition of something that the federal government already owned but wasn't actively using would be very much easier than to go out and try to buy private land, which the owner might not want to sell, or at least he'd want a high price for it. I think we found one or two other possible sites. But everything considered, the Livermore site, which is nice, flat land, near transportation--actually a couple of railroads going by, not that we needed the railroads, but good highway connections and so on--the railroad might be useful, you know, for some very heavy, large object. It just seemed to be by far the most attractive site. I don't remember what other federal agency owned it at the time, but as I recall, there was no great difficulty getting the transfer.

Hughes: When you used the term "practical AEC purposes" in regard to Lawrence's comment, was he meaning weapons?

Pitzer: I was using that as a somewhat more general phrase than just weapons. This is a statement I'm making from a retrospective point of view, not as something that was actively thought about at the MTA time. But for example, during the war, Lawrence was promoting the electromagnetic separation of isotopes on a weapons-scale, commercial-scale level. This was impractical on the Berkeley site, and it was done at Oak Ridge [National Laboratory]. If there had been some practical infrastructure and the Livermore site had been available at that time, I would estimate that it would have been done at Livermore.

But Oak Ridge had the advantage, of course, that both the laboratory and the diffusion isotope separation processes and the early reactor at the laboratory were going to be at Oak Ridge, so that, again, the infrastructure for that type of an operation was going to be set up there anyway. This electromagnetic separation process was carried out in a third subdivision at Oak Ridge. And most of the research and development for that did come from Berkeley, but it was implemented there.

Well, I think that's the MTA story, as far as I'm concerned.

Decision to Develop the Hydrogen Bomb

Pitzer: Now the question of the decision to develop, as we called it, "the super", the nuclear weapon based on fusion as well as fission, has of course been widely discussed now in various places, and this was primarily a weapons division problem--"military applications" within AEC terminology at the time. But again, I was in the communication channel and having my say, to some extent, in the background rather than in the most officially recorded places, although my general views are pretty well in the record there.

The account in this Volume II of the history of the AEC is generally very good.[1] I've reviewed it recently. There is only one real correction I want to make; otherwise, I'll just amplify with some personal comments and memories. For background, just recalling briefly, this all came up very suddenly after the U.S. Air Force discovered that the Russians, the Soviets, had set off an atomic bomb. The effluent in the atmosphere was captured by airplanes carrying filters, and it was identified unquestionably.

This happened a lot sooner than anybody expected it to happen, and it caused within the AEC circles quite a sensation and a questioning of what should be our response. The general advisory committee was called for a special meeting on October 28 and 29 of 1949. For the morning session on the 29th, it is said in Volume II of the history that the division directors were present. In fact, the division directors arrived expecting to attend the meeting but were told they were not to attend.

This was unprecedented as far as I was concerned. I had always attended any GAC [General Advisory Commission] session on a topic to which I could possibly contribute, except for an executive session of the committee alone, or of the committee with the commissioners. Thus, as I say, it was unprecedented that I'd be told I wasn't invited.

It was clear to me immediately that [Robert] Oppenheimer wanted to exclude me. He wanted to prevent me from presenting my views, or explaining and amplifying Lawrence's and [Luis] Alvarez's views, which I knew, to the other members of the GAC that did not have that background independently. At least, many of them did not.

Hughes: Now, explain why Oppenheimer would not want you to talk.

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[1] Richard E. Hewlett and Francis Duncan. *Atomic Shield, 1947/1952*. University Park: Pennsylvania State University Press, vol. II, 1969.

Pitzer: Well, the position that the committee took at that time, under Oppenheimer's leadership, and I have since learned even more strongly than I realized it at the time that James Conant was active in advocating that position, namely, that the U.S. should not start, shall we say, an emergency or vigorous program on thermonuclear weapons, that they should rather only explore it in a more relaxed or routine way. I am not prepared to explain why they believed that. This is in the books; you can read it in any one of a number of places.

Shortly after Lawrence learned of the Russian test, he and Luis Alvarez--Ernest Lawrence and Luis Alvarez had, if I remember rightly, gone by Los Alamos [National Laboratory] to pick up the thinking there about the thermonuclear possibilities. They had then come to Washington and talked to me as a preliminary as to what was going on within the AEC headquarters, sort of unofficially, while deciding what other contacts to make.

It happened that Alvarez had a second interest in some aviation-related question with certain members of Congress, but then Lawrence certainly had access to the chairman of the Joint Committee on Atomic Energy, Brian McMahon, so they did meet, not in an official committee session, but informally with two or more congressmen, or senators and congressmen, and I think this is all in the official history.

But by the time the GAC arrived, it was known, I'm sure to Oppenheimer and presumably also to Conant, that Lawrence and Alvarez were vigorously advocating an accelerated--I don't know whether you'd call it "all out"; at least a strong--exploration of the possibility of thermonuclear weapons, and that I had been at least in contact and presumably in agreement with Lawrence and Alvarez. There were nuances or details in which I actually did not fully agree with Lawrence and Alvarez, but those were secondary and were not really important in this. I thought their major message deserved to be heard.

Hughes: You don't think that your differences should be brought out?

Pitzer: No, I don't think so. I don't even remember them too well. Well, I do too remember. You see, a thermonuclear weapon would involve undoubtedly large amounts of tritium, as well as other thermonuclear fuel. In other words, the thermonuclear reaction would probably be a deuterium-tritium reaction, and deuterium you could get easily enough, but tritium would be hard. And they were thinking in terms of a special nuclear reactor, or reviving something like the MTA or something else. But they were really thinking of a nuclear reactor for the tritium production.

I thought that might be desirable, but that was not for Alvarez and Lawrence to do. They weren't "reactor" people. If you wanted a tritium-producing reactor, why, the Oak Ridge people or the Argonne [National Laboratory] lab people would be the people to make it. But that really was secondary to the question of should Los Alamos really bring in some top-flight additional people that had been in the weapons business during the war and had gone about their civilian activities elsewhere, as well as giving higher standing to Edward Teller, who was, of course, the central figure in terms of thermonuclear weapon research. This type of a major change at Los Alamos was what the General Advisory Committee was recommending against. They weren't saying, "Don't study it at all"; they were just saying, "Just study it the way you have been studying it."

But the other members of the committee were either relatively uninformed about this internal, behind-the-scenes discussion that had already occurred, or were only partially informed, and I think that's why Oppenheimer didn't want me in on their first sessions while they were formulating their recommendation. He wanted to be able to control the discussion with Conant's backing.

There was a later meeting to which I was invited, but by that time, their report had already gone to the commission, and they had met with the commission, and, as it were, the fat was on the fire in the whole atomic energy circle nationally, among those aware of what was going on. Harold Urey came out on, shall we say, the Lawrence side of the argument with his own touch to it, and others.

Hughes: How did you feel about being bypassed?

Pitzer: Oh, I was annoyed, but not too much.

Hughes: Why not too much? It was a big issue.

Pitzer: Well, what I mean is--oh, strike that. [laughs] I was annoyed. This was, shall we say, weapons, military applications' business, and it was the commission's option to invite others that were not actually in the military applications operations of the commission or not, as they chose.

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Pitzer: But I was able to voice my own view within the Washington circle of the AEC; the general manager had meetings with all the division directors separately. I was not always but ordinarily invited to commission meetings if the research side was at all involved, even though it wasn't immediate research division business. So I knew that I would be able to express my opinion within the Washington, D.C., headquarters of the AEC, both to the general manager and to each of the commissioners; I could go in and express my views to them.

Hughes: And did you?

Pitzer: Oh, yes, sure, I did. There was no doubt about it.

Let me state in my own words what my view was. I'm not an expert on the scientific potentialities and feasibility of the thermonuclear weapon. My view was just that you don't defend the United States by intentionally remaining in ignorance about something that might be very important. My interpretation of the GAC action was to intentionally not learn as much as you could with a reasonable effort about what was possible. Therefore, I thought the directions to Los Alamos, to Norris Bradbury, the director, should have been: "Put thermonuclear exploration along with other top-priority things, and if additional money is needed to pay for the program to bring back to Los Alamos people that knew that sort of business but had left after the war but would be willing to come back, to do so."

What was actually done was somewhat intermediate. David Lilienthal, the chairman of the commission, bought the GAC position completely, as nearly as I could tell. And now I have to be careful as to who was still on the commission at the time, but certainly Lewis Strauss was on the commission, and he was a vigorous advocate of, shall we say, the vigorous program point of view, the opposite point of view, and other commissioners were sort of intermediate. They weren't prepared to say "Go slow," but on the other hand, they weren't prepared to fully overrule their chairman and their primary advisory committee.

Well, the rest of it is in the history books, of course. There were meetings, and the Joint Committee on Atomic Energy got involved in the act. There was finally the committee involving the Secretary of Defense, the Secretary of State, and Lilienthal as chairman of the commission. The other two outvoted Lilienthal; then Truman accepted the report to go ahead with the thermonuclear program. It turned out that the initial design of the super probably wouldn't have worked at all. Somebody--I guess actually probably not Teller--came along--it's all in the books, who it was--with a different design that did work. This was in due time successfully tested. Lawrence gave up on the idea of his running a tritium-producing reactor as soon as he saw that the necessary tritium would be produced by some means by elements of the AEC better qualified to do it than he.

And then, of course, eventually we learned that the Soviets were going full speed on this and knew enough about the whole science that they didn't really make much use of espionage information for the thermonuclear weapon. They got it within, what, about two years after we did, or even less. I don't know whether Oppenheimer ever thought about what would have been the situation if he'd carried the day. The country would have been surprised by learning that the Soviets had a super weapon that we could have had and chose not to have. I think it would have been quite a crisis, that at least we didn't have. President Truman showed good sense, and lots of other people did, in the long run.

Administration and Research

Hughes: What was the scientific and administrative overlap, if any, between what you had been doing at the university and what you did at the AEC?

Pitzer: Relatively little overlap. Not completely separate, but the science that I was using and learning from the AEC side was very interesting to me, and various parts of it were more or less in territory that was quite familiar, but relatively little of it was in immediate areas in which I either had or would soon do my own research. But it kept me in scientific circles, and when I'd go to Oak Ridge or any of the other laboratories, in addition to dealing with, shall we say, science administration matters, I would make a point of talking to active scientists at the working level, partly to judge the local morale and atmosphere and so on, but also for the fun of the science.

I frequently caused some situations that were interesting. On going to one of the national labs, I would add to the agenda that I wanted to see so-and-so or such-and-such a group. I remember once out at Los Alamos that the director had never heard of that group. [laughter] It was too far down the line and off in a special area that he wasn't interested in. Usually, it was not that far off, and usually it was something that the director knew about. He may have been a little surprised that I wanted to spend maybe a whole half-day with them, or at least a couple of hours with them.

But this [had a] dual purpose: both interesting science to me and keeping a feel on the morale and enthusiasm and so on at the working science level in the laboratory, which sometimes the director was fairly well insulated from. He had a more direct authority over people so that he had to be a little more careful about getting out of line in the administrative structure.

Hughes: Were you able to keep any of your research going?

Pitzer: Oh, yes. I could get back here to Berkeley fairly frequently. [tape interruption; consults his bibliography] Just checking the list of publications. There were quite a number in 1949, but of course, they were essentially concerning work that had been done here in Berkeley before I went to the AEC. There are two in 1950 and one in 1951, where the co-authors were students that I'd had in Berkeley. For one reason or another, we were a bit slow [in getting the papers published], maybe made some additional calculations or something like that.

Then I also contributed to a special meeting of the Faraday Society in Great Britain where I gave a lecture about work that I had done earlier titled "Potential energies for rotation about single bonds."[1] [laughs] I found time to put that paper together and make the trip and so on.

There is one paper that involves George Pimentel [ref. #96], who had been my student for his Ph.D. but was by that time a junior member of the faculty here, and in a sense, it was a continuation of his thesis work, but not an immediate part of it.

In other words, there was essentially a two- or two-and-a-half-year hiatus in terms of anything like full-time active research, but I was sufficiently in contact with things that I could easily quickly pick it up again [when I returned to Berkeley in 1951].

Appointment of a Chemist as Director

Hughes: Why was a chemist appointed Research Director of the AEC?

Pitzer: Well, [Hewlett and Duncan] go into that some. I think Lawrence presumably had a good deal to do with it, but not too much. I think the commissioners and general manager wanted somebody who was not too intimately connected to any of the laboratory directors or to the strong advocates of their particular program,

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[1] K.S. Pitzer. Potential energies for rotation about single bonds. *Faraday Society Discussion* 1951, no. 10.

but who was well enough acquainted scientifically, had the scientific capacity, to understand what was going on.

And then as another side of it, there was, I think, a feeling that if atomic energy was going to be useful and not just a weapons program, that there were a lot of, shall we say, supporting aspects of power reactors and other things that were going to involve chemistry and metallurgy and non-nuclear physics --in other words, solid-state and other aspects of physics.

I was selected as somebody, even though from Berkeley, who was independent enough of Lawrence that I wouldn't be unduly influenced by him. And I had other qualifications that I've just been going over. It's I think quite clear that W. Albert Noyes had a good deal to do with recommending me, and I think that's in the book there. He was then head of chemistry at Rochester, but was very widely known for important scientific work done during World War II and for his national leadership role. And while I didn't know him that well or he didn't know me that well, we did know one another, he apparently respected what I'd done, and it is suggested that he had quite a little influence on this.

Hughes: Did your service during World War II as Technical Director of the Maryland Research Laboratory have some bearing on the appointment?

Pitzer: Not in any direct scientific way.

Hughes: I thought maybe somebody had spotted your administrative skills.

Pitzer: Oh, it was pertinent in that sense; I'm sure they thought that I could handle the administrative side. That's a primary thing. Not all good scientists are good administrators.

Hughes: You had also been tested in the College of Letters and Science by that time.[1]

Pitzer: Well, in a limited way, yes. Probably the fact that I had been on the Academic Senate Budget Committee had as much to do from that point of view, too. Yes, that all played a role.

Hughes: Did you like administrative responsibilities?

Pitzer: Up to a point, yes. And, of course, I got back into it for a longer period of time.

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[1] Pitzer was Assistant Dean, College of Letters and Sciences, University of California, Berkeley, 1947-1948.

Hughes: You're talking about the presidencies of Rice and Stanford.

Pitzer: Yes.

Dean, College of Chemistry, UCB, 1951-1960

Hughes: When you came back to the university in 1951, you were appointed dean of the College of Chemistry [1951-1960].

Pitzer: Yes. Well, I think it was quite clear that I would have been that earlier if I hadn't gone to Washington. I think you'll find it in the record somewhere that the department made a concerted request that a new longterm deanship appointment not be made when Latimer retired as dean, but that they wanted me to do it eventually and assumed I'd be back before too long. Hildebrand agreed to do it, even though he was practically at retirement age then. He'd done so many administrative things already that he would not normally have stepped in on that role. I can't lay my hands on the records on all that, but that was what I was led to believe, and I'm sure that there's evidence of it in the record somewhere.

VI RESEARCH (CONTINUED)

Relativistic Effects on Molecular Properties

Hughes: Well, after that detour, let's talk about relativistic effects on molecular properties.

Pitzer: Sure, let's do that.

I'll probably say something further about the science. This is an area where a lot of the science is very specific and detailed, and I'm not sure that in conversations of this type, I could really add much to what is pretty clear in the published literature.

Hughes: No, but maybe you could give the context.

Pitzer: Yes, but I will try to give some context.

Attraction to the Problem

Hughes: What attracted you to the problem?

Pitzer: Yes, all right, I could start on that.

When I decided I'd had enough of university administration in 1970, and I had essentially a sabbatical in late '70 and early '71, of course, my thoughts were on science. I kept enough active science during the years at Rice that I had some things I definitely wanted to get back to scientifically. I still enjoyed direct scientific work, and so there was no question in my mind but what I wanted to get back into direct scientific work.

I might add as an aside that this was in contrast to practically all of my fellow university presidents of that period that had difficulty with the Vietnam War protests. They became executive secretaries of foundations or took on some other position that was not going back to their immediate academic field.

I spent fall of 1970 at the University of Indiana. There were a few people there that I had known very well and had urged me very much to come and made it very cordial for me there. Then after some very pleasant travel in places like New Zealand, my wife and I settled down in Cambridge, England, for the spring of '71. And I was thinking during that time about what specific things to do.

There are at least two other research areas that will come up when I talk about some other topics, where there was a rather immediate indication that there was something more to be done, but that is not the case for this relativistic effects section. Here, it was just a matter of general interest in quantum chemical questions in relation to and comparison with experimental properties in which I am sure many others were aware, as was I, that as the atoms got bigger and had larger charges on the nucleus, relativistic effects would become important. But it wasn't easy to make more detailed calculations of experimentally measurable quantities.

Calculating the Role of Relativistic Effects

Hughes: Could you elaborate a little?

Pitzer: Well, chemists, of course, are very much interested in the periodic table. If you take groups of the periodic table, and we might just take the fourth group with carbon, silicon, germanium, tin, and lead, it isn't a smooth trend in properties all the way from carbon down to tin; the anomaly there is between carbon and silicon. Once you get to silicon, the trend of various properties between silicon and germanium and tin is pretty regular.

Lead is quite different; not totally different, of course. It's still got four valence electrons around a closed shell, but the primary stable compounds of lead are based on +2 ion, whereas tin is much more dominated by the +4 compounds, and germanium and silicon almost completely by the +4--or the four valent, whether you call it +4 or not.

Also consider the copper-silver-gold sequence and the size of the positive ion: copper is considerably smaller than silver; gold ought to be considerably bigger than silver; it isn't. It's slightly smaller, in typical compounds where the radius can be determined. Mercury is probably the most flagrant example. In general, the boiling points of the elements as you go down the periodic table may decrease or they may increase a little, but they don't do anything drastic like going from zinc to cadmium to mercury, which has a vapor pressure at room temperature and a melting point way below room temperature. Well, this is absurd.

Hughes: How did people explain it? Or did they?

Pitzer: Well, that's what I'm coming to. The standard explanation in the inorganic chemistry books was that this all happened because the 4F shell of electrons had come in with the rare earths, from lanthanum through lutecium. This is an inner shell of fourteen electrons with fourteen extra charges on the nucleus.

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Pitzer: For gold, for example, the valence electron is a 6s electron with no angular momentum. If you think of a pseudoclassical motion for it, it penetrates this spherical shell of fourteen electrons and sees the higher charge on the nucleus, and therefore, it gets attracted more strongly than it would have been in the absence of this 4f shell.

The idea that went through my mind was that relativity may be becoming important also, and how much of this may be due to relativistic effects, which may be going in the same direction as this 4f shell effect? Well, it seemed to me that it was feasible to explore this in 1970, whereas in 1960 or earlier, just the burden of electronic computer calculations would be probably too heavy.

I looked into this on a second-priority level; that is, there were two or three other things that I was going to start more immediately when I got back here and sat in this room, actually, [laughs] in 1971. But after I had gotten two or three other things started, along about 1974, something like that, I really began to spend a little time on this.

A Frenchman by the name of Jean-Paul Desclaux had published relativistic calculations for atoms, giving the various energy levels and relativistic wave function properties. There were comparable nonrelativistic calculations for atoms, so that just looking in these published tables, one could make comparisons. It looked to me like the relativistic effect, even on the outermost electrons, was beginning to be of the magnitude that could affect chemical properties. So I decided to look into that.

Student Collaborators

Pitzer: The first student collaborator was Yoon Lee, a Korean, who had already spent some time in this country, however, so that he wasn't coming directly from Korea. We started looking into that. I made contact with people down at the IBM laboratory in San Jose who were very good at demonstrating what IBM computers could do in the world, as you know, and were open to suggestions as to additional calculations that they might use their computers for.

Paul Bagus was my first collaborator down there, along with Yoon Lee, the young Korean. I persuaded Bagus to make, with Yoon Lee's cooperation, calculations for what I called pseudo-atoms. They were atoms in which I just suppressed the 4f shell, reduced the charge by fourteen, allowed the 5s, 5p, 5d and 6s orbitals to be filled, and compared those atomic properties with the real properties calculated relativistically or calculated nonrelativistically, with or without the 4f shell. I was able to show that the anomaly, say, of gold was about two-thirds relativistic and about one-third the 4f shell effect, both going in the same direction, both making the 6s electron more tightly bound, the radius smaller, and so on.

Well, there actually were some other atomic calculations that I made in the first three papers. I made a mistake, I think, in not putting the Bagus paper[1] in the *Selected Papers*. The content of it is in the review paper[2] of a few years later. But anyway, there were two other papers on atomic matters that I don't think are as interesting; they're there to be read.

The problem was to get into molecules. As comes up with me frequently, I need some help on using modern computers effectively. I was fortunate to get Walter Ermler to join as a sort of senior postdoc. He'd been a postdoc in molecular physics and molecular calculations with Robert Milliken at Chicago. I

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[1]P.S. Bagus, Y.S. Lee and K.S. Pitzer. Effects of relativity and of the lanthanide contraction on the atoms from hafnium to bismuth. *Chemical Physics Letters* 1975, 33: 408.

[2] K.S. Pitzer. Relativistic effects on chemical properties. *Accounts of Chemical Research* 1979, 12: 271-276.

believe his thesis at Ohio State had involved some molecular calculations, too. So he really knew how to make such calculations. With Yoon Lee and another man at IBM San Jose, Douglas McLean, A. D. McLean, and separately at times with Nicholas Winter, N. W. Winter, at Livermore, we set about calculating for molecules.

Approximating a Realistic Molecular Calculation

Pitzer: I'm not sure that I can orally contribute very much in comment about the detailed methods, but I will say a little bit in general; the reader really has got to go to the literature or papers. The relativistic orbitals, strictly speaking, are four-component or as compared to scalar mathematical functions, and that makes it terribly complicated. Then the question is, how can you approximate things to make a realistic molecular calculation? We did that for a series of problems starting in 1975, when Ermler joined the group, and for the next two or three years, and finally made reasonably successful calculations for diatomic gold and for a few other cases. But the methods were very complex and the results were still not entirely satisfactory.

In 1978, Phillip Christiansen joined, also as a postdoc, and he was, I would say, less experienced than Ermler but much better than I was in making the calculations. We made some improvements, including calculations for the more difficult cases involving the thallium-containing molecules--thallium hydride, diatomic thallium, and diatomic thallium with a positive charge. Thallium was more complicated, because you had to consider the 6p electron as well as the 6s electrons. The group of six 6p electron orbitals, including spin, split relativistically into a pair of 6p, spin one-half orbitals, net angular momentum of one half unit, and four atomically degenerate orbitals, each with angular momentum of 3/2.

This leads to some very interesting facts in that the relativistic effect is to lower the energy of the 6p one-half orbitals very substantially as compared to that of the 6p three-halves orbitals, whereas nonrelativistically, they would have all been at the same energy. And those results, published in 1980[1],

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[1] P.A. Christiansen and K.S. Pitzer. Electronic structure and dissociation curves for the ground states of Tl_2 and $Tl_2{+}$ from relativistic effective potential calculations. *Selected Papers*, pp. 170-173.

follow pretty nearly correctly the various properties, as known experimentally, but still, I was not very happy with that method.

By 1981, K. Balasubramanian had joined me as a postdoc. I can remember him walking through the door and saying, "Balasubramanian is too long a name. Call me Balu." [laughs] His first name was Krishnan; he's obviously from India. But he'd gotten his Ph.D. from this country at Johns Hopkins. He was a bundle of energy and a very agreeable young man. And with Christiansen and Balasubramanian, we developed still another general approach in which we removed the spin orbit term, which is substantial relativistically for these atoms, but we still took it out of the first stage of the calculation. We included the relativistic lowering of the 6s energy level, which has no spin orbit, or the average of the 6p orbitals, because of the relativistic motion near the nucleus, but we took out the spin orbital term from the 6p aspects of the calculation. On that basis, the orbitals become scalar functions, not multicomponent functions, and the nonrelativistic methods can be used.

Using Effective Potentials

Pitzer: Now, I should have said earlier that the key to doing any of this was the use of effective potentials. To make detailed calculations for all the electrons in a pair of gold atoms would have been ridiculously difficult at that time [1975]. But by 1970, other people in quantum chemistry had developed effective potentials for nonrelativistic calculations for, shall we say, diatomic copper. There are a lot of inner electrons even in copper that take up computer time, and they don't really affect the result for the molecule. So if you can replace the effects of the 1s and the 2s and the 2p electrons with effective potentials operating on the 3s and the 3p and the 3d, you can save half the electrons, or more or less half the electrons, and still get pretty good results.

Well, this methodology had been developed, and we used this right from the beginning. But now having pulled the spin orbit aspect out of the problem, we were able to make the first approximation calculation, say for a thallium compound, on the same basis as it would have been done nonrelativistically, provided we used these spin-averaged relativistic potentials, instead of nonrelativistic potentials.

Then, as the second stage of the calculation, we could put in the spin orbit effect, and introduce at the same time some terms for the electron correlation, which is always the difficult nuisance part of many electron molecular problems. By putting in the spin orbit terms and the electron correlation terms at the same time as a second approximation stage, it became feasible to do a lot better than we had done before.

The first major example of this was the paper on thallium hydride with Balasubramanian--this was in 1982--which is the final paper[1] that I put in this *Selected Papers* book, because that essentially completed the development of the methodology, so far as I was concerned. Balasubramanian continued with me for a couple of years, three total, and was remarkably prolific. Actually, there were six additional papers using this new methodology, but where the results were experimentally better known and there was less novelty in it, so I didn't include it in the *Selected Papers* volume.

After that, I essentially stopped my activity in this field, because it seemed to me that skill in electronic computer calculation was going to control further advances, rather than new conceptual approaches. I thought we had developed the optimum general approach. I did have one further student, Randy Neissler, who did calculations on the diatomic dipositive ion, Hg_2++[2] which is quite a novel, interesting problem, using the methods that had already been developed. But otherwise, I passed this on to others.

Balasubramanian went from his postdoc here to a regular faculty position at Arizona State, Tempe, where he's done very well indeed. We keep pretty close touch. He wants me to write a letter of recommendation every once in a while [laughs]. I checked with the latest list of publications that he'd sent in, and he has over 340 papers, which for a man of his age is well-nigh a record, far more than many people get in their entire career. They're not all in this field of relativistic quantum calculations, but most of them are. He lists ten book chapters,

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[1] P.A. Christiansen, K. Balasubramanian, and K.S. Pitzer. Relativistic *ab initio* molecular structure calculations including configuration interaction with application to six states of TlH. *Selected Papers*, pp. 181-186.

[2] R. P. Neisler and K. S. Pitzer, "The Dipositive Dimeric Ion $Hg_2$2+: A Theoretical Study," J Phys. Chem., 1987, 91:1084.

one of which I co-authored with him.[1] He's a very, very interesting person, and as I say, very successful by almost any standard.

Usefulness of the Calculations

Hughes: Perhaps you could say something about what the availability of such calculations allowed you and others to do.

Pitzer: There were some uncertainties about the experimental data on the thallium hydride and diatomic thallium and so on, where the calculations, I think, did help resolve ambiguities in the literature. But for the most part, so far as I was involved with it, the experimental spectroscopic and other information of that sort was pretty well established. We were just showing that our relativistic quantum calculations by the last method, not only for the ground state of the molecule, but also for various excited states, were valid, were leading to good results which would help guide experimental interpretations later.

I should reemphasize that that final paper on thallium hydride involved not just the ground state but a whole series of excited states, some of which were known experimentally. But once you go on beyond relatively simple diatomic molecules, there is a lot more uncertainty in the experimental situation, and theory is more likely to be useful guidance in interpreting the results. Balu, as we call him, and others have pursued that.

And again, my son, Russell, comes into the picture. He has great ability with computers, and he's very good on simplification of molecule calculations by use of symmetry if the molecule has symmetry. He's calculated a number of very complex examples involving heavy atoms, using relativistic effective potentials, including putting a heavy atom in the middle of a C_{60} Buckyball, for example. A thing like that, somebody has just created it experimentally or maybe is just trying to create it experimentally, and to make reasonably good calculations about the properties to be expected is rather exciting business.

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[1] [ref #308] K. Balasubramanian and K. S. Pitzer, "Relativistic Quantum Chemistry" in "*Ab Initio* Methods in Quantum Chemistry I," K. P. Lawley, ed., John Wiley and Sons, Ltd., 1987: 287-319.

Pyykö's Contributions

Pitzer: I mentioned the work of the Frenchman, Desclaux, for relativistic atomic orbital information that we use as the basis. I should mention another major figure in relativistic quantum chemistry for whom this has been his primary career. He's a Finn with the name Pekka Pyykö, that I very soon became acquainted with when I got into the field. We had a very interesting, constructively competitive, and friendly relationship for all the years that I was in the business.

At the same time that I presented in 1979 a review paper in *Accounts of Chemical Research*,[1] there's an accompanying paper by joint authorship of Desclaux, the Frenchman, and Pyykö, the Finn, that really described the situation at that time. Pyykö organized a meeting in Finland one summer somewhat later, and my wife and I attended, and we had a very interesting trip there. I found Finland a very interesting country that I'd probably never have gotten to otherwise.

Hughes: Now, is Pyykö a mathematician?

Pitzer: No, he's a physical chemist. His position was in theoretical chemistry or physical chemistry.

There's another interesting aspect to it: his name is clearly Finnish, not Swedish. There is a Swedish component in Finland, mainly on the Baltic coast and on the islands. But he got his Ph.D. in Sweden, and he can speak Swedish, and so his initial appointment when I was there for the meeting was at the secondary city at the base of the archipelago that goes out into the Baltic, can't think of the name of it right now [Turku]. There's a Swedish-speaking university there, and he was the professor who was teaching in Swedish.

Because of his command of Swedish, he got a position that represented a promotion for him in a Swedish-language university in Helsinki. I haven't had contact with him for several years, but so far as I know, he's still active. I find a big review paper on this whole relativistic bond area in some journal every once in a while.

Well, I guess that's about that story.

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[1] K.S. Pitzer. Relativistic effects on chemical properties. *Accounts of Chemical Research* 1979, 12: 271.

Hughes: Do you want to talk now about other work on molecules?

Pitzer: Why don't we go back, and you might interpose this ahead of the relativistic quantum mechanics.

Spin Species Conversion in Methane

Pitzer: The original idea was that one might find in methane a situation analogous to ortho- and para-hydrogen, where there would be separate long-lived gaseous species with actually the same atoms and molecular composition but diffrent quantitative properties. The actual behavior of methane is different, but it is also very interesting.

The first four papers[1] on spin species conversion in methane involved research at Rice. [R.F.] Curl was co-author on three of them, and [Harry P.] Hopkins, who was a postdoc of mine at Rice, on two. [J.V.V.] Kasper was also on two. [P.L.] Donoho was on the physics faculty at Rice, and he was the key figure for certain experiments. Curl, by the way, had been a Ph.D. student of mine at Berkeley earlier, but he was a regular senior member of the Rice faculty at the time.

Hughes: So that was just chance, that you ended up together again.

Pitzer: Yes, although I suspect Curl might have had a little influence in my coming.

Hughes: Ah. [laughter]

Pitzer: He wasn't senior enough to have had a major influence, but he's probably pretty clever at that.

From the work at Rice, we demonstrated that oxygen, with its triplet state with the magnetic characteristics, would tend to convert the spin state of something else. Really, the key thing there on the experimental side was the paper with Hopkins and Donoho.[2]

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[1] *Selected Papers*, papers 49-52.

[2] H.P. Hopkins, Jr., P.L. Donoho, and K.S. Pitzer. Oxygen catalysis of nuclear spin species conversion in solid methane. *Selected Papers*, pp. 343-344.

The two papers with Hopkins, one with Donoho, and one with Curl, established that there was something to it, but I thought more needed to be done about that. And so starting more or less immediately when I got back here in the fall of '71, G. J. Vogt was one of my first group of students, and Janice J. Kim was another one, but it's primarily Vogt's experimental work which was the really important thing there. He did the experiments with methane near and below 1 degree Kelvin, and there were two publications: one, a letter to the editor, and the other a detailed publication.[1]

It was feasible actually to get into that quickly, because I had had apparatus and had done some work down at that low-temperature end before I went to Rice as the university president. I had turned over my equipment to a colleague here [Professor Norman Phillips], and I was able to reclaim the equipment that would make these experiments feasible down near 1 degree Kelvin.

But with our experience from the Rice experiments, we could design the new experiments quite efficiently. With the oxygen, the spin conversion catalyst, we found this very striking thermal anomaly centered about 1 [degree] Kelvin. We followed it down to about four-tenths of a degree, so that we could get a pretty good estimate of the total entropy and heat capacity of it. This was published first as a letter to the *Journal of Chemical Physics* and then a full paper in the *Journal of Chemical Thermodynamics*.[2]

We investigated the catalyzed system at higher temperatures, but then we also did the experiments without the oxygen catalyst. The very low temperature effect, near 1 degree Kelvin, just didn't appear at all, but we found some effects at higher temperatures with some time delay associated with them.

Then in the interpretation, we found in the literature several papers about the detailed structure of solid methane, which is quite interesting. One of the structures that was proposed, which seemed to be about as likely to be true as any alternative, was a rather peculiar one for a very symmetrical molecule and for overall cubic symmetry of the crystal, in that one-fourth of the molecules were in different local sites from the

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[1]*Selected Papers*, papers 53 and 54.

[2] G.K. Vogt and K.S. Pitzer. Spin species conversion and the heat capacity of solid methane near 1 degree K. *Journal of Chemical Physics* 1975, 63: 3667-366; G.J. Vogt and K.S. Pitzer. Entropy and heat capacity of methane; spin-species conversion. *Journal of Chemical Thermodynamics* 1976, 8: 1011-1031.

other three-fourths. One-fourth of the molecules were in what were called cubic sites of very high symmetry, where there would be very little change in energy with possible orientation, and three-fourths were in less symmetrical sites. It was only in those less symmetrical sites that it was reasonable that we would get this thermal anomaly, this high heat capacity or broadened transition, as you wish to state it.

The amount of entropy we measured checked pretty well with this being caused by three-fourths of the sites. If all the molecules had been contributing, it ought to have been a bigger effect, and if less than three-fourths were doing it, it should have been smaller than we observed. So our results played a role, at least, in confirming that particular structure, and I had interactions particularly with a man in Japan, Y. Yamamoto, who was involved with that theory. That was rather interesting and a pleasure.

Well, I think that pretty much covers the spin species conversion and the Rice situation. Do you have any questions?

Research on Xenon Hexafluoride

Pitzer: Now, the other area that I took up more or less immediately after returning to Berkeley was with [Leonard S.] Bernstein on xenon hexafluoride,[1] and we might as well say all that we need to say about that. This is a pseudorotation problem. You may recall the discussion of cyclopentane as a pseudorotation case, and I think I said then that there were other types of pseudorotation.

The xenon atom has nominally a full quota of electrons, but you can pull some electrons off into the fluorine atoms, making the fluorines negative and the xenon positive. Of course, it was 1975, it was still not long since compounds of xenon existed at all. It was only Neil Bartlett, who's now my also-retired colleague here, who first discovered that xenon had real chemistry. Several other scientists actually did the xenon fluoride, but they wouldn't have done it without Bartlett's work earlier.

Well, after xenon provides six electrons to go with those fluorine atoms, its outer shell of eight has two left over. If

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[1] K.S. Pitzer and L.S. Bernstein. Molecular structure of XeF_6. *Selected Papers*, pp. 264-271.

you leave them sort of symmetrically around the xenon nucleus and have the fluorines in a regular octahedral geometry, they're just sort of interfering with xenon-fluorine bonding. So there is a tendency to squirt those two electrons out in a localized orbital and distort the--[telephone interruption]

Well, these two extra valence-level electrons are sort of squirted out in a localized orbital, and the xenon atom then has seven entities, six fluorines and this one electron pair. But how do you arrange the seven? And one of them is squishy and flexible, so you can probably rearrange the six rather easily. So we took that on as essentially a theoretical question of what's the structure of this pseudorotatable molecule going to be? There was some experimental information related to it, but it was one of these problems that is obviously not world-shattering. It's an additional example of a type that we've really met before, but it was lots of fun, and Bernstein's gone on with a very good career, so all to the good.

Use of the Term "Pseudorotation"

Hughes: Now, we had talked before about how others earlier had picked up your term "pseudorotation," but used it in a slightly different sense. Was your use of "pseudorotation" consistent with your use of it in the earlier work on cyclopentane?

Pitzer: Oh, I think this is purely a matter of preference about terminology, and if I implied that I really objected to somebody using that, I didn't.

Hughes: No, no, it wasn't that strong.

Pitzer: I thought sometimes it ought to have been called type II pseudorotation or something like that.

Hughes: That's what you said.

Pitzer: In this sense, maybe this is type III pseudorotation. [laughter] There were actually earlier examples of whether it was localized electrons as well as actual substituent atoms that were rearranging, so that if that's called type III, why, this is another example of type III. Type II, as I originally described it, was the five-fold substituent molecule. No problem with extra electrons; it's just, how do you arrange five? You put three in a plane and one above and one below, but it's awfully easy to move them a little bit here and then move them a little bit there, and interchange them. That was what I was calling Type II.

Well, I didn't start anything else in that period, but that gives you a picture of some other things that were started ahead of the relativistic quantum calculations, in part because I saw better how to actually do something about them.

Slow Interconversion in Methane

Hughes: Could you explain more fully the interconversion aspect? In the introduction to this section in your *Selected Papers*, you said, "It proved to be impossible to prepare separate gaseous species but very interesting properties, including slow interconversion, were observed in solid CH_4 [methane]."[1]

Pitzer: Oh, yes, the solid methane question. Yes, I could say a little more about the beginning to that. In fact, that is sort of fun too.

By analogy to ortho- and para-hydrogen, methane would actually have three such species. This has to do with the relative orientation of nuclear spins of the protons. In ortho-hydrogen, they're parallel, so that the molecule has a net nuclear spin of unity. In para-hydrogen, they're antiparallel, so it has a net nuclear spin of zero. This determines which rotational levels are allowed, quantum mechanically. The para-hydrogen can have zero overall rotational angular momentum, or two units, or four units, or six units; and para-hydrogen can have one unit of overall rotational angular momentum, or three, or five, or any other odd number.

The conversion of ortho- to para-hydrogen is slow in the absence of an inhomogeneous magnetic field. But it was learned fairly early in the game, in part by Giauque in this laboratory, that it could be catalyzed with iron, or another strongly magnetic substance, as long as it was fluid hydrogen so that the hydrogen molecules could get in contact close to the magnetic species.

Well, methane has three such species. A molecule could have two spins opposing the other two, a net spin of zero; it could have three one way and one the other, with a net spin of unity; or it could have all four nuclear spins lined up, with a net spin of

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[1] p. 331.

two. This controls the allowed rotational levels in the molecule. I had thought, well, couldn't we do something similar, maybe separate the different nuclear spin species of methane by some low-temperature treatment with magnetic catalyst? And then quickly evaporate it and do spectroscopic measurements, which would determine what had happened.

Now, with hydrogen, that's easy. The ortho-para conversion in gas at room temperature takes months, depending on the surface of the container and so on. But we were unable to find anything with methane. If we had made any conversion at low temperatures to a preferred form, it reconverted too fast. But that we rather soon understood--this was all at Rice, when I was in Texas--because ortho- and para-hydrogen never have the same overall rotational state. As you get up in rotational quantum number with methane, eventually you come to a level--and it's not terribly high, maybe four or something like that--where different spin species can have rotational states of essentially the same energy. And then with essentially zero energy difference, you expect interconversion to take place. In other words, any minute perturbation will cause otherwise exactly the same energy levels to convert one into the other.

So that's why, as I say here [reading from *Selected Papers*]: "It proved to be impossible to prepare separated gaseous species but very interesting properties, including slow interconversion, were observed in solid methane. And the associated theory exposed some other interesting properties."[1] Well, the first experiments that failed to show separated nonequilibrium speciation in the gas were done at Rice, while we examined the degree of conversion in the solid in the experiments with Vogt here.

Research on Polyatomic Carbon

[Interview 5: June 18, 1996] ##

Pitzer: Did I comment on paper 37 on carbon vapor in *Selected Papers*?[2] It's a paper with [Enrico] Clementi.

Hughes: No.

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[1] p. 331.

[2] K.S. Pitzer and E. Clementi. Large molecules in carbon vapor. *Selected Papers*, pp. 229-237.

Pitzer: Clementi was a postdoc, a quantum theorist, who was with me in the late fifties, and at that time, there was a major puzzle about the heat of vaporization and vapor pressure of carbon. Of course, carbon doesn't vaporize except at an almost impossibly high temperature, but experimental information was accumulating in large measure from spectroscopic measurements. My colleague Leo Brewer was actually more in contact with this situation than I was. And so when Clementi was with me, we looked into what theory could be contributed there.

Rather simple calculations in terms of the sophistication of the quantum theory indicated that small polyatomic molecules--three atoms, four atoms, five atoms, up to about seven or eight, maybe--would be probably linear, in some cases almost certainly linear, and that the odd-numbered species would be distinctly more stable, more strongly bound, than the even-numbered species. So far as this heat of vaporization and the equilibrium vapor was concerned, this meant that C_3 would be much more important, probably, than C_2. So insofar as monatomic carbon wasn't accounting for all the carbon being vaporized in experiments, in other words, while C_2 was not accounting for all the carbon being lost in addition to C_1, the explanation that we suggested, with pretty convincing basis, was that this absent thing was C_3. And the nature of the C_3 molecule, with no dipole moment, with a very high ionization potential, no low-lying spectroscopic states, would have escaped spectroscopic detection until somebody set out very specifically to detect it.

Well, that all proved to be correct. There were some minor points about C_2--well, they were significant, but I don't think they deserve any particular discussion now. But that initial paper with Clementi still gets cited in connection with polyatomic carbon. If we'd carried on far enough, we should have predicted the C_{60} and buckyballs, but we didn't. [laughs] But we did talk about ring molecules, just bending the chain around and making a ring out of it. Our predictions on that weren't as good as they might have been on hindsight.

But in the years shortly thereafter when I was now at Rice, I was still interested in this situation. Information became available that suggested that the bending force constant for C_3 and for longer C_n molecules that we had assumed for the initial paper was too high, in other words, that the chain would actually be less resistant to going around in a circle and biting its tail than we had calculated in the first paper.

So there was a second paper [#176] with Stu Strickler as a co-author out of Rice on the fine-tuning of the polyatomic carbon work, and I didn't put that in the selected papers volume because it doesn't affect the overall picture. It has in turn been superseded further, but it was interesting to do that. This is an example of one of the things I was able to do when I was at Rice as president. Strickler had intended actually to come as a postdoc with me here, and agreed to come on to Rice instead. He was later professor of chemistry at the University of Colorado at Boulder and served time as a chairman of the department, has had a very good career.

Early in that period at Rice, I became aware of and was very much interested in the chemistry of xenon, which Neil Bartlett had discovered, and then others had discovered with respect to xenon hexofluoride. So just by myself, in reading the literature about halogen polyfluorides, as chlorine trifluoride, bromine polyfluoride, iodine polyfluoride, there were some conclusions that could be drawn concerning the structure of xenon fluorides, which were interesting. On such a rapidly opening up field, they were a significant contribution at the time, even though they didn't involve very much expenditure of effort, other than familiarity with various parts of the literature.

In addition to Strickler, I had at least five postdocs while at Rice. There were usually two there at any one time. Jurgen Hinze was a German who had just gotten his degree in this country, and has had his main career back in Germany. [Jerome V.V.] Kasper and [Krishnan] Sathianandan, but I don't necessarily have much more to add about their work. Harry Hopkins was important on the experimental work related to the spin species conversion project, as well as making contributions to the calculations and other things. He's had quite a strong career in the Georgia State University that's right in Atlanta, which is not really a research university, but he's succeeded in carrying on quite a significant program. And finally [T.S.S.R.] Murty, who contributed some things at Rice.

VII PRESIDENT AND PROFESSOR OF CHEMISTRY, RICE UNIVERSITY, 1961-1968

Establishing a Laboratory

Hughes: Dr. Pitzer, you of course had to establish a laboratory as well as assume a presidency at Rice University. Would you comment on what that entailed?

Pitzer: Well, it entailed both personnel and physical facilities. I had arranged that I have what had been an intermediate-sized undergraduate laboratory room that was was not being used for that purpose, so that it was going to be rebuilt anyway. I arranged to have that available with about three-fourths of the space flexible for research space, and then the remainder a research office for me, where I could keep my research activities and talk to research people in the chemistry building. This all worked out very nicely, and amusingly enough, my successor, Norman Hackerman, who was also a chemist, took it over when he became president. And when I last visited Rice, it still looked essentially the same, although there have been two more presidents since Hackerman, neither of whom wanted that lab.

But let's go back to the more important side of having something going on in terms of people. I was in contact with postdoc candidates and was able to essentially take with me two young men who would have been postdocs here and were willing to come with me to Rice. One moved on fairly soon, but I was still in contact with various candidates for postdoc positions. Most of the time, I had two postdocs steadily, who would turn over from time to time during those years. We got quite a number of interesting things done. In addition to anything that shows in this book [*Selected Papers*] here, there are quite a number of other interesting things.

Research Conditions at Rice

Physical Setting and Scheduling

Hughes: Well, I was wondering if, as part of the negotiation to become president of Rice, you included stipulations about what you expected in terms of research conditions.

Pitzer: I'm sure there was some understanding, but I anticipated no difficulty in this regard. My predecessor, William Houston, a physicist that I'd known from my Caltech days, had maintained active participation in scientific matters, although he did not actually do any original scientific research himself or have postdoctorals with him. It may have been somewhat of a surprise to key members of the board of trustees that I wanted to do this, but they didn't have any objection to it. There was no need for any major or even substantial expenditures. I was raising separate money, so far as paying the postdocs were concerned. If I wanted to use a little of my own time, that's what Houston had done; there had been no problem about that. So I didn't see any particular problem, nor did they.

Now, some physical arrangements had to be made. What I did was take over one fairly large laboratory on the ground floor in actually the nearest corner of the chemistry building as I came in from the presidential office, and had this renovated with a modest-sized office in one corner where I could keep my scientific papers and do scientific work undisturbed by administrative things. In a room outside of this little office, there was also an area for the postdocs to have desks and bookcases, and then the rest of the room was for laboratory experimental equipment.

And for some more elaborate experimental things, these would be just temporary; I could arrange with whoever had that sort of equipment or facilities to, as it were, borrow them for a limited period of time while a given experiment was done, usually with that person as a collaborator so that they were part of the picture, too. So there was never any problem about this.

And schedule-wise, I always did my science the first thing in the morning. I found that once I had gotten my mind on some administrative problem, it was no longer productive to think about the science. Unless it was a lecture that somebody was giving that I wanted to hear or something like that. So I'd go over to my research office first thing in the morning and tell my secretary that I'd be over there [in the president's office] about ten-thirty--she knew that anyway; I didn't have to tell her every day--and if there was some crisis, to call me, but otherwise, tell people to wait until ten-thirty, and she'd call and get us together then.

Now, there were some days when something was going on that obviously needed early attention, so I just didn't do science that day. And then I'd have to be out of town on travel fairly frequently, but there was still a lot of time to do research on this basis.

Hughes: And the postdocs didn't feel neglected?

Pitzer: I don't think so. In fact, postdocs don't want to be supervised too closely. [laughter] The idea that they might be on their own occasionally for even as much as a week was all right.

Hughes: How did you find the research atmosphere in the department at Rice as compared to that at Berkeley?

Pitzer: Well, it's obviously very much smaller, and it isn't at what you might call first-line level of recognition and importance nationally and internationally. But in terms of quality, it had some very good people right from the beginning, when Rice was first founded in the early teens. So that excellent work being done at Rice was a familiar idea to other people, at least in fields where this work had occurred. Richard Turner was an excellent organic chemist of international distinction.

Laboratory Associates

Pitzer: Then secondly, two of my former Ph.D. students were on the Rice faculty, so that there were familiar faces there. Curl particularly, the younger of the two, had carried over and influenced the local operation in a very positive way, as he is still doing. This aspect of Rice--being small, but yet being absolutely first-class in some particular activities--is very clear today, particularly with Rick Smalley in the carbon-60 effect discovery, and all this work about nanotechnology, in other words, cluster molecules, carbon tubes, things of this sort that is a very exciting field. And Rice is right there at the forefront of it.

Hughes: Was that going on when you were there?

Pitzer: No, Smalley wasn't there, I just meant that Rice had been doing some things at the very top level of quality, and was recognized for this. They had trouble drawing graduate students of the highest quality unless the student was already interested in one of these particular topics, so that in general the students were not as good as we had here. But there were others who were really very good that wanted to go to Rice because of local geography or for other reasons. A postdoc is much more willing to go where there is at least one outstanding person that he's going to work with, and the fact that it's a smaller place isn't anything negative.

In addition to the two former students of mine, one of my very early appointments was John Margrave, whom I had known as a postdoc of Leo Brewer here in Berkeley. He got his degree with a student of Brewer's that I'd also known at the University of Kansas. He's in high-temperature materials research; still is. He was on the faculty at Wisconsin. I attracted him to Rice. So there was a third person that, while not actually a student of mine, was absolutely first-class and with a major impact in terms of recognition nationally and internationally.

VIII RESEARCH (CONTINUED)

Hughes: Did you want to say a bit more about condensed state research?

Pitzer: Yes, I think we might run down the few comments on the papers in here about the condensed state.

Other Condensed State Research

Entropy Discrepancy in Ice

Pitzer: I commented on the first two papers in connection with Latimer and his interests, and he's co-author of one of them. So starting with number sixty-eight, which is in that series that concerns the situation in normal ice crystals, there's an entropy discrepancy in terms of the Third Law [of Thermodynamics] in ice which was by that time--1956, say--well known.[1] Giauque here in this department was aware of it. Linus Pauling had actually proposed the mechanism for it, which concerns hydrogen bonds and possible randomness of the location of the proton [hydrogen nucleus] along an oxygen-oxygen line.

The stable position is not with the hydrogen in the middle but with the hydrogen closer to one oxygen than the other, and in a pattern such that H_2O molecules are retained, in the sense that each oxygen with four other oxygens around it has two hydrogens close and two far away, but in the entire crystal, there are random possibilities. Pauling had predicted the pattern, and in

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[1] K.S. Pitzer and J. Polissar. The order-disorder problem for ice. *Selected Papers*, pp. 490-492.

Giauque's laboratory, he and colleagues had done experiments both on ordinary water and heavy water and found the entropy discrepancy to be exactly what the Pauling theory indicated.

But the question was, how was this frozen in? Why didn't these hydrogen atoms or protons rearrange themselves into a lower-energy pattern at some low temperature? Well, it was fairly plausible that this would involve too much activation energy, because it involved simultaneous movement of a lot of atoms at the same time. Jan Polissar was actually an undergraduate interested in doing a little research in his senior year, and I suggested that he make some calculations about what the energy would be required to get this rearrangement to occur, but more particularly, what the energy differences would be if you did rearrange the protons into the lowest-energy situation, considering the entire crystal. We published this in a very short paper in the *Journal of Physical Chemistry*, in 1956, not with any particular attention to the feasibility of experimental measurements.

But thirty years later, Professor H. Suga in Japan took up this general problem with a very clever scheme in which he trapped into the crystal a little impurity of potassium hydroxide or some other hydroxide. If the OH^- ion goes in where a water molecule ought to be, it's short one proton, and therefore there's a chance for protons to rearrange sequentially, one at a time, in that whole region. They in fact found a thermal transition region of high heat capacity at around 70 Kelvin, and our estimate of around 60 Kelvin was remarkably close.

This experiment is one I might well have thought of. After all, this is pretty similar to my oxygen catalysis of spin species conversion. In other words, trapping some impurity in a crystal that catalyzes some process that otherwise doesn't take place in any measurable length of time. But I didn't think of it, and Suga did, so more power to him. He was quite an outstanding person; did a lot of important things.

Interaction between Molecules Adsorbed on a Surface

Pitzer: The next paper, with [Oktay] Sinanoglu, was theoretical calculations of interaction between molecules adsorbed on a surface.[1] He was a very interesting young man, born and raised in Turkey and still with connections in that country. He first took an undergraduate course in thermodynamics with me while in a chemical engineering major, went to MIT for graduate work in chemical engineering, and decided he liked basic science better than engineering. After getting a master's degree there, he came back here and wanted to work for a Ph.D. and do chemistry with me. I took him on. He was a very able young man.

The calculations we did about very simple molecules on a simple surface was a pioneering calculation which other people have followed up on in more detail. I didn't follow it up any further, and I don't think Sinanoglu did either. He went on to Yale, was promoted very rapidly there to a regular professorship, but I think was rather disappointing in his career as a whole, considering this very promising start. Nonetheless, he did commendable work at Yale, just not as outstanding as his early promise had suggested.

Bonding in Fused Alkali Halide-metal Systems

Pitzer: The next paper concerned nature of bonding in fused alkali halide-metal systems, which I did separately on my own.[2] It was actually a contribution from Rice. [laughs] (I should have mentioned that along with the one on the xenon-hexafluoride.)

This I suppose arose out of a general interest in high-temperature physical chemistry and thermodynamic phenomena. You don't think of a sodium metal dissolving in common salt, sodium chloride, under any sort of ordinary conditions, but of course, if you get the whole thing hot enough, things can happen that wouldn't happen at lower temperature. But the question of the nature of sodium metal dissolving in liquid, sodium chloride, say, or vice versa, sodium chloride dissolving in the metal, seemed to me to be an interesting and challenging one.

I was able to give a pretty good story about that by calling upon known experiments of others in which a bare trace of alkali metal, say sodium, was dissolved in solid sodium chloride. And

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[1] O. Sinanoglu and K.S. Pitzer. Interaction between molecules adsorbed on a surface. *Selected Papers*, pp. 493-502.

[2] K.S. Pitzer. Solubility and the nature of bonding in fused alkali halide-metal systems. *Selected Papers*, pp. 503-506.

that happens sometimes in naturally-occurring salt crystals. If you take some sodium chloride out of the ground, sometimes you have the so-called F centers in it, which are a few atoms of sodium, and there it is known that one single electron occupies the site that otherwise would be a whole chloride ion.

So just by transferring the properties of these F centers, with the idea that in the liquid, the size of the cavity might increase or decrease, more likely decrease, but still would be a distinct cavity in the structure, that worked out quite nicely, and for all the alkali metals and all the halide anions.

Model for Solutions of Alkali Metals in Ammonia

Pitzer: The next paper in [*Selected Papers*] is a revised model for solutions of alkali metals in ammonia.[1] It involved my faculty colleague here, William Jolly, and his student or postdoc, [Marvin] Gold. Again, it was actually published after I had gone to Rice, although it clearly arose out of my contacts here in the late fifties with Jolly.

This is a surprising system in which alkali metal dissolves in liquid ammonia without reacting chemically. At thermodynamic equilibrium, it would do what it does in water: it would react chemically, evolve hydrogen, and be no longer the metal. But for some reason, this reaction in ammonia is so slow without a catalyst that you can do rather elaborate experiments for long time periods without interference.

This problem is a little like two or three that I've talked about earlier in this discussion, in which, if we take sodium as a metal, the sodium ion, positive ion, can disassociate, can be solvated, by ammonia molecules with the nitrogen towards the positive charge and the hydrogens in the other direction. But what do you do with the electron? That, again, is sort of like the previous case I was talking about with the sodium metal and the sodium chloride in liquid: the electron goes into a cavity which is remarkably large and has the ammonias oriented in the opposite way.

The surprising thing about the experimental situation at the time this was written concerned the magnetic properties. If the

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[1] M. Gold, W.L. Jolly, and K.S. Pitzer. A revised model for ammonia solutions of alkali metals. *Selected Papers*, pp. 507-508.

electron is separated from other electrons, its electron spin is free to be oriented. There's a paramagnetic contribution there, but the actual properties of the typical liquid ammonia system, with reasonable concentration of the sodium, showed much less paramagnetism than you would expect. In other words, apparently a large portion was in the dimagnetic component, and the question was, how did it lose this expected magnetism?

We provided a plausible explanation of all this with essentially a two-electron species that would be dimagnetic. This general picture has, I think, stood up pretty well through the years, although as I say in my comment in *Selected Papers*, papers are still being written about the exact nature of that two-electron dimagnetic solute species.

Phase Equilibria for Highly Assymetrical Plasmas and Electrolytes

Pitzer: The next paper, "Phase equilibria for highly asymmetrical plasmas and electrolytes," was done in 1980.[1] I'd been back here in Berkeley for about a decade. It involved discussions with a longtime scientific friend, Bernie Alder, who was by then on the Lawrence Livermore Lab staff and has been for most of his career, but spent a certain amount of time in Berkeley and was interested in discussions of topics of common interest.

It comes up in part with the properties of the sun. As you go into the interior of the sun, the temperature is so high that the electrons are essentially dissociated from the nuclei, which are not just hydrogen but go up in appreciable numbers to as high as iron, so that they're highly unsymmetrical. In other words, the positive species is ultimately charged, and with as high a charge as that of iron, but also with other species.

The question was, what will be the physical properties of this fluid of highly asymmetrically charged species? And it involved some calculations in statistical mechanics that apparently hadn't been adequately taken care of earlier. I notice I do acknowledge conversations with Alder.

And the final paper in this miscellaneous group [in *Selected Papers*], also 1980, is entitled "Electrolytes. From dilute

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[1] *Selected Papers*, pp. 510-516.

solutions to fused salts."[1] In a sense, I'm getting ahead of what I guess will be our next session on semitheoretical electrolytes. But since it comes up here, I might as well say a few words about it, and then we can come back to it later.

That work that I'll be discussing next time provided a greatly improved, theoretically based but empirically completed, description of aqueous electrolyte properties up to the maximum concentration of solubility, equilibrium with solids, for most solutes. In other words, sodium chloride you think of as being pretty soluble, but it will only dissolve about 6 moles per liter.

The theory that we'll talk about next time is adequate for things like sodium chloride up to their equilibrium with their solids. But if you raise the temperature and if you select a particular highly soluble electrolyte solid to dissolve, you can get a situation where their solubility goes essentially all the way. A good example is, say, lithium nitrate, and I see the other example that I chose in this first paper was lithium chlorate. I've talked also about lithium-potassium nitrate, in other words, a mixture of lithium and potassium nitrates. And at not terribly high temperature, just 120° Centigrade, that's a continuous liquid phase all the way from dilute aqueous solution to the pure fused salt. Well, how do you represent that? [laughs]

This is the first paper that I wrote on that, which essentially just combined a Debye expression for the electrical interactions of the ions in the dilute aqueous system with the common formulation of the energies of short-range interactions of nonpolar, nonionic species at high concentrations. I found that these two concepts could be combined quite satisfactorily to represent the actual behavior, whereas neither one alone would come close to it.

As I say in the comment there,[2] this was the beginning of what's turned out to be a very extensive series of papers on more detailed treatment of a wider and wider variety of cases. It eventually led to a collaboration with a young Englishman by the name of Simon Clegg on systems that he was very much interested in in terms of atmospheric science, with aerosols as small aqueous particles that get into the air, with some salt in them. It might be spray from the ocean, it might be impurities coming out of some industrial smokestack and then condensing water around them. But they're long-lived, and in many conditions, they play increasingly

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[1] pp. 512-517.

[2] *Selected Papers*, p. 517.

recognized roles in getting things up into the stratosphere for ozone depletion and all sorts of things of that sort.

The quantitative description of these aqueous electrolyte systems under conditions in which as much of the water has evaporated as will, which means that the remaining salt or the acid, or whatever it is, is very concentrated, has led to a whole series of investigations that are still very actively going on today. I suppose I could add that I might have put in half a dozen more papers after this one, but I chose for the *Selected Papers* volume to put in just this initial paper, plus the Comment giving the reference for one of the more recent ones.

Hughes: Is this an area of your research interest at the moment?

Pitzer: I'm still involved with it, yes. One of my current postdocs is doing this type of calculation on sodium hydroxide, which didn't seem to be terribly important from the aerosol point of view; aerosols tend to have acids in them, rather than a base, sodium hydroxide. And if sodium hydroxide gets in, it gets neutralized by some acidic component. But sodium hydroxide is of some real interest anyway, and you don't have to go up very high in temperature before it melts. It melts at much lower temperature than sodium chloride, for example.

Therefore, you can have not at one atmosphere pressure but at relatively modest pressure, you can go all the way from pure H_2O to pure liquid sodium hydroxide. And at slightly lower temperatures, you can go all the way from pure water to a sodium hydroxide-water mixture that's, say, 90 percent sodium hydroxide and 10 percent water, and that in turn is in equilibrium with solid pure sodium hydroxide.

This system has proven to be interesting and challenging in that, for some reason, it is much more difficult to represent quantitatively than other systems in the same composition range. For example, nitric acid is nominally somewhat similar. Of course, there you can have a liquid phase that's all the way to pure nitric acid at room temperature, and a relatively simple equation of this type is adequate for nitric acid but doesn't even come close to fitting the sodium hydroxide. We're still working on that.

Clegg is still very active and quite widely recognized now, not only in this more or less basic science community, but more in the atmospheric science community. He'll be in this country for some international meeting in that field later this summer. I don't think we're going to get together, but we could. We frequently do after such meetings.

Hughes: Was there a sizeable research community interested in this field, or were you trailblazing?

Pitzer: Well, the aerosol business, that's quite recent.

Criteria for Choosing Papers for *Selected Papers*

Pitzer: I might make a few more comments about certain papers that I did not put in the *Selected Papers* volume but I think maybe deserve a few words, in part maybe for the people involved rather than the science.

Hughes: Maybe you could say something about your criteria for selecting papers for this volume.

Pitzer: Well, it was a question of essentially whether they had a significant impact on what other people did and on the later development of science in that area. In some cases, these have represented more or less completed separate packages, and in other cases, there has been an enormous literature subsequently.

For example, the acentric factor business, as I explained, is just one of the major things that chemical engineers involved in fluids or gas, vapor, liquid matters take as one of the basic items to be considered. On the other hand, some of the things that we've mentioned this morning, like that spin species conversion, seemed to be a very nice, neat piece of science, although they didn't really lead to much of anything of further interest.

It was a really subjective decision. I resolved some uncertainties in that regard by the citation index that you can get now. It's over in the Physics Library here. They have multiyear summaries of citations, and if there were a lot of citations [of any of my papers], I definitely put it in. If it was sort of iffy, then if there were several citations, I'd put it in; if there were relatively very few, I'd tend to leave it out, on the basis that it was still in the literature; it could be found all right.

In general, a book like this ought to justify itself on the library shelf in terms of somebody really wanting to use it. So the citation index was a pretty good indication of whether people would want to use it.

Hughes: Did the number of citations usually correlate with your personal opinion of the quality of the research?

Pitzer: Well, I would say they correlated pretty well with my opinion of the combination of the quality of the research and the applicability of that research. In most cases, the citations were about use of that work in a more applied sense than what I did--although I don't hesitate to do applied work with it; there's a limit to how much you can do, and if a hundred other people are doing it too, they'll produce a hundred times as much.

In other cases, the actual results of the work were not applicable, but the ideas, the methods and so forth, might well be applicable. In that case, you get not dozens and dozens of citations, but you get a few, because at least a few people that carry on in a similar way using a similar method will cite it. And then there are all sorts of mixtures of this.

C. N. R. Rao

Pitzer: In my complete list of publications but not in [*Selected Papers*], there is a paper by C. N. R. Rao,[1] [spells]--actually, the content is unimportant. Dr. Rao--"Ram" is his nickname--came to me as a postdoc with his own ideas of what he wanted to do, which was quite feasible experimentally so I was happy to set him up for it. He finished his Ph.D. at Purdue, I think it was. He was from India and returned there.

He went back to India, and as the years went by, he became unquestionably the top physical scientist in India. He essentially established and led the Indian Institute of Science in Bangalore. He's now retired from that but still active. He became a leading solid-state materials scientist with an essentially physical chemistry background, member of the Royal Society of Britain, foreign associate of the National Academy of Sciences in this country, and for a while was essentially science advisor to the prime minister of India, although the prime minister has changed now and I think he's no longer in that role. But it's interesting just in terms of human relations to be involved that closely, and we've maintained very close contact through the years.

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[1] C. N. R. Rao and K. S. Pitzer. Thermal effects in magnesium and calcium oxides. *Journal of Physical Chemistry* 1960, 64:282.

A very recent postdoc, T. Narayanan, that I had came from the Indian Institute of Science in Bangalore, not via Rao. This young man knew my science and wanted to come, and did, and was effective. He's back in India having a terrible time finding a regular, permanent position, but with my influence with Rao, he at least has a temporary position, according to the e-mail of last week.

Associations with Taiwan and Y. T. Lee

Pitzer: There are a couple of papers that arose out of a visit in 1960 to Taiwan. This was very early for Taiwan to be inviting first-line American scientists to visit. I suppose it happened because we had a Taiwanese senior scientist, K. Pan, not terribly distinguished, who was here for a sabbatical, and we got fairly well acquainted. I think he promoted the visit, and my wife and I spent two weeks there.

Hughes: Nothing to do with Y. T. Lee?

Pitzer: Yes, it will have, later in the story.

We enjoyed that a lot. We were very well taken care of. They gave us so many presents when we left, we could hardly get aboard the airplane. [laughter] They'd given them to us at the last minute; we couldn't have them all packed up ahead of time.

I gave a series of lectures, two in Taipei and two in a second location, which was not so far away, but the people went back and forth by bus or by train to attend either one. Y. T. Lee was one of the audience. So he calls me his first American professor. [laughter] Now he's back as the science advisor to the president of Taiwan, and the director of the Taiwanese Academy of Science.

Hughes: Was that your first knowledge of Y. T.?

Pitzer: I didn't even know him then. He was just in the audience. He came here as a graduate student. I was at Rice most of that time, but we did have some contact. Then he got his Ph.D. here and was at Harvard and then at Chicago and was coaxed back here on the faculty. I suspect my recent presence in Berkeley had a good deal to do with his coming here, because I'd given the lectures--see, this was 1960, just before I went to Rice. So he was familiar with me and familiar with my position here in arranging to come to this country and arranging to come to Berkeley. But I was away, so the idea of actually doing his research with me was out of the question.

But that's been an interesting human relationship through the years, and still going on, of course, although I don't see much of Lee any more. He pops back into this country to attend meetings and so on, but doesn't stay around long enough.

Research on Silver Oxide

Pitzer: There's a series of three papers on silver oxide, Ag_2O, involving two graduate students, Roger Gerkin and [Lawrence V.] Gregor, in that order.[1] There are some very interesting properties. There's a thermal anomalie down around 30 [degrees] Kelvin in silver oxide which is at least better understood after we got done working on it than it was before. [laughs] It doesn't actually cause an entropy discrepancy of significant magnitude at room temperature, but its properties are puzzling, and we did at least untangle that to a considerable degree.

Jenny and Andreas Acrivos

Pitzer: In connection with the ammonia solutions of alkali metals on which I've commented earlier in this session, which is in the *Selected Papers*, we did some other work on that.[2] Dr. [J.V.] Acrivos, nickname Jenny, was a postdoc with me in the late fifties doing magnetic resonance experiments on the alkali metal ammonia systems and related things. She's a very interesting person, too. She's from Cuba.

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[1] K.S. Pitzer, R.E. Gerkin, L.V. Gregor, C.N.R. Rao. Transitions and thermal anomalies in silver oxide. *Pure and Applied Chemistry* 1961, 2:211; K.S. Pitzer, R.E. Gerkin. Silver oxide: the heat capacity of large crystals from 14 to 300 degrees K. *Journal of the American Chemical Society* 1962, 84: 2662; K.S. Pitzer, L.V. Gregor. Silver oxide: the heat capacity from 2 to 80 degrees K and the entropy; the effects of particle size. *Journal of the American Chemical Society* 1962, 84:2671.

[2] K.S. Pitzer, J.V. Acrivos. Temperature dependence of the Knight shift of the sodium-ammonia systems. *Journal of Physical Chemistry* 1962, 66:1693.

I first met her at a meeting, or outside a chemical meeting in Minneapolis, Minnesota--hand-in-hand with a young Greek by the name of Andreas Acrivos who was on our chemical engineering faculty. Obviously, Acrivos is her married name. They were married shortly thereafter, and she came here both as his wife and as my postdoc. So we had a Cuban married to a Greek. [laughs] Their career--careers, plural in recent years--Stanford coaxed him away in chemical engineering, and she got a permanent job, regular faculty position, at San Jose State. They lived at Stanford, which is convenient commuting to San Jose.

Then Andreas got coaxed back to one of these very distinguished specially funded New York state professorships in New York City. They had no children, and he took that position, and she was unable to find anything in New York, at least to her satisfaction. So they've maintained a long-distance relationship, but they actually take sabbaticals and arrange long summer periods in Cambridge, England, and I think they have more married life in Cambridge, England, than in this country. And of course, he goes off to visit people in Greece. My wife and I actually visited his parents in Athens, and then the second time, his father had died, and we visited his mother. Science can lead to some interesting personal contacts too, and very pleasant ones.

Hughes: I've noticed how many foreign people you've mentioned. Is there anything to say about that?

Pitzer: Well, I in no way repel them, so if it works out that way, fine. I've mentioned enough people that weren't foreign, too.

Hughes: Do you think that there's anything atypical about the number of foreigners who have been associated with you?

Pitzer: Well, I doubt it. I may be somewhat more receptive, or they may be more comfortable with me for some reason. But there's no logic to that. Any foreign or overseas connections in my own family are far enough back that they really have nothing to do with it. To get back either to my surname, Pitzer, or even earlier, to my middle name, Sanborn, you'd have to go way before the [American] Revolution. You have to go back 250 years, or even 300 for Sanborn, New Hampshire. Let's leave it at this.

Ion Interaction Equations for Aqueous Electrolytes

[Interview 6: June 26, 1996] ##

Pitzer: The topic [for discussion] that I have written down here is ion interaction equations for aqueous electrolytes, now known generally as Pitzer equations. They're used very widely now not only in chemistry and applied areas, chemical engineering, but also in geology, geochemistry, chemical oceanography, and so forth.

Aqueous solutions with electrolytes, with salts in them, are commonplace. We're full of them. We have a lot of organic components in us too, [laughs] and that's not the feature here; it's just the salts and the other relatively simple electrolytes in solution. They have been of chemical interest for a long, long time.

Early Research by Others

Pitzer: The ones with ions, such as ordinary salt, for example, were puzzling as the physical chemistry of solutions developed. They didn't obey the rules that seemed to fit all mixtures of neutral molecules. And the untangling of this puzzle had various stages. It was quite an active subject early in this century, when the fact that it really was anomalous and needed a special explanation became clear. G. N. Lewis, who was really responsible for putting this department on its leading course, played quite a role in the period about 1918 to 1922.

I wrote a paper and gave it at a meeting in recognition of Lewis that was held in--at least the paper was published in 1984; the meeting might have been held a year or so earlier. There are a whole series of papers that were published by the *Journal of Chemical Education*, and I chose the title "Gilbert N. Lewis and the Thermodynamics of Strong Electrolytes,"[1] in which I summarized that situation, and how Lewis and his associates had made a lot of progress in untangling this puzzle, without really getting down to the deepest level. But at the more practical level, they had it pretty well worked out, along with a Dane by the name of [J. N.] Bronsted, who played a comparable role in those same years.

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[1] 1984, 61:104-107.

One of the interesting sidelights is that Bronsted, although Danish, published almost all his work in that period in the *Journal of the American Chemical Society*, not in the British chemical journal, not in the German chemical journals, but in the American chemical journal. Which indicates that Americans, and in particular Lewis, must have been playing an important role and showing great interest in that field at that time.

The deeper explanation came with Debye and his collaborator, Hückel, in 1923, which simply gave a more microscopically mathematical explanation, with some very clever approximations to a problem that had just been too complicated for anybody earlier.

Hughes: Can you say something about how they arrived at those approximations?

Pitzer: Well, Debye was very good at taking problems of great complexity and selecting an expression for the dominant aspect, which constitutes the approximation, leaving out less important things, to be added as second-level adjustments if need be. He did that with several other topics, some of which preceded this work in 1923. For example, his theory of the heat capacities of solids, which I guess goes back maybe a decade earlier, was a comparably important advance and a comparable sort of thing.

Well, after 1923, with Debye and Hückel's explanation in more detailed justification, together with Lewis and Bronsted's more empirical but very important contributions, there was a big wave of research in that field in the twenties, into the thirties, but the equations used to represent the solutions' properties with any theoretical basis still stopped at very dilute solutions. The Debye-Hückel theory was really just a theory of the behavior as the concentration approaches zero. It fits up to a very dilute solution, but there is practical interest in higher concentrations. People made measurements at higher concentrations. But the representation of the data at higher concentrations was just a purely empirical series of increasing powers of the concentration.

The Debye theory gave you a theoretical coefficient for the one-half power of the concentration. There was reasonable understanding as to the first power of the concentration term, although it had to be empirical. There was no simple theoretical way of calculating its numerical magnitude. But beyond that, it was uncertain whether you needed a three-halves power or the next power was just the second power of the concentration, and then how far up you had to go was a purely empirical matter.

This was clumsy in the sense that for almost any real system, the terms were of alternating sign, which indicated that they were not capturing anything of physical meaning. The contribution of one term was largely canceling out the one before and the one afterwards, and that type of an equation is never very satisfactory, although it can be used if you haven't anything better.

Pitzer's Entry into the Field

Pitzer: I was aware of this through the years, but I didn't really have any useful contribution to it. In the late 1950s, when Professor Brewer and I were revising the Lewis and Randall thermodynamics book, which led to its second edition carrying Brewer's name and mine, he undertook a scheme which improved the situation somewhat but still had some really unsatisfactory aspects to it that I won't bother to try to describe in detail.

When I was looking for new things to get into after my period as a university president and was back in Berkeley in the early seventies, I had this aqueous solution problem in mind. In fact, in the year before I was back at Berkeley when I was on sabbatical leave, if you wish, after the Stanford presidency, I did a little work on that topic in Cambridge, and there is a paper out of that, but it did not really solve the problem to my satisfaction [ref #195]. It just renewed interest and familiarity with the literature of the time.

Hughes: Now, were there reasons why you thought you could carry the calculations further?

Pitzer: Well, I would claim in a somewhat different realm that I had a reputation somewhat like Debye's, of being able to find a scheme of approximation for a complex problem that captures the most important aspect of it and puts that in the equation. Aspects of this sort appeared right from the beginning, in the internal rotation work, the ring molecules, the paper with Latimer on the free energy of hydration of ions, the various papers later all had elements of this in which I was able to make a good approximation for a complex system which was substantially different from what had been done before. There are degrees of departure from what's been done before, and this aqueous solution case was one where that was more striking and more important than maybe some of the others.

Actually, when back in Berkeley, this was not the first thing I worked on. I may have proposed it to graduate students, but they didn't take it up. My first collaborator was a young man, Guillermo Mayorga by name. That's William in Spanish, isn't it?

Hughes: Yes.

Pitzer: He'd gotten a master's degree at Hayward State College, and there was some program at that time of tiding over scientists until they found more permanent positions. He got a fellowship of some sort under this program, and I had him essentially just search the literature and then put the important data into his computer system, which was relatively primitive then, but still much better than slide rules and pencils and paper, and then fit these equations.

The Three Papers Forming the Basis of the Pitzer Equations

Pitzer: Now, that's getting a little ahead of the story. The first paper[1] has just my name on it, and I did it without collaboration after I had gotten students at work on, for example, spin species conversion for methane, and the work on the xenon fluoride, and a few other things that students took up with me in the early seventies. And it was in the very first paper that the essential approximation was developed.

This really depended upon familiarity with some rather complex statistical mechanics. I guess it was Joseph Mayer, and possibly his wife Maria may also have been involved with it, also Harold Friedman, in which they showed that for an ionic system, as compared to a neutral molecule system, the various terms involving representing interactions of unlike molecules as well as like ones would have what's called an ionic strength dependence if they were charged particles, whereas these would be just constant terms with the powers of density or concentration or whatever, for neutral molecules.

Well, the idea of ionic strength was one of Lewis' contributions back about 1920, so that wasn't anything new. What was new was that this ionic strength dependence was to be expected not only for the Debye-Hückel term, the half-order concentration

[1]K.S. Pitzer. Thermodynamics of electrolytes. Theoretical basis and general equations, *Selected Papers*, pp. 386-395.

term, but also on all the higher terms. And so far as I know, no one else had applied this in any simple way. Just to know it in principle didn't do you much good; you had to figure out how to represent it in reasonably compact fashion.

So I worked on that, and having successfully set up the form of the equation, which essentially just amounted, in addition to a constant term for binary interactions of both like and unlike particles, an ionic strength dependent term. But it involved devising a form of ionic strength dependency that was simple enough to be used conveniently and yet fitted the actual behavior of not one but maybe a dozen examples that I tried initially myself.

With that indication of promise, I put Mayorga to work collecting data for a large number of systems, which were all in the published literature then, for room temperature; that is, usually for twenty-five degrees Celsius, and applying this equation. I stopped the equation initially at three terms. There were two parameters for the first power of the concentration term, and then just a single parameter for the second power, the square term, so there were three parameters. And then I had Mayorga make the calculation to fit the data up to the maximum concentration if those terms would fit it, or otherwise, to fit it to as high a concentration as the simple equation would fit, but not to add any more terms, not to try to go any further.

For most solutions, it fit it over the whole range. Sodium chloride, for example, is soluble six moles per liter or per kilogram of water at room temperature, which is quite a lot higher than, for example, sea water. But the equation fits all the way up to the maximum. On the other hand, for some other things that are even more soluble, or some other things where there are more complex inter-particle interactions, the equations might fail around six moles, but the substance was soluble up to twelve or fifteen or twenty. Or, the substance was more complicated, frequently because the ions had larger charges on them, and then the equation might only work up to one mole per liter.

Well, those two papers, one of mine and then one with Mayorga, are essentially the basis [of the Pitzer equations]. Like a lot of other things, they got elaborated, but they're the first two papers in that section in the *Selected Papers* volume.[1]

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[1]K.S. Pitzer. Thermodynamics of electrolytes. I. Theoretical basis and general equations, pp. 386-395; K.S. Pitzer, G. Mayorga. Thermodynamics of electrolytes. II. Activity and osmotic coefficients for strong electrolytes with one or both ions univalent, pp. 396-404; *Selected*

The paper number two, including Mayorga, was limited to cases where one of the ions had only a single charge. The other ion could have a higher charge.

That covered a great deal of territory. We showed that, of the two parameters that were needed for the first-order term, there was a relationship between the two. It wasn't exact, but we showed graphs with one parameter plotted against the other one, and most of the examples showed within a 5 or 10 percent relationship of one parameter to the other for, say, 1:1 electrolytes, and then another graph for 2:1 electrolytes where that relationship wasn't anywhere near as good.

Still with Mayorga, we looked at 2:2 electrolytes,[1] say like magnesium sulfate, and for that, I had to add one additional term. It was of the same form as the ionic strength dependent term which was the key to the whole system before, but it had very different values of parameters to specifically represent an ion pairing tendency of the positive ion with the negative ion when they both had a double charge. I gave some theoretical basis for it, and it's stood up pretty well through the years.

The Fourth Paper, with Janice Kim

Pitzer: The first three papers were on pure electrolytes; in other words, one pure salt or acid or base, in water. The fourth paper,[2] which involved a graduate student, Janice Kim, was our move into mixed systems, although I had in mind getting into mixes right from the beginning. Indeed the publication date on this fourth paper is 1974; it's only one year later, so it was going on more or less simultaneously.

The new terms for the mixed system were only for interactions of ions of the same sign of charge. In other words, all the terms that involved a positive ion with a negative ion

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Papers, pp. 386-404.

[1] K.S. Pitzer, G. Mayorga. Thermodynamics of electrolytes. III. Activity and osmotic coefficients for 2-2 electrolytes; *Selected Papers*, pp. 405-412.

[2] K.S. Pitzer, J.J. Kim. Thermodynamics of electrolytes. IV. Activity and osmotic coefficients for mixed electrolytes. *Selected Papers*, pp. 413-419.

were carried over unchanged from that pure electrolyte. It was only if you mixed sodium chloride with potassium chloride that a new potassium-sodium interaction term appeared for the mixture. And it was small, in most every case. Bronsted would have said it was zero back in his work in the early 1920s. He suggested that since like-charged ions repelled one another, they wouldn't get close enough to have a significant interaction with one another, and he was about 90 percent right. [laughs] But to get quantitative agreement, we had to put in like-ion interaction terms, although in some individual cases, they were zero within the accuracy of the data.

Janice Kim was very effective. Again, she found a lot of examples in the literature and worked them out very efficiently. She was an interesting young woman, and she later decided science was too impersonal. She wanted to do something that involved more human personal relationships, and she decided to go to medical school. But then her pattern changed and after a relatively short attempt to get into ordinary private practice, she went back into medical research. [laughs] Not too many years ago when I was last in contact with her, she was over at UC San Francisco in medical research activity there.

The next advance was made when, for what reason I don't quite remember, I had occasion to be in Oak Ridge with the Oak Ridge National Laboratory group that I became very closely acquainted with through the years. I was there for two or three months in the spring of 1974. I suppose it was a one-quarter sabbatical or something like that.

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Pitzer: In the work with Janice Kim, there were problems when the two ions of the same sign had different charges. In other words, say, calcium ion interacting with sodium ion, with a double charge and a single charge, there seemed to be a little difficulty. But I didn't really run into it for doubly charged ions; I ran into it primarily for triply charged ions, say aluminum ion interacting with sodium ion.

So I looked back into this Mayer and Friedman theoretical work and found a basis there for an additional term that was added onto the general theory for that particular case of a mixed system where ions of different numerical charge in same sign were present in appreciable amount. That first paper showed that it was barely significant for 2:1 mixing if the data were accurate, but it was really a major effect for 3:1 mixing. Those calculations were actually carried out at Oak Ridge, and that's acknowledged, although the paper was eventually published after I was back here.[1]

Other Papers on the Thermodynamics of Electrolytes

Pitzer: The other papers that I put in the *Selected Papers* volume were just examples, I thought very interesting examples, that actually were next in time also. The first of those concerned phosphoric acid,[2] which is only weakly dissociated, but where there's multiple dissociation, possibilities of mixed solutions with salts and a number of interesting aspects.

The final one that was put in the *Selected Papers* volume concerns very important and common sulfuric acid,[3] where the first association of the hydrogen ion with the sulfate ion to the HSO_4^- ion, is relatively strong, such that in a concentrated solution, you have to recognize that it is a separate species. So although there are only two components here, we had to treat it as if it were a mixture of a three-component system.

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[1] K.S. Pitzer. Thermodynamics of electrolytes. V. Effects of higher-order electrostatic terms. *Selected Papers*, pp.420-436.

[2] K.S. Pitzer, L.F. Silvester. Thermodynamics of electrolytes. VI. Weak electrolytes including H_3PO_4. *Selected Papers*, pp.437-446.

[3] K.S. Pitzer, R.N. Roy, L.F. Sylvester. Thermodynamics of electrolytes. 7. Sulfuric acid. *Selected Papers*, pp.447-453.

Collaborations

Rabindra N. Roy

Pitzer: My collaborations for the phosphoric acid paper and the sulfuric acid paper were primarily with Leonard Silvester, who had gotten his Ph.D. up at UC Davis. But for the sulfuric acid paper, I had another collaborator, Rabindra N. Roy, who was professor of chemistry at a liberal arts college in southwestern Missouri, Drury College, and asked if he could come for--I guess it was for the summer. I probably found him some financial support. He participated and made real contributions in the sulfuric acid paper. I'm sure so far as that paper is concerned, Silvester and I would have gotten essentially the same results.

The interesting part is what's happened since with Rabindra Roy. He keeps wanting to collaborate, but doing most of the work there at Drury College. He gets undergraduate students to do the experiments, and then he wants me to help on the more sophisticated interpretation of them. He does the interpretation to a point, but if it's complicated, he wants me to help. Through the years, I keep suggesting to him systems that are complicated, for good reason: the simple ones have already been done. He is remarkably successful in getting students enthusiastically involved, getting some financial support for them, taking them to meetings.

He was born and raised in the northeast of India, maintains contacts there, takes students on trips to India, is active in international chemical organizations, especially those for the Pacific Basin, and always shows up at those meetings. Right now I'm in the midst of another collaboration with Roy on a system that has a particular complexity, namely, indium chloride in mixture with hydrochloric acid and water. He and his students have made a lot of measurements, and he has a postdoctoral fellow, a young woman, Kathleen Kuhler, who has helped supervise and is working on the interpretation. But they have come to some difficult problems and they want me to help out, and I find it very interesting. Of course, in suggesting the problem to them, I anticipated complexities. So this has gone on now from 1977, almost twenty years.

Hughes: What is the basis of his skill for getting students interested in problems such as this?

Pitzer: Personality. His own enthusiasm. I find it remarkable. In various meetings he'll take the students. For the most part, they won't give talks. There will be poster sessions, and they'll have their posters. There will be certain hours when the attendees at the convention are supposed to go look at the posters, so I always go look at Roy's students' posters and visit with the students, and they clearly are enjoying it.

The last time, it was the Pacific Basin Chemical meeting--they're always held in Honolulu, and this one was just this last winter, before Christmas. He had his group of students there, and I gave a talk on an only remotely related subject in the same session that he had his postdoc give one talk. And then, of course, he had his students giving poster sessions. It was fun to visit with him, and as I say, it's remarkable how much enthusiasm he generates. And he has other scientific activities, aside from the one that involves me. He's off now with a medically related program somewhere in Texas.

Hughes: How does he get funding for all these activities?

Pitzer: By making proposals to various organizations for grant funding. This postdoctoral student, Kathleen Kuhler, has what I think is called a Dreyfus scholarship. Dreyfus is trying to encourage young people with better than average scientific background and skill to go into college teaching, or conversely, if they go into a more research-oriented institution, to bring greater experience and enthusiasm for teaching into their later career. She apparently, I judge, has been at Drury College for probably two years. She's at the end of her time this summer and is going to Texas Tech out in northwest Texas, at Lubbock, on hopefully a regular academic career. I don't know her that well; I met her at the Honolulu meeting, and I'm having fax communications now about remaining calculations that need to be made.

Roy and I had an earlier collaboration on a 3:1, 1:1 electrolyte mixture, lanthanum chloride and hydrochloric acid. I then suggested, "Well, why don't you try a 4:1?" And the only 4:1 electrolyte that is really available conveniently is thorium chloride. His experimental measurements are electrochemical in cells with a hydrogen electrode and a silver-silver chloride electrode, which is sensitive to the chloride ion. So it really makes measurements just of the hydrogen ion and the chloride ion. The thorium ion, however, with its four-fold charge, has a strong influence on that but isn't measured directly.

Well, we very frankly ran into trouble interpreting the results, but I was familiar with a man who's up in Washington state at the Pacific Northwest Laboratory who had been doing other work on thorium salts, including some use of my equations. His name is Andrew Felmy. I got in touch with him, and he was interested in this, and he thought he could put the package together, and did. So we had a paper with about seven names on it, with Roy and Felmy and myself and four or five students that had made the measurements.[1] Maybe he had a more senior person who was supervising some of the measurements that were included.

Hughes: You seldom have a large number of authors on your papers. Is that true of physical chemistry in general?

Pitzer: I would say that's pretty true in physical chemistry. There are usually just three or four authors at the most, frequently just two. The more complex the instrumentation gets, the more you're likely to have people that, as it were, just keep the instruments running. They don't really do the experiments, but it's complex enough that you recognize them with co-authorship. And so on these physics papers where the list of authors is sometimes longer than the text of the paper or the letter to the editor, [laughs] most of those people, I judge, have just been running electrical tests and putting in new transistors or things like that.

Roberto Pabalan

Pitzer: I'd like to mention Roberto Pabalan, who was my first geochemical postdoc. He made measurements and calculations for high-temperature systems. Up until that point, we had done some calculations away from 25 degrees C., but we hadn't really pushed it up in temperature. Pabalan came from Penn State with a geochemical degree. There was a very excellent geochemical program at Penn State in those years. The whole idea was to push things up to high temperatures, both with experimental measurements here and with calculations based on high-temperature data elsewhere.

As a separate project, not particularly focused on the ion interaction equations, we had developed a high-temperature heat capacity calorimeter. A graduate student, P.S.Z. Rogers, and I designed the calorimeter, which was of a novel type, and had made a few measurements. Now it had become fully efficient and available. Pabalan was very effective in making a number of measurements on more than one system. But he also pulled information out of the published literature and found that the ion

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[1] R.N. Roy, K.M. Vogel, et al. Activity coefficients in electrolyte mixtures: $HCl + ThCl_4 + H_2O$ for 5-55 degrees C.," *Journal of Physical Chemistry* 1992, 96:11065-11072.

interaction or Pitzer equations worked pretty well for many systems up to about 300 degrees C., or 573 Kelvin, but not much further.

I didn't expect them to work much further, because these equations are based on the idea that ions are at least primarily dissociated. We know perfectly well that as the temperature goes up, the dielectric constant of water goes down. Then the ions tend to associate with one another to form neutral species more and more as you go up in temperature. For one or two systems, you can go up to 350 [degrees C], but for the most part, 300 is the maximum, and if you get multiple charged ions, you have to stop sooner.

Well, in addition to his own measurements on certain particular systems, Pabalan did go over the literature pretty generally and made calculations involving the solubility of the minerals right up to the saturation concentration, mineral solubility, in some but not all cases. The equations worked up to that limit in some, not in others.

##[1]

Pitzer: I'd like to say a few more words about my collaborators that had geochemical background. [Roberto] Pabalan came with a geochemical background with a geology degree, but a thesis on essentially chemical work of interest to geology. After Pabalan, I have had a series of people with essentially the same background, for the most part coming from either Penn State University or from the Virginia Polytechnic Institute and State University, where the program under Professor Robert Bodner is very active in this area, and Bodner in turn had come from Pennsylvania State University.

Both S. M. Sterner and Charles Oakes came later from Bodner's group. I had John Tanger also, who got his Ph.D. in geology here at Berkeley. Tanger, however, was more of a chemist than a geologist, and his work with me was sort of a transition, and I don't think he's done any geology since. He became more of a chemist or chemical engineer. In his work with me, there were really three quite important papers that are frequently cited, one on a new type of equations, one on equations for certain chloride water binary systems, another on the dissociation of water to

[1]The following is an excerpt from Interview 12, January 15, 1997, which was conducted to amplify Dr. Pitzer's earlier remarks on various subjects. Future inserts from this interview will be noted in the tape guide, which follows the transcripts.

hydrogen and hydroxide ions. All are for extreme temperatures; the last valid is to above 2,000 Kelvin.

In addition to these individuals who were postdocs with me, I've had a longtime cooperation and on a few occasions an actual collaboration with James L. Bischoff of the U.S. Geological Survey across the Bay in Menlo Park. Bischoff was a Ph.D. in geology here at Berkeley some years ago, but had gotten into some very important work involving experimental studies of essentially a chemical nature of very immediate interest to geology at high temperatures and pressures. This has been a very productive and very happy and friendly relationship through the years, and we have some nonscientific interests which we enjoy also, including the wagon trains by which people got to Oregon and California back in the middle of the 19th century.

Hughes: You mean the history thereof?

Pitzer: The history thereof, and visiting sites and so on.

John Weare

Pitzer: Well, now on to John Weare. He's a regular faculty member at UC San Diego. In the late seventies, he got in touch with me, I don't know whether it was just by telephone or whether it was some written communication. He indicated an interest in using my equations for mineral solubility calculations, and I said, "By all means, go ahead." I might have gotten around to it eventually. But we were more interested in moving to high temperatures for a few systems. Thus, I thought or said, "If you want to look at them more generally at room temperature, by all means do so."

John Weare had an interesting background in his doctoral work at Johns Hopkins University in that he'd had some geochemical projects with one professor, and then some rather abstruse physical chemistry, primarily theoretical chemistry projects, with a different professor. But that gave him an excellent background for this [research]. With graduate student collaborator [C.E.] Harvie for the first two papers, he made these calculations using my regular equations as they stood at that time. That included the additional terms that I've been talking about through the papers that are in the *Selected Papers* book, the special terms for highly charged ions and for unsymmetrical mixing and so on. He used very efficient mathematical techniques.

Weare covered a lot of territory and published very comprehensive papers,[1,2] one in '80 and another one a little later [1984], in the primary international geochemical journal known as *Geochimica Cosmochimica Acta*. This in effect introduced my equations to the geochemical literature, not that they hadn't been aware of it up to that time, but it was great advertising, if you wish, for me.

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Pitzer: Just a few words that probably would be unnecessary, but the work that John Weare did, about which I've already commented, also fell into this geochemical area, although his background was essentially chemistry.

Foreign Researchers

Pitzer: Another topic I thought we might add a few words about concerns foreign visitors. This was very uncommon in the early years, but after World War II it became fairly common.

Hughes: What was the reason for that? Financial mostly?

Pitzer: Yes, it's just money. Nobody had enough money to go wandering around the world.

Hughes: It must have had an effect on how science was done.

Pitzer: Yes.

Hughes: I mean, the fact that you didn't have this personal interaction.

Pitzer: Yes. You had communication, but not the personal contacts that became common later. Now, it may have been fairly common back in the twenties, when there was a relatively affluent financial

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[1]C. E. Harvie and J. H. Weare, The prediction of mineral solubilities in natural waters: the Na-K-Mg-Ca-Cl-SO_4-H_2O system from zero to high concentration at 25°C, *Geochim. Cosmochim. Acta*, 44, 981, 1980.

[2]C. E. Harvie, N. Moller and J. H. Weare, The prediction of mineral solubilities in natural waters: the Na-K-Mg-Ca-H-Cl-SO_4-OH-HCO_3-CO_3-CO_2-H_2O system to high ionic strengths at 25°C, *Geochim. Cosmochim. Acta*, 48, 723, 1984.

situation, but as I became aware of things--after all, at the time I was here at Berkeley with my bachelor's degree in '35, we were beginning to squirm a little with respect to the Depression, but we'd been in a state of depression for several years already, and really never came out of it fully until after World War II.

Well, anyway, the particularly interesting aspect of this, I thought, was that in the 1950s, I had two Japanese students. They of course were undoubtedly somewhat uncertain as to what sort of a reception they were going to get, but they handled themselves very well, and I thought it was fine to generate more person-to-person acquaintanceship among scientists with Japan.

Hughes: Was that not a common sentiment right after the war?

Pitzer: Well, I think it was welcomed as soon as it sort of became feasible and people started to do it. It was just that nobody was going to do it in the very next year or two. We had too much readjustment ourselves.

By name, it was Y. Mashiko and Tatsuo Miyazawa. Miyazawa accomplished quite a little while he was here, three papers in all, including two that are rather frequently cited. They concerned carboxylic acid dimers and their spectra, and of course, the particular interest on the spectra concerned the hydrogen bond situation in the dimeric molecule, where the force forming the dimer was a pair of hydrogen bonds between the hydroxyl groups. And then we were concerned with the entropy and other properties. And then also, the situation for some carboxylic acids which form linear polymers instead of dimeric molecular species. This work, as I say, was quite substantial and is still being cited fairly frequently.

Hughes: Did these two Japanese men come primarily because their scientific interests coincided with yours, or was there also an element of science being in disarray in Japan because of the war, or the aftermath of the war?

Pitzer: I think some of both.

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Pitzer: I've forgotten how the visits were financed. I think they both came with at least some money of their own. I may have supplemented this to some degree. Miyazawa actually came with his wife, whose English was much better than his. [laughs] She interpreted for him some of the time.

About the same time, I might just mention that an American Ph.D. student of mine, Roger C. Millikan, was also involved in the work particularly on formic acid, the simplest of all carboxylic acids, and his papers still receive quite a little attention.

V. K. Filippov ##

Pitzer: Interestingly, at almost exactly the same time as the Harvie and Weare work, V. K. Filippov in Leningrad, now of course St. Petersburg, started using the equations in a manner rather similar to John Weare's but aimed more at practical applied chemical and metallurgical interests rather than mineralogical interests. I was unaware of Filippov's work for quite a number of years, and John Weare hadn't even noticed it when I told him about it. By that time, Filippov had published at least eight or nine papers that were for the most part in journals that are translated into English, but I just wasn't monitoring them.

Hughes: How were you monitoring in those days?

Pitzer: Well, I would just read; pull the current journal off the shelf as it came in and glance down the table of contents.

Hughes: No computer searches.

Pitzer: Not yet. Computer searches are efficient if you have the name of the author. You can do it on titles and so forth, but it's really efficient for authors. Until you know that some author is likely to be publishing something, you don't put that name in.

Filippov really did a quite comparable array of work. It was published more piecemeal in the primary Soviet publications of the time, and then eventually, he began to put papers in the Western European or international journals. He was a very able man, too. In 1990, Filippov arranged for a visit here. I had been aware of him by that time for a couple of years, so I was most happy to see him. In fact, there was some sort of a joint University of California-Leningrad program to encourage exchange visits.

Hughes: In chemistry?

Pitzer: No, across the University of California statewide, not just Berkeley. I had actually begun to arrange to go there after a meeting in Italy; it was at Lake Como, if I remember rightly. But the arrangements proved to be very troublesome. I wanted to take my wife along, and she was going to be with me in Italy; there was no point in trying to not take her to Leningrad, but the agency at that end essentially refused initially to make arrangements for her. So I just canceled the Leningrad end and wrote Filippov that although I'd hoped to come--September, '89, I guess it would have been, '89--that the arrangements had just proven impractical and I wouldn't be there.

Well, the Russian mail service is absolutely abominable. Filippov told me that they actually went out to airport and train stations looking for me. I don't know how they even got a specific time. I had suggested an approximate time, within a few days after this other meeting. And the letter by mail came a few days later saying I wasn't coming.

But the arrangements coming this way worked all right, and Filippov came. I found he was in very poor health. We were able to arrange to get some medical examination for him, although I don't know that it did any good, other than saying indeed he did have very serious problems. I've forgotten just what they were. But we had a nice visit, and I sent him down to visit with John Weare, which was easy to do since it was within the university, although we could have done it anyway.

Filippov died within a year after he was here. That program has more or less fallen apart; that is, some other Russians are continuing activities of this type using my equations, but that very active group at now St. Petersburg has dissipated.

Hughes: Did you ever actually collaborate?

Pitzer: We didn't actually collaborate. Well, I didn't actually collaborate with John Weare either. We had very close discussions and so on, but I don't think there are any papers that have both names on them. Felmy was a student of his, so I've collaborated with a Weare student. Also, there is Sergey Petrenko, a man whose primary career had been in Moscow but who was with Filippov to help him with computations and whose name appears with Filippov. Petrenko is now here with me as a postdoctoral student.

Activity Coefficients in Electrolyte Solutions

Pitzer: There are a couple more collaborations I'm going to get to, but I think now is the time to take up the book entitled *Activity Coefficients in Electrolyte Solutions*, which now has gone through two editions. It's published by CRC Press. The first edition, edited by R. M. Pytkowicz, was published in 1979.[1] Pytkowicz had actually been a Ph.D. student here in Berkeley in the sixties when I was not here, or possibly even earlier, in the fifties.

He'd gone into chemical oceanography and was on the faculty of Oregon State. He invited me to prepare a chapter, and the chapter is essentially what's in the *Selected Papers* book, reorganized as a single chapter and with a few additional items, but not much different. But it was nice to bring it all together. That edition was published in two rather thin volumes, and with a lot of other work on aqueous solutions, some more theoretical, some focused on particular measurement techniques, and one or two by oceanographic people.

This was prior to John Weare's and Filippov's work, although it came out just about as they were starting. Along with the Weare and Filippov work, it played a considerable role in, as it were, advertising the method to people that might not have thought of it otherwise.

The second edition was planned for 1988 or '89, with Pytkowicz still editing it, but he was taken ill after a reasonable number of authors had been signed up, and actually he died within a year or so afterwards, but he had given up the editorship before then. By that time, I had pretty well done my revision, and I recognized that it was worthwhile. By this time, the citation index process was available, and I did observe that by the late 1980s my chapter was by far the most widely cited chapter in the book. In other words, most of the citations to the book were based on my chapter. That may be somewhat overstated, but to a considerable extent, it was true.

But CRC Press didn't seem to do much about it, and I didn't want my revised chapter to go [laughs] to waste. I communicated with another chapter author, Robert M. Mazo, who was at the University of Oregon, just down the road at Eugene from Corvallis, and who must have known Pytkowicz personally. We were talking about, well, what should we do about this? Then I got a promotional type of mailing from CRC Press, the substance of which was, "Suggest to us a new subject and possibly an editor, and we'll give you this advantage or credit for other CRC Press publications," or some other positive incitement.

So I called up the 800 number and said, "I don't have a new book to suggest to you; I suggest you get an editor for this one

[1] The second edition was published in 1991.

and get on with it." Within about two weeks, I got a call back from the head of the organization and said, "Will you be the editor?" [laughs] So I figured that was the way to get the chapter published.

I didn't have any difficulty with it. Most of the chapters just went ahead. I did think that the chapter on electrochemical cell measurements needed more up-to-date attention and got Rabindra Roy added as a co-author of that chapter so that it would be somewhat broader. I added a co-author to the isoplastic measurement chapter: Joseph Rard out at Livermore. He essentially did the chapter for the second edition, but he carried forward as a co-author the name of the one [Robert Platford] that had done the first edition.

For the second edition, I had already agreed to do a new chapter with Pabalan as a collaborator, essentially extending the work he'd done with me as a postdoc in high temperature and mineralogical systems. The first edition had had a marine chemistry or oceanographic chapter from a man named Michael Whitfield, a Britisher, whom I had become fairly well acquainted with. I actually had visited him at Plymouth, England, at his oceanographic laboratory during the mid-eighties. He was going to do a revision. He brought in a younger Englishman by the name of Simon Clegg as a co-author, and Clegg did most of the work on the revision. Clegg by that time had already gotten involved with the use of my equations for his own work, and I will come back and tell you more about Clegg later. I think that's a good way to leave it.

The only chapter I added beyond what Pytkowicz had intended was one from Oak Ridge with Robert Mesmer--he was the senior author--on ion association at high temperatures. That's not my equation business at all; the Oak Ridge people were using them where appropriate, but they had made very important contributions at still higher temperatures where the ion association was a dominant feature. I thought that was important to have in the overall picture.

Well, the new edition got published in 1991, and as far as I know, it's doing fine. There was an interesting sidelight on that: the CRC Press insisted on the editor making the financial arrangements with all the chapter authors and then implementing them. In other words, they sent me the whole chunk of money, and I had to parcel it out to the chapter authors according to the formula that had been negotiated and put into the contracts. So I had to learn how to deal with the IRS so that I did not pay taxes on their chapter royalties. [laughter] So I know how to do that now. I don't expect to have to do it again, but it all worked out.

Hughes: Is there usually just a flat figure for a chapter?

Pitzer: I think it related to the length of the chapter, just a flat figure.

Hughes: Per page?

Pitzer: A per-page type figure. Of course, I received that for my one chapter, and I think I gave the whole sum for the Pabalan chapter to him; I don't remember whether I kept any of it or not. And with the other co-author chapters, it was up to the co-authors to decide how to split it. Although they had to tell me, I think, how they were going to split it, because otherwise, the wrong person might have to pay taxes on it. Not that it was all that much. Now, I've forgotten whether there was any provision for further royalties. But I'm sure they were just to the editor, and I don't recall having gotten any more money from it, although I might have gotten a trivial check some year.

Well, so much for the second edition. It's done very well, and it provided a communication for a number of things.

Hughes: What sort of readership does it attract?

Pitzer: Oh, it's not really so much in chemistry itself, although it is substantial there. But it's in marine chemistry, aqueous geochemistry, some other aqueous geological fields, chemical engineering areas related to aqueous systems, some metallurgical separation operations; in other words, before you reduce the metal, you handle aqueous systems. There is not very much work now on improved theory or equations, although papers keep popping up. But the improvements, if any, are trivial usually. They don't really attract a lot of attention. But there are activities of that sort.

More On Collaborations

Simon L. Clegg

Pitzer: I thought I'd add some more about two collaborations. Going back to Clegg, Clegg's primary interest is in atmospheric science. He was collaborating with an oceanographer in the chapter in the book, but his own work is aimed at atmospheric science. To take an example, if salt spray from an ocean beach, or something like this, gets thrown up into the air and the particles are small enough, they remain suspended for a long period of time. There are some sea salts in the water, and if the humidity goes toward the low side, the water will evaporate and the solution can get very concentrated.

Well, now, this sort of thing has become very important lately. What is the catalyst for ozone decomposition in middle latitudes, which we're beginning to learn more about now, and particularly in the Antarctic, where the ozone seems to just plain disappear at a certain time of year? It's becoming more and more clear that it is these concentrated electrolyte aqueous particles, well below room temperature, probably with both sulfuric and nitric acids in them, that serve as the catalyst.

There is a letter or short report in the *Science* magazine that came yesterday on this very topic, essentially adjusting that theory, saying that they thought it was just sulfuric acid and maybe crystalline particles, but there's a lot of nitric acid in the particle and it is noncrystalline, it's just a very concentrated aqueous solution. One of the authors that was cited as primary source on this theory was Simon Clegg. Well, he got in touch with me about equations for these more concentrated solutions. Now I'll detour back to earlier work.

There's one article in this *Selected Papers* volume entitled "Electrolytes. From dilute solutions to fused salts," 1980,[1] in which I was not extending my regular electrolyte equations with just another term or two but was trying another approach that would go all the way to a fused salt. And there are a few examples in the literature through the years. One of the examples that I chose for this particular paper was lithium nitrate-water at about 100 degrees C., or a three-component system, lithium nitrate, potassium nitrate, water at a little above 100 degrees C., where there was data all the way from the pure liquid fused nitrate to pure water, partly from Oak Ridge but from a different group at Oak Ridge than the Mesmer group.

I showed that these data could be fitted by a combination of the Debye-Hückel term and what was known as Margules terms. Well, the Margules terms were just what you use for a nonelectrolyte, for a neutral molecule fluid, so there wasn't anything terribly surprising about this. The only surprise was why somebody else hadn't done it sooner.

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[1] pp. 512-517.

Hughes: Why hadn't they?

Pitzer: Why hadn't they combined a Debye-Hückel term with a typical nonelectrolyte equation? I don't know.

Well, this was not the only paper on this. In fact there were three papers later with J. M. Simonson when he was a graduate student with me.[1] He went later to Oak Ridge and has had a very fine career there. We applied essentially this same procedure. I think we added one additional Margules term. Also another person, O. Weres, who was interested in the method at the same time, published another paper in the same series.[2]

Clegg was aware of this, and he wanted to apply it to his room-temperature and even low-temperature systems, whereas work that I'd done had been high-temperature, all the way to fused salts. I might have tried it for nitric acid at room temperature; you can go all the way from pure liquid HNO_3 to pure H_2O at room temperature, but I didn't. Clegg in due time did, using exactly the equation formulation that Simonson and I had used earlier. [tape interruption]

As Clegg used this formulation, he had difficulty fitting some of the systems that were more difficult to fit. Admittedly, I suspect I had chosen ones where it worked and not spent too much time on ones where it didn't work. So he wanted suggestions as to what could be done to improve things, and out of my experience on the other equations which are for limited solubility, I had suggestions.

I should emphasize one difference between these two sets of equations. The regular so-called Pitzer equations use molality as the composition measure. That's moles of solute per kilogram of water. And you can't use that all the way to high concentration because the molality would become infinite for the fused salt. So you have to use mole fractions as the variable so that the mole fraction of salt becomes one for the pure salt, and the mole fraction is one for water at the other end and the sum of the two mole fractions, of course, is unity.

One feature of my equation was, they were intended for the range where the salts are dissociated to the ions so that the electrolyte is on a dissociated basis.

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[1]281, 296, 297.

[2]O. Weres and L. Tsao, *J. Phys. Chem.*, 1986, 90, 3014.

Well, the main suggestion was to carry over into the mole-fraction equations just the same term that I had devised back in 1973 for the molality equations. In other words, for the binary interaction, it was an ionic strength dependent term. So we just carried over the same exponential ionic strength dependency, but we had to define an ionic strength on a mole fraction basis, which is fairly straightforward.

And with that addition and extending the Margules series one additional term, Clegg thought he had an adequate equation for quite a variety of systems. There are two papers[1] in the *Journal of Physical Chemistry* of that period, it's not many years back, in which he formulated these equations for a mixture of unlimited complexity, in other words, unlimited number of salt components, or for that matter of neutral solvent components. The equations get ridiculously complicated. The first paper was for symmetrical electrolytes, where the positive and negative ions have the same charge, for the most part single charge, but it could be a double charge. The second paper is for unsymmetrical ones; the equations get even more complicated.

Those are the equations that are getting used by Clegg for these Antarctic aerosols. He typically publishes one paper in the chemical literature on a particular system or a certain level of complexity, and then publishes another paper in the atmospheric science literature and journals, in which it's focused more particularly on the atmospheric science application, in which he carries forward the equations from one to the other.

Hughes: How does he define himself?

Pitzer: Well, that's interesting. He's at the University of East Anglia, northeast of London, and there is a department or school of environmental sciences, and one of the subdivisions that they've gone into is this atmospheric science.

Earlier, I referred to the mid-latitude ozone decomposition, and that apparently is a salt solution case, but it's not quite so clear whether it's both nitric and sulfuric acids or maybe just sulfuric acid. The temperature is higher, of course, in mid-latitudes.

To come back to Clegg, he got his Ph.D. at East Anglia in environmental science, then he was with Whitfield at the oceanographic institution at Plymouth, England, for a short-term

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[1]351, 354.

postdoctoral. Then he received a special senior postdoctoral fellowship by arrangement back in East Anglia.

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Pitzer: His senior sponsor is P. Brimblecombe, an Australian who's been in England a long time now, although he's a relatively young man too. Brimblecombe was a co-author on some of this work with Clegg. Clegg seems to be happy in this arrangement, although he hasn't made any great effort to get a regular professorial position. I am sure he would be most happy to have it at East Anglia, but I think he's not terribly enthusiastic about starting from scratch someplace else where there might not be much interest in this rather highly specialized field.

Boris Krumgalz

Pitzer: I've got one more collaboration I might say something about. This is with Boris Krumgalz. He contacted me in late 1988 about a collaboration. He again is a chemical oceanographer, at least in his present position in Israel. He had been in Leningrad, and I'd been familiar with his name. There he was involved in high-temperature aqueous electrolyte type measurements, did some very good work. He's been in Israel now for quite a number of years and had previously had a collaboration with Frank Millero, a chemical oceanographer at the University of Miami, with whom I have been well acquainted. They actually made use of my equations some. Millero does fairly extensively.

The collaboration was to be based on a research grant from what was called an Israel-U.S. or U.S.-Israel Binational Foundation. But you simplify it: it's a way of raising some money for research in Israel if you have a U.S. collaborator. [laughs] We've seen more of that sort of thing very recently with the breakup of the Soviet Union, where there have been situations where you could raise money essentially for expenditure in the former Soviet Union, provided you had a U.S. collaborator. There have been European programs of this sort, too.

But particularly with Millero's recommendation that his experience had been satisfactory, I went along with it with Krumgalz. I thought he was going to do some experiments related to the very interesting question, "Suppose Mediterranean sea water was brought into the Dead Sea," which is below Mediterranean sea level, so there's a gravitational effect. But even though the Mediterranean Sea is fairly concentrated compared to open ocean water over there in the eastern Mediterranean, still it's very dilute compared to the Dead Sea.

But it turned out in fact what he decided to do was just to search the data for density information about aqueous systems, initially of oceanographic or geochemical interest, but eventually anything, fit my equations to them, and as it were, publish tables of parameters, and then make some applications, just with respect to density, including the Dead Sea, but that was only part of it.

Well, that's what he wanted to do; that was all right with me. He's had collaborators there who are efficient at searching the literature. The first paper was focused primarily on room-temperature systems of geochemical interest in a simple way, or oceanographic interest. I was a little concerned about this, because a young Frenchman, Christophe Monnin, had just done such an evaluation of data in this range and had published the paper in the geochemical journal, *Geochimica Cosmochimica Acta*. I thought he'd done quite a good job.

Monnin had come to me for a visit, oh, maybe ten years earlier, so I've followed his career with interest. I was not a collaborator on it; I may have given him a little advice or help. He may have acknowledged it, I don't remember, but everything was very friendly, and I thought that was fine. It wasn't clear to me that Krumgalz was going to do enough better job than Monnin did that it was worthwhile, but on the other hand, if he wanted to do it, why, I couldn't really tell him not to. I did insist that for particular systems where he'd made no improvement over Monnin, he included Monnin's parameters with credit rather than putting in what he thought were better ones. He did make improvements, but they were marginal.

This led to an amusing situation in publication. Krumgalz sent it to the same geochemical journal, and they turned it down, not on the basis there was anything wrong with it, but it was not enough different from the Monnin paper. So he wanted to know where to send it next, and I said, "Well, you collaborated with Frank Millero. He's editor of a journal known as *Marine Chemistry*. Why don't you send it to him?" And I don't know why he didn't publish it. [laughs] But he declined, and so I had him send it to the *Journal of Solution Chemistry*, and they were perfectly willing to publish it. But it is a journal of really very small circulation, and not much circulation in the marine chemistry or geochemical world.

Hughes: Is it rather typical for a review committee of a journal to reject a paper on a topic that had been published in their own journal, but they wouldn't necessarily look to see if a similar paper had been published in another journal?

Pitzer: This depends on the reviewer that happens to be asked to review. I was made aware of what happened on the *Geochimica Cosmochimica Acta* situation. They have at least two and usually three reviewers, and at least one of them, and maybe two of them, rather forcefully pointed out that the improvement was trivial over this other paper. And density of an aqueous solution of this sort is only of rather marginal geochemical interest anyway. If it has to do with the chemical reactions of this fluid, dissolving a mineral or precipitating something or causing some chemical change, that's much more interesting to that community than just the density of the fluid.

Now, just what Millero's feeling was, I don't know. I don't think he even sent it to review. I don't pay much attention to the *Journal of Marine Chemistry*, so I don't really know very well just what its readership is and why they might not have been interested in this. Now, when it came to the *Journal of Solution Chemistry*, they were more or less interested in any solution properties. Their readership by and large doesn't read the *Geochimica*, so that it wasn't a duplication there. So I wasn't too surprised that it was accepted there; in fact, I would have been surprised if it hadn't been.

Hughes: You encouraged Krumgalz to pursue publication because it would reach a different audience?

Pitzer: Yes, it seemed reasonable.

Well, then he went ahead and searched the literature completely for the same sort of information, that is, aqueous electrolyte density data without regard to any geochemical or marine chemical interest, and I think did a good job, on the whole. This was going way beyond Monnin, so this became really a comprehensive coverage. And for that, I suggested he send it to the *Journal of Physical and Chemical Reference Data*, which is run out of NIST, the National Institute of Standards and Technology, the old Bureau of Standards. It is intended just for this: for the most complete and thorough coverage of an area that's of some substantial technical scientific interest.

That paper has now been published, and amusingly enough, there may be a copy in Israel, but Krumgalz hasn't been able to find it. [laughs] So I had to send him a copy, which he has no doubt reproduced in substantial number. I pointed out a few embarrassing little errors, not very serious, but serious enough to raise the question of, should we have a published erratum? I'm sort of neutral about it.

Hughes: Are you a co-author?

Pitzer: Oh, yes, I'm co-author on both of them.[1] I sort of apologized to Monnin for the first one, but he understood. He's sort of a happy-go-lucky young fellow. He's doing very well. He comes by for a visit every once in a while; I take him to lunch. He's usually got some girlfriend I've never seen before. [laughter] So we have a good time.

Krumgalz has also presented a paper on mixed systems, including mixtures of Dead Sea composition and anything intermediate between that and the Mediterranean and so on. I'm a co-author on that one.

Krumgalz had one more highly specialized [paper], and I said, "Oh, you go ahead and publish that." I hadn't had enough to do with that to make it appropriate that my name should be on it. And I'm not sure what else he'll go on and do now. The money from this particular grant has long since run out, so there's no reason for my name to be on it anymore from that point of view.

The Robert Mesmer Group at Oak Ridge, Tennessee

Pitzer: I should probably say a little more about the collaboration with the Mesmer group at Oak Ridge. Mesmer was also a graduate student here, not doing his thesis with me, and I guess it was probably in the early sixties; he might have been here in the late fifties, but I didn't know him as a graduate student here. But we got acquainted very soon in the seventies. Oak Ridge has a very active and substantial program in aqueous electrolyte physical chemistry.

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[1] B. S. Krumgalz, R. Pogorelsky, Y. A. Iosilevskii, A. Weiser, and K. S. Pitzer. Ion interaction approach for volumetric calculations for solutions of single electrolytes at 25 degrees C. *Journal of Solution Chemistry* 1994, 23:849; B. S. Krumgalz, R. Pogorelsky, and K. S. Pitzer. Ion interaction approach to calculations of volumetric properties of aqueous multiple-solute electrolyte solutions. *Journal of Solution Chemistry* 1995, 24:1025. B. S. Krumgalz, R. Pogorelsky, and K. S. Pitzer. Volumetric properties of single aqueous electrolytes from zero to saturation concentration at 25°C represented by Pitzer's ion-interaction equations. *Journal of Physical and Chemical Reference Data*, 1996, 25:663.

They were interested right from the beginning in using my equations not only for the properties that I immediately considered, namely the chemical reaction properties of activity coefficients of the salt and the osmotic coefficient, in other words, the reduction in the vapor pressure of water because of the salt. But also for other properties like changes in thermal properties, heats of mixing, heats of dilution, and densities and so on, and going on up in temperature.

I suppose the program in a sense came from the old Oak Ridge program to develop an aqueous homogeneous nuclear reactor way back in the late 1940s and into the 1950s. This proved to be an impractical thing to do, but in the course of gaining all the information related to it, they had become essentially one of the major laboratories for work in these high-temperature aqueous electrolyte systems for any practical use. Well, one of the practical uses is geothermal energy. The hot water or steam that comes out of the earth isn't pure. There are a whole bunch of things like that that are of practical interest. Even the steam that goes into an ordinary electricity-generating turbine system that is nominally pure isn't completely pure, and it causes problems. The salts come out on the turbine blades, and they crack and break off, and the turbine has to stop.

There's a research group near Stanford funded by joint contributions of various power companies around the country--EPRI, Electric Power Research Institute. They give some support to Oak Ridge for their program and sponsor some work elsewhere. I haven't had any funds from them, but I've followed their work reasonably well.

We've actually had some direct collaboration with Oak Ridge, in particular the very general paper[1] with R. H. Busey, one of the Oak Ridge people. He is a co-author with myself and with one of my associates here. But we've had a very close relationship through the years, and of course, now J. M. Simonson is there too. He was a student with me.

[1]K. S. Pitzer, J. C. Peiper, and R. H. Busey, Thermodynamic properties of aqueous sodium chloride solutions. *Journal of Physical and Chemical Reference Data*, 1984, 13:1.

Frederick B. Rossini ##

Pitzer: I'd like to say just a little about an extended collaboration with Frederick B. Rossini on hydrocarbon properties, research beginning in 1940. Rossini was at the National Bureau of Standards [NBS] at a very senior level, headed a substantial crew. That's now the NIST, the National Institute of Science and technology.

There are in all twelve papers involving coauthorship, mostly published in the Journal of Research of the NBS, as well as two books to which I'll refer a little further in a moment. These essentially just put together my results calculating the entropy and heat capacity and those related properties for various hydrocarbon molecules, together with the heat of formation, which was measured experimentally by Rossini and his immediate associates, or that he collected from measurements of that type from elsewhere in the world. This information was very important for the petroleum industry, and part of the work both here and at NBS was supported by the American Petroleum Institute, which in turn, of course, was supported by the various major petroleum companies.

The books were both entitled Selected Values of Properties of Hydrocarbons. The first edition was in 1947 and was published by the National Bureau of Standards as one of their publication series. A few years later, Rossini had taken an early retirement from the National Bureau of Standards, or possibly had resigned, and was then professor at Carnegie Institute of Technology in Pittsburgh.

The second edition was published for the American Petroleum Institute [API] by the Carnegie Press, under the same title but with an augmented series of coauthors, including George Pimentel, who was by then assistant professor here at Berkeley, who had been my own research student and had participated in the API-supported work. It's a considerably larger volume, but under the same title. The second one was published in 1953.

Subsequent to that, the project financed by the API and at Carnegie Tech continued, but it became a multivolume, looseleaf operation which is now about ten volumes, and subsequently was transferred to other hands and moved to Texas A&M University, where it still continues. Rossini himself moved on to Notre Dame University in an academic-administrative position, and then retired and is no longer living.

The Meaning of "Semiempirical" Equations

Hughes: Well, I have a very basic question. Why are your equations described as semi-empirical?

Pitzer: Because they're semi-theoretical.

Hughes: [laughs] Isn't that a tautology?

Pitzer: Well, no. There are purely empirical equations. In other words, where there's no theoretical reason for choosing those particular terms, or at least where any theoretical reason is so trivial that what the individual did was to just try all sorts of combinations of terms and pick out ones that were the best. And I would put the Margules equations for those very concentrated solutions essentially in that category. The theory there is so trivial that they're essentially a purely empirical equation.

Prior to my equations, the electrolyte equations past the Debye-Hückel term were purely empirical, as I said. Some people said they needed to have molality to the three-halves power, second power, five-halves power, third power, fourth power, etc. Other people said, "Well, you don't need those half powers in there. Just use the unit powers." There was no theoretical justification to amount to anything for a half power; on the other hand, there was no reason against it.

I had a clear theoretical basis for this ionic strength dependency for this exponential term that I put in the so-called "second virial coefficient," i.e., first-power molality term, modifying it. What I mean by semi-empirical is that there's a theoretical basis for it, but the theory hasn't been carried through in complete detail. It just suggested a curve of about this shape, and that looks like an exponential, so you pick out an exponential. But there was theory for that shape.

Hughes: Could you have called them semi-theoretical equations?

Pitzer: Yes, sure.

Hughes: Which would have meant the same thing.

Pitzer: Yes.

Citations of the Pitzer Equations

Hughes: You commented off tape a few sessions ago that you came up with sixty-one entries when you made a computer search for "Pitzer," and you found that sixty of them referred to the Pitzer equations.

Pitzer: Yes, that's right.

Hughes: Which I guess is a pretty good indication of how widely they're used.

Pitzer: Yes, I would suspect there would be about an equal number of papers in which they were used in a substantial way, but it didn't get into the title.

Pitzer Equations for Concentrated Solutions

[Interview 7: July 10, 1996] ##

Extending the Equations

Hughes: Dr. Pitzer, I understand that you wish to talk about concentrated solutions.

Pitzer: Yes. The success of the equations that are now called Pitzer equations arose to a considerable degree because they were conveniently and accurately valid to a higher concentration with fewer terms or complications than any previous equations. Even so, they are in general valid only to a concentration typically about five or ten moles per liter or kilogram of water, which, however, covers the full range of solubility for many electrolytes up to their equilibrium with solids.

The equations have been extended by additional terms by various individuals, including to some extent myself, but more so by the Russian V. K. Filippov. But that is still a limited range. Indeed, the very measure of composition molality cannot be used all the way to the pure liquid salt, because the molality becomes infinite. It's a ratio of electrolyte to water, and if there's no water, the ratio is infinite.

Charles Kraus

Pitzer: Thus, I had in the back of my mind the problem, an interesting opportunity to improve the situation for equations that would be valid all the way to pure fused salts, which would have to be based on the measure known as a mole fraction. This had been a topic of interest and some appreciable amount of publication by a Charles Kraus, whose major career had been at Brown University. I knew Kraus personally from various national meetings, the National Academy of Sciences, and some interactions during World War II with scientific work. I chose "Electrolytes. From dilute solutions to fused salts"[1] as the exact same title as the 1954 paper of Charles Kraus, which was a very good summary of the situation at that time.

Before I go on with the science, however, I think a few comments about Kraus as a person might be of interest. He was a very colorful and sometimes controversial figure. Gilbert Lewis thought very poorly of him, [laughs] but I found that in some respects, there was really much to be said for him.

Hughes: What did Lewis criticize?

Pitzer: Well, I think they just rubbed one another the wrong way. [laughter] Among other things, Charles Kraus was very much inclined to drink alcoholic beverages, always under control, and for a relatively small man, seemed to have a remarkable capacity. During World War II times, there were stories that there were tense relations between scientists and the army officers of the Manhattan [Engineering] District sent in to govern the particular project administratively, and that Kraus as a consultant served a useful purpose to take the army officer out [laughs] and outdrink him! Whereupon he'd be more tractable the next morning.

This was believable to me. I had no firsthand experience with it, but at American Chemical Society meetings, if you wanted to find him, the nearest bar was a good place to look. [laughter]

Well, I tell this in part because of an episode in 1961 when I was on the way to Rice University as president. I went to MIT for a previously invited and accepted lectureship, and was asked to come over to Brown University, which is nearby, for a single afternoon and lecture. When I looked up in the lecture hall, there was a picture of Charles Kraus. He was no longer living. A new generation of people had largely taken over the department.

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[1] K.S. Pitzer. *Selected Papers*, pp. 512-517.

In opening my lecture, I made remarks in a positive sense about the figure in this picture at the head of the lecture room, and went on with my lecture. Afterwards, a number of the students greeted me and said, "Well, that was an interesting lecture." But what interested them most was that I knew the figure that they'd been looking at and didn't know anything about. [laughs] Apparently, the younger faculty had so little knowledge of him that they avoided talking about him, but they left his picture there.

Well, anyway, most of Kraus's work had been done earlier. He had made a very significant positive contribution, not a major one, probably.

Extending the Equations (continued)

Pitzer: My equation for electrolytes over this full range was based on the assumption that they were dissociated into ions, which would not be correct for many examples but would be a good approximation for others, and I focused just on those. I simply combined the equation for the Debye-Hückel effect, which is well established theoretically for the dilute range, with the so-called Margules expansion, Margules parameters, which are commonly used for nonelectrolytes in equations of state. There's no great originality about this, but as far as I know, no one had done it before.

This didn't occupy a major part of my time any one year, but as years went by, we made additional use of these methods from time to time. With one student, John M. Simonson, we actually made some additional measurements in the high concentration range and tested the equations somewhat more precisely than I had done before, with one additional term.

This equation generated quite a little interest, and I was invited to a symposium in Stockholm mainly on that basis a few years later, and on another occasion to a meeting in Germany. I gave talks based on essentially the same work as the 1980 paper.

Then Simon Clegg was interested in this, and I talked a little last time about collaboration with Simon Clegg, the young Englishman. And actually, the extension of this mole fraction-based equation with Clegg is the most complete and most complex extension so far. The main contribution of the Clegg work was to go from a maximum of two electrolytes plus the solvent, presumably water, in the earlier work, to an unlimited number of electrolytes and an unlimited number of neutral molecule solvents. The equations become ridiculously complicated, but of course, one can simplify them for any particular example.

This work continues, and one of my current postdocs, a young man from Russia, Sergy Petrenko, is using this in connection with sodium hydroxide, which at not terribly high temperatures is soluble all the way to liquid sodium hydroxide and fits the model in the aqueous range. As nearly as we can tell, it's dissociated into separate ions as long as there's water there to separate them. These mole fraction-based equations are going to be less widely used, because they are not required and are less convenient. Nonetheless, they are important when they're necessary. I expect in the next few years to put together an array of parameters for use of these mole fraction-based equations in mixed electrolytes of increasing complexity in somewhat the same sense as the very successful equations based on the molality composition. I guess that's the story on that. [tape interruption]

Fused Salts; Ionic Fluids

Pitzer: Another topic that we might discuss, relatively recent research, is at the other end of the range from dilute solutions to fused salts; this is just the fused salts, just liquid ionic fluids. Pure ionic fluids for the most part exist only at high temperatures and therefore are inconvenient to investigate, but they have been studied through the years to some extent. And they show characteristic differences from neutral molecule liquids. This isn't a qualitative difference; they have liquids and vapors, and at high enough temperatures, you get to a critical point where the vapor has gotten the same density as the liquid, and above that, there's only a single phase. But for a typical thing such as sodium chloride, this occurs at such a high temperature that it's hardly accessible.

Ammonium chloride has a critical temperature a little above 1,000 Kelvin, which makes it much more readily accessible, and it was investigated at Karlsrühe in Germany by Professor Ulrich Franck. He was one who called attention to the rather distinctly different pattern quantitatively for an ionic fluid from an ordinary fluid of neutral molecules. It's the liquid that's primarily different. The rate of expansion as you raise the temperature on the liquid is much greater. In other words, the density decreases and the volume increases more rapidly than for a neutral molecule fluid.

I made some calculations related to this, and in 1984 published one more detailed paper [#283] and then a review, which is under the classification as a feature article in the *Journal of Physical Chemistry*.[1] There, I give the underlying explanation for this difference in behavior for the liquid, in that in a dense liquid or solid, positive ions are surrounded by negative ions and negative ions by positive ions, but one positive ion isn't very far away from another positive ion, and likewise, negatives are close to one another, so that although the totality is rather strongly held together, there is only a relatively moderate fraction of difference by which these unlike-charge interactions exceed the like-charge repulsions.

As you expand the pattern, you can maintain almost the same net attraction by getting the like-charged ions further away from one another while still keeping unlike-charged ions close to one another, and eventually you end up with a nominally linear chain in which each ion has two oppositely charged ions next to it, and the like charges are twice as far apart as the unlike charges, so that there's still quite a lot of net attraction.

So I discussed that, and surveyed the situation. My quantitative estimate for the critical temperature of sodium chloride turned out to be too high, however. I had all the qualitative ideas present, but the quantitative estimates weren't too good.

For the lower temperature range, sodium chloride and other alkali halides are reasonably well known, for the vapor as well as for the liquid. And in addition to ion pair molecules in the vapor, there is a dimer which is essentially a square or a diamond structure with the like ions in the alternate locations, so that you get four plus-minus attractive interactions and just two like-charge interactions at the diagonal distance, so that it has relatively low energy.

In 1984, I was aware of and recognized that as the temperature increased, a linear pattern of either dimer or higher polymer, which would be nominally linear but actually very floppy and flexible, would begin to be more important, but it was hard to make an adequate quantitative correction for that.

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[1] K.S. Pitzer. Ionic fluids. *Journal of Physical Chemistry* 1984, 88: 2689.

I returned to that subject just this year, and there's a paper in the *Journal of Chemical Physics* early 1996[1] in which, with improved accuracy, I calculated the properties of the nominally linear but very flexible dimer, two sodiums and two chlorides, and showed that while it was unimportant that near the melting point of sodium chloride where it had appreciable vapor pressure, there were data at high enough temperature, well above the melting point, near the one-atmosphere boiling point of sodium chloride, that showed the very substantial presence of this linear dimer. By converting this into a more detailed picture, I could predict the properties at still higher temperatures. If one approached the 3,000 Kelvin temperature, it would be almost purely the properties of the linear species that would be dominant for the vapor. And thus, the critical temperature could be just a little above 3,000 instead of up near 4,000, as I'd suggested earlier.

This is a fairly local but quite an interesting topic in the sense that this is a prototype case, and if and when one is interested in other similar substances like sodium chloride, why, the pattern is established and one could treat other cases, by doing some further experiments and calculations.

Near-Critical Properties of Some Fluids

Pitzer: A related but really separate chapter on ionic fluids concerns properties very close to the critical point, within a few degrees of the critical temperature. I'm going to ramble a little. [laughs] Suppose you have an equation of state that shows both a liquid and a vapor and a critical point. For example, the one that Van der Waals proposed about 115 years ago is the prototype, although it doesn't fit real fluids terribly accurately. But if you take any equation like that and expand it mathematically in the vicinity of the critical point, you can define and evaluate what are known as critical exponents that have to do with the quality, the shape, of the curves for various properties without regard to whether they're at this temperature or that temperature, or this density or what, in the vicinity of the critical point.

Well, the one that I find of greatest interest has to do with the vapor-liquid variation with temperature. As you come down from the critical point, the two phases appear, and the vapor

[1]K. S. Pitzer, Sodium chloride vapor at very high temperatures: linear polymers are important. *Journal of Chemical Physica*, 1996:104, 6724.

phase decreases in density, and the liquid phase increases in density from the critical density. If you express that in an appropriate way, the Van der Waals equation will give the parabolic shape of the curve, which involves a square, the second power, of the temperature difference with respect to the critical point. In terms of exponents, the custom has been to take the reciprocal of that exponent, i.e. one half. It is given the symbol beta.

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Pitzer: But for most real fluids, the shape is a lot closer to cubic than to quadratic; beta is about one-third. That means that it's flatter on top. This was essentially ignored for years.

Contributions by Others

Pitzer: There's an interesting historical paper about this by Dr. [J. M. H.] Levelt Sengers, who just retired but is still professionally active at the National Institute of Science and Technology. It used to be the old Bureau of Standards. I've gotten to know Dr. Levelt Sengers, whose nickname is Annika, and hold her in very high regard. Indeed, I helped get her elected to the National Academy of Sciences this past year.

She was born in Holland, which of course is Van der Waals' country. Her maiden name is Levelt; she married Sengers, also Dutch, I think shortly after they both came to this country, but they might have married in Holland. He's interested in the same field but with somewhat different emphases, and although I know him, he isn't particularly pertinent to what I'm talking about here.

Annika wrote this historical paper which indicates how Van der Waals and his senior colleagues overlooked this contradiction even though a young member noticed it, published a paper in the *Proceedings of the Royal Dutch Academy of Science* or whatever the proper title is.

As the years went on, this discrepancy between mathematically convenient equations of state and the cubic shape became even more established. But it was only after World War II, and really, I guess, into the early 1960s, that the new fundamental theory was developed, for which Kenneth Wilson got the Nobel Prize, although several other people, in my opinion, contributed about equally. No criticism of Kenneth Wilson [laughs], but among others, Michael Fisher, who will appear in this little episode later, seems to me to have contributed about as much.

They showed that as you got close to the critical point, there are very wide fluctuations of density that, if these were taken into account properly, gave this locally cubic shape which could then be a part of the more general cubic shape very comfortably. Well, so much for the background on neutral molecule fluids.

Ionic fluids appeared to be more nearly quadratic. Professor Franck and his student in the work on ammonium chloride pointed out that their data fitted a quadratic description for a beta of one-half over the full range, up to somewhat over 1,000 Kelvin, as I recall, for the critical temperature, although the precision wasn't terribly high. Franck was a wonderful high temperature investigator, but those experiments are difficult.

Contributions by Pitzer et al.

Pitzer: Well, I decided to see if I couldn't come up with a two-component ionic fluid which could be investigated near room temperature. The critical argument is a corresponding states-type of argument with respect to the critical temperature of an ionic fluid, which makes it proportional to the square of the electrical charge divided by the distance of closest approach of the ions, and the dielectric constant, if it's other than unity.

Well, if for something like sodium chloride, this is 3,000, and you'd like to get it down to 300, you've got to get a factor of ten. You can't change the charge, so you have to get the factor of ten in the denominator. It seemed to me feasible to get about a three-fold increase in diameter for the particles, and then if they could be dissolved in the solvent with dielectric constant about three or four, one could get the temperature down by a factor of about ten.

Our first attempt was published in 1987;[1] it involved a postdoc, Dr. [Donald R.] Schreiber, and a visitor, a young woman visitor from Portugal, Dr. [Conceicao P.] de Lima. We came up

[1] D.R. Schreiber, M.C.P. de Lima, and K.S. Pitzer. Electrical conductivity, viscosity, and density of a two-component ionic system at its critical point. *Journal of Physical Chemistry* 1987, 91: 4087.

with an ionic substance in the solvent with a critical temperature of about 140 degrees Celsius, still a little high for convenient investigation. It clearly showed the quadratic shape, and this was published. But I thought it was worth looking for a better example rather than to expend too much effort on this one system, which seemed possibly to be not quite chemically stable, although we didn't pursue that aspect further.

With another postdoctoral, Rajiv Singh, who was originally from India but got his Ph.D. elsewhere in the United States, in particular at the University of Tennessee, we searched through the literature for other examples that we might test and came across one where the positive ion is an ammonium ion with four alkyl groups but not symmetrical completely. In other words, three of the alkyl groups are the same but one is different. But the negative ion, except for the center, has exactly the same alkyl groups, but it has a boron atom at the center, which makes it a negative ion. So there was a salt with positive and negative ions, almost although not exactly spherical, but with only alkyl exterior, so that there was no detailed local attraction. It's just the overall electrical attraction of the positive ion and the negative ion.

It turned out that phenyl ether as solvent gave a convenient critical temperature, just above room temperature. For the investigation of the critical properties, I suggested that we try measuring refractive index with a conventional prism, except that we'd make it a hollow prism and put the test solution inside the prism.

This was, I would say, highly successful. We were able to thermostat it to a thousandth of a degree temperature. Rajiv Singh was a very skillful inorganic chemist in synthesizing this compound, which had to be done in the absence of water and air. But as long as water and air were kept away, the material seemed to be completely stable. We found that this critical exponent, beta, was a half, within relatively narrow experimental uncertainties, down to a deviation in temperature from the reduced temperature in the critical point of one part in ten thousand, which is very close indeed.[1]

Well, this result caused quite a stir in the community of theorists on critical exponents. A number of other people investigated similar systems and got similar results, except that for the most part, they were less accurate. The systems were less

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[1]R. R. Singh and K. S. Pitzer, *Journal of Chemical Physics*, 1990, 92, 6775.

ideally coulombic or ionic, and there was some indication that there might be a so-called crossover in the critical exponent.

Hughes: Why were you able to obtain better conditions than the other workers?

Pitzer: The other systems were much easier to prepare. The one that Schreiber and de Lima and I used and published involved tetrabutyl ammonium picrate. Now, tetrabutyl ammonium salts or hydroxide are available on the storeroom shelf, and likewise picric acid is a common chemical. So that this was easy to come by, and you can use it in different solvents. Picrate has three nitro groups on it and one oxide, and the charge is initially distributed around all these. But as a picrate ion gets close to a positive ion, that charge can localize on the nearest nitro or oxide group, much more so than the boride ion that we had in our second salt.

In part also, these other workers just used different solvents. As long as the dielectric constant is in the general vicinity of three or four, different solvents can be used that may be more or less inert themselves. Our solvent was 1-chloroheptane. It had one chlorine substitution on a hydrocarbon. Some of the other systems that were studied used a long-chain organic alcohol, but its OH group is much more electrically active locally, shall we say, than even the chlorine substitution or the ether in the case of the other system.

Well, as I say, the theorists, and in particular Michael Fisher, who had been involved with critical exponent work and is a very top-flight theorist, and George Stell, similarly first-rank, who had been interested more in the ionic systems in the past, took up the question: "Is there a theoretical basis for this critical exponent of a half for an ionic fluid?"

We did one further piece of work here. A young physicist from Bangalore in India, where they were doing some first-class work in critical systems using light scattering as a measuring technique, applied for a postdoctoral. I was aware of Bangalore as a high quality institution scientifically. There is a personal connection in that at that time, the director of Bangalore had been a former postdoc of mine in the late 1950s, C. N. R. Rao. He had not only developed a remarkably good scientific institution there but is a very prominent scientist both in India and internationally. So that I was interested to have this young man come, Dr. [T.] Narayanan by name.

I suggested that we choose some less perfectly ionic systems and see whether we would find a crossover in this exponent beta, possibly from a half near the critical point to a third further away. There was an example that appeared to have shown that. It had been in the literature since 1970. It came from Cornell, Professor [Michael J.] Sienko there, who had been a student here in Berkeley and I knew, favorably. And a French visitor, [P.] Chieux. He has since done significant work in this general area. Sienko was a very good scientist, who has since died. His was one of these unfortunate premature deaths.

They measured sodium and liquid ammonia, and reported results that seemed to show a clear crossover with a beta of a half out to about a reduced temperature 0.01 away from the critical point, and then a value of a third further away.

Now, sodium and ammonia is sort of an ionic system. The sodium is essentially dissociated into positive ions surrounded by ammonia, and the electron in a cavity surrounded by ammonia may be two electrons in a specially shaped cavity. There is some debate about the details, but it clearly is to a considerable degree an ionic system, so that this example was pertinent to our thinking.

Well, with Narayanan, we went back to the tetrabutyl ammonium picrate salt, which is easy to prepare and handle, and chose a series of solvents, with different dielectric constants, and indeed found this crossover in beta essentially like the sodium ammonia. In the best case, we were able to quite clearly identify a region in which there was a beta of a half, and another region further away from critical point with a beta of a third. In other cases, it was less clear, but still suggestive of the crossover.

This near-critical work was summarized in a feature article in the *Journal of Physical Chemistry*[1] about a year ago, but it has been well summarized in papers of others too. Both Fisher and Stell wrote very general papers from the theoretical side. Fisher still can't decide what the theory really is. [laughs]

Fisher was here as the special G. N. Lewis lecturer less than a year ago, and he focused on this topic very much in his lectures, to my pleasure, obviously. He emphasized our experiments as really the key, although others contributed. And within the past week, I got a preprint from Fisher and essentially another review in connection with a scientific meeting in which he still can't decide what the theoretical property for an ionic fluid ought to be, although he has various things which are suggestive or indicative of the behavior that we in fact found.

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[1]K. S. Pitzer, *Journal of Physical Chemistry*, 1995, 99, 13070.

Hughes: Are you able to help him out?

Pitzer: Well, we have constructive and interesting discussions. In fact, when he was getting into this before we did the last round of work, he was here for some meeting, I think probably up at LBL, and he called me and wanted to come down. We spent most of the afternoon talking about that sodium-ammonia work, and our own experiments.

An interesting aspect of this near-critical work concerned a comment that almost immediately followed the paper of Singh and myself from [A. L.] Kholodenko and [A.] Beyerlein at Clemson University. Kholodenko was the professor there. He claimed that his already-published theory, of which we had been unaware, predicted this beta of a half, but the arguments didn't seem terribly convincing to me.

The more interesting aspect was that Kholodenko's theory predicted an exponent of two for the exponent gamma, which has to do with the behavior just above the critical point in the single phase, with respect to compressibility, but in particular, it is measurable in terms of light scattering. The classical value is one, and the accepted neutral fluid value with the modern theory is 1.24 exactly.

Well, this motivated some experiments that were done first in Germany.[1] While the difference in exponent between a third and a half might be difficult to determine, the difference between one and two or even between one and a quarter and two is a much more drastic difference.

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Pitzer: Their results were unambiguous in the sense that this exponent gamma was not two. They couldn't decide for sure whether it was one, or maybe crossed over between one and one and a quarter, but it was clearly not two.

Also, the Kholodenko theory was attacked very thoroughly by Fisher theoretically. Thus, when Narayanan came with me, we used that light scattering technique, but by that time, we were not disproving Kholodenko. That had already been done.

I should mention the names of those in Germany. Hermann Weingartner was a young man at the postdoctoral level who was very

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[1]H. Weingartner, S. Wiegand, and W. Schroer, *Journal of Chemical Physics*, 1992, 96, 848.

much interested and effective. He came originally from Karlsrühe but then was in Bremen, with Professor Schröer there. He had a postdoc associate in his laboratory, a young woman named Wiegand. They did very good work.

Then Dr. Levelt Sengers collaborated with an optical experimenter at the University of Maryland. They used the same system that Singh and I had used, the boride system, with light scattering, and they found a gamma of 1, that is, the classical value, within their experimental error. Their experimental cells broke before they got as close to the critical point as they would like to have gotten. [laughs] And as I mentioned before, once exposed to the air, the sample was spoiled, and they were not themselves in the business of making the sample. They had hired somebody, some commercial firm, to make it for them. So that experiment was somewhat abruptly terminated, but nonetheless, it did support our general position, that the classical exponents were correct.

Well, just to return to my summary before, as of today, including this unpublished paper of Fisher's, he can't decide on a really rigorous theory. He has all manner of theories that suggest that ionic systems are different and that the classical exponents may well be correct, but it's still up in the air. I won't try to summarize the Stell situation, but it is somewhat similar to that.

Theoretical vs. Experimental Approaches in Chemistry

Hughes: I notice throughout your discussions of research that you seem to make a distinction between the theory and the experimental evidence. Are most people trying to do both?

Pitzer: No. [laughs] In my younger days, there was no separate community of theorists in chemistry as there was even by then in physics. Certainly from the turn of the century or earlier, there was essentially a separate theoretical physics community with Boltzmann and Einstein and Max Born and Schrödinger and so on. In chemistry, there were people that did more theory than others, but most everyone was primarily an experimentalist who did enough theory to interpret his experiments. Linus Pauling did or sponsored experiments almost to his final day, and he certainly was a great theorist.

But we physical chemists or theorists of those days used fairly simple theories. These became much more complex numerically once we had electronic computers to help us, but in terms of the abstract mathematics, they weren't really at the frontier of mathematics, let's say. Whereas some of the physicists were more or less pushing the frontier of mathematics.

More recently, there has developed a separate group of theorists. There are several in the department here. Bill Miller is strictly a theorist. He's got his feet on the ground in terms of contact with experimentalists; he's been department chairman, well recognized and so on, but he doesn't pretend to do any experiments. The theoretical chemists tend to get more into the complexities of theory, of the mathematics side of it, than experimentalists would.

There are a good many experimentalists who still go ahead and do theoretical interpretations of their experiments up to a certain level. Certainly in my own career, there are some chapters that are primarily theoretical. Some of the chapters, such as the very last one I was talking about, are essentially purely experimental in terms of my contributions. Although my theoretical background left me in a better position to understand the theory and to possibly have judgment about the theory than some other experimentalists would have.

On the other hand, say in our electrolyte solution research, while we've done experiments there, our contribution to that field was almost purely in better theory and efficient use of that theory in the sense of treating data rather than our actually contributing very many additional data. In fact, our contributions at room temperature are practically trivial there; the literature was full already. At high temperatures, we have contributed. We've contributed experimentally not only in terms of measurements but in terms of the experimental techniques.

We designed and built a new and different type of high-temperature heat-capacity calorimeter, for example. The same essential elements were embodied in the one done at the University of Delaware simultaneously, and the two of us were in communication about it. We along with [Robert] Wood at Delaware devised an experimental calorimeter that was widely used elsewhere in subsequent years.

Hughes: Does the state of the field largely determine the degree to which you emphasize the theory versus the experimental component?

Pitzer: Yes. I'm an opportunist in the sense that I look at the field and say, "Well, now, what does this field need, and is it the sort of thing that I might be able to do?" In the room-temperature, aqueous-electrolyte field, there was an enormous body of experimental measurements already. What it needed was a better theory; not a deeply abstract complex theory, but a better, what I call a semi-empirical, semi-theoretical theory in which you use as much theory as is efficient. But then you do some empirical evaluation of parameters where the system is just too complex to calculate from first principles.

That was one situation, and this last one that I was talking about today, near-critical properties of ionic fluids, there was zero experimental literature. There were a couple of suggestive things--the sodium ammonia system and the ammonium chloride from Franck's laboratory at fairly high temperatures. But beyond that, there was a complete blank in terms of experiments. So the first thing was to invent the model ionic fluid that you could experiment with at room temperature with high precision, and then to design, for example, the hollow prism and put the sample inside it, so you controlled the temperature accurately. So our contributions there were on the experimental side.

And I could think of some other examples where it was one or the other. Go back to the acentric factor area; there again it was like the aqueous solution area, an enormous body of data already measured, and it was a matter of systematizing it for convenient use.

Hughes: From your remarks, do I understand that you and I assume also your colleagues do not prize theory over experiment? It's much more a question of which the problem needs.

Pitzer: That's right.

Hughes: An older notion, especially in physics, is that a theoretical scientist was somehow of higher value [laughter] than one who dirtied his hands with experiment.

Pitzer: Well, I'm sure you're speaking with some reality with respect to the real world of physics, yes. Certainly in any area, there will be numerous people who will do relatively routine experiments, where the intellectual quality may be high in terms of selection or complexity of synthesis or preparation or something like that, but in terms of the scientific essence of it, it's relatively routine. It adds some data in a table or something like that.

But experimentalists can be much more intellectually advanced in the sense of inventing. A major invention is a new type of measurement, a new instrument that measures something that wasn't measurable before. That's one of the major advances in science, and it's essentially an experimental one, although somebody's theory may have made this device possible, but the device itself is an experimental thing.

While the examples that I've cited in terms of my own work are relatively low-scale in terms of novelty, the prism for measurements of a near-critical phenomenon as far as I know is novel. I don't think anybody did it before; maybe they did. And our new calorimeter for high-temperature heat capacities of aqueous solutions was new. I don't claim anything major for it. But as you get into experimental fields that are more completely new in a qualitative sense, then it can be a major advance.

Pitzer's Books

Quantum Chemistry

Hughes: In 1953, as you're well aware, you published a book called *Quantum Chemistry*. I wondered what prompted you to write it.

Pitzer: Well, it came out of my feeling that physical chemists, chemists in general, needed to learn some quantum theory. Although quantum theory is essentially physics, in the same sense, all of physical chemistry is essentially physics. It's just selected to be chemically applicable and valuable to chemists. I thought that general point of view ought to be carried over into newly developed quantum mechanics and to make it convenient and available and accessible to chemists.

There were two books that were somewhat in that direction. The Pauling and Wilson quantum mechanics book was one that I had had. And, there's an Eyring book too, but a little later.[1]

I'd introduced a course primarily for graduate students at Berkeley. In accordance with Lewis' desires, it was given an honors undergraduate number initially, but it was later converted to a graduate course. Willard Libby and I taught it jointly once, and then the war intervened, and one thing and another. After World War II, I was going to be teaching this modern chemistry course, and while the existing books could be used, I thought a

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[1] Linus Pauling and E. Bright Wilson. *Introduction to Quantum Mechanics with Applications to Chemistry*. New York: McGraw-Hill, 1935; Henry Eyring, John Walter, and George E. Kimball. *Quantum Chemistry*. New York: John Wiley & Sons, 1944.

different mix which brought in a bit more statistical mechanics along with the quantum mechanics would be valuable and convenient for the sort of course that I was teaching at the time. So this was essentially written as a book for the first-year graduate level quantum chemistry course.

Hughes: And Pauling and Wilson's book hadn't brought in much statistical mechanics?

Pitzer: Oh, essentially it didn't bring any statistical mechanics in. But it's a great book. It's lasted far better than mine through the years, as a matter of fact.

Hughes: Why is that?

Pitzer: Well, I can't help but think that Pauling's name might have helped some, but it's a purer introduction to the Schrödinger quantum mechanics of molecules. Mine, as I say, is somewhat more of a mix and is in a sense maybe scaled down a little bit for the students.

Hughes: What audience were you hoping to attract?

Pitzer: Well, beginning graduate students. Well, it was to be useful later, but that's where it would be used and was used fairly extensively.

Hughes: Did it change curricula in chemistry?

Pitzer: Courses were introduced very widely at that time, not all using my book. Until after World War II, the few chemists that learned much quantum mechanics seriously did it by taking physics courses or reading the literature and studying it on their own, as I did.

Hughes: And did that method serve just as well?

Pitzer: Well, it will get you there, but you can do a more efficient job and make it more attractive to the chemists, and therefore include an introduction to quantum mechanics to a wider number of chemists, if you design a course more specifically for them. And that's the essence of physical chemistry generally. That is, you can learn thermodynamics in mechanical engineering if you want to. It's a piece of physics. But the physicists are hardly even interested in it any more. [laughs] And insofar as it's important to chemistry, the theory isn't all that complicated; you might as well teach the necessary theory along with some applications that are of chemical interest. Well, what we were doing in the *Quantum Chemistry* book was to teach quantum mechanics at a minimum but adequate level, with applications built right in there that looked like of chemical interest, rather than otherwise.

With respect to that book, I seriously considered a revision in the mid-sixties, but it just fell apart. I was busy as president of Rice, and I thought of involving my son, who was getting into quantum theory. He's more of a quantum theorist than I am. But somehow, that didn't fly, and I thought of a co-authorship with somebody at Rice, and that somehow didn't work out. In other words, everybody had other things that they were more interested in doing, so we never got around to doing it.

Hughes: Had the publisher approached you?

Pitzer: Oh, yes, the publisher was quite happy to have it done. And in subsequent years, there are six or eight or ten books more or less designed for the same purpose, some of which have come and gone too [laughs].

Second Edition of *Thermodynamics*

Hughes: In 1961, you and Professor Brewer published a revision of Lewis and Randall's 1923 book on thermodynamics.[1]

Pitzer: Yes. The Lewis and Randall book had enormous impact in the U.S.

Hughes: Why do you emphasize the U.S.?

Pitzer: Well, because there were differences in terms and definitions, not of any real science, but terminology, symbolism, things like that, that were never accepted really in Europe. So the book was on the library shelf in Europe, but was virtually never used as a textbook. But it was very widely used in this country.

Hughes: Was that a matter of conservatism or chauvinism, that the American terms weren't used?

Pitzer: Well, this is an interesting subject.

Lewis chose to depart from what was on the way toward being recognized as the standard terminology and symbols. To him, the most important function was what is now called the Gibbs function,

[1] Kenneth S. Pitzer and Leo Brewer. *Thermodynamics*. New York: McGraw-Hill Book Co., Inc., 1953.

Gibbs energy, or the Gibbs free energy, and he wanted to just call it free energy. But the term free energy, or at least the German equivalent, had first been proposed by Helmholtz for what is now often called the Helmholtz energy or the Helmholtz free energy, also devised by Gibbs but unnamed. Gibbs used Greek symbols that nobody really liked.

The European community preferred to keep the word "free energy" for what we'll call the Helmholtz function, with the symbol "F." Lewis chose the symbol "A" for the Helmholtz function, "A" being for the German word "Arbeit," or "work," as a work content function, with a certain rationale. The Germans used "G" for the Gibbs function.

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Pitzer: That use of "G" wouldn't have been really much of a controversy. But using "F" differently was a real controversy.

Now, a great many Americans had contributed substantially to chemical thermodynamics in the period of the later twenties and thirties, using Lewis and Randall as their guide. And this had continued on through the forties. McGraw-Hill, the publisher, approached me to revise the book. I was by that time involved as dean here and with a good many international committee appointments and things like that.

Hughes: This was right after the war?

Pitzer: No, this was in the mid- to late fifties. Leo Brewer was here, was well established as a major figure in chemical thermodynamics too, so I approached him to be a co-author on the project, and we agreed to do it and did it.

We in effect ducked this question of terminology by saying we were revising the Lewis and Randall book. We realized there was controversy about it, but we thought it was appropriate to stay with Lewis and Randall symbols and terminology. And that carried the book forward for another period of time. I contributed very substantially. But after I had gone as president to Rice, the later stages of proofreading and all that sort of thing, Brewer handled very efficiently. The book was quite widely used in this country.

Hughes: How much did you change the original?

Pitzer: Well, let me continue.

The symbol for the Helmholtz energy, "A," was not conflicting, it was just different from "F" being used in Europe, and to a considerable extent, the European terminology was used by physicists in this country too. But "A" didn't conflict with anything, and the chemical engineering community and even a number of people in Europe took up the symbol "A" for the Helmholtz energy, so that that was part of the situation.

Third Edition of *Thermodynamics*

Pitzer: We might as well go on now to the third edition [1995]. The publishers, McGraw-Hill, approached both of us in terms of doing a revision, and neither of us was terribly anxious to put the time into it. But after I was going to be retired with respect to teaching or administrative obligations of any great consequence, I figured I could put some time into this. Brewer was not quite as close to retirement, but he was essentially retired by the time this was going to be implemented. He agreed to go along with it.

But then we didn't do much of anything for a year or two. Finally I did get to work on it and revised a large portion of what I thought I could handle best, was willing to take the initiative on. We were in reasonably close communication about it, including these sensitive questions. But by this time, it was quite clear that there was no point to retaining all of the Lewis terminology. The thing to do was to adopt what, shall we say, the American Chemical Engineers had found comfortable, namely, "G" for the Gibbs function, which really ought to be credited to him, and why Lewis wouldn't do it I never did understand, but "A" for the Helmholtz function, which doesn't really offend anybody, avoids confusion, and you've got "F" then for the Faraday constant, which is important too, and you had to use a different typeface or something or other to keep that straight. Now you didn't have any problem. And terminology-wise, give up "free energy" and just call it Helmholtz energy and Gibbs energy. Or Gibbs free energy and Helmholtz free energy.

So I was quite satisfied with that symbolism and terminology, and Brewer wasn't disagreeing about it. He just never got around to doing anything. That's a tale of human affairs and so on that I don't want to particularly go into. He still was somewhat active scientifically. What happened was his wife died. Somehow, he had less energy to put into anything that wasn't immediately pertinent. One could speculate further, but that's getting more into somebody else's human relations than I would want to go.

So in due time, I just had to bring the matter to a head and asked the McGraw-Hill editor to come out. It was decided that there would be roughly a one-year window in which Brewer would either decide to make a substantial contribution or not, and he didn't. So I finished it off.

He was very helpful on any particular topic. If I said, "Well, now, look, you've got information, literature or something or other, on this particular topic. Either give me some guidance or actually let me look at your file," and he was always very helpful about it. But that's as far as it went.

Hughes: Was that a disappointment to you?

Pitzer: Well, I would like to have had Leo carry it through; there's a limit to how much you're willing to essentially do it all yourself.

There's another aspect to the book that I thought was important to change, and that was, in a sense, a difference of emphasis. The original book was essentially based on room-temperature, mostly aqueous solution systems, whereas now, thermodynamics is at least as important in terms of high-temperature systems or nonaqueous systems that are important in mineralogy and oceanography and chemical engineering and metallurgy and so on.

It seemed to me that the approach ought to be much more nearly equal balance between the more traditional chemical areas and the applications that are chemical in the sense they involve many different substances. And this seemed to me to be important.

Actually, the amount of teaching of thermodynamics as a separate subject in chemistry has diminished. The thermodynamics tends to get incorporated into physical chemistry and other courses, but it is much less taught as a separate subject. The book has sold moderately well, but my guess is to a considerable extent it's for these wider range of applications where chemical thermodynamics is a more active topic than it is in physical chemistry itself.

IX TEACHING

[Interview 8: July 17, 1996] ##

Importance

Hughes: Dr. Pitzer, what weight do you put on teaching, as opposed to your other responsibilities, primarily research?

Pitzer: Well, I regard teaching as the most important thing that a university or college does. The research is certainly at the comparable level in a so-called research university, on down to the junior college or state college where the research has virtually no role at all. That doesn't mean that teaching necessarily occupies more than half of the time of a typical faculty member, and certainly if one is heavily involved in administration or other obligations that are important to the university, it is commonplace to relieve that person of some teaching duties during that period, and because there's an important continuity to research that, if too seriously interrupted, is harder to start again.

Hughes: Well, I was going to quote you G. N. Lewis, but you pretty much said it, because he regarded the training of men, as he put it, for basic academic research to be the most important of the department's functions.[1] You too apparently think that teaching is the most important aspect of the university.

Pitzer: He's taking a narrower view than I was.

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[1] John W. Servos. *Physical Chemistry from Ostwald to Pauling.* Princeton: Princeton University Press, 1990, p. 246.

Pitzer's Teaching History

Pitzer: In my own case, I would teach a few years, and then something would arise, such as when I was director of research for the Atomic Energy Commission and clearly wasn't teaching anywhere for two and a half years. But I had no trouble reinstating and starting teaching again thereafterwards, and was pleased to do so.

I was probably most unusual in the years at Rice in that I at least did a little teaching when I was president. Not very much; it was always to take a joint role with someone else in teaching a particular course, with the understanding that if my obligations as president were overriding at any particular time, the other person could maintain the continuity for the students. In the brief period at Stanford where I was contending with student disturbances and so on, I didn't pretend to teach at all. But I had no trouble taking it up again as soon as that period was over.

Hughes: Had you intended to teach when you first went to Stanford?

Pitzer: No. Stanford is a much more complex university [than Rice], and while I didn't anticipate as much difficulty as we had at that time, I was sure there were going to be tensions such that it would be foolish to think one would teach.

Another aspect with respect to my own teaching is that I have not generally taken on very large-enrollment elementary courses. In fact, the only time I did that regularly was the first two or three years after I got my Ph.D., when I taught, in the off-semester, the course that began Chemistry 1A in the spring semester instead of the fall semester, and it was rather fun. But that takes additional preparation, and if one does it properly, one needs to be accessible to students additional hours as compared to a more advanced course, even if there are a fair number of students in the advanced course.

So most of my teaching has involved either upper-division--junior-senior level--physical chemistry courses, or graduate-level. Frequently, these actually enroll some graduate students and some undergraduates at the same time, as honors undergraduates will be in a graduate-level course, or occasionally a would-be graduate student finds it necessary to go into a somewhat more intermediate-level course to get up to speed.

Hughes: You choose upper-division courses mainly because they take less time?

Pitzer: No, not entirely that. I think I can do a reasonably good job of an introductory course, but there are others who are better showmen, as it were, that can do a better job in a freshman course. My predecessor and mentor in so many things, Joel Hildebrand, was a wonderful freshman lecturer. Probably my most outstanding graduate student, research student, George Pimentel, was a wonderful freshman lecturer. I don't think I ever came up to quite that level of style. It is true that when one has substantial other obligations, it's easier to feel that one's doing essentially a 100 percent job of a more advanced course, where the students are more mature and are more committed to the course anyway.

The Chemistry Curriculum

Types of Courses

Hughes: How broadly based was the curriculum in chemistry when you first began to teach?

Pitzer: Oh, it covered very broadly chemistry as it was then viewed. The freshman course and to a considerable degree sophomore courses were designed not just for chemistry majors but for all sorts of other fields, or for general education, breadth of education. Thus, the enrollment in the freshman course was maybe only about 20 percent chemistry majors, and the others were engineers and premeds and biological science people and so on.

At the sophomore level, it's intermediate; it's become mostly chemistry or chemical engineering, in the later years chemical engineering majors, but there is still quite a component, particularly in the organic course, which is usually a sophomore-level course, of biological science majors, premeds and so on.

Hughes: Did you attempt to add material or change your delivery in any way because you knew that there were chemical engineers in your audience, as well as chemistry majors?

Pitzer: Oh, sure. Where you have to have some example of a given concept or equation or something, you just choose an example that a chemical engineering student would recognize as being pertinent to chemical engineering. Or if you have a lot of premeds in the class, why, you'd try to choose one that looked like it was biologically interesting. It wouldn't be medically interesting, but it might be biologically interesting and that they would recognize.

Hughes: So you did adapt your style to your audience.

Pitzer: Oh, yes. But this does no harm to the chemist at all.

Hughes: Was the emphasis on teaching of students bound for an academic career characteristic of the department in those days?

Pitzer: Well, I think the attitude was that teaching should be done in a fashion that is first class for that category, but there's no reason why it can't still be quite appropriate and effective for students who have a broader range of interests and prospects and future plans, within reason. But along that line, I might comment that chemistry hasn't given a completely nonprofessionally oriented course, such as physics and some other departments have. In other words, with physics, it's commonly Physics 10 by number that is directed toward the arts or social science student who has no intention of using physics in further work.

The physicists usually also have two other courses, one intended for people that are really serious about physics, that the chemists take and that most engineers take. And then there was an intermediately serious physics course for biological science, premeds and so on, requiring less mathematics, that ought to have some more serious physics than the physics for artists course, or whatever you want to call it.

Well, chemistry has felt that a general introductory chemistry can be taught at least for a semester or for a year that is perfectly appropriate for would-be chemistry majors and is appropriate for any reasonably serious student, regardless of their future interests. This view is influenced--at least it was in my time as dean [1951-1960]--by the knowledge that most all of our students that enroll have had high school chemistry. Relatively few have had high school physics. This was typical of incoming high school students both in my age in high school and certainly through the forties and fifties. So a very thin introduction to chemistry would be repetitious for these students.

On the other hand, the general chemistry course was nonetheless given in a fashion that a good student could handle it without having had high school chemistry. In other words, high school chemistry wasn't a prerequisite. Now, I haven't kept up to date concerning the pattern with respect to high school background, and it may or may not still be valid. But, the department here still is along that line.

Recent Biological Orientation

Pitzer: The major change that has been made here is to deal with the greatly increased number of biologically oriented students that want a quite serious organic chemistry course. Now, I was never involved in teaching organic chemistry, but we did have two courses, one aimed for premeds, as it were, or those that wanted just a limited introduction, and those that were not just chemistry majors or chemical engineers but including biological people that were really serious about the organic chemistry. The population in that area has greatly increased, so that that has become a major teaching obligation now.

Hughes: That's tied in with the flowering of the biological sciences?

Pitzer: Yes. And the present faculty is taking that very seriously. In fact, there is a tendency to go to organic chemistry even in the second semester of the freshman year, and then come back to some additional inorganic and analytical chemistry later.

It was always a pleasure to find at least a certain number of students who were bold enough to come in and get acquainted personally. I always made a point of keeping good enough records that I could always write pertinent and appropriate recommendations even years afterwards. Not too many students at the levels that I was involved with asked for this or made these contacts, but I welcomed and enjoyed those who did. Of course, the more you got into graduate level, then you frequently got on an oral examination committee or a thesis committee, and then, of course, it was obvious you were going to keep a detailed record and be prepared to give evaluations of the students later.

Graduate Students, Postdoctoral Fellows, and Visiting Scientists

Hughes: Would you care to talk about the process of accepting a graduate student or postdoc?

Pitzer: Sure. There are great differences. Graduate students may be attracted by a given faculty member, but they're admitted to the department. They're expected to interview at least two or three potential research directors, even though they may have a fairly strong pre-decision toward a particular person. But I think it's important to maintain that process.

A postdoc, on the other hand, comes to a given faculty member by invitation of that faculty member, who is going to provide financial support for them in almost all cases. Now, occasionally, a postdoc with some postdoctoral fellowship, brings his own support, at least with respect to his personal expenses. But even then, it's predetermined that they're coming to work with a particular person and that there is space in his laboratory and facilities available and so on.

I emphasize this difference because it's important to recognize it when retirement comes along. With the department here, the pattern was established by Hildebrand, who maintained scientific activity years after he retired, but he stopped taking graduate students. I know perfectly well why; I heard him talk about it, and I agree completely. If you were still to take graduate students when in retirement status, it puts you in competition with your still-active colleagues that are carrying teaching obligations and administrative obligations. Furthermore, of course, it means the student has to gamble on your maintaining good health [laughs] and capacity.

But it's totally different with a postdoc. You're not in competition with your colleagues. The postdoc was not thinking of going with any one of your colleagues. He might have gone to Chicago or Columbia University or somewhere else, but not with one of your colleagues. If you're attracted to them and vice versa, and you have whatever financial support is needed and some space, then that's quite a comfortable relationship.

Hughes: Did you stop taking graduate students for that reason?

Pitzer: Oh, yes. Well, I could add that in later years, it became a definite policy.

Hughes: Of the department?

Pitzer: Of the college, yes, for both departments. In fact, it was detailed even further that in anticipation of retirement, people should stop taking at least any appreciable number of graduate students, so that all those in process will complete their work at least within, say, a year or two after [the professor's] retirement. In marginal cases or with rare exceptions, one might take a graduate student jointly with someone else who is perfectly prepared to carry on with that student separately if anything happens to the older person. But that's rare and tends to occur only where there's some highly special technique or instrumentation or something or other that is involved.

Hughes: Do you have postdoctoral students at the moment?

Pitzer: Oh, yes.

Hughes: I knew you had students around, but I wasn't quite sure how they were classified.

Pitzer: Well, they're not all postdoctorals. At the moment, I have just two, and both postdoctorals supported by the Lawrence Berkeley Lab with Department of Energy money. I have a modest amount of campus money in addition. One of these people ran out of time on the length of time that LBL normally supports a postdoc, and I carried him on for another six months or so, something like that.

But, over the past two or three years, I had in addition one man on a German full-support fellowship who wanted to come with me for a limited period of time, six months or nine months, something like that, but then carry on his multiyear postdoctoral fellowship back in Germany with someone that he wanted to have his primary contact with there. And I get every once in a while a sabbatical visitor, a more senior person who is on sabbatical leave from wherever he is, and sometimes needs a little financial support, which we find.

Or, and I've had three of these, I have a visiting senior scientist, nominally fully supported by the Chinese government. [laughs] Their support is marginal, and in each case, I have usually had to supplement it a little bit. But that's gone reasonably well. The first man, Yi-gui Li, twelve, thirteen years ago, was excellent and has a major international reputation now. He goes to international meetings, comes by to visit, will frequently fly in and out of San Francisco. The second, and the third, who has just left a few months ago, were not up quite to that standard, but they did good work. We got publishable research done that was worthwhile.

Hughes: Is it you that usually takes the active role in inviting visiting scientists?

Pitzer: For the most part, I just receive inquiries. I make a quick judgment as to whether to encourage the inquiry enough to find out more about the person and decide whether to make an offer or not, or whether just to steer it away. Occasionally, when I have a particular piece of apparatus and I want someone that has some competence to operate that piece of equipment, I'll send out a letter to maybe ten different people that are likely to have graduate students that have interest or experience relevant to this area. And in a number of cases, that's led to the appointment. But the two that I have now, in both cases, had written to me and expressed interest.

Hughes: Is that usually on the basis of shared research interests?

Pitzer: Yes. In other words, they are interested in research along a line that I have been publishing, and what I put them into isn't necessarily exactly that area. [laughs] Frequently, I think that [research problem] was pretty well established elsewhere, and I want to do something slightly different, not grossly different.

Hughes: Is the research project determined by you or the postdoc?

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Pitzer: If I have invited the person here and I'm providing the full measure of support and so on, I determine the initial project. As time goes on, we will have conversations as to what the next project ought to be. If they come up with a good idea that may not have been the one I would have otherwise chosen, but it's about as good as anything else, why, I'll encourage them to do that.

In the case of the fellow that had the German fellowship, however, he had a very specific thing that he wanted to do, and essentially just wanted me to be a sort of consultant and advisor while he did what he wanted to do. And that's fine. It's not [a problem] that I would have chosen, although it was something that I was very much interested in and very happy to see somebody who really felt competent to do it. He was far better able to do it than I was, as a matter of fact.

Hughes: Which is the reason that you wouldn't have chosen to do it?

Pitzer: Yes.

Mentoring

Hughes: Please describe your style of mentoring.

Pitzer: Well, I try to adapt that a great deal to the mentee. [laughs] Of course, for a postdoc, you assume that they've had research experience, they know how to go about research, and they either have more or less a full background in the area in which we're proposing to do research generally. Or else we agree that they ought to do some independent study and gain that immediately, audit a course or whatever.

Then beyond that, it is essentially just a matter of beginning to do it. For example, my most recent postdoc, Peiming Wang, is taking over a piece of equipment that needed some further calibration and upgrading. I think she's handling it very capably. It's causing us more problems than I had hoped, but that's the way things go. We've already agreed on the particular experiment she's going to do as soon as this recalibration and apparatus improvement is done.

The Oak Ridge National Laboratory has some programs quite similar to things that we're doing here and has some people in it that I know very well, including former students. In this case, for example, I made inquiry there as to whether they had any unpublished work or work in progress that would relate to what this young woman is doing here, and indeed, they did have, and we have got a long preprint of a paper. Among other things, she's been putting those equations into her computer, and it didn't seem to work right, so we've got communication problems. Maybe they'll send a complete computer program for it.

You never can tell whether what's wrong in a case like this is something that came into the manuscript or whether it's possibly a glitch in their program and that they have a mistake. We've had experience with all of those things as time goes on. But one can transmit an entire fairly complicated computer program, if each computer is compatible with the other, and then begin to iron out any difficulties.

Hughes: What about degree of independence?

Pitzer: Well, this depends a good deal on the individual too. I want to keep closely informed about how things are progressing, but if the individual seems to be making the right decisions and going ahead and doing the right things, then I essentially just encourage them. If on the other hand not much seems to be being accomplished, or if I think they're going off on, shall we say, time-wasting diversions, then I'll take a stronger hand and say, "Well, now, let's get this particular sub-project done with top priority." It's highly variable in different cases.

Hughes: How would you describe the social relationship? Are you the professor and they are the student?

Pitzer: I am pretty old-fashioned about that. [laughs] I try to be very friendly and all that, but I think this is partly a generational question. Somebody even in his upper sixties, let alone eighties, is not an everyday pal of somebody that's in their twenties. So there is a distinction there and there's no use pretending not.

In past years, I went a good deal further in terms of trying to have relatively frequent social affairs and so forth. My wife [Jean Mosher Pitzer] is all in favor of being friendly, but she's gotten older, too. She did a lot of entertaining, both at Rice and at Stanford as a president's wife, and now we do less of that than earlier. What I tend to do now is have [postdocs] over to the Faculty Club for lunch when there's some visitor in town, or just occasionally anyway. Then about once a year, we'll have a more formal dinner, again maybe at the Faculty Club or some restaurant, with any wives or other particularly close friends that are involved, and make more of a party out of it.

Human Relationships and Ethics[1]

Hughes: Were there nonresearch aspects of science that you thought should be imparted in training a student?

Pitzer: Well, there are always human relationships in this world, and you assume that students are absorbing various aspects of these human relationships along the way. Sometimes you make a point of talking about something, some situation maybe that involves other people nearby that they're familiar with, rather than anything they're immediately involved with, as a way of, as it were, teaching a lesson, giving some guidance. But I think that follows fairly generally around the world.

But of course, in some cases, some people do get into trouble. There is one that I'm familiar with particularly, but only indirectly, because it occurred at Ohio State University where my son is not only an active faculty member but was chairman of the department for part of this time. A very prominent organic chemist was accused of misusing information that he'd received confidentially. It turned out that the way this got misused was mainly when he was sent, say, a proposal to referee, to give an opinion to a science foundation or whatever organization. He'd invite one of his students or postdocs to do it instead.

Then by one means or another, some of these ideas for future research got incorporated either into papers or proposals of one of his postdocs who'd gone on someplace else, or even of the professor himself. I don't know the details, but they had a formal investigation of this in which the dean, up one echelon,

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[1] For better chronology, this and the following subsections were moved from their original position at the end of the transcript of this session.

selected the committee and received the report. I guess it was all more or less worked out; I don't know what the relations are now with some of the federal government agencies.

But human nature will sometimes, as it were, get off the appropriate track, and the boundaries of an appropriate pattern are not always obvious. People can be tempted to do things they ought not to do and not realize, or if you want to be less sympathetic, think they can get away with.

Hughes: How did you pass along ethical standards?

Pitzer: Well, I tried to do this by occasionally discussing things like this and saying what I think was appropriate, and why.

Social Status

Hughes: In my reading of the early correspondence concerning the College of Chemistry,[1] there seemed to be an awareness when new people were coming of what their social status was. There were in letters of support terms such as "He"--and it was always a "he" in those days--"comes from a good family," or "is a gentleman," et cetera. Would you like to make a comment about how sensibilities such as that might have changed?

Pitzer: I suppose there is some change. Well, there's certainly a change in that it will frequently be "she" now. [laughter] Mostly, that sort of thing comes along with the more specific recommendations to the person that's considering candidates [that he] needn't have any concern about this person in terms of personal factors from that general point of view. If their background is less obviously satisfactory and commendable, then one might comment more particularly in terms of how they have handled themselves in various situations.

All this sort of thing involves human relationships. It does change with time some, but I would think not all that much.

Hughes: Is what you're talking about geared now to personal rather than class characteristics?

Pitzer: Yes.

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[1] College of Chemistry papers, 1912-1945, CU-5, University Archives, The Bancroft Library.

Hughes: I picked up from these earlier documents that there was a certain expectation that members of the college would come from the upper class. Was this true when you first entered the field?

Pitzer: Oh, I don't think so. Certainly at any time, the chance that a young person develops an interest in and a high capacity to succeed in what I call intellectual activities, such as university faculty positions and so on, there's a greater probability that that's going to develop for someone with a home influence or a neighbor influence and so on that has stature and recognition of that type of thing. It's going to be a more exceptional young person who actually does follow successfully an educational sequence starting from a family background that has practically nothing in this regard, but it happens. I could even mention names, but I don't know that there's any need to. A few of my best graduate students were of that sort, and others, in contrary, had family background that was very strong in that respect.

Specific Students and Postdocs

Hughes: Do you wish to say anything about specific students or postdocs? I think particularly of George Pimentel whom you've mentioned a couple of times.

Pitzer: Well, there have been a wide variety. Pimentel is clearly outstanding by any standard. He was elected to the National Academy of Sciences, was a wonderful freshman lecturer, and led the high school level textbook project known as Chem Study. I was on the steering committee or whatever it was, and Glenn Seaborg was chairman of it. But the generation of the textbook was Pimentel's, although various chapters were written by other people. He's written other books, and he's had excellent students. In a sense, a great many of my most distinguished academic offspring are actually grandchildren via George Pimentel. [laughs]

I've had some other very able students, too. The group right after World War II was absolutely first class. Bill Weltner was present at the same time as Pimentel. He is still active in research at University of Florida, making a very substantial contribution still.

William Gwinn, one of my very first students, was on the faculty here, and did very well. When I was away on leave at times, he would take over things for me, and we did some things jointly through the years, in the internal rotation area particularly. He was a leader in using World War II technology in radar for microwave spectroscopy. He got tired of research late in life and retired a year or two early, and is still active in some consulting activities.

A great many others that I could name have very commendable either academic or nonacademic scientific careers, but I don't know that there's any particular point in trying to name particular individuals here.

During the period of the fifties when I was dean, I had quite an active graduate program, but the number of students wasn't terribly large. The one that certainly does deserve mention during that period is Robert Curl, who is on the Rice faculty and was recently department chairman. He has done some very important research, although it's not quite up to the Pimentel level, but very commendable work, and a very fine person.

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Pitzer: I should add a few words about Robert Curl. Since we dictated the earlier part, he has received a Nobel Prize [in chemistry, 1996]. Along with his Rice University colleague Richard E. Smalley, and their collaborator from England, Sir Harold W. Kroto. The award was based on their discovery of the so-called buckeyball or C_{60} molecule, which has certainly had a major impact on the science of the element carbon.

Robert Curl was clearly one of my very good students, and I collaborated with him some again when I was there at Rice as president. Part of his Ph.D. work involved the work on the corresponding states, the program involving the definition of the acentric factor and the evaluation of numerous experimental data in that program. I gave an invited speech[1] about that in London at an international conference in 1957. The Institution of Mechanical Engineers of London awarded their Clayton Prize to Curl and myself on the basis of this in 1958. The prize had a dollar amount, not very large. I gave it all to Curl. I enjoyed the trip to London. But a number of people, I'm sure, through the years and particularly since Curl's Nobel Prize have wondered just how and why he, a physical chemist, got an award from the mechanical engineers in London. [laughs] But it was well justified at the time, and an interesting element to a very good career.

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[1]The Thermodynamics of Normal Fluids, Kenneth S. Pitzer and R. F. Curl, Jr., Proceedings of the Conference on Thermodynamics and Transport Properties of Fluids, 1957, Institution of Mechanical Engineers, London.

Hughes: Could we revert to a question that occurs to me?

Pitzer: Sure.

Hughes: And that is, your opinion of the Nobel Prize itself. I'm thinking of the controversy over the fact that it is awarded usually, if not always, for one piece of work, rather than the individual's entire scientific contribution.

Pitzer: Well, your statement is correct. As such, since it is openly acknowledged, I don't think there's any reason to object to it, except that the overwhelming attention that the Nobel Prize gets as compared, for example, to the National Medal of Science in this country, or within chemistry, the Robert Welch Foundation Award, sort of throws things out of balance. In other words, those latter two are more or less, at least, on a whole career, or at least a major portion of a career. Also the Nobel Prize is shared, whereas, of course, the National Medal of Science is an individual award. But the rather arbitrary rules as to how the Nobel Prize can be shared leads to some rather peculiar things at times.

Hughes: What are you thinking of when you say that?

Pitzer: Well, just for my own case, the prize went to Hassel and Barton, not for one piece of research, actually, but for the combination of the identification of the axial and equatorial substituents in the structure of cyclohexane, which arose from the internal rotation barrier, and the more general introduction of the effect of that internal rotation barrier on more complex organic molecules. Now, sort of the origins of that was my work on the internal rotation barrier in the first place. I've never known what the nomination situation was, but a lot of people have said they didn't see why it wasn't split three ways, and in view of my earlier but fundamental component. Now, in the published statements about that award, my work is acknowledged, but that's different from getting a third of the prize!

Hughes: Yes. [laughter]

Pitzer: And there are innumerable cases through the years of that sort, where--well, my colleague, Harold Johnston, on the next to the last Nobel in chemistry, could easily have been one of the awardees, but wasn't. In that case, I think it would have been going from three to four. Now, why they could go to three in one case and seemed to feel they had to stop at two in another case, I don't know. But there's all sorts of internal affairs there. I guess that's enough on that topic.

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Hughes: Is there an explanation for the group of outstanding people right after the war?

Pitzer: Yes, they were a little more mature, and they were anxious to get on with their career. The war had delayed their career, and they wanted to get on with it. So they were very effective, got very commendable thesis research done, and I was fully aware that they wanted to get on with their lives. We helped them finish off and encouraged them to finish off a fully acceptable thesis and then go on with their career. Of course, Pimentel stayed here on the faculty, as did Gwinn. Gwinn started pre-World War II. [Ray] Sheline was another one who had a very good career, in Florida at Tallahassee, and Weltner was at Gainsville. There were others not quite that outstanding, but that was a very good group. Curl came along a little later.

Hughes: I've noticed throughout our discussions that you often mention associations with universities that are not considered to be in the top rank. Is there any explanation?

Pitzer: Well, it's true that of my students that have gone into academic work elsewhere, few of them have shown up in what you'd call Ivy League or other truly first-line institutions. Well, the one that would have been most capable of doing it, Pimentel, was here and stayed here, as did Gwinn. Curl was from Texas in the first place, in fact had been a Rice undergraduate, and to what extent he might have attained a position in one of these other institutions I think would have been an open question. He did have a short postdoc period at Harvard, but I'm sure Rice was coaxing him back, and he looked with favor at that. It's a very good institution. It's at a very high quality level, but it's small. And he is the one who won a Nobel Prize.

Amusingly enough, the one that did end up in an Ivy League institution, as far as I was concerned was almost a washout. [laughing] A very interesting chap, of Turkish origin, Oktay Sinanoglu. [tape interruption] He'd started out to be a chemical engineer and decided he liked physical chemistry better, did a very nice thesis on molecules observed on a surface, which has been taken as a basis for a good deal of further work by him and by others. Three papers came out of his thesis work, all very commendable.[1] He very soon landed a regular position at Yale and was strongly backed by a theorist there. Sinanoglu was purely a theorist. But I don't think that he came up to the level of

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[1]*Journal of Chemical Physics*, 1959, 30, 422; 1959, 31, 960; and 1960, 32, 1279.

accomplishment later of several of the people that I have already mentioned.

Edgar Westrum was awarded his Ph.D. in 1944 and was in war research until 1946 when he joined the faculty at the University of Michigan. It's in the top group of state universities. Some additional research had to be done on his thesis problem, so it wasn't published until 1949.[1] It was important at that time when the location of the proton in a hydrogen bond was of great interest and some dispute for the F^{-}-H^{+}-F^{-} case. We showed the properties of solid KHF_2 were consistent with a symmetrical pattern for the HF_2^- ion in this case, in contrast with the unsymmetrical pattern in some other cases.

Westrum measured the heat capacity of both KF and KHF^2 from 16 to 500 K and the properties at 500 K for the reaction $KHF_2(s)=KF(s)+HF(g)$. I might have included this in the Selected Papers volume but didn't; it isn't cited often now.

Westrum has continued, even in retirement, to publish many, over 500, papers on thermodynamic measurements. Some are very similar to his thesis with low-temperature heat capacities as the core. Some involve collaborations with others at different laboratories in the U.S., Canada, Europe, or Asia. But all are on chemical thermodynamics, and few involve issues arising in other areas of science.

Another person that was a postdoc in the late 1950s is a very interesting case, Enrico Clementi. His research on polyatomic carbon vapor I already described (pp. 127-128). He did not go in the academic direction, but after a second postdoc at the University of Chicago, he went with IBM and became a major figure in demonstrating how to do theoretical chemistry with an IBM computer. IBM supported him very strongly, primarily because this sort of work would illustrate and help sell the computers, I presume.

He was of Italian extraction, and at one point was invited as a professor at--I wouldn't say a leading, but--an important Italian university. I judge that didn't work out very well, and I don't know all the details of Italian universities, so that it may be nothing critical of Clementi. It may have just been an unrealistic expectation of his to go. So he came back, rejoined IBM. Initially, he'd been at the IBM at San Jose here in California. Later, he was at IBM in their main research lab in upstate New York. Then they had a reduction in their budget and

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[1]Journal of the American Chemical Society, 1949, 71, 1940.

they chose to spend somewhat less in this area and arranged to set him up with some relatively more modest program back in Italy. [laughs] Still under some IBM connection.

I followed a lot of this because he had problems with the immigration authorities, and then had them again when he was coming back from Italy, so I had to write letters for him. But I maintain quite close contact with him, and he's a high caliber person, but he didn't go into the academic side.

Hughes: Did that disturb you?

Pitzer: That he didn't go academic?

Jobs in Industry

Hughes: Well, did you have any preference about where your students ended up?

Pitzer: Well, I hoped they would make good use of their abilities. Oh, I suppose other things being equal, I would encourage them to the university side. But if they were at all uneasy about teaching, then I would not encourage them that way. It may work out all right for the individual, but it's really not best for the university system and country. Nowadays, there are plenty of opportunities in other research establishments for those who really don't want to give their full attention to teaching.

Hughes: What rough percentage of your students went into industry?

Pitzer: I'd really have to run a count to be realistic about this, but I would say it wouldn't be very far from a third in industry, a third in academic university appointments, and a third in other nonteaching [positions], either government-funded national laboratories or other institutions of that general character, rather than industry, where they're of first quality in terms of research although they may have some fairly strongly focused long-range application. Some of them that have gone into industry I judge have done very well, but that work tends usually to be confidential. And none of them have popped up as president of the company.

X COMMITTEE WORK

Hughes: Shall we go to committees?

Pitzer: Might as well.

General Advisory Committee, Atomic Energy Commission, 1958-1965

Hughes: Well, first on my list is the General Advisory Committee of the AEC, where you were a member from 1958 through 1965, and chairman from 1960 to 1962.

Pitzer: Well, that was an important committee at that time. I'd had a good deal of interaction with that committee earlier, when I was with the AEC as director of research. I was quite willing to serve. I didn't want to continue as chairman, because by that time I was at Rice as president, and I thought that it was better for someone else to carry the chairmanship. There were many important topics, but I wouldn't say there were any of enormous consequence that would have come into the public view.

President's Science Advisory Committee, 1965-1968

Pitzer: I think I actually resigned with maybe a year or so left on a term, because I was asked to go on the President's Science Advisory Committee in 1965. This was during Lyndon Johnson's presidency. I thought that was under the circumstances probably more important and had a broader sphere. That, of course, became a more and more tense situation as the Vietnam War became more overbearing on the American scene, and there were some fairly critical things there. We were briefed by very high-level people with respect to the military situation, but it really wasn't something that high technology had much to do with.

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Pitzer: As the years went on, very severe pressure developed, and I don't think they were listening too much to the Science Advisory Committee, although the chairman at that time had full nominal access to the President.

One amusing thing came up that I might mention. We were in the middle of a meeting and a message came in urgently to the chairman, and he asked me and one other member to stay after the meeting. It turned out the president of Korea was visiting, and I think I'm not exaggerating this: he was going to the White House that very evening--maybe there was one more day to spare--and the President wanted something to offer him in his toast. [laughs]

The three of us[1], and I will have to do a little memory research in order to supply the additional names, invented for him a research institute, the rationale for which would be that it could attract very able Korean scientists that were in this country, or possibly in Western Europe, back to Korea for a new institution in which they would play a leading role. Whereas they would never go back to a traditional Korean university where they would be fighting old hands and trying to overcome rigid rules and so forth that would be unattractive to them. We were able to do this because we knew individuals, not necessarily in large number, that fitted this mold, and therefore, it wasn't an empty gesture. Well, Lyndon Johnson took it up, and in due time it was established, and at least one of my people did go back there. [tape interruption]

I have to amend that a little bit. I don't really recall who I had in mind as candidates in the mid- to late sixties when we were suggesting this to President Johnson. It was a little later that Yoon S. Lee was with me in the period of research in the early to middle seventies, when we were doing quantum mechanics where relativistic mechanics was required. He was of Korean origin and did join this Korean research institute soon thereafter.

Hughes: In both the case of the AEC General Advisory Committee and the President's Science Advisory Committee, do you know how you came to be appointed?

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[1]Donald Hornig was the science advisor; the other member was Herbert York.

Pitzer: Well, on the AEC, I think it's rather clear: I had been Director of Research, and I had background that was obviously valuable to the committee. I had worked with the committee, and in a sense against it, during the question of the decision to go ahead with the hydrogen bomb project. By that time, 1958, the commission was committed to that project, proceeding with it, and while it was going along well enough, I was obviously someone that had background in the area. Also, I had contacts more broadly, which a member of that committee is supposed to have.

I suspect [my appointment to] the President's Science Advisory Committee probably goes to Lyndon Johnson himself. I was in Texas at that time as president of Rice. I had met Johnson; I wouldn't say I knew him well, but we at least had met on occasion. My board chairman at the time at Rice, George Brown, was a close friend of Johnson's, so it would not be surprising that I came to the president's attention. Brown might well have said, "Well, Pitzer's in Texas now; why don't we have him on the committee?" There probably weren't any other Texans on the committee. I was maybe a bogus Texan. [laughing] I've never investigated that.

My general stature in government circles, as obviously from the AEC side, would have made it appropriate. But I've always suspected that Lyndon Johnson had something to do with it, too. Probably a fairly long list of names were discussed, and I would have been legitimately on it. But it could well be that either Johnson himself saw the longer list or somebody else on the White House staff saw the longer list, recognized my name as being in Texas at the time, and called it to the president's attention.

Hughes: As a member of that board, did you have personal association with Johnson?

Pitzer: Yes and no. That is, he virtually never met with the committee. I don't say he never did, but he very seldom did. The chairman of the committee, Donald Hornig, of course, would have a briefing session with him no doubt afterwards and with other top White House people. But he did meet with the group once in a while, less frequently as time went on, actually. And then I saw him occasionally more or less in social circles in Texas.

Council of the National Academy of Sciences, 1973-1976

Pitzer: I suppose next we might talk about the National Academy of Sciences. I was on the council, an elected council member, for two terms rather separated one from the other, 1964 to '67 and then later 1973 to '76. Although the council is in a sense important just to the Academy, it's also important to science nationally.

Also, I was on and frequently chairman of very important committees of the Academy. I was involved I think three times on selection of the president of the Academy. The only recent one that I wasn't involved with was the present president; in other words, for the preceding two or three presidents, I had a major role in either selecting or in nominating for a second term.

There were a number of other quite sensitive or important policy questions that the Academy needed to study in greater detail than the council could do itself, and I was involved in frequently chairing a committee.

Board of Directors, Owens-Illinois, 1967-1986

Pitzer: Membership on the board of directors of Owens-Illinois was an interesting insight on the industrial world. It's not a high-tech company; but at times it attempted to be a high-tech company. It never worked out very well, and partly for internal reasons that I understood but couldn't do anything about. I suppose I got onto that board because the chairman of the board, Preston Levis, was also a trustee of Cornell University and asked individuals at Cornell who would be a scientist and administrator that might be an appropriate member. They had quite a lot of business in Texas, and he might well have asked, "Is there somebody in Texas that he might think about?"

Anyway, I was invited to do it. It didn't meet very frequently, which is one of the reasons I accepted it. Four times a year, plus a special meeting maybe, whereas many boards meet every month, and that would have been much more of a burden. But it was quite interesting, and as I say, was an insight on how the industrial corporate world operates.

Consultantships in Industry

Hughes: Has there ever been any prejudice in academic chemistry about serving as a consultant or on an industrial board?

Pitzer: Well, I would say there are differences of opinion as to the appropriateness and to the extent of commitment. Of course, more often it was a matter of a consultant rather than a board member. The involvement of a board member is very limited time-wise, even if they meet somewhat more frequently than Owens-Illinois did. The person is at arms' length with respect to the actual operations of the company; although he has great influence, he's not actually pulling the levers, writing letters and making appointments or passing out money and so on directly.

The sensitivity develops much more with respect to consultants, who may get drawn into spending too much time being remunerated somewhat in terms of the importance of their work to the company. In other words, it's a temptation to a faculty member to spend more time than he should probably. Also, the corporate work is almost certainly confidential, and if it is close scientifically to what he's doing in the university, there's a danger of inappropriate handling of confidential information there, either discouraging open discussion of what's going on in the university, or otherwise.

A member of a board doesn't get into that. That is, there will be confidential things, very confidential, but they're so far removed from the day-to-day work with your graduate students or postdocs that there's no real problem there.

Hughes: Did the university have any rules at that time about consultantships?

Pitzer: There's almost always a rule about time. If it's more frequently than one day a week, then it's too much. But it's also a matter of scheduling the time, and it has to do with what your teaching obligations are too. If it's a fairly advanced-level course and there's somebody else on the faculty, or maybe even one of your postdocs who can fill in for you effectively, then an absence isn't so serious. But if it's a big lecture course, even if your substitute is good, it will be a break, and unless it can be arranged to be some special sort of peripheral topic that is nice to bring in, why, it upsets the instruction.

Appointments on Other Committees

Pitzer: Well, let's see. The Carnegie Foundation for the Advancement of Teaching [1966-1972] was interesting but not terribly important.

A peripheral [appointment] in a sense, too, somewhat like the O-I [Owens-Illinois], although for a much shorter period of time, was the Federal Reserve Bank of Dallas [director, 1965-1968]. The time obligation there was almost trivial; in other words, it was a day every two months, something like that, but that was just hop on a plane in the morning and have a midday meeting and be home at night, even in time for dinner.

The Houston Chamber of Commerce [Director at Large, 1965-1968] again was interesting, local.

Hughes: Why were they interested in having a scientist on that board?

Pitzer: It wasn't a scientist; it was a university president.

Hughes: Oh, I see.

Pitzer: A regional Federal Reserve Bank board has three types of members. There are I think three of each. There are three bankers, that are presidents of banks in that district. There are three, shall we say, industrial people, bank customer types, if you wish. And then there are three public members that are supposedly not really involved directly with the banking business or interests, but with public welfare with respect to banking. I suppose there's a story why I happened to be involved, but I don't think it's particularly important.

The Rand Corporation [1962-1972] is an interesting situation, but those trustees only met twice a year, and I don't know that we need to say anything more about it.

Universities Research Association, Council of Presidents, 1965-1971

Hughes: What about the Universities Research Association?

Pitzer: Well, that was something that we invented. That was right at the end of my AEC General Advisory Committee membership.

There had been quite a controversial situation with respect to the location of a major new accelerator facility. It was important not only to de-politicize the contest for geographical location but also for its management. Several of us invented this Universities Research Association to be of a national character so that it would be impartial with respect to actual geography, both in terms of initial location and later management. Of course, I was president of Rice at the time.

I thought it was a good idea to try to get the university presidents involved, not in the active management--there would be a board of directors or trustees that would have been selected by the presidents--but to get the presidents involved to that degree. I think in that respect it was probably unsuccessful. Except for that aspect, however, I believe that the organization has been successful. The accelerator was actually located in Illinois, but in contrast to the Argonne [National] Laboratory which is contracted out of the University of Chicago, it is contracted to this Universities Research Association, so that somebody from Colorado or MIT or California feels on an equal basis in going there.

I haven't followed it more recently, but my impression is that the board of trustees more or less recommends its own successors. But then the fact that there is a mechanism to review that may still be a beneficial one.

American Council on Education, Board of Directors, 1967-1971

Hughes: Another committee membership is on the American Council on Education.

Pitzer: Well, that's interesting. That is the most inclusive of the higher education associations. In other words, including all levels, from state colleges, private colleges, on through the top-level universities.

Hughes: Now, are you talking about membership in general, or membership on the board of directors?

Pitzer: Membership in the organization. The board of directors is fairly small, as I recall, and my involvement there again I think had a good deal to do with the Texas situation. My membership began when I was still at Rice. The president of the American Council at that time, Logan Wilson, had been in Texas recently, at the University of Texas. I had gotten to know him personally during the 1960s.

The council is important, I suppose, in representing the broadest view to Congress or to the public when questions concerning higher education arise, appropriations are being considered, and so on. In addition to helping with possible controversies, there is one association of the very top research universities--I'm not sure I have the right name for that right now--and then there's a Land Grant College Association, which is all the state universities that are of high enough stature to have agricultural research programs and things like that. And then there's a Private Undergraduate College Association, and there are probably a couple more that I don't think of.

Sometimes, these have conflicting points of view, and they lobby Congress in conflicting directions, and the American Council will tend to ameliorate those possible difficulties or complications in relationships with the broader public.

XI SCIENTIFIC PUBLICATION

Student Participation

Hughes: How do you go about writing a scientific paper?

Pitzer: Well, the first question is, who does the first draft? Do I do it, or does the student or postdoc do it? In the case of students, it's clearly an important part of training to get them into writing about their own work. So in that case, you tell the student to write a first draft, which will presumably be a chapter in their final dissertation, although there may be exceptions to that situation.

Then after that's been written, one makes the judgment as to whether the student is able to efficiently get it condensed somewhat and rearranged to have the appropriately publishable paper, or whether it's better for me to just go ahead and do it and assume maybe the student will do it later for another chapter, assuming that there are several, as there usually are. I find in general students do pretty well. They may have been relatively poor in writing earlier, but once they've got something they know and they're committed to writing about it and doing it efficiently, they do.

Now, when it comes to the postdoc or the senior visitor or others, there's more of an option as to who does the first draft. If a person is clearly somebody who can do a reasonably good job, why, I'll always have them do it. Most recently, this man was from China and had been here only a little less than a year. He was about to go back within another month or two. To have expended the time for him to do a first draft that would almost certainly be almost impossibly far from the final thing would have just used up very valuable time, so I did a draft. Then I said, "Now, look, you go over it and be sure it's correct in all these details that I may have just guessed at," and that went very efficiently in this case and in earlier cases. It all worked out satisfactorily, and I'm sure they were satisfied.

In other cases where the person has grown up in this country or Western Europe and is used to having written up their own thesis or other papers based on it, why, I always have them do a first draft, and frequently it's very close to what gets published. In some cases, I don't change it as much as I should. There was one case I think of in particular concerning one of the more important things we've done in recent years. This young man, Rajiv Singh, was from India originally but, after all, English is a perfectly good language in India too. He'd gotten his Ph.D. at the University of Tennessee. He'd done very fine work.

I had him go ahead and write up a paper for the *Physical Review Letters*. This was on the near critical ionic systems. We never got it published there. I realized afterwards that if I had done it myself, I probably would have been able to put into the introduction phrases that would have caught the eye of the editor as being something that belonged in *Phys. Rev. Letters*, which is a very selective journal, as you probably know. We probably would have gotten it published there. As it was, I relatively soon sensed that this was just marking time, that I might get it published, but that I'd get it published sooner by just sending it to the *Journal of Chemical Physics*. They would publish it right away, and would allow us to go into more detail than *Phys. Rev. Letters* would have allowed. So we did.

Well, this is the second chapter in a sense in that a different man, directly from India, [T.] Narayanan, was in essentially the second round of work in this same field. In this case, we had what was actually a second chapter less exciting than the first chapter, but I was taking no chances about getting this published. It was easy to do it gracefully, because there was a Festschrift number to be published in the *Journal of Physical Chemistry* in honor of C. N. R. Rao of India, a former postdoc of mine, to which I was invited to contribute a paper. I couldn't think of anything more appropriate than this paper with this young man from India, and so we sent it in in the appropriate format for this Festschrift.[1]

But the greatest real interest in this work was in the physics community, and so I wrote up a brief edition for *Phys.*

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[1] *Journal of Physical Chemistry*, 1994, 98, 9170.

Rev. Letters, and they published it not as expeditiously as it seemed like they ought to have, but they did publish it.[1]

For the other work, Narayanan always did a relatively good first draft, not only of this Festschrift number but also a more comprehensive paper including a lot of his later work before he left. I only edited those rather lightly. Then, later, I did an invited review paper just in my own name that included this work, but it also included other things.[2] There was no question who did the draft there.

Hughes: Is facility with English a consideration?

Pitzer: Oh, yes. If they have reasonable facility with English, then it's fine for them to go ahead and do a first draft, and then I can clean up the English very easily. But if it's going to be too awkward, well, then just too much time gets spent. Well, right now, I've got this young man from Russia whose English is better than the Chinese man's. He's actually published in the English language in American journals before he came with me. I've had him do first drafts, and depending on the subject and other aspects, they are sometimes very good and can be just modestly edited, and in other cases, it turns out it's just better to start over. So we're near a rather comprehensive paper, and it's going to be a mix. There will be pieces of it that he did with minor editing on my part, and there are other parts that I wrote separately.

Choosing a Journal

Hughes: On what basis do you choose a journal?

Pitzer: Well, I do that in large measure in terms of readership, and, except for the *Phys. Rev. Letters* possible situation maybe, I consider whether there will be any real question about getting it published promptly without a long wrangle with the referees. So a great many of my papers I send either to the *Journal of Chemical Physics* or the *Journal of Physical Chemistry*, which has similar but not exactly the same readership area, but they're both in all the libraries. They come to the attention of people. There's a

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[1]*Physical Review Letters*, 1994, 73, 3002.

[2]*Journal of Physical Chemistry*, 1995, 99, 13070.

special short-article publication, *Chemical Physics Letters*, that I've used occasionally.

But there are some things that are more a matter of getting detailed information into the permanent record and before a relatively limited audience. There is a *Journal of Solution Chemistry* and a *Journal of Chemical Thermodynamics* that I have used a good deal from that point of view. They are more comfortable in terms of the amount of space you can take, giving full detail and so on, than the wider circulation journals, yet they have a wide enough circulation that the paper is not getting lost there.

I just mentioned the *Journal of Chemical Thermodynamics*. It had for a while an editor that was very particular about all the details being done exactly his way, and so I tended to avoid it. I just didn't want to get into a long argument with him. I could win the argument, but it was a nuisance. [laughs] We were friendly personally. For example, one was an invited talk at an international chemical thermodynamics meeting, which traditionally was dually published, one in an official journal of the International Union of Pure and Applied Chemistry, and in this *Journal of Chemical Thermodynamics*. So it went in both places, and the editor of the *Journal of Chemical Thermodynamics* wanted to rewrite it. I told him, "Look, I'll compromise a little bit, but otherwise, you print it the way I sent it to you, or else you just won't publish it. It will be in this other journal anyway." Although that's more of an archival journal. He took it. [laughing] He's retired now.

Hughes: Was he objecting to the way you presented the science, or was it the English format?

Pitzer: Almost purely details of presentations of units and symbols, in which I had favorite ways that were widely acceptable but not to him. He'd been active on some international committees that make particular recommendations, but had options, also acceptable. I was using the "also acceptable" options, and he didn't like that. But he would agree eventually. But why have an argument when you don't need to?

Determining Order of Authors

Hughes: Right. Well, then what about determining order of authors?

Pitzer: Ah. I take the position that as long as the other person or one of the other persons has really carried the major part of the project, with limited guidance and so on, their name always comes first. In some cases, I tell them, "You publish it all by yourself. You can give me an acknowledgement in whatever words you want." But in most cases, if I've had some substantial contribution to it, why, I put my name on, and then I usually handle the publication with the editor.

Hughes: And what would be your position in terms of the order of authors, or does that vary?

Pitzer: Usually, my name's at the end.

Hughes: Because it's your lab?

Pitzer: Yes. But you can get into alphabetical ordering, and that's useful in some cases, but from a practical point of view, I don't pay much attention to that. That is, the first named author, if there's more than two, is cited as "Jones et al." [laughs] And so if we have, say, three authors, the first one, in my view, ought to be the one that had the most important role, even though he's further down the alphabet than the others.

Now, there are marginal cases where the alphabet is a good decision basis to fall back on, but I don't use it in very many cases. Well, I guess that's enough on that.

Scientific Citation Index

Pitzer: Now, I'm just going to add a few words about the [Scientific] Citation Index. You're familiar with that, I presume.

Hughes: Yes.

Pitzer: I find the Citation Index interesting. It's not a complete indication of the significance of one's work, but it's not without significance on the other hand, either. So I've enjoyed occasionally and again recently taking a look at the Citation Index for my own work in this. Since I frequently put my own name either last or late in the list of authors, it means I have to look up under the coauthors' names in order to check. But my most recent survey, at least for the single most recent year that the index has been fully published for the whole year, 1994, there are about 480 citations, which I think is pretty good.

I was particularly interested in the very first one, which I didn't expect. That's the little paper on the crystal structure determination that I did while still an undergraduate at Caltech on cadmium tetrammino perrhenate, which has cubic symmetry. That's why Linus Pauling suggested that I do it as sort of an exercise in learning how to do a crystal structure on a fairly simple compound that had just become available. Rhenium as an element had only been obtained in significant quantity for general sort of scientific investigation.

Well, here was a citation to that paper. It came from an important laboratory in Strasbourg, Germany, and this paper is in German. I had to sort of brush up my German to read the paper, but it's clearly the same structure for a much commoner substance, zinc tetrammino--that means four ammonias around the zinc--perchlorate. Well, perchlorate is tetrahedral just like perrhenate, and the structure is the same, so it's not of any really great significance, but it was fun.

Hughes: What is your hesitation about citation as an index of scientific achievement?

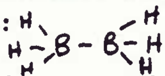
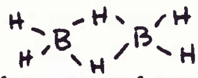
Pitzer: Well, I think any one index is incomplete. And as time goes on, of course, some discoveries become so incorporated in the general knowledge that they don't bother to cite. For example, my much more important work on internal rotation the very next two years, '36 and '37, is cited more often than this crystal structure paper, but not much. But not much more because that knowledge is already incorporated in the literature.

Selected Papers

Pitzer: I thought I might say a little bit about a couple of papers that might well have deserved inclusion in that volume of Selected Papers but were not included. One is that paper with Roger Millikan related to the Miyazawa papers on the carboxylic acid, and I've already commented a little about it.

Another is a paper published in 1948 entitled "Repulsive forces in relation to bond energies, distances, and other properties." I got into it really on the basis of the puzzle as to why the oxygen-oxygen single bond was weaker than a sulfur-sulfur single bond. In general, you'd expect the bonds between smaller atoms to be stronger than the bonds between larger atoms. That trend, although less dramatic, carries over comparing fluorine with chlorine.

I discussed this in terms of the sort of relative mean radii or orbital sizes for the second and third rows in the periodic table, and the fact that there apparently was more repulsion to be overcome in that second row--nitrogen, oxygen, fluorine atoms--as compared to the corresponding larger atoms in the next row of the periodic table. This could be derived now on a much more sophisticated and quantitative basis, but for the purpose of explanation and relatively elementary chemical discussions, apparently it's still useful, and that's indicated by the fact again that it's frequently cited.

A third paper of 1945[1] might also have been included in the Selected Papers volume. In it I joined one side, the correct side, of a debate about the structure of diborane, B_2H_6. Under Linus Pauling's leadership, Simon Bauer had made electron diffraction measurements and reported the structure to be the same as that of ethane:  But B_2H_6 has two fewer electrons and there are not enough for seven electron-pair bonds. Others had proposed the structure  with just six electron-pair bonds, i.e., each B-H-B bond has only one pair. I joined the advocacy of the second structure with B-H-B bonds for B_2H_6 and went on beyond to propose structures for the larger boron hydrides, B_5H_9, B_5H_{11}, B_6H_{10}, and B_6H_{12}. The debate was soon over and our structure has been accepted so long that this paper is seldom cited.

Well, that's the end of my list here.

Hughes: Good additions.

Pitzer: Good.

[1]Electron Deficient Molecules. I. Principles of Hydrocarbon Structures. Kenneth S. Pitzer, Journal of the American Chemical Society, 1945, 67, 1126-1132.

XII THE RESEARCH PROCESS

[Interview 9: July 23, 1996] ##

Physical Chemistry

Definition

Hughes: Dr. Pitzer, I thought we should start out today with your definition of physical chemistry.

Pitzer: Well, in brief, I think the best description of physical chemistry is it's physics applied to problems of chemical interest by someone who understands the chemistry as well as the physics. Physicists tend to have their interest decrease after they've solved the general principles of a problem and demonstrated their understanding in the completeness of the theory by a few examples. In other words, physicists tend to emphasize the similarity of different substances with respect to the physics involved.

Chemists are intrinsically interested in the differences between substances even if of similar general character. They regard it of interest to consider all the examples of a given type, all the elements in the periodic table and their various combinations, although beyond a certain point, that becomes less interesting unless there's something of practical importance. But in general, chemists are interested in additional examples looked at from the point of view of differences at least as much as similarities.

All chemists are going to use a certain amount of physics. Physical chemists have studied physics more deeply, feel more comfortable even to add to the physics of the problem if need be, rather than just use the well established physics as taught at elementary and intermediate level.

Hughes: Well, Atkins, whose text you lent me, characterizes physical chemistry as having three main approaches: thermodynamics, spectroscopy, and the analysis of rates and mechanisms of chemical change.[1] Would you agree?

Pitzer: Yes, that's reasonable.

Hughes: Is that sufficient to define physical chemistry?

Pitzer: Oh, no. To cover the full array of what physical chemists do, you probably find some things that could only be very loosely attached to one of those topics, and not what one initially thinks of. But those three topics cover a lot of the central parts of physical chemistry. The chemical equilibria are connected to thermodynamics, and then statistical mechanics gives a more detailed approach for these same thermodynamic chemical equilibrium properties, if you have enough detailed knowledge of the molecules or otherwise of the system involved. Statistical mechanics can also treat the rate processes. Then spectroscopy is important in giving detailed atomic molecular or crystal structure information in order to, shall we say, make statistical mechanics applicable.

Pitzer's Approaches

Hughes: You, I believe, have used all three approaches.

Pitzer: Yes. Most of my work has been on the statistical mechanics-thermodynamics side dealing with equilibrium properties, but not entirely. In fact, there were rather important contributions back in the mid- to late fifties with Harold Johnston, who was still at Stanford at that time but later was with our faculty through retirement and still here as a retiree.[2] We dealt with a number of systems with respect to the rate processes in order to make the statistical rate theory applicable effectively and correctly on more complex systems than it had been applied to earlier.

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[1] P.W. Atkins. *Physical Chemistry.* San Francisco: W.H. Freeman & Co., 2nd edition, 1994.

[2] H. S. Johnston and K. S. Pitzer, Rate constants and molecular structure. *A.I.Ch.E. Journal* 1959, 5:277; also, an earlier paper in 1956.

But by and large, I've been concerned more with equilibrium properties. And from time to time, with the spectroscopy, to get information that is pertinent to it.

Origin of Pitzer's Scientific Ideas

Hughes: Where do your scientific ideas come from?

Pitzer: [laughs] Well, you first notice in your reading or in listening to lectures or seminar talks something that is puzzling. It's either admitted and stated to be a puzzle, not understood, or else you're impressed by the idea that the explanation that was offered by the speaker or the writer doesn't sound very sensible and is probably wrong.

Now, you notice lots of these things, and the question is, which ones are significant enough to be worth one's time, but more prominently, the question is, does one oneself have any idea as to how to solve the problem? From that point on, I suppose you examine how brain cells work. [laughs]. But with experience, you find that you have been successful in a number of situations of this type, so you go on trying some more.

Occasionally, someone knowing of the areas in which you have made successful contributions may bring to your attention a problem and ask you if you wouldn't be interested in working on it, but that's rather rare. It's usually things that you notice just from having a broad range of interest and curiosity, and one thinks oneself of the idea that that's an interesting possible research problem.

Now, as I said, chemists are interested in more than one example, and once one has discovered or developed a new approach to a particular type of problem, then it's a natural series of steps thereafter to apply that approach to a variety of substances that are of importance and of general interest to the chemistry or applied chemistry communities.

Hughes: I noticed you've done that time and time again.

Pitzer: Yes. And then after a certain point, you've dealt with what seemed to be the most important examples, and others that were conveniently available, and then you assume that the rest of the world can carry on further. But it depends. In practical work in dealing with students or other young scientists, visitors and so on, they want a problem that is tractable for solution in a reasonable length of time. In fact, I try to keep a list of a few problems of this type that I can suggest to them that will be significant additions to knowledge and presumably soluble by established methods. But you always keep your eyes open, because sometimes the established methods don't work without some further elaboration or improvement.

Hughes: So you're always looking for anomalies.

Pitzer: You keep your eyes open for anomalies, yes. They become more interesting than the ones that just work out in the normal course of events.

Thinking Through a Scientific Problem

Hughes: Do you have a standard way in which you think through a problem?

Pitzer: Oh, to some extent, I suppose so. I don't think there's very much point in trying to formulate that. Clearly, you refresh your memory, your mind, about any physical principles and any relevant factual data so that it will be fresh in your mind or on a relatively limited number of notes at the top of the pile, and then go on from there. But that's just common sense, an orderly way of doing things.

A Quantitative Approach

Hughes: Are you aware of using certain types of mental language when you're thinking scientifically? For example, a visual approach or a mathematical or many other ways of thinking about anything.

Pitzer: Well, usually anything I'm involved with is quantitative in the sense that one needs to be considering the actual numerical values and not just a qualitative yes-no, this-bigger-than-that type of thing. And that means that one needs to have numbers, and they frequently need to be on a sheet of paper or on a graph. There's no point to memorizing a whole long string of numbers; you'd get them wrong anyway probably.

I suppose nowadays, one could organize that sort of thing to be displayed on your computer screen very satisfactorily. I'm old-fashioned in the sense that I still am more comfortable with hard copy in terms of tables of numbers or graphs, but I certainly do use the computer.

Hughes: So you think mainly quantitatively?

Pitzer: Yes.

Examples: Ring Molecules and Spectroscopy

Hughes: When you were talking about ring molecules and the substituent groups and the chair formation, et cetera, that to me was very visual.

Pitzer: Well, let's go back to that. The cyclohexane ring, for example, on the basis of the bond angle strain, you remember, Baeyer's strain, would have either the so-called chair form or boat form at equilibrium. Now, the Baeyer strain itself is, however, a numerical thing. It's saying that the carbon-carbon bond angle around the six-membered cyclohexane ring wants to be roughly the tetrahedral angle, 109 or 110 degrees, not 120 degrees.

Now, in benzene, where it's C_6H_6 and not C_6H_{12}, the equilibrium angle is 120 degrees, and so it wants to be flat. In other words, here are qualitative differences, but they're based on numerical differences as to what the equilibrium bond angle is.

The contribution of the internal rotational potential or strain indicated that the boat form would be higher in energy than the chair form by having two of those internal rotational angles at the top of the potential instead of the bottom, so that it's a qualitative difference between boat and chair forms, but it's based on a quantitative potential. In the free rotation model, with Baeyer strain but free rotation, the boat and chair forms were the same energy. So from one point of view, you're getting a qualitative conclusion out of it, but it's only because you put numbers in, at least numbers of about the right magnitude.

Now, if you change that potential barrier by 10 percent, that doesn't change the qualitative conclusions. But in my own approach, not necessarily on the first round, but eventually I would treat those molecular problems more quantitatively so that the actual numerical value was important in the end.

Hughes: So the quantitative aspect comes first, and everything flows from that?

Pitzer: Well, no, I think it sort of oscillates back and forth. Well, it depends on what you learn first too. In that case, the thing that was new to the scientific community, that I contributed to the scientific community, was this internal rotation potential. Then others were thinking, and I was thinking, how does this impact other things that are known but not known in full detail or not fully understood? And your first thoughts about some of those things may well be pictorial or qualitative, but it's with at least the approximate quantitative quantity in the back of your mind as being applicable to it.

Hughes: You seem to be talking as though there's a give and take between the quantitative and the qualitative. And that's it?

Pitzer: Yes, well, the same sort of thing comes up in spectroscopy. The precise frequency or wavelength may be important, or for some purposes, it may be just a matter of identifying a vibration or some other motion as being the one you're interested in, and whether it's at 1,005 cm^{-1} or 1,006 doesn't really matter; it's just that that's close enough to prove it's the motion that you had in mind. Then the question might be, what's the intensity of that feature, or how does that feature vary as you make some substitution in the molecule?

Changing Initial Approaches

Hughes: Well, I'd like to quote Kenneth Pitzer. [laughter] A good source, you must admit. This comes from your introduction to *Molecular Structure*. "When the [research] territory is just being opened up, initial approaches are only partially successful and must be abandoned or altered, but a clear map eventually unfolds."[1] Would you like to expand or give examples of that statement?

Pitzer: Well, I certainly am happy to be associated with the statement. I think that's certainly true. I suppose one could pick various areas in which that sort of thing happened.

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[1] *Selected Papers*, p.vii.

Examples: Aqueous Electrolyte Equations

Pitzer: One series of examples might be on the aqueous electrolyte equations. The formulation of a new equation was in one paper, with a relatively modest number of examples. Then with a co-author, we covered the conveniently available literature that conveniently fitted the equations as initially formulated. [laughter] That was paper two. But there were some that didn't fit.

Hughes: What did you do with them?

Pitzer: They're the subject of paper three. I presume we acknowledged in paper two that the 2-2 electrolytes and in general electrolytes with both ions, positive and negative ions with multiple charges, didn't fit the equations with simple numerical parameters as presented. Now, even when I wrote that, I may have had some ideas as to what the solution of it might be. But we went on and were able to describe what was happening, which was formation of ion-pair molecules in the case of the 2-2 electrolyte; they would be neutral. But they never became more than, say, a moderate percentage of the total, possibly a third or something like that.

And then the oppositely charged species more generally came close enough to one another that trying to identify a clearcut ion-pair molecule became more or less meaningless, and I devised a way of expressing this behavior that proved to be quite efficient and satisfactory. Well, that covered the main map, shall we say, of pure electrolyte solutions as just one electrolyte plus water or some other solvent.

And we went on to mixtures. Well, I'm sure I had mixtures in mind right from the beginning, and in fact probably did something about them in the first paper, but with a different co-author now we again went through the convenient literature. It was already reasonably well organized. And again, we found some cases that didn't fit and were able to define what was the characteristic that seemed to be critical in whether they fitted or not. The question was one of the relative charges of ions of the same sign that were getting mixed up. If they were the same magnitude of charge, in other words, a +1 with a different +1 ion, or a +2 ion with a different +2 ion, it was no problem.

But in the extreme case when it was a +1 ion with a +3 ion, something more complicated was happening, so we simply passed that one by at that point. And again, I came back to that later. By that time, I had found in the much more abstract statistical mechanics literature a theory which yielded a term for that specific phenomenon of mixing of ions of the same sign but of grossly different charge.

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Pitzer: And that, when added to the previous semiempirical equations, fitted the 3-1 charge mixing. The term in principle was present for 2-1 charge mixing, but it was so much smaller that it was down in the realm of an experimental uncertainty in most cases, and still is. You can find in the literature even recently people that include that term for 2-1 mixing, or ones that don't, or ones that try it both ways and don't see much difference. But for 3-1 mixing, it's agreed you have to put the term in.

There are two or three different convenient calculational ways in the literature. I think the one that we proposed initially is about as good as any, but there are others that other people favor. But they all get the same results, so it doesn't matter.

Hughes: When you pass something by, as you expressed it, do you acknowledge in your write-up that you are doing so and may return to it at some point?

Pitzer: I think so, but I'd have to check those papers [laughs] and see how much was said about that. I think we did say that. Well, we certainly had to say it in the sense that that category was not being included in the present paper. Now, to what extent I would indicate that it was not included because the present methods didn't work, as compared to that we just hadn't gotten around to doing it--I think I would have said something along the line of the former, that there were difficulties there that needed further examination, or something like that. And in some cases, I might even have said that the cause of the difficulty is undoubtedly along this line, but some additional work is needed as to just how to deal with it, or how to put that identified phenomenon into the mathematical equations in a convenient, practical manner.

Hughes: Could you have been confronted by your critics if you had not made some allusion to the omissions?

Pitzer: Oh, I suppose so. I certainly avoided making any claim of having completely covered a field when I hadn't completely covered a field. In other words, I certainly would have acknowledged that there was an incompleteness, and I'm almost certain that it was acknowledged that it was not just having run out of time; it was that there were further problems that hadn't yet been solved.

Now, occasionally, you just say that this other area is one that a different co-author has been working on and will publish a follow-along paper in the near future, or something like that. But it's not usually quite that way.

Creativity

Hughes: In what areas do you consider that you have been especially creative?

Pitzer: Well, I suppose that's pretty well indicated by things that were selected for the *Selected Papers* volume that World Scientific published. In that volume, I included some things that were additional examples with no appreciable originality or new introduction of new concepts or anything. But for the most part, although we published a good many papers of that sort, where we were just using existing methods to go on to another problem which we thought was of interest to the practical world but had no real new ideas to it, in general, I didn't put that sort of paper in that volume.

Starting right from the beginning on the internal rotation business, there are several additional papers in which an internal rotational barrier as well as other information was either measured, or maybe thermodynamics properties were measured to be fitted by these methods. But if it was an the additional example of something that was already pretty clear in an earlier paper, I didn't put it in that volume.

Now, the acentric factor business represented an approach to something that mainly the chemical engineering community had been dealing with in a less satisfactory way for at least a decade or so. But they'd been correlating with respect to the compressibility factor at the critical point, which in principle was an equally good way of doing it, but in a practical way was unsatisfactory because it depended on the measurement of the critical volume or critical density which was very difficult to get accurately and unambiguously.

My contribution of making the third parameter the vapor pressure well away from the critical point was just using an easily measured, in fact already measured in most cases and in the literature, piece of information, instead of one that was very difficult to measure accurately and therefore was just a source of ambiguity or in fact, if you wish, error in terms of actual application.

In a number of cases, I think my contribution was a combination of not necessarily what's more accurate theoretically, but what was a better combination of valid, accurate theory and what is conveniently measurable or has already been widely measured. And in some other cases, of course, and this is where a lot of advances in science come about, a new measurement becomes possible in terms of instrumentation and so on, that although existing in principle is never practically available. That's where a lot of advance come, but I can't really claim that for myself in very many cases, other than just taking it up after somebody else developed the instrumentation.

Experimental Research

Importance

Hughes: In the course of your career, how important has it been to spend time at the lab bench?

Pitzer: Well, it's very important to have done some early on.

Hughes: Why so?

Pitzer: If you have never actually done experiments with your own hands, I don't think you really understand fully enough what the problems are to be an optimum guide for your students or other associates. However, I don't say it's impossible, but I think the easiest way to learn anything like that is not only to have instruction as to the theory, but you actually do it. I can't imagine anybody being a good driving instructor who'd never driven a car himself, even though he had the best of instruction from somebody else. [laughs]

And this is certainly a matter of personal relations, too; that is, insofar as you're trying to guide somebody in making some measurements, the way you talk about it and so on, and the way they react to it, will depend upon whether it's plausible in their minds that you ever did it or would be capable of doing it if you wanted to.

The way you read the literature about experimental work will be somewhat affected by whether you have done experiments at least somewhat similar to those. It's not that you can't get along without it, and there are lots of purely theoretical physical chemists nowadays that do very valuable things without themselves ever having done any experiments remotely resembling the type that they're dealing with the results of. But I still think it's probably a net advantage to have done some experiments, and in my generation, purely theoretical physical chemists were practically unknown. Almost everybody, even those whose primary contributions were theoretical, had, at least early in their career, done some really publishable experiments, not just exercises in the course of instruction.

Hughes: Is the electronic computer an impetus?

Pitzer: Oh, it's changed the world enormously. It makes calculations feasible that just were completely impractical before.

Hughes: But does it also move work towards the strictly theoretical?

Pitzer: Yes. In fact, I might have pointed that out. The theoretical chemist would probably be regarded by some types of theoretical physicists as computational chemists. In other words, their skill is in not advancing the most fundamental and abstract theory; it's rather in making computations of great complexity with respect to the real chemically interesting systems, which are frequently more complex than the theoretical physicist would have ever been interested in anyway. No, as I think of my theoretical colleagues both here and elsewhere, and including my own son, they will add to the abstract theory some now and then--at least some of them will. But in lieu of experiments, what they do is complex calculations that would never have been attempted without the electronic computer.

Balancing Research and Administration

Hughes: Can you designate when you ceased to do experimental work on a fairly regular basis?

Pitzer: Well, it tapered off gradually. That is, before I was distracted by World War II, before this country was involved in World War II, I was doing a considerable number of experiments personally as well as having a relatively small number of students who were doing them under my guidance. After World War II--in other words, the second half of the forties, roughly--I had a large group of very able students, and I suspect I did very few experiments with my own hands. But I probably did a few.

Then when I was back after the period as director of research for the AEC, I was dean of the college and heavily involved in administrative things, as well as having a fair number of students and a few postdocs. I doubt if I did any experiments then. Subsequently, I've actually done experiments only if they were probably fairly simple [laughs] and where I wanted the answer in a hurry and didn't want to wait for somebody else to do it, rather than because I undertook any fairly complex experimentation. Oh, I would do an experiment in the sense of testing out a new piece of apparatus or something like that, just see how it works. But my research program has had a major experimental component throughout.

Delegating Research Problems

Pitzer: The question more often in recent years has been how elaborate a computation I would do myself, as compared to enlisting a graduate student or postdoc to program up the electronic computer and check it out for errors and so on, and then make an elaborate computation. There's a recent example of that, a paper that was submitted for publication either early this year or the end of last year, in which I actually did the numerical computations in a problem that ordinarily I would have handed out to somebody else.[1] But my new postdoc from Russia seemed not particularly interested in taking up this particular problem, so I got him to write a couple of simple little programs for me and put them in the computer, and then I went ahead and did the calculations myself.

He was doing more complex calculations and is doing them quite successfully. I was a little surprised that he didn't take this up as an opportunity, but it was more foreign to his past experience than his major problem, and he didn't want to be distracted from it, apparently. So I guess it was all satisfactory for all people concerned.

Hughes: You usually rest happy when a student decides that a certain problem is not something that he wishes to tackle?

Pitzer: Well, if he's working on something that's at least as interesting to me, [laughter] and he's more enthusiastic about it, why, I don't have any particular difficulty.

Hughes: So it's not sufficient that the problem is interesting to him?

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[1] K.S. Pitzer. Sodium chloride vapor at very high temperatures; linear polymers are important. *Journal of Chemical Physics*, 1996, 104, 6724.

Example: The Critical Temperature of Sodium Chloride

Pitzer: Well, in a sense, this was a case that was really untangling a situation that had led me plausibly, but in retrospect erroneously, to make of certain predictions. Specifically, the question is, What's the critical temperature of pure sodium chloride? It's at least 3,000 Kelvin. I used a published theory, a numerical theory, an experimentally based but theoretically formulated expression for the sodium chloride vapor, and it suggested and indicated that the critical temperature ought to be up near 4,000 Kelvin. And yet, as time went on, there was more and more indication that that was too high.

It occurred to me what the explanation was: the dimeric molecule of two sodiums, two chlorides, which had been formulated previously only as a ring, could also exist as an open-chain molecule, and that at very high temperatures, the open chain would become more abundant, because it had a higher entropy but also a higher energy.

Well, this little calculation that I was referring to was just putting real numbers, plausible numbers, into that open-chain sodium chloride molecule, using some methods, actually, that I had used back in the late thirties on open-chain hydrocarbons. It seems to have resolved the puzzle now, and as far as I know, the world has all agreed that the critical temperature is about 3,100, a little above 3,000. Maybe somebody can even do an experiment at that high temperature, but they haven't yet.

But that last little problem that I was talking about, my getting into it involved not only having a puzzle to get resolved, but it was useful to feel at home with, let's put it that way, the German literature in the 1920s and thirties where some important measurements were made that were relevant to this problem. Although not obviously relevant to it, they turned out to be relevant to it. Nowadays, the Germans publish all their work in English, and therefore, the rest of the world doesn't bother to learn German.

But for this particular problem, it was important to read German accurately. [laughs] I have to refresh my capacity there, but at least I can do it. It helped to know that there were actually two German investigators who were important in that field, that they both had an excellent reputation, that they were virtually never wrong. Therefore, I was comfortable basing my new paper on their measurements. I was confident that they'd measured it accurately enough that I could draw the conclusions that I was able to draw.

Judging Accuracy of Others' Research

Hughes: Now, how do you determine the accuracy of previous work?

Pitzer: Well, the authors generally give some estimate of accuracy. But what I was alluding to in the previous remarks, is that one frequently learns, if one is widely enough informed, whether a given investigator's results in the end turn out to be about as accurate as they thought they were, or whether they frequently turn out to be seriously in error. In that case, it's a mistake to build any new contribution based on the assumption that their work was accurate. And that's really what I was alluding to, and it was helpful to me to know that these two investigators had a reputation of being accurate. Whereas I was aware of some other people where this was more doubtful.

Hughes: Is there a common thread running through your research?

Pitzer: Well, I don't have any suggestion of an additional common thread beyond those that we've already talked about. I don't have some snappy word that would be particularly pertinent there, I don't think.

The Art of Approximation

Hughes: Would you like to expand upon what you called in the Ridgway interview "the art of approximation"?[1]

Pitzer: Well, I think one or two talks have been given under that title, as well as in that interview. In a sense, that's the essence of what you might call computational physical chemistry, in going beyond what the physicists do in opening up a field. In developing a theory or a field, they usually treat the simplest examples. Chemists are interested in more complex examples with additional numbers of atoms or more complex structures in one way or another. And the question is, Can you apply the theory to this more complex situation in a--

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[1] p.11.

Pitzer: --calculation that is comparable to experiment in terms of accuracy or in general is accurate enough to be useful for future work and so on.

Well, now, by approximation there, I don't mean just numerical approximation; I mean formulating the theory maybe in alternate ways of expanding it in terms of a series or something like that. How many terms of the series do you have to include, or if you can think of some alternate and possibly closed form rather than a series that's still computationally feasible, that may be a better way of doing it. In other words, over time, you develop experience in what I call this art of approximation of devising some satisfactory way of making use of the theory with respect to that particular phenomenon.

Example: Aqueous Electrolyte Equations

Pitzer: This is basically what I did on the aqueous electrolyte business. The initial theory was a major advance when it occurred. It was that of Debye and Hückel in 1922, '23, something like that. But that theory was essentially the first term in a series, the first term related to the correlation of location of the positive and negative ions at relatively long distances from one another. At long distances from one another, only the charge mattered. No shorter-range forces had anything to do with it.

So from there on, what theory needed to contribute was some way of dealing with the shorter-range forces between the ions in solution. They are not obviously known in detail. They have to do with the water molecules interacting with the ions, assuming there's water in between the ions. Debye and Hückel assumed the ions couldn't get closer together than some repulsive distance. It was not accurately known, but that led to the inclusion of a second approximate term in addition to the exact term.

The tradition in aqueous electrolyte thermodynamics for years and years, then, was just to add an expansion in powers of the concentration--molality, whichever concentration measure you use. There was some argument as to whether you used just integral powers or half-powers or possibly even quarter-powers, although I don't think anybody took the last seriously. For the most part, they just used integral powers.

Well, my contribution really in 1973 was to show that that very first expansion term in the first power was not a very good approximation, and that it was feasible to improve that approximation with a modification there. Instead of having a power series that went on maybe to six terms, with alternative coefficients positive and negative so that one was canceling most of the other one out, that one additional term, in other words, the modified first term, which now had two parameters in it, plus one additional term, which was usually very small, frequently negligible, would fit the whole range of known information in many cases.

This was a case where, while there was a conceptual theoretical basis for this modification, the implementation of it was just in a clever choice of mathematical form that was convenient to use and yet worked, fitted. I could probably point out the same sort of thing in a variety of other cases, but one example is good.

Research Funding

Hughes: Is there anything you'd like to say about funding, both in terms of obtaining it, and also how it may possibly have shaped the sort of research that you did?

Pitzer: Yes, that's a good subject. Well, in the beginning, there was virtually no external funding for modest-scale work, and there was no necessity. Students were also either part-time teaching assistants or they had some fellowship. There were some sort of prize fellowships available. It was only post-World War II that the idea of external funding became relatively widespread, even for fairly modest levels of expenditure such as I was involved with. One then had to spend a certain amount of time making applications and maintaining good relations with the source of funding.

I had some federal government funding and also some from the American Petroleum Institute. This is an organization of the major oil companies, petroleum companies. They had a very clear idea of what areas of research in which they were going to be competitive, and those in which they were going to be cooperative. If you were a catalyst chemist, that was a competitive area and you couldn't get any money from them for open university research. You could be a consultant under confidentiality restrictions and help them in their own laboratories.

But things like thermodynamic properties in a very broad sense, or spectroscopic measurements that would help identify or analyze, those were areas in which the petroleum industry decided they would cooperate. They would share information one with another, and they would support work in universities in that regard. I had support from them for many years there, not terribly large, but large enough to maintain a moderate program fairly comfortably. Of course, the whole acentric factor work was within that framework.

Hughes: Just because that was where your interests were?

Pitzer: That's right. In other words, if I had had some idea in the competitive area, I wouldn't necessarily have stayed away from it. I just wouldn't have asked the petroleum industry for money for it, knowing their point of view.

For my project, we had a small, I think it was a four-member advisory committee that would meet maybe twice a year, and the chairman of this at least would also go to the general API [American Petroleum Industry] meetings. In general, there was a chain of human relationships, and the number of people that got API support wasn't terribly large, but there were a considerable number.

Now, for other things, I had funds from the Office of Naval Research for a while, and then in the years at Rice--oh, even before I went to Rice, I had some Atomic Energy Commission support, which has carried on through essentially ever since. It's become the Department of Energy now, but that's been a continuous trail, essentially. And in the years at Rice, the Welch Foundation, which is limited to Texas in making research grants, but does so almost automatically up to a fairly modest level, for commendable work within Texas.

I've never had to spend very much time on the money-raising side, in part I think because in the later years, anyway, I wasn't interested in trying to get a very large research group. I wanted enough people to really go ahead and implement some of my ideas, but I wasn't trying to assemble a large group, as some people have done, although usually earlier in their careers. In the middle of my own career, I was heavily involved in university administration a good deal of the time, and I didn't pretend to want a large research group; a modest size was all I was seeking.

So fund raising is an important part of the picture. It can take a good deal of time on the part of the research scientist who is a lead investigator. But it can also be handled with a relatively modest amount of time, if done skillfully and with good human relations, too.

Chemistry Consultants in Industry

Hughes: Have you ever been a consultant?

Pitzer: Yes, but only for limited periods. For a few years I was a consultant for the U.S. Rubber Company. Later, I was a sort of general corporate consultant for Union Carbide for a while, and then I was on the board of directors of Owens-Illinois. But in the second and third cases, I was not a consultant on any particular research or development field. In either of the latter cases, I was advising them, or even going beyond in the case of a member of the board of directors. But primarily then it was also advising on whether their research and development program was healthy, whether it was being well-guided, whether it was wasting a lot of time and money on things that the corporation wouldn't know what to do with even if they made some development.

Well, this was so far removed from my own research that there was no sensitivity or embarrassment at all. That is, if I was telling Union Carbide that their laboratory outside of Cleveland was wasting money on some particular project, this had essentially nothing to do with what my own students or postdocs were doing. There was no need to tell them about it.

Hughes: Did you deliberately choose it that way, or did it work out that way?

Pitzer: In a sense, both. At least in my relationships with the petroleum industry, I had chosen on the basis of scientific interest and so forth to work in what happened to be their cooperative area. So it just naturally worked out that money was available there without any confidentiality problem. So it becomes just a "what-if" question without a real example. No, I've never spent any appreciable amount of scientific work on anything that wasn't going to be publishable openly at the end, which means it must have been in this cooperative framework, if it had industrial association support. And what I did during World War II, or possibly was aware of during the Atomic Energy Commission period or in these three corporate affiliations, was essentially separate from anything I was doing with my own research group.

Of course, there are other fields where this would not be the case, as I was pointing out.

Hughes: Did the College of Chemistry have any rules and regulations about consultations, above and beyond what the university imposed?

Pitzer: I don't remember with great clarity about that. Lewis discouraged anything of this sort, but the opportunities were relatively limited anyway.

Hughes: You mean then?

Pitzer: Then. In the post-Lewis period, either under Latimer and Hildebrand or in my own deanship, I don't recall any real difficulty. Some people were getting into some consulting relationships of various types, and there were discussions about what was a reasonable amount of time to spend, and how to handle things with your own research group that was not privy--in other words, essentially don't get your own graduate students involved in doing work that they can't talk about because of your consulting relationships.

For the most part, people that I recall on the faculty in those days might be a consultant to DuPont or someplace like that way across the country. What they would do is spend a few days with DuPont maybe in connection with a trip to an American Chemical Society meeting that also was taking them across the country. Well, if they were extending this unreasonably, they would have had to take a leave of absence. But as long as the trip was partly to a scientific meeting and was only extended by a very few days for a consulting relationship, we usually just overlooked it, and somebody substituted for them for teaching during that period.

As the chemical engineering side of the college developed, it was clear that there would naturally be more sensitivities there. As far as I was concerned, that was one of the reasons for establishing the Department of Chemical Engineering separate from chemistry, because in the long run, they were almost certainly going to have to have more specific understandings as to what sort of consulting relationships were appropriate, or when it could grow on beyond a consulting relationship.

Suppose the faculty member is the owner or partner or partial owner of the separate business, and he wants to spend two days a week with that business which is nearby; in other words, he can go there instead of coming to the campus.

And that sort of situation has developed, and it can then get to the point where you have to have a decision by the faculty member. We'll tolerate this for two or three years, but then you've got to decide which is your primary career. Either scale down your affiliation off-campus to a one-day-a-week-maximum consultantship, or else go the other direction primarily, and maybe we'll invite you to give a series of lectures on the campus occasionally or even regularly. In other words, come on campus and give two lectures a week for one semester. And we've had that relationship with people that are primarily in industry and frequently never had regular faculty appointments.

But that's much less common in chemistry, although it can happen there. Usually, it can be worked out with goodwill and common sense, but sometimes one has to have firm regulations and enforce them.

Pitzer's Choice of Scientific Directions

Hughes: Were there periods in your career when you were conscious of having a choice of directions to pursue?

Pitzer: Well, I suppose that times when this was a particular decision were when, after some period of essentially full-time administrative commitment, I'd come back into the laboratory. This would have been three or four times. There's after World War II; there's after the AEC period; there's after the university presidency period. The most recent one is probably the easiest to discuss, although it's over twenty years ago now.

What I did then in the early seventies was to start by taking up problems that I was aware of that I either hadn't done anything about before or that I had done something concerning but where there was obviously some more to be done. Now, the spin species conversion activity had been started at Rice, and we'd gotten certain basic aspects of it or general aspects of it outlined, but it was obvious there was more to be done, so I took that up.

The electrolyte solution business was one where the unsatisfactoriness of the established way of doing it I'd been familiar with, among other things from the second edition of the thermodynamics book. Therefore, it was on my background agenda for giving it another thought, another exploration, if and when the opportunity arose. That was one that I didn't get to immediately when I came back in late '71. It was a couple of years before I really did anything with that.

There were one or two other things that I took up right away where there was some background or I'd had essential ideas that I might have done something with earlier, except I'd been occupied with other things.

Now, I might, I suppose, have decided to try something more grossly different, in the early seventies, but I don't think I ever seriously thought about it. As long as I had some ideas that I wanted to work on, why not go ahead and do it, rather than think about the possibility of something really more generally different?

Hughes: Chemistry in general remains a small science, particularly compared to what's been happening in physics and biology. Do you have an explanation?

Pitzer: Well, small in the sense that any one project or effort involves only relatively few people. The total number of chemists is larger than the total number of physicists.

Hughes: Yes.

Pitzer: And of course, biology is a more comprehensive subject, and if you subdivide biology, why, the numbers at least come down to the level of chemistry or less.

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Pitzer: Well, in the first place, chemists are involved in extending to additional substances methods which for the most part have already been developed for at least a few examples in physics. Now, we're talking about physical chemistry primarily here, but it's true of chemistry generally. Very large fundamental particle physics projects are involved with properties deeper in the nucleus and involving those high-energy particles that get spun off when you blow up a nucleus, or maybe come out of the sun, cosmic rays that come from somewhere in outer space. Those projects don't generally have any extension into different chemical substances, and therefore there's no particular occasion to use that sort of technology.

Motivation in Science

Hughes: What motivates you to keep doing science?

Pitzer: I enjoy it.

Hughes: Why? What aspects give pleasure?

Pitzer: Well, it's like solving crossword puzzles, except that when you solve a scientific problem, why, you write a paper about it and talk about it at meetings and have your friends discuss it and so on. As far as I'm concerned, it's purely a matter that I find it pleasurable, both doing the science personally and having the associations with particularly the postdoc level, people about the age of my grandchildren [laughs]. I like to see my grandchildren, and here are some extra grandchildren to be associated with and enjoy constructive relationships.

Some people my age, good friends, colleagues, take up some really different activity on retirement and seem to enjoy that a lot. But that's pretty rare. I've known a few that start something of this sort and then it doesn't amount to much. Of course, at this age, there's no obligation there. In addition to Social Security and your university retirement fund, you're under no obligation to do anything. You can go play golf all the time if you want to. But I find it just more fun, more pleasure for me, to be in contact and to have my even though relatively modest part in continuing to solve some scientific puzzles.

Honors

Hughes: Well, you've received the National Medal of Science [1975]. Was that given for a body of research, or for a particular piece of it?

Pitzer: The National Medal of Science is essentially for the body of research. The various [honors] that I have received--the [Robert A.] Welch Foundation Award [in Chemistry, 1984] and the American Chemical Society Award [in Pure Chemistry, 1943], the Priestley [Medal, 1969] are all more or less for scientific careers, or at least a major portion of a career. They're not focused on any particular discovery. There are some others of that category but lesser stature. There are some that are more specific as to field.

The American Chemical Society Award in Pure Chemistry, which in my case goes way back in time, had an age limit on it. Of course, at that age, probably what you contribute was in some fairly narrow field, but it didn't pretend to be a particular single discovery. In fact, interestingly, they raised the age after a few years. [laughs] I could brag about having gotten it at a younger age than a great many of the subsequent recipients.

Hughes: Why did they raise the age limit?

Pitzer: Well, [laughs] anything I say now should be verified in the facts, because I don't have the facts in mind. I'm quite sure I'm correct that they did raise the age limit, but not as to when it was done or why. A plausible explanation would be that after World War II, the people that would have been eligible under the old age limit, so much of their career had been taken up by war-related activities that they were essentially foreclosed from ever getting it. See, I got it in '43 but it was based on work through '39 or '40 before we were in the war.

Many of the straight scientific awards may have had some focus or another. The Gilbert [Newton] Lewis Medal [1965], which hasn't been maintained--it had only a few years of existence--was specifically for theoretical work. The Clayton Prize [1958] from the Institution of Mechanical Engineers [London] concerned something of interest to mechanical engineers, namely, the acentric factor work. They wouldn't have given it to me for any of the other activities. I guess that's enough on that.

Most Significant Scientific Contributions

Hughes: What do you consider to be your most significant scientific contribution?

Pitzer: Well, to say that one is more significant than another is a little hard. The internal rotation work way back at the beginning of my career had an enormous impact, but of course, other people were involved in the later stages of the impact. And if I hadn't done it, somebody else would have pretty soon anyway.

At the other extreme, the aqueous electrolyte equation work, as we've noticed, is recognized simply by my name in the title of a very large number of papers [by others], even though the conceptual basis--it was real, but it was fairly limited. Most of the work that people are giving me credit for having a part of might well have been done anyway, but more clumsily, or somebody else again might have come along and done the same thing that I did. There was an opportunity that was sitting around for twenty or thirty years, and somebody else could have done it any time in that period and didn't. Of course, I could have too and didn't, until later. One can have various speculations here.

XIII FAMILY

Jean Mosher Pitzer

Hughes: Well, you've mentioned your son on and off, but you haven't said much about the rest of your family. Do you care to say anything about your wife and two other children?

Pitzer: Well, yes, although it doesn't get into science particularly.

Hughes: No, but it's part of your life.

Pitzer: I have been very fortunate in picking out a very attractive girl in high school. We went to college separately but kept in contact. After graduation, we married and have been very happy. She was not, shall we say, ambitious in trying to have a substantial second career. She was happy to devote herself primarily toward raising three children, getting them well started in the world, giving me the backing and support and so on.

After the children were all essentially off on their own, she developed a significant interest in archaeology with respect to Indian artifacts in this general region, and did some significant research on that, has several publications in lithic technology, and went to some meetings. She still has boxes of things to sort out and maybe will do some more work on it, but apparently it's down on her priority list fairly low.

Part of this has some connection with the place that we explored and then finally bought property on the shore of Clear Lake, which is 100 miles north here. The beach had some artifacts on it, where the Indians apparently had made arrowheads and other tools. That Clear Lake place has been a part of our family and very happily so for many years, and still is.

Ann Pitzer

Pitzer: Our oldest, Ann, is a very capable, strong person. After her B.S. at Davis and an M.S. in home economics at Berkeley, her first position was in Hawaii. She has had several different careers, one with some connection to a previous one, and has reasons for making the shifts. She didn't marry until recently--not terribly recently now--but she has no children. She married an already-retired navy captain somewhat older than she but not all that much. Her more recent career has involved essentially the electronic computer as applied to certain interesting things.

Ann was in Houston roughly the same time that we were at Rice, although she was living separately on her own. She got into the use of the computer in the medical world at Houston. When an M.D. from Houston moved to the Salk Institute down near San Diego, he invited her to set up computational facilities for him. When they got set up, he decided that he didn't need her any more, but the director of the institute kept her on the staff. Soon she had made enough contacts with others in similar fields, not necessarily medical, that she was offered a good position with a company that uses electronic computational capacities for various applications, such as the post office in handling mass mailings, and the navy in finding enemy submarines, and so on. She's had interesting travels in connection with the navy contracts. She's only working about half-time now; she and her husband are great travelers. They like all sorts of exotic trips.

Russell Pitzer ##

Hughes: Dr. Pitzer, I think you wish to revise your comments about your older son.

Pitzer: Yes. Russell Pitzer is also a scientist, undergraduate at Caltech and Ph.D. from Harvard in 1963. He was then back briefly at Caltech as an instructor and assistant professor before a more senior appointment opened up at Ohio State University in 1968, where he's had his career.

His thesis was a deeper level quantum mechanical explanation of the barrier to internal rotation in ethane. As I explained earlier in our interviews, the calculation involving the detailed motion of the electrons in ethane was utterly too complex and beyond the scope of the computers in the 1930s when I was involved with the experimental data and the evaluation of the overall potential barrier.

By the early 1960s, when Russell was in graduate school, it became feasible to do this calculation with appropriate approximation, but otherwise recognizing all the detailed terms in the potential interaction of all the electrons. He got very good agreement with the value that we'd inferred from the experimental data.

He continued through his career with quantum mechanical calculations on more complex molecules than ethane. He's a great expert on the use of symmetry to simplify calculations for any molecule that has symmetry.

Russell is also a very good and warm person in human relations. He's active and effective in various organizations, served a full term as chairman of the Department of Chemistry at Ohio State. He declined an early retirement offer; he's still enjoying his science and academic activities. Also, he's now a trustee of Pitzer College, Claremont, and as it were, represents the family in the affairs of the college that my father and his grandfather were involved in establishing.

<u>John Pitzer</u> ##

Pitzer: John, the youngest, I think felt he had to run pretty hard to keep up with his older brother and sister, [laughs] but he did. None of the three went to the same college. Russ went to Caltech; Ann was at Davis; and John went to UC Riverside just after the Riverside campus was established. So it was rather fun to be on the scene when a new institution was getting developed.

John got into mathematical economics with a master's degree in mathematics at Wisconsin, and then he's been with the government in essentially applying this. He eventually got a Ph.D. on a part-time basis with American University there in Washington, and is still active with the Bureau of Economic Analysis in the Commerce Department, and is at quite a senior level, involved with measures of economic accomplishment.

John was involved with the change in definition from the Gross National Product to the Gross Domestic Product. It was the same thing so far as most business within the country, but insofar as an American corporation has business overseas or a foreign corporation has business in this country, the Gross Domestic Product has more emphasis on what actually occurs in this country as compared to who owned it. He will be in an international meeting in Norway, I think it is, within a few weeks now, on making economic reporting and statistics as comparable as possible among different countries, and sometimes under UN sponsorship, sometimes just in voluntary international associations. This leads to interesting opportunities for travel.

Both John and Russ married relatively young, Russ essentially at the same age that I did, John two or three years older, but not very much. John has two children that are younger than Russ's three, but not very much. Russ's wife, Martha, is a nurse who has actually had a usually part-time but very substantial career in nursing education with advanced degrees and faculty appointments at Ohio State University. John's wife, Claire, is also an economist with an M.A. degree, and she has a part-time job actually in the same unit, the Bureau of Economic Analysis. Her job is more in terms of the actual statistical measures that are released to the public frequently. They both get interesting opportunities for foreign travel here and there, and then opportunity to extend it as they wish, as Jean and I have done through the years.

Well, we're very happy with the family. Some people have trouble with their children. [laughs] We may have had tense moments, but we've had very happy relationships in the long run, and with grandchildren too. One could go on and on indefinitely there; I guess that's enough for your chapter.

Hughes: Well, is there anything you more you want to say?

Pitzer: [laughs] Well, I've pretty well talked myself out right now. I don't think I have anything else right on the top of my mind now.

Hughes: Well, I thank you.

Kenneth Pitzer, Rice University, 1963.

Photograph © Gittings

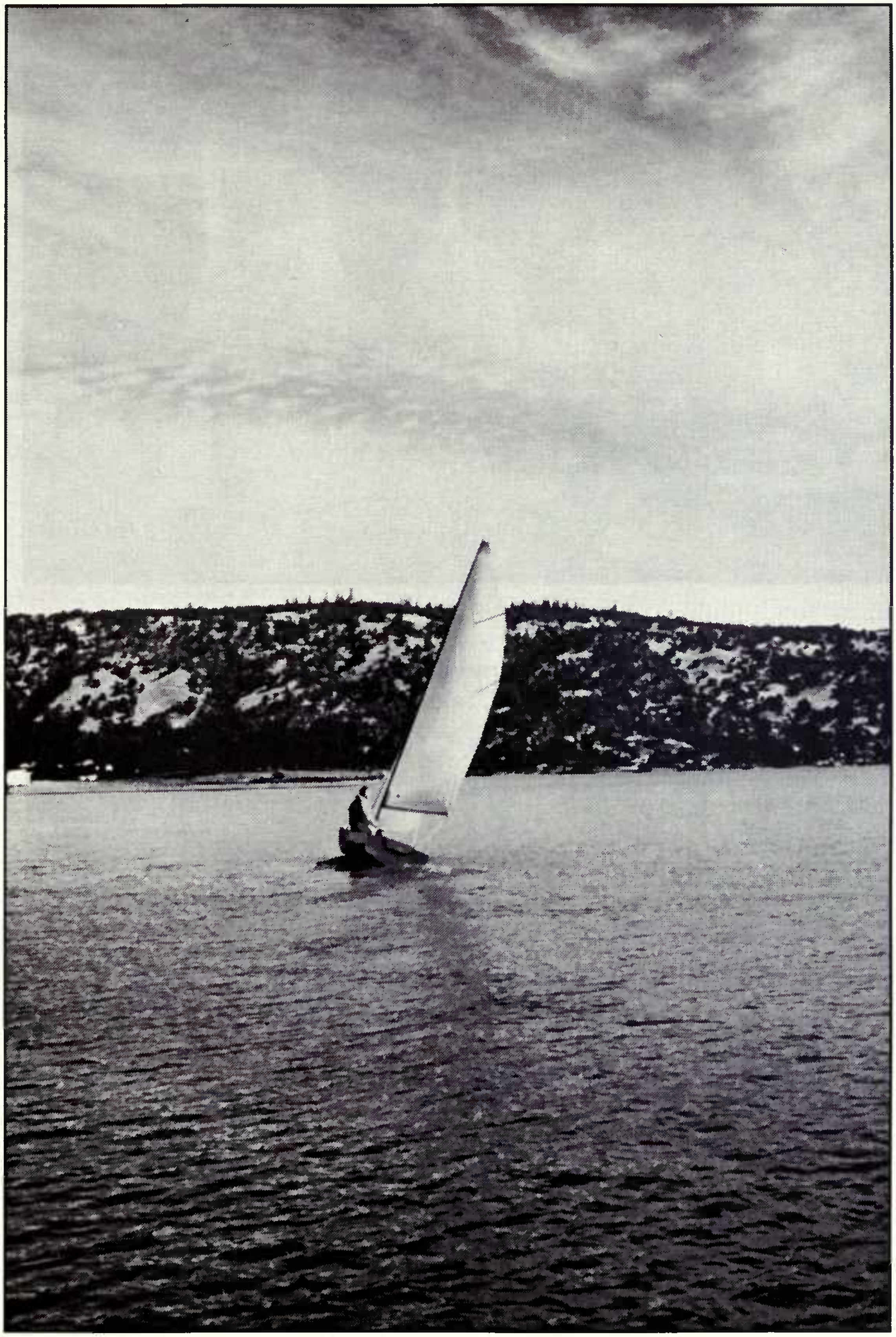
Jean Pitzer, Rice University, 1963.

Kenneth Pitzer's Stanford Inauguration, 1968.

Photograph courtesy News and Publications Service, Stanford University

Kenneth Pitzer receiving the Priestley Medal of the American Chemical Society from President Wallace Brode, Spring 1969.

Kenneth Pitzer receiving the Presidential Medal of Science from President Gerald Ford. (Linus Pauling in background.)



The Susan on Clear Lake, 1972.

Top: Kenneth and Jean Pitzer, 1996.

Bottom: Pitzer family, December 31, 1997. Standing, left to right: Fred Bromley (Ann's husband); John, Russ, and Gregory Pitzer; Willie Zahn; Susan, Ken R., Claire, Matt, Mary Lynn, and David Pitzer. In chairs, left to right: Arthur Browne, Jean Pitzer, and Constance Mosher Browne. In front: Ann and Martha Pitzer.

XIV FAMILY BACKGROUND AND CHILDHOOD IN POMONA

[Interview 10, with Germaine LaBerge: August 14, 1996] ##

The Pitzers

LaBerge: Well, this morning we're going to start with the beginning, even though this is midway in your interview. I know you were born in Pomona, but why don't you give me some of the details?

Pitzer: [laughs] Well, Pomona was essentially an agricultural town then. The primary basic source of income was citrus orchards, mostly oranges, some lemons, some grapefruit. Some walnuts, things like that. A little bit of commuting to Los Angeles, but not very much.

LaBerge: Commuting by your family, or the people--

Pitzer: No, I'm just talking about in general.

LaBerge: Yes, in general, okay.

Pitzer: This is just in general, the character of the place. It was very pleasant.

My grandfather on the Pitzer side had been a farmer, came from farming background in western Iowa, and had been quite successful. Also, the family was very much troubled by asthma, and the ragweed season--I guess it was mainly ragweed--caused him to move to different places to try to ease it, not for my grandfather himself, but for others in the family.

LaBerge: What was his name?

Pitzer: Samuel Collins Pitzer. He enlisted in the army at the time of the Civil War but was never sent to the southeast; he was rather sent to the north to fight Indians. He enlisted in Council Bluffs, Iowa, and he maintained a diary for a while which I have.

LaBerge: Oh, that's wonderful!

Pitzer: And I wondered if you'd be interested in adding this as an appendix or something to this oral history. [See Appendix]

LaBerge: I think that would be wonderful. Either that or to deposit it--well, no, your children would probably like it. I think that would be wonderful.

Pitzer: Well, I think it should be copied appropriately so that it's still within the family, but it just occurred to me that it might be of interest.

He moved the family, as I said, once to northern Nebraska, although that was more for cheap land. Then to Boulder, Colorado, hoping to improve the situation on allergies, but also to get the children educated--the older children at the University of Colorado. My father was too young for that; it was his older siblings that were actually at the university in Boulder.

Then they came to Pomona, and my grandfather--

LaBerge: Do you know about what year that was?

Pitzer: Oh, it would have been about early 1890s, I would guess. Well before 1900. He started a little orchard work near Pomona, but it was mainly older siblings in the family, that is, older than my father, who got into the orchard activities, or married somebody who was in that. There was just one girl and three boys in my father's family.

My father, as I say, was the youngest, so he still had some high school to do in Pomona. By that time, Pomona College, which had started in Pomona, had moved to Claremont. The reason for the move primarily was that the Santa Fe Railroad had built a hotel in Claremont and didn't find any business for it, so they coaxed the college to go to Claremont to take over the hotel as their number-one building [laughing]--

LaBerge: My goodness!

Pitzer: --and at least provide some railroad business, although I don't think they paid anything for the hotel.

So when it came college age for my father, the convenient place was Pomona College <u>at</u> Claremont.

LaBerge: What was your father's name?

Pitzer: Russell Kelly Pitzer. He then graduated in 1900 from Pomona College, went to law school at [University of California] Hastings [College] in San Francisco for three years. As he then said repeatedly in later years, he hung out his shingle as a lawyer and nobody came. [laughs]

LaBerge: And he hung it out in Pomona?

Pitzer: In Pomona, on the side, having joined his older siblings and father in orchard activities, so that growing citrus became his primary early life activity. He used his legal training purely as an aside, but it was a background of skill and competence in business generally that was of value to him through his whole life.

The Sanborns

Pitzer: On my mother's side, I have actually less information.

LaBerge: What was your mother's name?

Pitzer: Flora Anna Sanborn. She dropped the Anna later on and became Flora Sanborn Pitzer.

LaBerge: And that's where you got your middle name.

Pitzer: That's right. I have quite a lot of information about that. Sanborn is an old name in New Hampshire. You will find two villages, one Sanbornton and one Sanbornville. Sanbornton I visited several times because it's right off the north-south interstate through the middle of New Hampshire, and I had been going to some meeting or had other reasons for being there. Sanbornton has a post office, a church, I guess there is a fire station, a bicycle repair shop last I knew, but no grocery store. [laughter] The railroad was built about eight miles away, and the town that's on the railroad more or less took everything away from Sanbornton, but didn't completely drop it from existence. But the Sanborn family goes back into that period.

There was quite a little tradition of interest in education in some members of the family. My grandfather on that side died when I was so young--I guess he was still alive when I was born, but he died so early that I have no real memory of him at all. But his wife was quite interested and supportive of the schools in Pomona, and was I think on the school board for a while. She was a graduate of Mount Holyoke College in its second class.

LaBerge: So did your mother grow up in Pomona?

Pitzer: Yes. And she went to Pomona College offset one year later from my father. She came to Berkeley and spent a year, certainly got her teacher's credential, I think got a master's degree along with it, although I'm not absolutely sure of that, and then taught mathematics, high-school level, in Pomona roughly until the time she married my father, and then I was born. Those aren't exact, but I think for this purpose, that's accurate enough.

She had an additional connection in Berkeley--she was the youngest of the family, too--her next sibling going up in age, also a girl, was married to Professor James Allen, who was professor of Greek here at Berkeley. So she just lived with her sister and brother-in-law during the year that she was here.

Older Sanborns--the Sanborn family had been in Pomona, but except for my mother, there were none around there in my day. They had all gone--well, the one was married and was in Berkeley; others, married or not, were all in the Los Angeles area somewhere. So we visited them frequently, but they weren't down around the corner.

LaBerge: And did you get to know your grandmother, your mother's mother?

Pitzer: Yes, yes. She however had gone practically blind at the time I was old enough to have much contact with her, so we had oral contact but she was more or less of an invalid and was being taken care of by two older sisters, older than either my mother or the one here in Berkeley, one of whom was also a mathematics teacher in high school in Los Angeles, the other--neither of them, however, married--and the other one essentially kept house for the other sister and the mother as long as she lived.

But there were a couple of men in this family too, one of whom was a post office employee assigned mainly to railway post office activity, which people don't know about any more but was a fairly important thing in those days. I didn't get to know them particularly, although I met them occasionally.

[It seems appropriate to add a little concerning the background and antecedents of my grandfather, Samuel C. Pitzer. The first to come from Europe appears to have been an Ulrich (or Willery) Pitzer (or Bitzer) arriving near or at Philadelphia in 1727(?). He moved west in Pennsylvania and then to western Virginia where he died in 1769 or 1770. The record indicates a daughter, Anna Mary, and sons Christian and John.

From this point the record is clear. John (b. 1735?, d. 1824) had a large farm in Botetourt County, Virginia, as did his oldest and next oldest sons Frederick (my ancestor) and John, Jr. It was along the James River, and John, Jr., had a beautiful house overlooking the river. I have visited the house; it is well maintained.

The next links are: Frederick Pitzer (b. 1770, lived in Kentucky and then in Macoupin County, Illinois, where he died in 1839); Claiborne Pitzer (b. 1802, moved from Illinois to Madison County, Iowa, in 1847, and then to Mills County, Iowa, where he died in 1864); Samuel Collins Pitzer (b. 1844, moved to Ainsworth, Nebraska, then to Boulder, Colorado, then to Pomona, California, where he died in 1919). I have visited the Nebraska and both Iowa locations and found Claiborne's gravestone in the rural Hillsdale Cemetery. I have been in Boulder, Colorado, many times.

An older brother of Samuel C. Pitzer, Henry Littleton Pitzer, went directly to Colorado and was a merchant in mining areas rather than a farmer. His son, Robert Claiborne Pitzer, was a ranch hand, a gold miner, a newspaper reporter, and then a Presbyterian minister in Pennsylvania. He was also an author of western stories and on philosophical topics. He wrote a biography of his father under the title *Three Frontiers* (Prairie Press, Muscatine, Iowa, 1938). The descriptions of the Kentucky, Illinois, Iowa periods are equally applicable to my ancestors. My wife and I visited Robert Claiborne Pitzer and his family in Lebanon, Pennsylvania, when we lived in Washington, D.C., in 1949-51.

The Sanborn family history has been researched and published in several volumes. Most pertinent is *Genealogy of the Family of Sanborne* or *Sanborn in England and America*, V.C. Sanborn, privately printed 1899, reprinted 1969, Goodspeed's Bookshop, Boston. It gives considerable information about Sanbornes in England and their arrival in New Hampshire in 1640 or a little earlier, and then a complete account through 1899 where on page 588 it shows my mother, "Flora Anna Sanborn, b. June 3, 1879; a student at Pomona College."

There is an active Sanborn family organization that has meetings and publications. I have not been active in it, but I have visited the two villages, Sanbornton and Sanbornville, in New Hampshire. On a visit to the Sanbornton post office I found that the postmistress was a Mrs. Sanborn.][1]

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[1]Bracketed material was added by Dr. Pitzer during the editing process.

Influence of Elmer Kelly, M.D.

Pitzer: Now let's see. In terms of general family background, my grandmother on the Pitzer side, who was a Kelly, she was as Irish as the word Kelly would suggest. [laughter] Apparently--it's quite an interesting story--the original Irish movement in the line was out of Ireland in potato famine time. The young man was sort of adopted by a farmer in the southeast who converted him from no doubt his boyhood Catholic religion to Protestant, and I guess he was a Methodist. At least all the Kellys turned out to be Methodists once they got to California.

And this was a very large family. My grandmother was not the oldest but near the oldest. Another member was a leading physician in Oakland. A couple of them were Methodist ministers, and in the next generation, one of that Kelly line was actually the Berkeley health officer for the city government. I met some of these people; I didn't really know many of them.

But there was another member of that family considerably younger than my grandmother, but a younger brother, who was an M.D. and in Pomona, Elmer Kelly by name, and he was really quite an influence, as far as I was concerned, because he was quite alert scientifically and, while I wouldn't say that he had any major role in my going into science in the long run, I can't help but think that in the very general and sort of subconscious way, that he probably was more influential in that respect than any other one person or thing.

LaBerge: Did you spend a lot of time at family gatherings growing up?

Pitzer: There were quite a few family gatherings.

Oh, there was another in that Kelly line too, toward the younger end again. In other words, considerably younger than my grandmother, I suspect even younger than Elmer, who taught junior high school level in the Pomona schools. I was in her class. Never married. Did a great deal of traveling. She taught geography; it was a course in those days. She would teach some other things too, but it was actually a junior high school course, not a year long, maybe a semester long, in geography per se.

She was a great traveler, so that she could teach geography not only as an abstract subject but as stories about "When I was there."

LaBerge: Do you remember her first name?

Pitzer: Effa. I'm quite sure I'm right on that.

LaBerge: Okay. Your grandmother's first name was--?

Pitzer: Alice.

LaBerge: Alice, okay. Well, from both sides of your family, there's such an emphasis on education, even in those early days. I think that that's unusual.

Pitzer: Yes, I think so. Either education or people that were going into the ministry or going into medicine and so on, that obviously had gone to a fairly high level in education and appreciated it, yes. I think that's true. In terms of actually the highest level of education, only my father went to law school, but his siblings were all college graduates at the bachelor's level. But even then, they were all very respectful of it, let's say, and regarded it as important.

LaBerge: For the early 1900s, that was something.

Pitzer: Yes.

LaBerge: Do you have any siblings?

Pitzer: For practical purposes, the simple answer is no. My mother died when I was in junior high school, of cancer. After several years, my father married again. They had no children, except by adoption. The adoption occurred long after I had left home and was married and was living away from the area. I met the individuals involved there. It was a debacle. [laughs] My stepmother was--if I may be so blunt--was <u>not</u> a good mother, and the two girls had already had a troublesome background. It might have been straightened out, but she was not capable of straightening them out. Only the younger girl, Jean, was actually adopted. So things sort of drifted apart, and while I have records of it, I certainly met them, I had no real acquaintance, and for all practical purposes, I was an only child.

LaBerge: So you were born in 1914?

Pitzer: January 6, 1914.

LaBerge: Any more anecdotes on the grandparents and uncles and aunts? It was such a rich, warm family, it sounds like.

Pitzer: Oh, yes. The geography teacher, with all her travel, she didn't drive an automobile. The story is she tried to learn once and something went wrong, and that's the time when I was too young to know much about it, and then it was not widely advertised. She decided to make another try. She ran into a fire hydrant, knocked the fire hydrant off, the water obviously--a geyser--right in front of my grandfather Pitzer's house! [laughter] Anyway, she gave up on that.

Religious Background

LaBerge: Did you have a religious background growing up?

Pitzer: Oh, yes. We should say something about it.

My mother had been a Congregationalist. There was a very good Congregational church almost within a block or two of our house. My father had come from a Methodist family, but I don't think very ardently so, at least as to denominations, and so he happily became a Congregationalist instead. They were, I would say, moderately active in the church. They attended, but were not terribly intense about anything there. But of course, the Congregationalists aren't, by and large. They are relatively tolerant, relaxed about others having somewhat different beliefs.

Pomona had lots of churches, and around the small city generally, it was a strong influence. Now, let's see what else one might add there? I guess that's--

LaBerge: I guess I was asking that, too, to wonder if that has influenced you throughout your life.

Pitzer: Yes, but not very much. My wife--remember, she grew up in Pomona too. Her immediate family was, I would say, about the same. They were in a different denomination, but they didn't feel very strongly about which Protestant denomination one was affiliated with. But neither of us were very strongly committed, and we sort of drifted away from being active members of any religious denomination. So I won't say that--I would say that it did not have any very strong influence on my life, except that the general sort of personal guidance of morality and so on that all these denominations would agree upon, that was quite a strong influence, I would say. But any particular detail religiously has not been a strong factor.

Schooling

LaBerge: What about your childhood schools?

Pitzer: Well, I guess that's a good subject to go to now.

My mother had been a teacher. She stopped teaching, had stopped at the time I was born. She decided to teach me at home for a year or two, so I never went to kindergarten, never went to first grade. I guess I started in the second grade. But she'd done a good job, and--

LaBerge: You probably started off reading and knowing the math--

Pitzer: She taught me to read, she taught me math. She had all sorts of little math games, puzzles to build up your math skills, so that I went into the--I guess it was the second grade, possibly it was even the third grade, but I think it was second grade--with no trouble so far as the school work was concerned. I may have been slightly handicapped in terms of social relationships, having been more isolated.

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LaBerge: We were talking about being an only child.

Pitzer: Well, I think it had some continuing influence, particularly through the lower grades and even through junior high school. I think that my relations with fellow students were not as comfortable as they would have been had I gone to school earlier, but this is a minor difference. It caused no serious problems.

LaBerge: No, and considering everything you've done in your life since then--I mean, being university president and having to really be quite social, it certainly didn't harm you.

Pitzer: Well, it just meant you may not have been--you might have to be a little more conscious about it than you might have been otherwise. No, I'm thinking more, even the time I was in high school, I think it had pretty well gone away. I think I was socially quite at home with fellow students even in high school. Certainly once I was in college, it had no significant effect.

I went to the other various public schools in Pomona, which were on the whole pretty good.

LaBerge: Did you have favorite subjects?

Pitzer: Yes. I was always very much at home in mathematics at any level.

There is one story there that actually I guess made it into the national press later. The teacher of plane geometry, which was the standard tenth-grade subject in those days--an elderly man, name of Bartlett or Brackett, I think Bartlett, but no matter. Nearing retirement. About halfway through the course, he said that he was going to assign a problem, and if anybody could solve it, that he'd guarantee them an A on the final grade. [laughs] It involved some complex geometric proof, and I succeeded in solving it.

Now, the reason it got into the national press is that a classmate in that class by the name of Jack Beardwood, who later became a reporter, I think it was for *Time* magazine, and when I was appointed as director of research for the Atomic Energy Commission, he volunteered to write a story about his old high school classmate, and among other things, tucked that story in.

LaBerge: You probably were going to get an A anyway without solving the problem.

Pitzer: Well, yes, I think so. [laughter] And I didn't ignore the rest of the course because the A was guaranteed.

Actually, the woman I had for the advanced algebra I think was a better teacher, did a better job, and it was a much more important subject from the point of view, really, than the plane geometry. I don't have any very clear memory of the fourth year's trigonometry and solid geometry courses, but they went all right.

I took both chemistry and physics. The chemistry was taught very competently, but not particularly inspiredly. But the instructor was always there, we did lab work, it was really very well done.

The physics, on the other hand, is much more of a story that I tell from time to time. By and large in those days, most of the college-bound students took chemistry. There were quite a number of sections. Relatively few took physics in addition, and even less probably physics instead of chemistry, although I don't know about that.

As it turned out, the physics instructor, who was reasonably good in physics but not terribly good, was also the manager of athletics, essentially, for the school. And so he would frequently be called out, called away from the class, because there was a crisis about arranging for refereeing in the football game next Friday afternoon or Saturday or something. He decided that I knew the physics about as well as he did, as a matter of fact, [laughter] and certainly better than anybody else in the class, and so he told me to take over as physics teacher.

Well, if I was going to be called on to teach, I'd better at least study it ahead of time, so I would not be too much taken aback if I turned out to be the physics teacher instead of the teacher. I think that really helped.

LaBerge: So essentially, you taught yourself, if you had to study ahead.

Pitzer: I taught myself physics, yes.

All in all, that was a very good high school experience.

Hobbies

LaBerge: And did you play athletics?

Pitzer: No, not to amount to anything. I have been a reasonably healthy person but never to the edge of skill in terms of accuracy of movement and balance and all these things that make the difference between an athlete and somebody who can get along in life but doesn't really excel.

LaBerge: Did you have some other hobbies, like music or--

Pitzer: Well, I like to work with my hands in terms of mechanical or carpentry or that sort of thing. I used to make model boats and things of that sort.

LaBerge: In the present day, you build boats, don't you?

Pitzer: I've built boats later on, several of them, yes. We'll talk about that later. We can come back to that.

LaBerge: But that started early on in life.

Pitzer: That started early, and my father encouraged me. He had a collection of tools but not very much of a one; he wasn't much in line in that direction. But he'd buy some more tools or give me the money to buy additional tools and supplies and so on, and encouraged it generally.

Let's see. I took two years of Latin, which I didn't excel in that. I had no trouble passing it, and it proved to be in a way valuable in the long run. On the other hand, it might have been better to have taken French or--I don't know whether German was offered; I'm sure French was. You picked up a bit of Spanish just because, as in much of California today, there are people that speak Spanish around, and therefore, you pick up a little of that.

LaBerge: Do you feel that you needed French or German later when working on your Ph.D. or--

Pitzer: Well, yes, I had to study it later. Well, we'll come to that. We talked about college a good deal in the other interviews--

LaBerge: But some things like the classes you took--it was more the scientific part of college.

Pitzer: Yes. But for example, at Caltech, they offered German and if you wanted, a brief switch-over to French, but the German was purely aimed at reading German to pass a language examination for an advanced degree. I don't mean that they never did any pronunciation, but they did not really attempt to make you competent orally. French, it was less than a year. They didn't care much about what you read in French, and [laughs] and I remember one about some adventure story in Alaska written in French.

But this was all useful, and in those days, German was enormously important in the physics-chemistry literature, which it is no longer. But the Caltech German was adequate for that purpose. You know, German went out of high schools very heavily during World War I, and I'm not at all sure that it was offered at Pomona even as late as late twenties, '30, when I was there. Certainly French was offered, there's no doubt about that, and Spanish.

Well, let's see.

LaBerge: What kind of books did you like to read as a child?

Pitzer: Oh, I suppose what you might call adventure stories of one sort or another, involving travel and exciting activities of that sort. And some more classic things, but just--in other words, I was not allergic to more classic things from school, but I suspect at home in my spare time, more the other.

LaBerge: Did you do a fair amount of reading in your spare time?

Pitzer: Quite a little, quite a little.

My father was primarily a citrus-orchard farmer. He got into other business activities partly because he did have a law degree. He was on the board of directors of the local savings and loan, serving as a first-line--as a member for business purposes, but also on legal matters, he would give a simple answer. He wouldn't take a complicated case, he'd say, "Take that to such-and-such a law firm." But he'd save them money, as it were; it was a relatively small operation. They didn't have to have an in-house salaried lawyer. But as I say, it was mostly citrus. But we actually lived in town. He went out--it was a small town then--went out by day, and he had one or two full-time farm employees out actually in the orchard. But he did quite a little himself.

Getting a Driver's License, Then and Now

Pitzer: I remember one time, I was beginning to learn to drive, it was picking time. We had a truck and my father was going to toss off empty boxes for the hired picking crew to come later and pick the oranges and put them in the boxes. He said, "Well, you've more or less learned how to drive this truck now. You drive the truck and stop at every tree, every pair of trees, and I'll toss out the box or two, and then you can go ahead."

This was perfectly legal; it was on his own orchard. A little while--well, I almost ran over an orange tree once, but that's--[laughter] I did find the brake. Eventually. Well, we did that sort of thing.

So when it came for me to get a driver's license--of course, I had been coached by my father on the road--but this is a small-town atmosphere, informality, that we went in together, and the fellow who was issuing driver's licenses said, "Well, R. K."--Russell Kelly, they called him R. K.--"Well, R. K., does the boy know how to drive?" My dad said, "Yes," so he issued the license. [laughter] No written exam--

LaBerge: No practice--

Pitzer: No practice. [laughter]

LaBerge: That's pretty good. I think a lot of kids these days would love that.

Pitzer: Yes. Well, we had almost as good a story as that with my older son, the middle child, who's also Russell. He was an athlete in El Cerrito High School, and I guess it was track--he was a shot putter. He'd done his practice driving under my supervision or his mother's, but he couldn't find time to go down and take the driver's test because of this athletic schedule. Suddenly, a given track meet was called off, and he called home, hoping that his mother would take him down and let him take a driver's test, but she was out somewhere. So he called the mother of a close friend, and she agreed to go down with him--in a different car than he'd ever driven before. He got out with the driving examiner, and he couldn't figure out how to start the car! [laughter] The examiner said, "Well, I don't think you'll ever have a collision under this condition. Now, if you'll do that, I think it will run." So he passed.

LaBerge: My goodness. So he came home, and you didn't even know he had a driver's license by then!

Pitzer: No. [laughter]

Working in Father's Citrus Orchard and Other Jobs

LaBerge: Oh, that's very good. Well, did you do other things at the citrus orchard? Did you do some of the--

Pitzer: Oh, yes. Irrigation, particularly. I would--see, this is all irrigated agriculture, and there was a source, either a ditch with little gates in it or--well, that was it, it was a ditch with a fairly firm wall and then little gates for rows of furrows down between the trees. And what you had to do was adjust the gates properly so that water would get to the lower end of the orchard but not go to waste, not much go to waste. And there was always the danger that there was a gopher somewhere, and that it would go down a gopher hole instead, so if the water didn't seem to be getting there, you'd have to walk up that row and find the gopher hole and at least fill it up to the point that the water would go on by, and adjust things and so on. That was one thing that I did often during the summer, this was essentially summer activity. There were other things, but that was one that I got called on to do quite a little.

I did other things during the summer. I remember one time--I don't know why my father bought this half-burned house, but anyway, for some reason or another, he had this house that had had a fire in it, which didn't involve anything--any of our living in it at all. And it wasn't on one of his major primary orchards either. I don't remember why he had it. Maybe he'd lent somebody some money on it [laughter] and foreclosed it, after it had half burned.

Anyway, one summer I was a carpenter's helper fixing up this house, and that was a nasty job. Every time you drove a nail, why, the soot would fall all off on you. So various activities like that.

I did not in general have paid summer jobs outside the family. In other words, it was within my father's orchard activity or other things like this that I would get involved with.

And I started building even a human-size sailboat; I guess it was one summer after I was at Caltech. I had built a somewhat smaller boat even earlier. We frequently went down to the Newport-Balboa area on the shore for holiday vacation periods, and one could trailer a boat down. So that I got first into some model sailboat activity and then, I guess it was a rowboat, and then an eighteen-foot sailboat with a little cabin on it. That I built during a summer when I was at Caltech otherwise.

Family Vacations

LaBerge: Other family travels?

Pitzer: We did quite a little traveling by automobile within California. I had seen Yosemite more than once with my family. I can remember the old times--didn't make any difference which of the main entrance roads you used in those days. As you got close to the Yosemite Valley, it was steep and one-way width, so it was under control. In other words, you had a period of time going this direction and the other direction.

But then near there, after you're outside the control area, you still had this problem of meeting somebody coming the other way. There was quite an interesting custom about that in that if this was a group of cars that had come through under a timed control, they'd still be more or less a group on beyond. So you frequently put up fingers, "five more to come," or "four more to come," that sort of thing. Because once you got a wide spot in the road, you could let the people by you, just stay there and let the rest of them come by too.

But Yosemite was fun in those days. I don't say it isn't fun now, but it was relatively empty. And there were things that the environmentalists later abhorred and stopped, of course; they pushed the fire off of Glacier Point and let it slide down the cliff next to Camp Curry. We stayed there and watched the fire, and they shouted back and forth. It was quite a show. I don't see that it did any harm, but it certainly wasn't natural as things had been made before.

More frequently, we'd go up to Lake Arrowhead or Big Bear Lake, northeast of San Bernardino. It was only a two-hour or three-hour drive even at relatively low speeds those days. There was a family friend that had a summer house on Big Bear Lake. I think it still carries that name. We'd either stay with them or camp in the yard and be with them during the day. The man in that case was a carpenter. He built cabins up there during the summer, and a rather extended summer, and I think they had another home outside the mountains that they lived in part of the year, too.

We made trips also but not too infrequently to Santa Barbara. I don't know whether my father had some of the savings and loan activity business there or not. I remember occasionally going with him to Imperial Valley, the El Centro area. That was a savings and loan business trip when they had some legal problem down there, and he wanted to go down and talk to the local counsel who was handling the problem.

There was a solar eclipse that was full at San Diego but not at Pomona. We went down to the northern outskirts of San Diego and camped overnight to watch the solar eclipse there, I remember.

We went to Los Angeles quite a lot, of course. That was simple, and optional as to method. There was an interurban electric train service from Pomona to Los Angeles, and then a local streetcar out to where the Sanborn family was. We drove more often than the other, but we did take the public transportation sometimes. On the other hand, if we were doing something in central downtown Los Angeles, we usually took the Pacific Electric cars, which delivered you within walking distance then at that end. And the Pacific Electric line was right in front of our house in Pomona, so there was no difficulty there.

My father would have some occasional business in Los Angeles, not very often, but I guess it was more to visit Sanborn relatives than it was business there. But between the two, we'd do that quite a little.

LaBerge: Did you ever think of going in the citrus business yourself?

Pitzer: Not seriously, no. If I hadn't had a really quite strong bent toward mathematics and science, I would have never gone to Caltech; I would have gone to Pomona College, probably, possibly to Stanford or some other place, but not to Caltech. And since I liked Caltech, and as it were, Caltech liked me, [laughs] I--

XV UNIVERSITY OF CALIFORNIA GOVERNANCE

[Interview 11: September 11, 1996] ##

Loyalty Oath Issue, 1950

LaBerge: Last time, we finished your childhood and growing up, and today we're going to go back to the university and talk about various issues and possibly the Academic Senate. One big issue was the Loyalty Oath in 1949, and even though you were I think at the Atomic Energy Commission then, you--

Pitzer: Yes, I went to the Atomic Energy Commission as director of research at the very end of 1948, and I was there beginning January 1, '49, through '51, which was the hot time.

LaBerge: Right. [laughter]

Pitzer: So although I was informed about it, I had no real role in it one way or another. I don't have any comment, other than just saying it was sort of a very unfortunate situation. But other people have given their opinions, people that are better informed than I, so I don't really see very much point to my talking about it.

LaBerge: Did it impact on the Chemistry Department or the College [of Chemistry]?

Pitzer: I don't recall that it had any very serious impact here. My former student, who was by then on the faculty, George Pimentel, I know was active in trying to sort of calm things down and minimize the damage, if you wish, and keep people doing things that they normally were doing rather than getting overly worked up about this. But I think that's in the record, and as I say, I wasn't here, so I can't really add anything to it. I'm sure more senior people were even more involved in that respect, but I comment about George Pimentel because he was active as a very responsible, but very junior, faculty member at the time. And well, I'm not sure--I guess he was just barely on the faculty, he had just finished his degree.

College of Letters and Science, Assistant Dean, 1947-1948

LaBerge: How about your position with the College of Letters and Science? How did that come about? You were assistant dean, right before--

Pitzer: Yes, I was an assistant dean for one year, and I'm not sure just which year it was, but it's pretty well pinned down around--

LaBerge: '47 to '48, I have.

Pitzer: --'47-'48, probably. I presume that I was recommended by Professor [Joel] Hildebrand for that. In fact, he was actually dean of Letters and Science several years later. [The dean of Letters and Science was Alva R. Davis in 1947-48.]

It was essentially a sort of second level of student advising, with authority to adjust or waive minor regulations when justified. Student advising in the College of Letters and Science has always been a problem, and to the point of being almost nonexistent so far as regular faculty is concerned in some certain times and places and levels. I think I had been just an advisor to Letters and Science majors in chemistry the year before. Most of the students in chemistry beyond the elementary level were in the College of Chemistry, and for those students, there was good advising from regular faculty. And then there were people with pre-med or biological or engineering majors that were pretty well advised in those circles.

[The College of] Chemistry has provided an advisor for those who were chemistry majors in the College of Letters and Science, of which there always have been some, but for the freshmen and sophomores particularly, before they had chosen a major, the College of Letters and Science has always had trouble getting real faculty attention to advising.

I agreed to help out for one year, not as an initial advisor, but after some problem had arisen or the student had complained that he wasn't getting attention, or his mother died, or all sorts of things like that. I was one of a few assistant deans who spent a certain number of hours on that. I think we went to the dean's headquarters, rather than receiving people at our own offices, but I could even be wrong on that. This doesn't have a very prominent place in my memory.

Faculty as Administrators

LaBerge: I have a quote from you, and I don't know if I got it from one of your speeches or where it was, but it goes like this: "A research scientist should be willing to devote a portion of his career to a management position, if it were important to science and to the community generally." I'm just wondering if that was part of your philosophy as far as taking on something like this, or taking on more of an administrative role.

Pitzer: I could well have said it. [laughter] That's what I believed for quite a number of years, and acted on.

LaBerge: Do you think that's unusual in the faculty, to feel that way?

Pitzer: No, I don't think it's unusual, but it is probably, depending on the degree, rather in the minority. Some people by their personality and background and so forth are better equipped to do some administration than others, and there's no reason to push those that are poorly qualified and not so inclined into it. They'll do a poor job of the administration, and both they and those administered will be unhappy about it. So the alternative on the other direction is to have people that essentially may have been trained in science and had little experience, but then they essentially give up their science for administration. Some people do that very successfully and very well, at least for quite a number of years, but others get into the situation that in order to satisfy their ego, they become too heavy-handed and not sufficiently consultative of active scientists, at least after a few years.

I think when the person has the inclination and qualifications, that it's better for science and high-level education to have it handled on this basis of relatively limited terms, by people who not only are active scientists but expect to continue to be or to return to their science, rather than to have administrators that have only some training in science but no immediate prospect of personal activity there.

You didn't mention, but as I recall, I was on the Budget Committee of the Academic Senate.

LaBerge: Yes, I think I have it written down, anyway.

Pitzer: I actually have a clearer memory of that than the assistant deanship, not that it's very clear. [laughs] And I don't remember which year it was.

Academic Senate

Budget Committee

LaBerge: Let's see. I have Budget Committee, that you were replaced in 1949 on the Budget Committee, so that leads me to believe you were on it before 1949.

Pitzer: Yes. Well, I think it was the year after I was assistant dean, '47-'48; that would have ended in June of '48. Then I guess I was on the Budget Committee, but that would have been just the fall of '48, because I left at that time. So it was really only six months, although that's an active time on the Budget Committee. The faculty promotions are all presented during the fall, and the promotion appointment activities that go through on schedule go through during the fall. Only the ones that are delayed or get overly complicated or something get into the spring. So I saw a fairly substantial part of the cases of 1948-1949.

And this is something which is almost unique to the University of California. It was certainly developed in Berkeley and, insofar as it operates equivalently in other campuses, it's by following the tradition in Berkeley, in which a campuswide committee has that much involvement and that much influence in terms of reviewing the promotions that are recommended by a department or the dean, at least the dean of a relatively small department group. I think it's had a very significant role in maintaining the excellence of the Berkeley campus through the years. Having had the experience with it as a very junior member from the committee side, even for six months, it meant that I could deal with it from the side of the dean with both understanding, maybe greater assurance, maybe a little greater influence, at least greater appreciation, too, and willingness to give the Budget Committee views full measure.

LaBerge: So how does it work?

Pitzer: Well, what happens is, say on an appointment at a high enough level, or a promotion, the department chairman recommends, with whatever consultation with the members of the department or the senior members of the department, to the chancellor. The dean comes into this going up the line, either before or after the Budget Committee. The chancellor's staff immediately refers it to the Budget Committee. So depending on the importance of the case, or on its complexity as compared to its simplicity, the Budget Committee may just commend it without further review, but almost always, the Budget Committee will appoint a special review committee with a chair from a different department from that of the candidate, and the majority from other departments, but a minority from the recommending department, that is, the candidate's department. And then, on receiving their report, the Budget Committee makes its own comments in forwarding the case to the chancellor, or the president in pre-chancellor days.

The chancellor or president is not obliged to follow the advice of the Budget Committee, but he almost always does. At least, he very seldom promotes or appoints somebody that the Budget Committee recommends against. He may wait another year before promoting somebody that they were favorable of, depending on other factors, or the budget, or one thing or another. But the influence is very great, and at least in all the time that I was close to it, even including later years when I was just occasionally on a review committee, it was done very thoughtfully and relatively impartially. In other words, the tradition was very much against letting personal prejudices enter into it.

Now, as I said before, I think it's been an important factor for the campus. I've known a number of members and chairs of Budget Committees in later years that would talk with me occasionally about how it was going, or what might be the best way to handle some emotionally very tense situation.

LaBerge: How would a smaller committee go about evaluating the faculty person's--

Pitzer: Well, they would look at what the chairman had said. The chairman will have had outside nominating or recommending letters. That's gone almost overboard now in the large number of such letters required. In my day, three was really quite sufficient. If they were sufficiently distinguished and covered various aspects of the candidate's career, maybe it seems to me we even got by with two once in a while. Now, if the case is controversial, and some of the letters are either lukewarm or tend to have a negative tone to them, why then, if you still want to make it a positive recommendation, you get a lot of additional letters.

Now, the review committee will be sufficiently well informed or will have independent sources of information to judge whether these were the appropriate persons to evaluate that candidate, and in particular, was there some person that ought to have been asked to evaluate the candidate but wasn't. This is the sort of information that the chancellor isn't going to have, and even the central Budget Committee may not have, unless one of its members is close to that particular field. But with enough experience in how the university world works, this all goes, as I say, really very successfully.

LaBerge: When you're in the Academic Senate, are you automatically assigned to, say, Budget Committee or to some other committee, or do you volunteer, or--

Pitzer: No, no. The Academic Senate is a world of committees, and among other things, at the top level, there is a Committee on Committees. That, at least ordinarily through the years, has been elected by the total membership in a mail ballot. The Committee on Committees essentially appoints. Now, they may ask for volunteers, but I would think it would be unfortunate if they limited themselves purely to volunteers, because some of the most valuable potential members are not going to be anxious to spend their time on this, but might be willing to if asked.

Vice Chairman (Now Called Chairman)

LaBerge: Were you ever on the Committee on Committees?

Pitzer: I don't think so. I think I would remember if I were. I was vice chairman of the Academic Senate in the late fifties, as I recall. And at that time, the president of the university was ex officio the chairman of the Academic Senate, and the person either elected or appointed by the Committee on Committees, or whatever the appointing mechanism was vice chairman. But in effect the chairman of the Academic Senate was called the vice chairman. This distinction has come up recently in that there was an occasion honoring all the former chairmen of the Academic Senate, [laughs] and I was not recognized on that basis, although I was invited to the dinner and all that. I needled somewhat the people that might have gotten that straightened out and didn't. It's not all that important, but--

LaBerge: No, but if you in fact were the chair, because nowadays, it's called what, the president of the Academic Senate?

Pitzer: It's the chairman now.

LaBerge: And the president doesn't have a role, or--?

Pitzer: The president nowadays in the multicampus university would hardly expect to come regularly, but he's undoubtedly welcome if he wants to come as an ex officio member. But he's not really likely to be there. The chancellor is the one who frequently comes, and is on the agenda, has an opportunity to make remarks, and of course, can always speak about other items of business if he wants to. And that's what actually happened with Chancellor [Clark] Kerr when I was presiding. He wasn't the chairman; it was still ex officio the president, as I recall. I could be wrong in this memory; I haven't checked it exactly, but that's as I remember it. [laughs] Just for amusement, one session that I remember presiding over, the subject was parking.

LaBerge: Oh, that's a very big topic on campus. And a privilege.

Pitzer: Various people had quite emotionally charged speeches about parking. Of course, the Academic Senate had virtually no authority on it, but they were listened to.

LaBerge: Did the chancellor have authority on that? Who does decide the parking?

Pitzer: Oh, I'm quite sure the chancellor had it, on a campus basis, yes. There may be some statewide policy that the chancellor has to remain within, but I'm sure the chancellor can essentially determine it. Of course, the other limitation is just how much parking there is.

LaBerge: Right. So essentially, as the vice chair or the chair of the Academic Senate, what are your duties during that term?

Pitzer: Again, my memory is somewhat fuzzy there. But for the chairman, the formal duties are just to preside over the meetings when the full Academic Senate is called to meet. They met as a body more times a year then in the late fifties than they do now. That is, the numbers were smaller and people were more inclined to take that limited amount of time off and attend. I suppose the attendance was never even a majority, but it was a very substantial portion of the total membership.

The chairman of course has to have a role in putting together the agenda and checking the agenda as being appropriate, and being sure that the particularly important people are notified of the agenda ahead of time, in addition to the regular notices. He wants to think about it a little bit as to what is likely to be controversial and how much time will be required, and will he need to control the length of time that any one speaker has.

The other less definite thing is to keep some degree of cognizance over the activities of the various committees, as to whether they are taking care of problems or playing their appropriate role. I suppose if there's a controversy over which committee has jurisdiction over something or other and it's ambiguous, then the chairman could probably straighten that out. But in this I am more or less reconstructing just from general background and experience rather than remembering detailed episodes.

LaBerge: Did anything controversial come up that you remember when you were--

Pitzer: [laughs] The only thing I remember is parking!

LaBerge: And actually, the fifties were kind of a quiet time.

Pitzer: They were relatively quieter, yes. Now--

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Pitzer: One could no doubt find in the files the minutes of that period, and one could look through and see if there was any reason to comment, but I doubt it.

LaBerge: I have written down also that you were on at one time the Academic Freedom Committee. Do you recall that?

Pitzer: Well, I think I was, and I don't really recall anything particular there. Do you recall what the years were?

Universitywide Committees

LaBerge: No, I didn't find any issue, just that that was one of the committees you served on.

Pitzer: [looking through files] I'm not sure whether that's recent enough. Certainly it's not very recent. I've been on various other committees, including the one on emeriti.

LaBerge: Oh, I was wondering if you were still active in the Academic Senate.

Pitzer: I've served several years on that, and I'm off now. Others more appropriate are on. There is one--I can't find anything. [still away from microphone] I've forgotten this but just before I went down to Texas to Rice [University] as president, I was apparently on the intercollegiate athletic advisory council. I had forgotten that. That's one that Glenn Seaborg was on just before he was chancellor, and I guess I must have taken his place afterwards. Oh, I've got a whole set of files here about the different committees, not necessarily Academic Senate committees. There are other Berkeley and University of California committees.

LaBerge: But when you came back in the seventies, you were active--

Pitzer: [returns to microphone] I don't recall being particularly active within the Academic Senate. I was undoubtedly on some review committees and so on. But I was also on special review committees for the director of the Lawrence Berkeley Lab, or the director of the Livermore Lab, and that sort of statewide presidential-level ad-hoc committees from time to time. There was also a standing advisory committee for the Lawrence Berkeley Lab that I chaired for four or five years, I guess, three or four years, which was a presidential committee, but of course, it involved, among other topics, the relations with the Berkeley campus, and that was one thing that I was very much interested in being sure were maintained as well as possible.

LaBerge: Anything with Los Alamos?

Pitzer: Well, not with respect to Los Alamos particularly, but Los Alamos might have come into it along with Livermore as the two atomic-energy, weapons-level laboratories. These committees were active during the times that Charles Hitch was president and when David Saxon was president--I knew both of them very well--and one or the other of them asked me to do things of this sort. I don't think any of those really need comment further, but I might look through the drawer that I just looked at and see if there's some that I would like to expand on, we can come back to that some time.

University Presidents

LaBerge: Okay. Well, just on that subject, universitywide, you have served under many different presidents.

Pitzer: Yes, more or less intimately, I have known them all. I knew [Robert Gordon] Sproul quite well, and of course Kerr, and then my memory plays tricks on me, who was--

LaBerge: Well, Harry Wellman was acting, I think, before--

Pitzer: Of course, I knew Wellman very well. He was just acting, though. Then it was Hitch, wasn't it?

LaBerge: Yes, I think so.

Pitzer: I knew Hitch quite well. Saxon reasonably well during his time as president, but I have known many of these others over longer periods of years and in other connections as well as this, whereas Saxon I knew well only in this immediate connection. The current president, [Richard] Atkinson, I had first known at Stanford. As a matter of fact, he lived within a block or so of the president's house, and I'd see him almost as a residential neighbor. That's a long time ago, and we had a friendly but not particularly close relationship then. But I've followed his career since, and I wondered whether, as he was there as director of the National Science Foundation, I was wondering whether, when the Stanford [presidential] vacancy occurred, whether Atkinson would be appointed instead of Don Kennedy, whom I also knew in the Stanford time. But Atkinson turned up as chancellor at [University of California at] San Diego instead.

Robert G. Sproul

LaBerge: Would you have any comment on any of the presidents, particularly President Sproul and how he related to the faculty?

Pitzer: Yes. Sproul was a person who genuinely enjoyed warm, personal relationships, and was at ease in knowing and dealing with a large number of people. Now, of course, the university was much smaller, but in those pre-chancellor days, he at least maintained a remarkably good acquaintanceship and real personal understanding and relationships all the way down to departments and the more significant faculty members at Berkeley. In some of the later years, he would go down and spend months at Los Angeles in order to do somewhat the same thing down there. I don't think he ever maintained quite the same relationships, and the provost or whatever it was at UCLA came closer to what you'd call the chancellor's role there.

I think that sort of broke down during the oath period. There was a lot of tension there, and then I was away, although I was back and had contacts with President Sproul afterwards. We always had very friendly relations.

Let's see, when was the chancellorship established?

LaBerge: You know, I don't have it written down here, but it was some time in the fifties. I'm guessing around '55 or '56, but I'm not positive.[1]

Pitzer: Yes, I think that's probably it. But I know my initial appointment as dean was by Sproul, and I am told that--I suspect this is in the record somewhere, that when I went to the AEC as director of research, at a time that Wendell Latimer had just resigned, the department members and faculty in some form petitioned Sproul, asking that he make Hildebrand, who was then over-age to be a dean really, although not yet retired, to make him dean during the interim while I was away, and hold it open for me to be dean when I came back from the AEC. I don't think there's any question but what that is substantially correct.

LaBerge: Right, I have read that, and I think I may have read it in some of the presidential papers.

Pitzer: But it's something that ought to get into the collection here, and I'd really like to see it, as a matter of fact.

LaBerge: So they essentially were waiting for you to get back to be dean.

Pitzer: That's right.

LaBerge: That you were the choice.

Clark Kerr and Three Departments of Biochemistry

Pitzer: Clark Kerr, as I say, I knew, of course, particularly at the time he was chancellor. One of the extra things he got me to do was, we had three biochemistry departments. [laughter]

LaBerge: In the College of Chemistry?

Pitzer: No, none in the College of Chemistry. There was one that was really a San Francisco medical school department. For many years the first-year medical students were at Berkeley and took their biochemistry in the first year physically at Berkeley, and then went to San Francisco. Then there was a biochemistry component of the College of Agriculture, which got into some pretty fundamental biochemistry, rather distinguished biochemistry. And then, Wendell Stanley was appointed with the project of

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[1]Clark Kerr became the first chancellor in 1952.

establishing an academic department of biochemistry, which he did. But they didn't always cooperate. [laughter] And there were some controversies.

Actually, there was not much involved in the medical department, because they were separately accommodated and everybody knew they were going to go to San Francisco eventually, so there really wasn't very much there. It was between the agriculture people and the Stanley people that were both physically in the building that has Stanley's name now, on separate floors or something like this. Then eventually, the academic department got a separate building. I don't remember really what the details were, but they really weren't all that difficult, except the personalities that were involved. Clark Kerr needed somebody that could sort of read the fine print and also had some feel for the personalities.

Relationship with UC Davis and Other Campuses

Pitzer: I'd had a close background with respect to [UC] Davis, too. Davis had been--this is the Davis campus--had been almost purely agriculture with just enough chemistry as required for undergraduate programs. But that was gradually developing, and as a part of the development of the multicampus, multi-general campus university, they clearly wanted to go into graduate work. We set it up so that, for an interim period of a few years, people could get essentially a joint degree, and students would usually spend, I think it was one year, either commuting to Berkeley or moving here and spending their full time here. And we'd get them a teaching assistantship here to help pay for it. And then maybe another year, they would commute for one day a week or something like that for a seminar or advanced course, one thing and another.

So I got pretty well acquainted with people at Davis more broadly, and that background was useful in other connections, but also in this biochemistry situation. My daughter Ann was later a student at Davis and graduated there. I don't know, you'd better make a special heading about that Davis relationship.

LaBerge: Okay.

Pitzer: Maybe put it somewhere else in the story, whatever you want.

LaBerge: Were you also active in helping some of the other new campuses set up their departments?

Pitzer: Yes, but not as much as Davis. Of course, in many cases, I'd get a letter or a telephone call from somebody, what did I think about, or who would I suggest for a candidate, or judge between two or three candidates, or something like that. In one or two cases, this became somewhat more extended in that there would be a multiplicity of these cases where I'd actually pay them a visit and review the situation more generally. That happened at least over a limited period of time at Riverside and San Diego and maybe at Santa Barbara; I don't have a clear memory of its happening there.

At Riverside, it was a little more substantial in that a good friend here in Berkeley, Robert Nesbitt, was--I guess he was dean of Letters and Science, or he was the chancellor at Riverside. Anyway, Nesbitt, although he was a sociologist, I'd known quite well, and he would ask for comments or discuss matters with respect to more or less the whole science side of the campus, not just chemistry.

Turning Down Administrative Posts

LaBerge: When we came to visit you just informally, you mentioned that there was a story about you possibly becoming chancellor, either here or then at some of the other campuses.

Pitzer: [laughs] Yes. I suppose maybe I ought to check the details. Well, I'll go ahead and tell the story as I remember it, and we can decide whether to redo it later.

This is now a time when Clark Kerr is president. I'm trying to think of what the schedule is. Anyway, a period just before Glenn Seaborg became chancellor briefly. Clark Kerr asked me if I'd be interested in the chancellorship. He went off on a trip to Africa, as I recall, and I sent him a message somehow saying that, well, I'd be willing to consider it, or would have some interest in it, particularly at Berkeley. Well, he decided to offer the Berkeley one to Seaborg, and Seaborg took it.

He then offered one of the other campuses, probably more than one of them--[laughter] obviously only one to be accepted--but by that time, I was negotiating with Rice about the presidency there, and about the same time, Glenn Seaborg was going to Washington as chairman of the Atomic Energy Commission. There was some maneuvering as to whether I might follow as chancellor at Berkeley, but I was not inclined to do that. I thought that one chemist right after another was not really a very good idea, and well, it obviously put me in as a second choice, too. But I had some backing from the Regents with respect to the Berkeley petition. There was some tension between Clark Kerr and the Regents. But I decided to go to Rice. So none of these others really progressed very far. There was no formal offer for any of them. But they were serious discussions.

LaBerge: You had some offers from other universities, too.

Pitzer: Yes, of differing--there are all sorts of levels of seriousness of these things, you know, where somebody of some stature at the other university asks you whether you would be interested and then maybe you get another inquiry from somebody who's probably closer to the decision-making center or group. I don't remember, but I undoubtedly got a number that I just turned down without more or less a second thought. In the years at Rice, roughly just before the time that I actually did go to Stanford, I was approached by MIT [Massachusetts Institute of Technology] and later by Caltech--of course, I was an undergraduate at Caltech.

The Caltech one came late enough that I was already so seriously involved with Stanford negotiations that I just didn't think I wanted to complicate the picture. And I really wasn't particularly interested in the MIT one. It's a great institution and I have very high regard for it, but one of the things that my wife and I learned after seven years in Texas was that we still had quite a little California in us, [laughter] and the idea of really committing ourselves to the Boston area for an extended period of time wasn't too attractive, even though it's an excellent institution. So I didn't let it go very far. But that one, I think, might have come through if I'd encouraged it.

LaBerge: And was there an offer from Dartmouth also?

Pitzer: If there was anything from Dartmouth, I didn't take it seriously at all. I don't really recall that. But there were ones something like that. In other words, whether it was Dartmouth or something else, some other place, there were such. There were special characteristics in the situation that made Rice attractive to me that we can go into sometime. It has a heavy emphasis on science, but it also is a more general university. Two of my own former students were on the faculty, and I knew the retiring president quite well, so there were special reasons why that was interesting.

Of course, the other great challenge about it was two major things that needed to be changed about it, which we've gone into, and those are well documented in the record, and a challenge like that is interesting. Well, is this about it?

LaBerge: Sure, should we end there, and then when you come back from your trip, we'll decide when we're ready to go into the other things?

Pitzer: Yes.

XVI COLLEGE OF CHEMISTRY GOVERNANCE

[Interview 13: February 4, 1997] ##

Faculty Selection Process

LaBerge: We thought today we'd talk about the College of Chemistry, specifically when you were dean in the fifties. And a little bit about how you make faculty appointments, the tenure decisions. Why don't we start with faculty appointments?

Pitzer: All right, well, faculty appointments are extremely important to the future of the program and the university in general. At that time, things were relatively informal, as compared to what they are today at the College of Chemistry in that the chairman and dean for chemistry--I was both--can essentially seek as much advice as he wishes, and depending on the particular subdivision and information available from recommendations, the feasibility of having an interview without undue expenditure, which would never be omitted, I think, now, but we did omit at some times. Those are all things to be considered. Actually, during my period the number of new recruitments was fairly modest. There had been a substantial expansion during the latter forties. After I took office in 1952, there were promotion decisions for these appointees but not too many open positions for new selections. But that doesn't mean that they're <u>unimportant</u>; everyone is important, but I handled them on a case by case basis in terms of the need for someone of a particular area of specialization. Not narrowly defined, but broadly defined in order to maintain a reasonable strength in various subdivisions of chemistry.

And then, depending on my travel schedule or that of others, I might--. Suppose one obtained two or three interesting candidates: If my travel, for other reasons--meetings, committees, and so on--made it convenient to interview them at their location, I would regard that as quite adequate. Or, if one of my particularly trusted associates were to do it, that would frequently be regarded as adequate. And by that I refer particularly to Professor [R. E.] Connick, who later followed me, of course, as dean, and was vice chancellor and all sorts of things later in his career here. I would have trusted his judgment essentially as much as my own. I don't mean to denigrate that of others, but I don't think the others were as skilled and wise about it as he.

In many cases, the person did visit and gave a seminar, but not always. I remember in one case Samuel Markowitz was actually in England at the time. I happened to be in London about the right time, not for this purpose obviously, but I was able to invite him from, I've forgotten whether it was Manchester or Liverpool, someplace where he was actually located. He came into London, and had dinner and visited.

I guess, in retrospect, the most important recruitment in chemistry was Andrew Streitwieser, who is a very top level physical organic chemist who is a member of the National Academy [of Sciences] and so on. I don't have any very distinct memories of that recruitment. I could look it up in the records.

LaBerge: Where did he come from?

Pitzer: He was a Columbia University Ph.D. but was then a postdoc for a year at MIT.

Next let's consider the chemical engineering side. Early in my period, chemical engineering was established as a division, and with a chairman. [Theodore] Vermeulen, who was the senior faculty member, was the initial chairman, but only, I think, for one year. For two reasons: one, he'd served as informal chairman for several years ever since he came because he was the senior chemical engineering faculty member; and secondly, because his interpersonal relations with either junior faculty there or with senior people around the university were not absolutely the best, and by this time Charles Wilke, although considerably younger, was regarded in the chemical engineering profession as much more "one of them." Vermeulen was a physical chemist who was only sort of halfway a chemical engineer, and this may be even the more important reason, whereas Wilke had been trained right from the beginning as a chemical engineer and was so regarded nationally and was beginning to gain general recognition. So it seemed really much better to make him chairman as soon as it could be done politely. And once that was done, I left the initial round of recruitment to Wilke.

One very important recruitment was John Prausnitz, who has been a wonderful colleague through the years for me, too. I can't claim any great detail about that, but his field of research was one that was close enough for me to recognize quite clearly, and I'm sure I had a good deal of confidence once I'd seen the evidence, and possibly interviewed him, although I don't remember that, but I had confidence that he was an excellent choice.

LaBerge: What was his field?

Pitzer: Well, he was in chemical engineering, but in the sort of borderline area of chemical thermodynamics and statistical mechanics as applied to topics of engineering interest, and of course that was to a considerable degree one of my major topics, too. And in fact it was at about that same time as I was developing a theory using corresponding states with the acentric factor, a method which has been adopted by chemical engineers worldwide. Thus, our fields were relatively close together. He again has the highest international standing now. There were other chemical engineering recruitments, of course, but I don't know that there is any need to discuss them in detail.

Well, I could probably say a little more about, not individuals, but in general. Most of these recruitments were, of course, at the assistant professor level. We'd ceased recruiting as instructors by now, but assistant professors were not tenured. The very important decision, of course, is made whether to retain the person permanently with tenure or to say, essentially, "We're happy to have had you, but we think you'd better look elsewhere for your own career." While the chairman has a leadership role in this regard, this is very much a decision of the already tenured faculty. After a few years the non-tenured member had become well known, at least to some of his colleagues if not all, had given various seminar talks, or had begun to receive national recognition, hopefully, and had a record of publications, and his teaching was also well known--.

LaBerge: For instance, would you sit in on each other's classes?

Pitzer: Occasionally. Not as a regular specified procedure, but particularly if the person were giving a large lecture class where you could [laughing] slip in the back row quietly, without drawing attention to yourself. I have done that, and I remember [Joel] Hildebrand doing that, too, to me once in class, and that was fine [laughs]. But some people were more sensitive to that, or reacted differently to that than others. I regarded that as very important, and there were one or two faculty that had really quite promising research to show early in their career, but seemed to me to be not really interested in their teaching, and I think it's not just skill in teaching, it's genuine interest in teaching that is really important. Because over a career, if you don't really enjoy teaching, and enjoy the relation with the students, even good skills tends to be rather artificial and not very effective. So, in one or two cases, where, without making a real issue about it, I'd just encourage somebody to go somewhere else. I think in one or two cases they got an offer--and the question is, should they accept it?

One said, "Well, now, I can't predict what the reaction would be if you turn it down and stay here, but it might be just as well if you went ahead and accepted it." And a comment like that is usually enough to indicate that you're not going to fight for them very hard. It might be the better thing to do.

I don't think I let any real superstar get away on that basis. One or two of them had actually quite distinguished careers in major universities. The person we did promote instead was at least as good, everything considered.

I should comment on [George] Pimentel. By '52 he was already a non-tenured faculty member having been appointed in 1949. Of course, he was a graduate student of mine, and I was very enthusiastic about him. He had a major career, not just in research but in all sorts of things. The big lecture hall was named for him, as you know, properly. He was a wonderful teacher at all levels, even for the big freshmen lectures, a great showman--much better than I, at that. There was no question about retaining him or promoting him. It was a matter of how soon is it appropriate to promote to tenure. In that case there was absolutely no question about it.

<u>Outside or Inside UC System?</u>

LaBerge: How much did you go outside this campus to look?

Pitzer: Oh, that's a good question. During the [Gilbert] Lewis period there had been practically no appointments from outside, except those that Lewis brought in when he came. Essentially everyone thereafter was somebody who had gotten their degree here. And this may have been all right under those circumstances at that time, and certainly the people were very good, but as a long-range policy it's not good, and in the immediate recruitments after World War II, [Wendell] Latimer went outside for the areas in which Berkeley had not been strong. This was something that he obviously should do, but he had, as it were, already here in the physical chemistry area, people such as Glenn Seaborg and myself, and Leo Brewer, and Robert Connick.

It would have been really foolish to have gone outside and recruited somebody else. They couldn't possibly, on the average, have done better, and furthermore, this avoided any sort of hiatus here. We were already familiar with the scene and prepared to give our full response. Now, of course, I've forgotten just when various of those recently named got tenure, but it was so early in this whole period, this was all past by the time I was dean. That had been done earlier.

Specific Hiring as Dean, 1951-1960

LaBerge: I have a list of a few people that you hired, and I'll just name them, if you want to say anything.

Pitzer: Okay.

LaBerge: Rollie Myers.

Pitzer: He's an academic grandson of mine, you know. He was a student of [William D.] Gwinn. I haven't mentioned Gwinn. Gwinn was one who--well, I should have put him immediately with Leo Brewer as someone who had been here long enough that it was not a matter of recruitment in the immediate post-World War II period. He's in physical chemistry, of course, too.

So, Gwinn was promoted to tenure, and he picked up a new area of research after World War II. It was based on World War II technology--microwave spectra--that was sort of a brand-new physical chemistry research field, and did very well in it. Although we'd never had it before, essentially nobody else had ever had it before either. It was taken up strongly in Harvard and other places after World War II, so that was very successful. Myers was a student of his. He was an attractive candidate.

LaBerge: Okay, well, William Jolly you've mentioned. Where did he come from?

Pitzer: He was an inorganic chemist initially from the University of Illinois. He had gotten his Ph.D. degree here with Latimer and then been at the Lawrence Livermore National Laboratory for a few years. He also had a professorial offer from the University of Illinois but preferred to come back to Berkeley. In later years he wrote a book about the history of chemistry at Berkeley.

LaBerge: Okay, fine. John Rasmussen.

Pitzer: He was a nuclear chemist with a Ph.D. with Seaborg. He was then a postdoc for a year in physics at Stockholm before his appointment here. He became more of a nuclear physicist than a nuclear chemist and spent most of his time at LBL. Thus, his contribution to the college was limited.

LaBerge: Frederick Jensen?

Pitzer: Jensen was an organic chemist with a Ph.D. from Purdue. He was recruited primarily by our most senior organic chemist, James Cason, who was acting dean when I was on sabbatical for six months, and by William Dauben--younger but more or less comparably senior among the organic chemist leadership. I assume that they must have been the primary recruiters in this case, although I was, of course, aware of it.

LaBerge: Norman Phillips?

Pitzer: Norman Phillips has a Ph.D. from the University of Chicago. I was fairly heavily involved in recruiting him. I was through Chicago frequently, and undoubtedly saw him there. His line of research was physical measurements at very low temperatures. This is an area that I was moderately familiar with, had been involved with, so that I certainly was able to recognize his qualities very easily. In a sense he was a replacement for Giauque. He was later dean of the college.

LaBerge: Oh, he was? I didn't realize that.

Pitzer: He's very active. He turned down the VERIP (early retirement) offer. He has made a major contribution.

LaBerge: Okay, Bruce Mahan.

Pitzer: Yes, he was from Harvard. I don't have any very clear memories of his recruitment, although I must have been involved. He was, of course, a great contribution and served a term as department chairman. He died prematurely of Lou Gehrig's disease. Very sad story.

LaBerge: Okay, Ignacio Tinoco.

Pitzer: Yes, that was a move in the direction of getting someone in what we call biophysical chemistry. In other words, with physical chemistry capabilities, but focused more specifically on biological problems. We have a very active and distinguished group in that area, and he was the beginning of it. His Ph.D. is from Wisconsin and he was a postdoc at Yale. Tinoco has made a very important contribution.

Harold Johnston and Dudley Herschbach

LaBerge: I just picked these out, just kind of to see if it jarred your memory. Harold Johnston?

Pitzer: Well, that's an interesting story. That is true, that's within my area, isn't it? [laughs] That's quite a story. I'd known him slightly from research during World War II. Research on gas cloud behavior, which was precautionary against possible chemical warfare. I'd been in it for a period with Gwinn, and then when I went east for a different research activity, Gwinn took over the role here under Latimer. Johnston was then affiliated with Caltech. A very able physical chemist specializing in reaction rate measurement and theory.

We had the practice of inviting someone that we might want to get acquainted with further, to teach a fairly light-load summer session course, plus give a seminar in his own field of interest. We had several people in on this pattern over some of the years that I was dean and chairman, and this must have been in the mid-fifties. He was at Stanford at that time. It turns out that we had a very close overlapping interest in statistical rate theory using my background and capacity with respect to the internal rotation potentials, and in general potentials associated with activated complexes. And his interest in prediction or evaluation of some of the rate measurements that he was familiar with or had made himself.

And so, we began, we started a little collaboration and he said, "Well, we'll make some more detailed calculations, and I've got a student over at Stanford, an undergraduate student who is very bright, and I'll urge him to go ahead and make some of these calculations and come over and visit and get advice as to details." And actually Richard Powell, who was with us then, was also involved.

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Pitzer: The paper that we published was really quite important. I regard it as Harold Johnston's paper, primarily. I did not put it in my volume of selected papers because I thought this was primarily his.

The other part of the story is that the student, who was named Dudley Herschbach, who went on to Harvard for a Ph.D. and stayed at Harvard for a while, came here for a while (which I'll come back to) then went back to Harvard, and eventually shared a Nobel Prize with Yuan T. Lee. Well, Herschbach and I started a personal friendship, not terribly close, but a warm friendship in that period. As luck would have it, or whatever, Herschbach showed up here yesterday as a Miller Professor for four months, so our acquaintanceship will get reinforced again. Well, let me continue about Herschbach for just a moment.

LaBerge: Could you spell his last name for me? You see, I'm not as familiar with--.

Pitzer: [spells name]

LaBerge: You were going to say something more about him, I think. Either what his paper was about, or--.

Pitzer: Well, no, I think that's enough about the paper. After he finished his degree at Harvard, we got him here as a junior faculty member, but Harvard apparently had a stronger attraction on him, so he went back to Harvard after only two or three years. He was here at the time I went to Rice as president so that we overlapped. I was no longer dean, although I may have been involved with offering him an appointment. Connick was dean, by that time. But there's an odd little item maybe worth putting in. I want to come back to the question in a moment.

Now one of my major things during my deanship was getting new buildings. We can come to that later. The plans for this building, Hildebrand Hall--down four doors from this office on the same floor, there's a room that, in the early planning stage, had my name on it as a professorial office. I was probably no longer dean at the time it was written, but I hadn't gone to Rice yet. As I understand it, when I left, Herschbach was looking very attractive as a faculty member, permanently. His name was written on it. Then he went to Harvard and never occupied the room. The third name that was written on it was Mahan, who did occupy it--became department chairman and was a research director for Yuan Lee who shared the Nobel Prize with Herschbach, whose name had been on the room, but had never occupied it. [laughs] So, interesting interplay--.

LaBerge: Right. Who's in it now?

Pitzer: Paul Alivisatos is in it. One of our very promising young people that Harvard tried to hire, but he decided not to go.

LaBerge: In those days, was salary an issue the way it is today?

Pitzer: Well, it was always somewhat an issue, yes. The numbers were a lot smaller than they are today, but I think it was about as much of an issue. Not an overwhelming issue. In other words, any institution that was going to be in the first rank had to have more or less competitive salaries, but there are other attractions. People don't move just because the other place offers them a <u>little</u> more money. On the other hand, if the offer is enormously more money, it's pretty hard to turn it down. Through the years, one had to keep pushing the president's office and the Regents and to keep the salaries competitive, but I don't think we lost people very much on that basis. Now I want to come back to Harold Johnston.

Shortly after this episode involving Herschbach, he [Johnston] was coaxed back to Caltech where he'd been a graduate student, and where he had been when I knew him slightly during World War II. It was for a rather specific position. I never quite fully understood, but it implied some restraints in terms of what his teaching assignments were, or something or other. He wasn't very happy with it. So, I remember that he called me up, got me at home, and said he was not very happy with his Caltech position and could we do something about it. I said, "Well, as far as I'm concerned, yes, by all means. It will take me a little while to arrange it."

Now, this was rather late in my deanship (1957). In this case, one didn't need any further interviews or anything. He's well known and an absolutely superb member of the faculty, and was dean for a while. A great international reputation. He is the one that more or less killed the supersonic aircraft as a passenger transport, internationally, on the basis it would contribute to the stratospheric ozone depletion problem.

Filling Vacancies and Making Promotions

LaBerge: Now in a case like that, how would you get his appointment through? I'm assuming you did it more informally than you would have to do it today.

Pitzer: Yes, I can add a little more about that. The department and the college, through the years, has followed the pattern of having several junior level appointment staff positions--regular faculty, but as assistant professors, of course, that used to be instructors. Some would be promoted, and some would not. If they were not promoted, then there was an effective vacant position on the budget. I'm quite sure in the case of the Harold Johnston appointment it was not a matter of finding a position for him, it was a matter of getting a position upgraded from an assistant professorship. For a person of sufficient promise and distinction, this university has always been very good about that.

Now, if the department had gone out of reasonable distribution in terms of wanting everybody to begin at the senior level, well, I'm sure the central administration--the chancellor or president--would have been reluctant about it. But, since we had a certain number of retirements, of course, and occasionally somebody would leave, and there was still some expansion going on. So, at least during my time, there were always several junior level people with some not getting tenure, and so on and so forth. You could put somebody in.

LaBerge: Obviously, from what you said, you didn't do it just within the department. You went through the chancellor? Or who did you go through?

Pitzer: Well, to get either a promotion or a senior level tenure appointment, the chairman simply recommends it with all the supporting evidence. On this campus--this is rather special--there's this Academic Senate Committee on Budget and Interdepartmental Relations, which is really in large measure, a committee on academic personnel. They appoint a special committee for each case, or occasionally for a couple of cases if they're very similar.

In this case, for example, I would have had to recommend it. The budget committee would have had to have appointed a special committee for the Johnston appointment, and then that special committee would have reported to the budget committee, who would have reviewed it and commented on it to the chancellor. The chancellor would have had the authority to approve, in this case, both the upgrading of the position and the naming of Harold Johnston.

Most of the major first-line universities have something somewhat similar to this. The budget committee here is more independent of the chancellor or the president than in many other institutions. The same sort of evaluations take place, but there's more independence in the selection of the members of the committee, and so on.

LaBerge: How often does the chancellor or the president disagree with what the budget committee proposes?

Pitzer: Well, we've had very few cases of any disagreement, at least to my knowledge, and certainly during my time. Nor have we had essentially any appreciable number of disagreements with the chairman's recommendation. On the other hand, I never resented this procedure because I thought it was important to the standing and quality of the university generally, which we're interested in. I thought it was appropriate, too.

The chairman's recommendation would include letters of recommendation from others outside this university, and some type of documentation from others within the department here, or possibly from other departments here, if somebody was familiar with the case. This is somewhat more formal in terms of the number of letters of recommendation now than it used to be; but that's a relatively minor change.

LaBerge: And are you still involved in that at all?

Pitzer: Oh, yes. I see these cases. For example, if the chairman is recommending a given action, why, the tenured faculty or senior or full professors are invited to come examine the situation and comment to the chairman if they want to. If they think he's wrong, they can say so.

LaBerge: Okay. Do the Regents ever have anything to say on this?

Pitzer: This has changed through the years as to how high the level is at which a decision actually goes to the Regents. I don't think the appointment of a full professor has gone to the Regents for many years, either the president or the chancellor would sign off on that. If the salary is high enough, so that it's a salary above the sort of standard salary scale, then there have been times when that went to the Regents, although I don't think it does now.

For administrative positions, say a deanship, things like that, that would normally go to the Regents, although they wouldn't ordinarily pay any attention to it. They'd just say yes. If it appeared to be controversial, they might really look into it.

Other Recruits

LaBerge: I have a couple other names. You spoke about Sam Markowitz. David Shirley?

Pitzer: Oh yes, David Shirley was a student of [William F.] Giauque here. He later had a major role in the Lawrence Berkeley Lab, or Lawrence Berkeley National Lab, as it's called now. He was director for a while. We were--became quite close personally, through the years. I think his time must have been rather late in my deanship, but I'll check. (He was appointed in 1960.)

LaBerge: Well, his is the last on my list, so I think I must have found it chronologically.

Pitzer: Fairly well through his career, he went to Penn State as an academic vice president or vice president for research or maybe both, and has now retired--is back living here, although I haven't seen him around the university appreciably. He may be around up the hill at LBL.

I had an interesting experience with him recently when he was at Penn State. We were doing the biography of Professor Giauque for the National Academy of Sciences, and Shirley is an Academy member, too. Since he was the one that had been Giauque's student, although I'd known Giauque very well, I kept pushing him into doing some work on it. I'd already done the shorter biography for the American Philosophical Society, and helped with the one just within the university, here. I thought he ought to work on it a little bit. Finally, I got him to spend a little time on it, and we got it done. That was sort of fun. He's a very good person, and one that I enjoyed. This was really fun, too. That's the end of the list?

LaBerge: That's the end of my list of people that you hired. That doesn't mean that's all. That's just the ones that I found.

Pitzer: I think that's probably enough. Well, I'm afraid this is going to be pretty unorganized--this section.

Informal Student Evaluations

LaBerge: Oh, I think that--we're kind of focusing just on faculty and how you find them, how you hire them. Anything else on that? For instance, would you get the student evaluations, to evaluate the teaching?

Pitzer: Formal student comments--that came on later than my period. That doesn't mean that one didn't try to sense this, particularly if the person were somebody you pictured as giving large freshmen lectures, and so forth. You might informally have an occasion to be talking to a student, engage them in conversation or something like that; but we didn't have formal student evaluations, no.

New Buildings: Latimer and Hildebrand Halls

Financing

Pitzer: The other subject, I don't know if we want to go into at all, was the question of new buildings.

LaBerge: Okay, do you want to do that today?

Pitzer: Well, we might have a shot at it.

LaBerge: Okay.

Pitzer: As I saw it, in 1952, although it was only a start on providing the additional physical facilities that the college needed. So, this was really my number one priority, to get major new buildings; and this, of course, involves working with the top level administration, and with the Regents. We got an appropriate priority position, in terms of getting state funds. In those days, getting state funding for the entire cost of the building was feasible. It doesn't seem to be anymore. You've got to get private donations for most anything. But it was a time when the state was doing reasonably well, so that it was feasible to go after substantial funding. So, that's what I did.

LaBerge: Did you go up to the legislature yourself?

Pitzer: I never actually went to Sacramento, but I remember being invited to escort a member of the legislature, important to the university, who was going to attend the Charter Day ceremony or the commencement, which in those days was a big thing, you know, in the football stadium--one end of the stadium. I remember [laughing] marching the legislator through what was then the old chemistry building, which had been built before the turn of the century, you know--a building that obviously was obsolete [laughs]. How much of this had anything to do with getting the approval or not, I don't know.

It meant at least some real contact with the Regents, and there were various ways in which this was maneuvered. So eventually we did get the approval for what was contemplated as a three-unit sequence with the largest unit, including an auditorium to be first. The second unit was to follow, more or less immediately after the first, but it was important to do it in sequence because an important old building must not be torn down until you've built the first one [laughs]. Then the third one was to be primarily for chemical engineering as it gradually developed and grew. That's the one that we dedicated this year.

LaBerge: Oh! You started way back then. Was that in the plans?

Pitzer: It was in the plans. It should occur.

LaBerge: Wow, so what buildings--?

Pitzer: So, Latimer Hall was the one that was the major first step, and that was designed carefully. Leo Brewer had a major role in helping design it, in terms of appropriateness for the physical chemistry community, and William Dauben correspondingly for the organic community.

We had one setback in terms of financing in that the first time we went out to bid, the bids were over the appropriation by just too much to bridge. So we had to scale back the design a little bit, and try to get the state to raise the appropriation a little bit. By that time the bidding climate, by accident, had improved, so we actually had a little money to spare, which I guess we got to use for equipment. I'm not really sure about the details now. We never moved into it while I was dean, but it was so far along the way that it was completely assured. Then, this building we're in now, Hildebrand Hall, was on the site of the original chemistry building. It was designed by the same architects to follow on, hopefully, more or less immediately.

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LaBerge: Okay, who were the architects? Do you remember?

Pitzer: Anshen and Allen. I was very pleased with them. They did a good job. I was surprised by the rather significant difference in architectural styles between the two buildings, but they fit together usefully.

Auditorium for Freshman Lectures

LaBerge: As a part of this building, did you work with the campus architect also?

Pitzer: Oh, yes, we had to work with the campus architect, and, as I say the details were delegated mainly to the two people I mentioned.

LaBerge: Leo Brewer and--?

Pitzer: And William Dauben, but others in the faculty participated in that.

There was another addition that had to be made, and that's the large auditorium we use for the freshmen lectures. It was decided that should be joint with physics, and was called the Physical Sciences Lecture Hall until it was named George Pimentel Hall. That was part of the first stage, and I don't remember whether it was funded separately, or whether it was funded as part of the Latimer Hall project. I presume the same architect handled it, although I'm not explicit in my memory there.

I had occasion to recall some of this because [laughs] the question came up recently of naming the lobby of the lecture hall for Harvey White, who was in physics and was on the committee that designed the lecture hall, and had some role in the very clever rotating demonstration unit or stage, which had three positions so you can set up demonstration experiments back in the preparation room, then rotate the stage out for your lecture, then rotate it away again for the lecture immediately afterwards. Without that it was not feasible to have lectures without gaps in between lectures for the preparations for demonstrations, and so on.

So it was a real contribution. How much other people contributed to it, I don't know, but I was asked to evaluate the appropriateness of this recognition of Harvey White. So I had to look it up to some degree, and found out that actually, Richard Powell, who was the chairman of the committee that was involved with the design of the auditorium; and he was the one that was then giving freshmen chemistry lectures. So I'm sure he must have had something to do with it, too; but he died many years ago now, and Harvey White isn't living either. Nobody seems to remember any further detail. As long as it's kept modest, and as long as there's some recognition of Powell, as well as Harvey White, why, I think it's all right to have some recognition of Harvey White. I passed that word along, and I think it's all going to work out.

Naming Process

LaBerge: Now the other names--I mean, they're obvious why they were named Latimer and Hildebrand, but did you have to vote on that?

Pitzer: Latimer died, and there had been no name assigned to it and I recommended immediately that Latimer be recognized with the name, and there was no controversy about it. After all, he was really, more than anyone else, responsible for the whole nature-scope plan of the college, including chemical engineering.

The next stage was really decided after I was out of the deanship, although I probably was involved, and certainly I had the highest regard for Hildebrand. He was living; indeed he lived many more years. Naming it for somebody still alive was a bit novel, not totally unprecedented, but unusual. Certainly I'd not the slightest objection once we talked that through, and thought it most appropriate in terms of the person. So that was done, but I think Connick was dean by that time. As a part of those moves, another still very useable building, Gilman Hall, which had been my office as dean, and so on, was turned over to chemical engineering in full. They got individual space in Hildebrand, very substantially, too.

There is one additional building that might be mentioned, and that's the purely research--specialized research--space, named for Giauque, that predates these others. I guess that was done during my time as dean. Some special funds were obtained--I'm not sure just how, now--for this very unusually designed building to allow certain special experiments to be done. Giauque, by that time, had won the Nobel Prize, you know, and was widely acclaimed. People wanted to let him do whatever he wanted to do. That's the building that is between the others. It was built with Gilman on one side, and Hildebrand was built onto it on the other side. Yuan Lee was able to make use of these specialized spaces. Norman Phillips also has part of Giauque Hall. In terms of the general needs of the college, that was a footnote, almost, in that it was so highly specialized.

Well, I think this was one of my major accomplishments as part of the deanship--was getting the buildings well on the way. So that wasn't so much a matter of expanding the faculty a great deal further, but a matter of having <u>space</u> for them to expand their activities--increase number of graduate students, particularly increase number of post-doctorals, other research personnel, and a modern pleasant space, both for classes and a lecture hall, and so on.

LaBerge: Did you realize that when you came into the deanship that was going to be one of your tasks?

Pitzer: Yes, that was one of the things that I saw as dean as a major need. In a sense, selecting good faculty is very important, but it's the sort of thing you keep doing and is within the pattern that you understand. Trying to get major new buildings [laughs] is not so ordinary. Therefore, that took some special thought and arrangements and so forth.

One could go on in various directions here. I think this is a good time to stop.

LaBerge: A good time to stop? Okay, then we'll pick that up. I want to ask you more about Regents meetings--.[1]

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[1]Unfortunately, no more interviews were held before Dr. Pitzer's untimely death in December 1997. The Appendix includes Dr. Pitzer's notes for future interviews.

INTERVIEW WITH JEAN PITZER

XVII BACKGROUND IN POMONA AND UC BERKELEY, 1935-1960

[Interview 1: March 3, 1998] ##

Meeting Kenneth Pitzer in Pomona Schools

LaBerge: We thought it would be good to have a little personal background. I think that you grew up in Pomona. Why don't you tell me a little bit about your growing up and your family?

J. Pitzer: I was born in Pomona, California.

LaBerge: Do you mind telling me the year?

J. Pitzer: September 1914, the same year my husband was born. His birthday was in January 1914. I had two older sisters. We were all just a year and a half apart in age. My older sister Constance was a brunette, my middle sister Mildred was a blonde, and I was a redhead. Actually my hair was a dark auburn--and brown eyes, and I had a complexion usually associated with red hair. I thought I had more than my share of freckles during my pre-teen years. Much to my delight the freckles disappeared in my early teen years.

LaBerge: Did you go to all Pomona schools?

J. Pitzer: Yes. I started kindergarten when I was four years old. This was permitted in spite of the customary five-year admittance age because it was wartime--World War I, and my mother was in charge of a Red Cross sewing group. It was essentially a child-care situation. This meant I had two years of kindergarten! I didn't become bored with it--I loved going to school--just like my sisters were going to school.

My mother had taught me to read before I ever entered kindergarten. This resulted in my skipping the second semester of the second grade because I already knew how to read and write and I was promoted to the third grade then. This eventually had a very felicitous result in the future--although of course I didn't realize it then--because I was then in the same grade as Kenneth Pitzer throughout our school years in Pomona.

I first met Ken in the third grade, I think. He had been taught at home through the first grade, I believe. His mother had been a teacher and was a mathematician. That's the first I remember. We went to the same elementary schools near my home and also near his home. In the third grade, my aunt, Ella Mosher, on my father's side was a teacher. She taught us both in the third grade. And in the fourth grade his great-aunt, Effa Kelly, taught us.

LaBerge: I think he mentioned her name, but I didn't realize that she taught you both.

J. Pitzer: Yes. And we went through fifth, sixth, and I guess the seventh and eighth grades together, but without any particular reference to each other [laughter]. His Aunt Effa later taught us in the seventh grade geography class. She loved to travel.

LaBerge: Pomona must have been a very small community.

J. Pitzer: About 30,000, surrounded by orange groves at that time, not subdivisions of houses as it is now.

LaBerge: So when did you really become friends? In high school?

J. Pitzer: Yes, it had an enrollment of about 400. Everyone knew everyone else. I had spent a year--the tenth grade, I think--in Ventura. My father had been a teacher in the Pomona High School. He taught what we call mechanical arts now, but they called it shop then. My mother was also a teacher in the junior high school in Pomona. My father had gone to Stanford University during summer sessions and received his degree during summer school. In 1923, when I was eight years old, my father attended the summer session at Stanford and my family spent most of the summer in Palo Alto.

We went to the Stanford Museum, which at that time primarily contained Mrs. Stanford's collection which she had acquired in Europe. I was enthralled. I realize that the Stanford collection has been downgraded by museum directors and art critics as second class, but it was able to impress an eight-year-old with the interaction of art and emotion with history--something that an exhibit of contemporary art could not do. I think it was the only museum able to do this on the West Coast at that time. I think I read every description and every date on all the labels. I asked to be taken back several times before we left Palo Alto. A few years later my father graduated from Stanford and I spent another afternoon in the museum.

My father was appointed vice principal of Ventura High School. My middle sister, Mildred, and I went up there and kept house for him and went to high school there for a year. Then he was offered the principalship in Beaumont, California. We returned to Pomona to finish our high school. The year in Ventura was a period when Ken and I weren't acquainted.

When I went back to Pomona High School as a junior and senior, there was a group of friends that I had previously known that went around together. I dated other boys in my junior year occasionally; I don't think I dated before my junior year. Ken and I had most of our classes together. The first time Ken asked me for a date was when we were juniors. There was a miniature golf course across the street from the high school, and he asked me to go over there during the lunch hour [chuckles] and play a game of miniature golf, which we did, and that was fun.

There was a group of six or eight that would go out together--have dates to go out to dinner and dance and that sort of thing. I remember one date that was lots of fun. I think it was Glenn Miller that was playing at the Ambassador Hotel in Los Angeles. They had tea dances then in the late afternoon [laughs], and so we went to a tea dance there, which was fun. But the lunch hour golf game was the beginning. Our group also went for day-long hikes to the top of nearby mountains and also day trips to the beach or desert--Palm Springs--which then was just a few palm trees around the springs.

LaBerge: What about college? Had you always thought you were going to go to college?

J. Pitzer: Oh, yes. My parents were very strongly in favor of that, or just planned on it. It was rather a struggle because that was the Depression era, and our family was living on two schoolteachers' salaries. My two older sisters went for two years to the Santa Barbara State College; it later became the UC [University of California] Santa Barbara campus. That was also where my mother got her college degree and teaching credential. And then they finished up at Pomona College their last two years.

All three of us were just a year apart in college; to have three girls in college at one time was quite a struggle during the Depression. They were both at Pomona College in their junior and senior years, so I went to Pomona Junior College, which was located at the Pomona High School, and later became Mt. San Antonio Community College. That was the beginning of the junior college development and most school districts had one. The community college concept grew out of the junior colleges. It was good. It was a small student body located in the high school, but it had some excellent teachers. I felt that was a very good experience for me. We knew everybody. All my friends played on the same hockey and basketball teams and other organizations. I was also editor of the weekly junior college newspaper and also president of the Associated Women's Students.

LaBerge: These are girls' hockey and basketball teams?

J. Pitzer: Oh, yes. Even the girls' basketball game was entirely different in those days. The court was divided into three sections, and there were two players in each section, and you couldn't go over the boundaries; they didn't think girls were capable of playing the full court at that time [laughs]. I was playing running center on the basketball team, and I had a lot of fun doing it. We played teams from schools in nearby towns --Ontario, Redlands, Riverside, San Bernardino, Anaheim, et cetera. And I loved the field hockey games.

LaBerge: I used to play field hockey too. What did you play?

J. Pitzer: Center half. We won the league championship in 1933.

LaBerge: What did you major in in college?

J. Pitzer: In college, sociology. After I graduated from junior college, I went to Pomona College to finish up the last two years. We dated the last year in high school and through his first two years at Caltech [California Institute of Technology] and my two years in junior college. We dated each other exclusively the last two years of college. He would invite me over to Caltech dinners and dances and come and get me--it's only thirty miles, you know--and take me home afterwards. Also I invited him to Pomona College events. In those days college girls had to have permission to stay out after the lockup, which occurred at ten o'clock or something. You could only have three or four late-night permits a semester. Quite different than what it is now. However, since my parents' home was in Pomona, I would check out to go home for the weekend and could get home late from frequent dances at Caltech.

The senior year of his college undergraduate work at Caltech, he applied for graduate work here at Berkeley. I don't think he applied elsewhere; he wanted to come to Berkeley. He had very good recommendations because of his excellent scholarship record at Caltech. I think he applied probably the first of January, and two weeks later he got a letter from [Dean of College of Chemistry Gilbert Newton] G. N. Lewis accepting him [laughs], which was quite unusual because the deadline--they weren't supposed to accept graduate students until March or sometime. After he received this acceptance to be a teaching assistant and his acceptance into graduate school, he phoned me--I was living in the dorm my senior year--and told me that he had been accepted. Our engagement was announced soon thereafter. We knew then that we would get married as soon as we graduated in June, which we did. We both graduated in June 1935 and were married on July 7, two weeks after graduation.

LaBerge: That was quick [laughs].

J. Pitzer: Well, we had to be up here for the fall semester in August for his graduate work.

LaBerge: This was 1935?

J. Pitzer: Yes. I was twenty and he was twenty-one when we married.

Graduate School at the College of Chemistry, 1935-1937

J. Pitzer: We rented a studio apartment down on Etna Street south of the campus, within walking distance of the campus. A year later we rented a somewhat larger apartment on Hilgard Street which had a small room for our first child and also was near my sister Connie.

LaBerge: What did you think you were going to be doing--both with your degree or when he was a student at Berkeley?

J. Pitzer: I rather thought I would be getting married [laughter] the last two years, and I didn't plan to be a career woman. There was no necessity to do so for financial reasons. The only employment for sociology majors at that time was in social service, in Roosevelt's "New Deal," so I imagine I would have gone into that if other things hadn't worked out the way they did. Remember, it was hard enough for men to get jobs during the Depression, and men with families to support were favored over women. But my parents believed strongly that a college education was of value even if there was not a paycheck attached to the diploma.

LaBerge: What were your first impressions of Berkeley? What did you do as a graduate student's wife?

J. Pitzer: It was a somewhat smaller community then and a smaller enrollment at UC. We loved it. Once a month we would take the train, then ferry, to San Francisco and have dinner and see a show. My oldest sister, Constance, and her husband were here at the time. He was a graduate student in economics.

LaBerge: And what was his name?

J. Pitzer: Arthur Browne.

LaBerge: Constance and Arthur Browne.

J. Pitzer: So that was very nice. When we first arrived in Berkeley after driving up with all of our wedding presents in our rather old car--it was a 1928 Pontiac sedan that Ken had been driving all during his high school and college years; we loaded all our wedding presents up and drove north--for the first few days we stayed with his aunt and uncle, Professor and Mrs. James Allen. He was a professor of Greek in the Classics department. We stayed a few days with them until we found our own apartment shortly thereafter. It was very wonderful to have my older sister here. We did our shopping together. They didn't have a car, and I hadn't learned to drive; I didn't have my driver's license until shortly after I arrived in Berkeley, but she did. So we would go shopping together in our car and do our washing together and things like that. Her apartment had a coin-operated washing machine, and ours didn't. This of course was an old-fashioned washing machine with a wringer, not an automatic. It took all morning to do a wash and hang it up to dry.

LaBerge: You were in Berkeley from then on until you went back East during the war, is that right?

J. Pitzer: Yes. In 1938 we bought a small two-bedroom house in Kensington on Avon Road, not far from where we live now on Eagle Hill.

When we first arrived it was still during the Depression --1935--and not many of the graduate students were married. As other members of the faculty who are now on the faculty have said, the year that Ken entered graduate work in chemistry was an exceptional group. There was Glenn Seaborg and [Willard F.]

Bill Libby, both of whom got Nobel Prizes later, and [Samuel] Sam Ruben, who co-discovered carbon-14 and would have received a Nobel Prize for that if he had lived. A really exceptional group that were all studying for their Ph.D.s. (I have given you an article by Glenn Seaborg about their sixty-two-year friendship.) [see Appendix]

LaBerge: All under--

Faculty of the College of Chemistry

J. Pitzer: Different members of the faculty. Ken's faculty advisor was Professor Wendell Latimer. Ken received his Ph.D. in two years, which was rather unusual.

LaBerge: That's for sure.

J. Pitzer: It was more usual in the sciences, I think, than in the humanities. He was immediately appointed as an instructor. He received a fellowship as soon as he got his Ph.D. too. In those days teaching assistants were paid six hundred dollars a year, and we lived on that, except for the expenses for the car which we paid for out of savings, out of our bank account. The additional income of an instructor's salary was welcome. We lived on that salary although it was not necessary for us to do so.

LaBerge: So how involved did you get--or when did you become involved as a faculty wife?

J. Pitzer: The first occasion that I remember after we arrived was going to the [Joel] Hildebrand home out here in Kensington, just down the hill from Eagle Hill. They had a large house with a barbecue pit out in the garden. They had a large garden. They invited all the graduate students and their wives--there weren't very many married students then--and there weren't very many graduate students either; there was only a total of about thirty, I guess, at that time. Including the wives. They invited us out for a supper. It was a very congenial group.

I became acquainted with the other wives of graduate students (only three or four), and when he [Ken] was promoted to instructor, he had a graduate student who was married--Lowell Coulter was his name. We were very close to the Coulters. Sam Ruben was an instructor and married to Helena Ruben. And there was one other instructor who was married, Edward Lingafelter--he later went to the University of Washington--on the faculty.

Chemistry Teas for Faculty Wives, 1930s to 1960s

J. Pitzer: That group of ours got together every month or so, and we invited all the other married graduate wives. We would meet maybe once a month at each other's houses or apartments. And that gradually developed into what became known as the Chemistry Teas. You probably haven't heard about that, but it became quite an institution in the chemistry department.

After a year or so of meeting together, we younger people in the department invited Mrs. Hildebrand. The Hildebrands, of course, were always congenial and interested in graduate students. And Mrs. Latimer and Mrs. [Ermon] Eastman and Mrs. [Axel] Olson. They were the faculty wives we saw the most of. We invited them to meet with us, and it gradually developed that the older women in the chemistry department would entertain us all in their homes because their homes were larger. Gradually it developed into the whole chemistry department--all the chemistry wives met once a month. It developed into a very enjoyable association which lasted through the sixties, anyway. There would be parties in the afternoon for thirty or so--all the wives in the chemistry department meeting once a month. It was a very congenial group.

LaBerge: And then when there would be new faculty coming on board you would invite the new spouses?

J. Pitzer: Oh, yes. We made a point of including new people even if they were only visiting. Especially if they were visiting, to get them acquainted.

When we went back in the late forties to Washington during the war, Mrs. Hildebrand--Hildebrand was dean then--took over the management of the teas. She later told me that other departments told her they were envious of the chemistry department [laughter] because of their congenial association. It was very congenial. My best friends were chemistry wives. That was what I missed when I was the wife of a president. You can't show partiality for any friend in that position.

After we came back from Washington and Ken was appointed dean, I again took over the management of the teas. We still meet two or three times a year, but the younger faculty women are too busy now. Even the young wives of faculty members work, so it's usually just a few of the old-timers that get together. We got together a few weeks ago.

Another thing that was started when Ken was dean was a reception--cider and doughnuts--for all of the graduate students and all of the faculty and their wives. It was held on the chemistry terrace between Hildebrand and Latimer Halls. That would be at the beginning of the school year.

LaBerge: Did you have activities too?

J. Pitzer: Oh, there were all sorts of activities--in the chemistry department, do you mean?

LaBerge: Yes.

J. Pitzer: Yes. There were guest lecturers and dinners following guest lectures. But the University Section Club with its many active sections was also a source of friendship. And Ken's aunt Amy Allen--his mother's sister and wife of Professor Allen--sponsored my attendance to the Section Club and college teas.

Collegiality of the Chemistry Department

LaBerge: During those years, who would you say were your husband's mentors?

J. Pitzer: When he was a graduate student?

LaBerge: Yes. Besides Professor Latimer.

J. Pitzer: Wendell Latimer was not only a brilliant scientist but he was the best and most astute judge of character whom I have ever known. Also if any graduate student had a personal or financial problem, he would go to Latimer and was always helped.

The chemistry department I guess is unusual--I hope not too unusual--for being very collegial and cooperative. We were fortunate enough to inherit that sense of collegiality when Ken became dean. Some of the younger new people that have just joined the department in the last few years have mentioned it, that it's still there. I'm delighted that it is, and that it has been maintained. The way the current new young faculty express it, there's respect for each other's opinions and they're interested in each other's work even if they're unfamiliar with or don't work in that particular field. They stimulate each other. People interact very agreeably here in the department. I'm just delighted that that's been maintained through all these years.

Ken especially interacted with Professor [William] Giauque. Giauque's lab was just around the corner from Ken's lab in Gilman Hall. Professor Giauque was a very strong-minded person; very easy to argue with. However, if you stood up to him, which Ken did, he respected it [laughter]. He enjoyed his association with him. Ken wrote the obituary of Professor Giauque and also Professor Hildebrand for the National Academy of Sciences. I am sure you have them on file. And Ken always felt particularly close ties to Professor Hildebrand. In later years, in the 1970s when Hildebrand was emeritus and we had returned to Berkeley, he and Ken had research interests in common in solution chemistry and collaborated on some papers. And earlier Hildebrand had recommended Ken to be assistant dean of Letters and Science in 1947-1948.

All the older people were very helpful. I remember Professor and Mrs. Gerald Branch used to entertain all the graduate students and their wives for Thanksgiving dinner. Of course there weren't the numbers that there are now. It would be a matter of thirty people or so, I guess. They cooked two big turkeys. The [G. Ernest] Gibsons and Eastmans were very interactive with the young people. All of the professors were; they took a very deep interest in the graduate students. The older group--Lewis, Hildebrand, Latimer, Gibson, Branch, [T. Dale] Stewart, [Gerhard] Rollefson--all of them, the older group, used to meet several times a year and have a formal dinner together. Black tie. They would take turns in various homes and play bridge. Ken was the first young faculty member appointed and invited to this older group, this older generation. And we were invited to play bridge with this group. It was a very nice association. These parties ended with the beginning of World War II--no one had time for them then.

[Ken was also a member of the small group of chemistry faculty who would play hearts at the Faculty Club every day after lunch. The group included G. N. Lewis, Wendell Latimer, and I think George Gibson--maybe also Gerald Branch. It was a very "cut-throat" game and Ken would sometimes come home saying he had won that day--which would please him.

When he was promoted to the rank of associate professor, he was elected to membership in the Kosmos Club. As you probably know, this is a club composed of members of the faculty from all different disciplines. They meet once a month for dinner at the Faculty Club and one of the members would give a talk about his field. Kenneth enjoyed that very much also.

When we returned to Berkeley in 1971 he was readmitted--or perhaps his membership had never lapsed. It was rather a select group and he considered it an honor to belong.

During the 1950s he was elected to membership in the Bohemian Club and invited to join the encampment which had many members associated with UC Berkeley--President Robert G. Sproul, Governor and later Chief Justice Earl Warren, Regent Don McLaughlin, Roger Heyns, President Charles Hitch, Professor Charles Townes. He enjoyed that association very much too. When he became professor emeritus in 1984 he felt honored to be chosen as a member of the Berkeley Fellows.][1]

LaBerge: How did that then translate when you became the faculty wife and your husband had graduate students?

J. Pitzer: When we lived in a small apartment I used to send over newly baked cakes and apple pies and cookies when they had to work at night. We entertained the graduate students and their wives. We had two young babies then. We had a home out here in Kensington that had a patio with a fireplace outside, and I think we would mostly serve hot dogs and potato salad and homemade chocolate cake and ice cream. We weren't much older than his graduate students [laughter].

Our daughter [Ann Pitzer], when she was here last January, told me something about the later years when Ken was dean. She said that we didn't really have many close family here, just my middle sister Mildred's family with her two children. Mildred was married to Professor Clarence Glacken of the geography department, and later chairman of the geography department in Berkeley. So Ann considered the chemistry department her family because we would frequently have cocktail parties and dinner parties in those days. Nowadays they serve wine. And our children were old enough to help serve and that sort of thing. So she felt that they had a very intimate association with the chemistry department as family and

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[1]Bracketed material was added by Mrs. Pitzer during the editing process.

friends. She vividly remembers babysitting for Bill and Lorelei Libby's twin daughters during a dinner party. They were two or three years old and very active. She had to call in a friend to help.

When Ken was still an instructor and assistant professor, he and a group of graduate students and other instructors--like Gwinn and Connick--who enjoyed hiking and backpacking would go on three- or four-day hikes in the Sierras during the spring break and Thanksgiving holidays.

Sam and Helena Ruben and Carbon-14

LaBerge: How involved did you get in the science part of it? Like just even when you told me that Sam Ruben discovered carbon-14. I don't know what carbon-14 is.

J. Pitzer: Actually, that is what Bill [Libby] used to devise his method for dating archaeological material. It was carbon-14 that Melvin Calvin later used to trace the process of photosynthesis. It was Sam's basic work for both of these men's--Libby's and Calvin's--Nobel Prizes.

Sam was doing war work when he died, if you recall. We were very close to the Ruben family. We were in Washington at that time; the first period in Washington when Ken was director of the Maryland Research Laboratory--I think; it must have been, if it was wartime--when we heard of his death.

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J. Pitzer: Ken had just received the American Chemical Society Award in Pure Chemistry [1943]. And we knew that Helena Ruben would have to support their three children, one of whom was an infant. So Ken gave half of his $1,000 prize to Helena at that time. That was before the establishment of the UC Retirement System. I believe, incidentally, Professor Axel Olson of the chemistry department was chairman of the faculty committee which established that plan. As I recall, Sam had neglected to sign papers for an insurance policy for people doing war research. Professor Latimer was instrumental in getting Helena the insurance.

LaBerge: That's wonderful.

Interest in Ongoing Work

LaBerge: It sounds to me like you know a fair amount of chemistry yourself.

J. Pitzer: No. I did take chemistry in high school, but no--.

LaBerge: Would your husband, for instance, come home and talk to you about what he was doing or try to explain it?

J. Pitzer: Yes. He would give me a very good conception of his work. Also hearing him discuss it with his students and with other members of the faculty or listening to one of his lectures gave me some idea of what he was doing. Just listening to them discuss their work together, I understood what they were trying to do.

LaBerge: It seems that you need a very understanding spouse to support the ongoing research.

J. Pitzer: I think that is true of any faculty wife in any discipline. When we were first married, the first two years before he got his Ph.D. degree when he was just a teaching instructor, a graduate student, he and one of the other students were making what they would call measurement runs of temperatures. These runs I guess were made with--I don't know whether you'd call them primitive--calorimeters. The measurements would go on continually for about forty-eight hours, and Ken would have to alternate with another graduate student every few hours. That was day and night, you know [laughs]. That took a lot of his time.

But this measurement of heat capacities was the sort of work that resulted in his understanding of restricted rotation in ethane, which is one of his outstanding pieces of research. Nowadays I guess, they have calorimeters that give them results in a comparatively short time. Plus computers give answers which used to take hours on hand calculators. Our son Russell did his Ph.D. thesis at Harvard in theoretical chemistry in this field.

LaBerge: For instance, during that time would you go with him to the lab or not?

J. Pitzer: Occasionally. Never in the middle of the night. It was too intense work. They'd had to record temperatures every so often and be quite alert. I did send food over.

Maryland Research Laboratory During World War II

LaBerge: How did the two of you make the decision to go back for the Maryland Research Laboratory job during the war?

J. Pitzer: Well, this was during wartime, and Ken had been doing research in this area up in the Yolo Bypass area where he would be gone a week at a time. I think Sam Ruben was also in that group and John Thomas and [William] Bill Gwinn (later Professor Gwinn). It was a group put together by Latimer, who was then dean. They were doing research on the behavior of chemical gases that might be used in wartime, not with the view that our forces would be using the chemical gas, because gas warfare was outlawed. But it was in case the enemy--Germany or Japan--started using gas, they would know how it would behave in different weather conditions.

[Things were rather tense during those first months after Pearl Harbor. There were frequent blackouts and air raid warnings. The sirens would awaken our two older children and I would take them to the window to show them the lights of the area go out district by district, including the Richmond shipyards, then come on again later. It was a beautiful sight. We could see the whole Bay Area--180 degrees from our windows. This was strictly against advice--you were supposed to stay away from windows in case of breakage if bombed and keep the draperies closed. But it helped to keep the children from being frightened. Ken was usually away during these air raid warnings doing the research at the Yolo Bypass.

Due to gas rationing, there was very little driving for pleasure. But if you were caught driving on a road or highway during an air raid warning, you had to pull over, stop, and turn off your lights. Also, during the nightly blackouts, volunteer block wardens would patrol every block to guard against glimmers of light.

It wasn't until after our victories at Midway and Attu Island in the Aleutians which denied Japan bases within range of our coast that this threat was removed.][1]

Dr. Thorfin Hogness, the University of Chicago chemistry professor who was the first director of this Maryland Research Lab which was associated with the O.S.S. [Office of Strategic

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[1]Bracketed material was added by Mrs. Pitzer during the editing process.

Services], probably phoned Latimer and Latimer talked it over with Ken. I know he did after he received a call from Dr. Hogness. He wanted Ken to come and be his assistant director. Latimer and Ken both thought it was important work to do, because the Germans had overrun France, and that was during the perilous days of that first period of the war when Germany was winning. They desperately needed devices for the underground French Resistance to do what they could to operate underground. So we went. No sooner had we arrived than Dr. Hogness had to leave.

He told Ken that he was being asked to join another very important project, and it later became obvious it was the Manhattan Project. Ken was asked to take over as director, which he did. The Maryland Research Laboratory was just getting started--Ken organized it and hired most of the research scientists and the staff. The laboratory had taken over the Congressional Country Club for the duration and Ken's office was in what had been former President Hoover's suite.

LaBerge: Did making the decision take a long time for the two of you?

J. Pitzer: Oh, no.

LaBerge: Did you have two children then or three children?

J. Pitzer: Three children. No, it was wartime; you did what you could.

LaBerge: So at that time you had this house on Eagle Hill already.

J. Pitzer: We built that in '41 just before Pearl Harbor was attacked. We had only lived in it for a few months.

LaBerge: So did you rent it when you went to Washington?

J. Pitzer: Yes, we rented it.

LaBerge: I'm thinking of all the work it would take for you with three children.

J. Pitzer: It did [laughter]. That was during the time of war rationing, of course, of gasoline and a housing shortage. So we got the house ready to rent and got very good renters. It actually was a family whose husband was in the service, as I believe, over in the Pacific. They took very good care of the house, fortunately.

But we traveled east by train. Our youngest was about two, I guess. He loved to climb up and down the ladder--we had a compartment, and he spent the whole night climbing up and down the ladder [laughter] between the upper and lower berths.

My sister Connie, and Art Browne, were living in Washington then. He was working for the Agriculture Department then, and continued to do so for a good many years. So they were there to receive us when we arrived. My sister Mildred later came to Washington to work as private secretary to Nelson Rockefeller in the State Department--Latin America. Her husband was in the army then. She was fluent in the Spanish and French languages.

LaBerge: That's really nice. What was that like to be in Washington at wartime?

J. Pitzer: Very sobering. But a sense of cooperation and a unity of purpose. Of course, there were a lot of men in uniform everywhere. Also young women. The railroad station was a very sad place to be--young couples and families saying goodbye. Things were rationed, especially gasoline--also essential food. The Maryland Research Laboratory people had arranged for a house for us to stay in until we found a more permanent house, and this house was in Bethesda. It was owned by the director of the Corcoran Art Gallery [laughter]. It was full of very valuable antiques. I believe they were a bit hesitant to rent to someone with three small children, and I can understand that hesitancy because there were some lovely things in the house. But our intention was to only stay there until a more permanent house was available, which we found within a month or so--another house out in the outskirts of Bethesda, which was more family-oriented. Anyway, all the antiques survived [laughter].

We moved to this other house that was right adjacent to an open field with a dairy, which was fortunate because it was very difficult to get milk delivery in those days. But we did get on their list. And there was a good elementary school nearby for the two older children. We went back a few years ago, and that dairy, of course, is all subdivided [laughs]. Our milk was delivered before dawn every morning by a horse-drawn milk wagon. The milk was in glass bottles.

LaBerge: So you were there for two years?

J. Pitzer: Approximately, yes.

We had some very nice neighbors in Bethesda. All of them were connected with the war effort, and were easterners and midwesterners.

I remember one amusing incident. There was a big tall dead pine tree in our back yard. It was quite a large yard, with other trees, and surrounded by neighbors' houses and fences.

We received permission from the rental agency to cut down the dead tree. Our neighbors were quite apprehensive about this westerner felling such a tall tree. They feared for their fences or houses--probably both. But Ken told them where he would lay it down. He proceeded to do just that--with an axe. (That was before the days of chainsaws.) The tree landed precisely where he wanted it to and just a few feet from the rear fence, and then all of the neighbors brought their saws and axes to cut it up and take home what firewood they wanted. There was plenty for everyone.

I also learned to drive in snow and icy streets in this period.

Incidentally, Ken could make or repair anything. He could repair or rewire electric things, do all sorts of plumbing, and loved to work in wood. He had had a lathe since he was a boy. Before we were married he made me a beautiful powder box, a pair of candlesticks, and a bud vase, all made of orange wood from his father's orange grove. He also made me a beautiful lamp made from a piece of mesquite log which we got from the desert before we were married. I took it to college with me and am still using it. He had a complete shop back here at home and also at our place in Clear Lake. And of course he designed and built several boats.

One amusing incident--during the sixties he was discussing in a shop at Lakeport the repair or replacement of the pump which we used to pump water from the lake to water our orchard at Clear Lake. As he was leaving the shop, the proprietor offered him a job, saying, "I need a good pump man." Ken politely declined, saying he already had a job. This was when he was president of Rice University.

He also enjoyed doing all of the work in our orchard at Clear Lake--planting, weed cutting, pruning, spraying, watering, etc. He enjoyed the vigorous exercise.

He supervised our son Russ's overhaul of the engine in a Model A Ford sedan which Russ had in high school, as well as the installation of hydraulic brakes. Russ later drove the Model A to Pasadena for his undergraduate work at Caltech.

LaBerge: Did you know any of the work that your husband was doing [in Washington] or was it top secret?

J. Pitzer: It was top secret, and he couldn't discuss it with me, no. But I had some general comprehension. After the war I was told that some of the devices the laboratory produced were used by the French Underground Resistance to completely tie up and halt the movement of German troop and supply trains in occupied France.

[A member of the British Intelligence who worked with the French Underground Resistance, George Millar, wrote a book about wrecking these trains using those devices. I think the title was Maquis. His books are in the UC library. Carleton Coon, the anthropologist, has written a colorful chapter in his book Adventures and Discoveries about his work in the OSS in North Africa preparing for the landing of General Mark Clark. He also worked with the underground.

Ken received several patents for devices he invented there--all of which were assigned to the government. One of the patents was for a sighting device for the bazooka rocket. This enabled one man to sight and fire the bazooka. Previously this had been an awkward operation requiring two men. General Eisenhower has been quoted[1] as saying that the West's four key weapons during the war were the DC-3, the jeep, the bazooka, and the atomic bomb.][2]

One of the men he hired--he hired several people that he was acquainted with here at Berkeley and Caltech, and one of the men he asked to come and work on their projects--particularly the projects that related to underground warfare in the Orient, in Japan--was a Chinese named Lu Jiaxi, who had received his Ph.D. at Caltech. My husband knew him at Caltech. He had left his family before the Japanese took over China and couldn't get back and was at loose ends, but he was a very intelligent and capable person. So Ken hired him to come back and work with him at the Maryland Research Laboratory.

After the war, Lu went back to China, became a professor, and eventually was president of the Chinese Academy of Sciences for many years. During the eighties he invited us over for a visit sponsored by the Chinese Academy of Sciences and planned

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[1]The Economist, April 10, 1999, p. 86.

[2]Bracketed material was inserted by Mrs. Pitzer during the editing process.

out a visit to the various installations, various universities and laboratories directed by their institution, Ken lectured at the several universities, which was very interesting. Lu had made several trips back to this country--one to the National Academy of Sciences and other trips--so we had seen him meanwhile, before we did go to take this trip to China.

LaBerge: This was when China was restricted. Somehow you kept up contact?

J. Pitzer: No. When Lu was working for my husband in this laboratory, the Japanese had taken over China. I don't know whether he had any news from his family--I don't believe so. He had married and left a wife and son in China. I don't know whether he had any communication with them at all during that period. But he went back--when was it? I guess before the Chinese Communist, Mao [Zedong], took over. Then during Mao's period we didn't hear anything from him for many years.

LaBerge: That's what I wondered, how you kept up.

J. Pitzer: We didn't hear anything from him until after [President Richard M.] Nixon resumed contact with China. And then Lu started contacting people in this country whom he knew. I guess the first time we saw him was when some Chinese friend of Lu's down here in Alameda phoned and said that Lu was coming for a visit and asked if we would have dinner with him. So we went down and had dinner with them.

Lu had a very interesting story. He had several children during this period when he was out of contact, and he said at least two of his children were sent out to work in the fields, and in spite of being very intelligent, talented people, they were members of that "lost generation" as far as education is concerned. But his youngest child was a girl, and he was bringing her over to enroll her in an American university on this visit when we first re-contacted him.

I think Lu was responsible for the regeneration of science in China after Mao. He may have been appointed or elected as president of the Chinese Academy of Sciences because he knew so many people here and had contacts here. Certainly that helped.

[An echo of the circumstances and atmosphere of Ken's administration at the Maryland Research Laboratory during the war occurred when he was a member of the advisory board of the U.S. Naval Ordnance Test Station in China Lake in 1956-1959. He was chairman of that board in 1958-1959.

His friend and former Caltech classmate, physicist William (Bill) McLean, was the technical director at the test station. Ken and his committee backed up and endorsed and stood behind Bill's efforts to develop the Sidewinder missile in spite of the opposition of his superiors in the Defense Department. I include an interesting article about this which appeared in the Wall Street Journal many years later, in 1985 [see Appendix].

The Sidewinder missile was not only the first, but also the most dependable and inexpensive and effective air-to-air missile which has ever been developed.

Bill had also done valuable work during the war when he was with the National Bureau of Standards. I think he did some of the preliminary work on the proximity fuse. He later became a member of the National Academy of Engineering as well as the N.A.S. Other members of that advisory board were the legendary navy flyer-Admiral "Chick" Hayward, later C.N.O. (chief operations officer of the navy), physicist Bill Shockley, and I think one or two professors from Caltech. It was a very interesting and amusing committee. I sometimes went with Ken to China Lake for meetings. We stayed with Bill and La V. McLean. On one visit they took the committee for a day-long jeep trip to see petroglyphs in the Coso Range--very interesting.][1]

Family and Home on Eagle Hill, Kensington

LaBerge: What other people did you meet? For instance, was your husband in contact with the people who were working at Los Alamos during this time?

J. Pitzer: Yes, there were a lot of people from the Berkeley faculty working there, particularly [J. Robert] Oppenheimer, and of course the Oppenheimer family were neighbors of ours up here on Eagle Hill. Ken had many friends in the physics department here, especially Luis Alvarez. [Ken and Luis published a paper on research they did together. We knew all of the chemists and physicists in the university and industrial world and at the AEC laboratories and were friends with many of them--Libby, Urey, Teller, Fermi, Du Bridge, Conant, Joe and Maria Mayer,

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[1]Bracketed material was added by Mrs. Pitzer during the editing process.

Rabi, Gamow, Segre and others--Wigner and Von Neumann and Bacher.][1] And of course when he was director of research for the AEC he had direct contact and supervision of all of the AEC laboratories and of their scientific grants. We moved in here, number 12 Eagle Hill, in November of '41.

LaBerge: Right before Pearl Harbor. You had hardly been here.

J. Pitzer: We had hardly been here. The Oppenheimers had bought their house up here, number 1 Eagle Hill, I think just a few months previous to that. Not only the house, but they bought all but one of the vacant lots between us. The vacant lot that's next to us now was not for sale at that time. And we have a half-interest in that lot now, along with our good friends and neighbors--it was the Daubens; he [William G. Dauben] was a former professor of chemistry and died a year ago. We each own a half-interest because neither one of us wanted a house built that close to us.

LaBerge: So this is a little Chemistry Hill [laughter].

J. Pitzer: Well, Oppenheimer was in physics [laughter]. But anyway, they were here then, and we really didn't--Ken had met Oppenheimer in seminars and that sort of thing, but we weren't too well acquainted with him. They weren't particularly--we didn't see them very often. Ken was only an assistant professor at that time. And it wasn't long before they went to Los Alamos. It wasn't until after the war when they returned that we became better acquainted with them.

Their son, Peter, was almost exactly the same age as our youngest son John. Peter particularly enjoyed being friends with John [laughs]. He loved being down here playing with our boys. We had planned this driveway, it's quite a flat area; it's a regulation badminton-size court, actually. But we had planned it with the idea that it would be a good place for children to skate and ride their tricycles and that sort of thing. We also had a swing, a bar, and a basketball hoop. The neighborhood children gravitated here because there was no place anywhere flat enough in this area to do that. Also Ken wanted a flat area where he could build a boat--which he did. Peter would come down, and Russ had a wagon and sometimes he or I would take it with John or Peter up to the top of the hill and we'd both sit in the wagon and come down and bump over the curb and into our driveway. It was lots of fun. But Peter and

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[1]Bracketed material was added by Mrs. Pitzer during the editing process.

John were in the same kindergarten and first grade class. They started kindergarten together. He was a delightful child; I enjoyed Peter very much. At that time the Kensington school had a bus that would pick up here at the circle near Arlington, a block and a half away. Ann and Russ, our two older children, saw that these kindergarten boys got the right bus [chuckles]. Every school day morning Peter would ring our doorbell so that he could walk to the school bus stop with John.

I became quite well acquainted with Kitty Oppenheimer. I wouldn't say we were friends in the strongest sense of that word. They spent a weekend with us at Clear Lake and we were entertained occasionally by them. When Kitty had to be in the hospital for a few days for minor surgery, she asked me to care for Peter after he returned from school until his father came home from the university--which of course I was glad to do. Peter was generally down here in the afternoons anyway. Not much is known about Kitty as a person. There was an excellent article about her in the November 1995 issue of Berkeley Insider--a very fair biographical sketch. [See Appendix].

Both Robert and Kitty Oppenheimer could be extremely charming and also extremely arrogant, sometimes simultaneously. There were also some charming and amusing moments of self-deprecation.

In my opinion, Robert was a very complex man. Professor Abraham Pais, who worked with him at the Institute of Advanced Studies for sixteen years, characterizes him, in his autobiography, as the most complex personality he ever knew.

I thought one of Robert's outstanding characteristics was his love of power and the exercise of power to influence policies. The combination of this with the aura of a mystic and his superb command of the English language resulted in the development of a cult of admiration, imitation, and followers which ranged from his graduate students to mature eminent scientists. After the war he was the premiere science advisor to the president, the secretary of state, secretary of defense, and other important agencies. I think that may have been why he later rather opposed the establishment of a President's Science Advisor as William Golden mentioned. It would diminish his role.

Kitty was fiercely protective of Robert, shared his ambitions, and tried to enhance the aura of genius which he projected and used.

LaBerge: Did the Kensington schools go through the twelfth grade?

J. Pitzer: Sixth grade.

LaBerge: And then where did the children go from there?

J. Pitzer: To the junior high school in El Cerrito down the hill a little distance from us.

LaBerge: So that's where your children went to high school?

J. Pitzer: Yes. Junior high first and then high school. Those schools were very good at that time.

LaBerge: Then when you came back, or even before, what about the larger group of faculty wives besides just the chemistry? Were you involved in the Berkeley faculty wives'--

J. Pitzer: Yes. There was the University Section Club and the college teas at the Women's Faculty Club. I would go to meetings. I would go to section meetings of this club as I had time for. Actually, that was during the time when our children were in junior high and high school, and I wanted to be home when they arrived home.

LaBerge: I know your husband told me this story. I'm not sure if it was John or Russ, when he was getting his driver's license. That was a funny story. And finally a neighbor took him [laughs].

J. Pitzer: Yes. He had taken driver's training--

LaBerge: Was this Russ?

J. Pitzer: Yes. In high school. And he had asked me to be home to take him to the DMV [Department of Motor Vehicles] to get his license. There was some emergency with my daughter; she had to have some dress or something like that [laughter]. Some small emergency. And I forgot all about it, to my regret. The mother of a good friend of Russ's, whom we knew very well, picked both boys up after school and took them to do some errands. Russ asked if she would take him down to the DMV. So she did. But he had never driven her car [laughter]. When the examiner got in the car with him, Russ didn't even know where to put the ignition key; it had to be pointed out to him by the examiner [laughter]. But he passed. Russ remembers this differently. He says he stopped by his friend's house on the way home and his friend's mother offered to take him to the DMV. But the result was the same.

Atomic Energy Commission, 1949

LaBerge: Then you had another trip back to the East Coast for the Atomic Energy Commission. Now how did you make that decision? It was 1949 or so.

J. Pitzer: Latimer was dean then, and of course Ernest Lawrence was much involved with the Atomic Energy Commission policy. And Luis Alvarez wasn't on the commission, but--the physicists had been very much involved in planning and in administration of the Atomic Energy Commission, including Oppenheimer and Robert Bacher. The director of research that Ken succeeded was James Fisk, who was a physicist. He was at MIT. At that time the AEC was a very important department of the government on a par with the State Department, army, and navy--because nuclear weapons and nuclear power were part of national strategy--more so than today.

I think there was a general agreement--at least the chemists agreed--that chemistry had been neglected. And the Atomic Energy Commission was establishing these research scholarships or fellowships, and they wanted someone from an academic background, preferably in chemistry, to sort of correct this overbalance of physics and to establish the policy of academic fellowships. This is as I interpret it; I hope it's correct. I guess Latimer put it up to Ken.

One of the things that Joe Platt, who spoke at Ken's memorial service at Pitzer College, said--and Joe knew because Ken hired him to be the head of the physics division of the AEC research division--Ken asked Joe to come and be the head of the physics division. Joe was at the University of Rochester then. So Joe was in right at the beginning of Ken's administration at the Atomic Energy Commission. He and John R. Thomas, who was a Cal graduate here in chemistry--a student of Gwinn's, I believe; and Gwinn had been Ken's student--Ken hired Thomas as the director of the chemistry division. Thomas later went on to become president of the Chevron Research Corporation.

Ken nominated Joe later as the founding president of Harvey Mudd College, which he became. [With emotion] Anyway, what Joe said at this memorial service a couple of weeks ago was that in 1949 most people in academic life had been doing war work for years and had neglected their own research but were then resuming their own research, and that was a real sacrifice and a patriotic thing to do to go back and to leave their research at that time--such a short time after the war.

LaBerge: In 1949?

J. Pitzer: So soon after the war, instead of resuming their own research interests. Well, Ken had taken up his own research, some very important research as it turned out later--in fact, George Pimentel, who was one of his most outstanding students, was his graduate student at that time and he hated to leave him; but George was a very resourceful person and older than most graduate students, having served in submarine warfare. He was able to not only direct his own research with a little help from the rest of the faculty and consulting Ken by phone--but I know it was a great sacrifice for Ken to leave his research here at UC.

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J. Pitzer: And Ken felt it was an important thing to do.

Importance of Academics Serving in the Government

LaBerge: Were you making it a commitment for so many years or limited--

J. Pitzer: I think the conception was it would be two years. Ken felt very strongly that there should be an interaction between citizens--especially in the academic world, perhaps especially in the sciences--and the government; that there shouldn't be a class of government people who were completely dissociated with scientists out in [pause]--

LaBerge: In the real world? [laughter]

J. Pitzer: In academia, yes. He thought that it was for the benefit of both government and science to have this interaction and have citizens serve for, say, a two-year period. In fact, later when George Pimentel was asked to be an assistant director of the National Science Foundation, he reiterated that conviction to George, and I think that was one of the things that convinced George to leave his university work here and spend two years there. I think both the government and the university benefited from it. In a sense it meant that new attitudes and opinions would be infused into the government agencies which would be "recharged."

Of course, Ken was there at the very important time when it was decided whether or not to build the hydrogen bomb. He felt strongly that it should be built because he felt that if the Russians developed it first we would be at their mercy. The idea of mutual deterrence was more convincing than if we did not develop it. I think the hydrogen bomb played a major part in the success of the policy of containment and the end of the Cold War. I believe the Russian scientist Sakharov confirmed that recently in his memoirs.

[An excellent account of this controversy and also the history of the development of the hydrogen bomb in both the United States and Russia is in Stalin and the Bomb by David Holloway. As I understand it, Oppenheimer based his opposition to the hydrogen bomb on the belief that if the U.S. didn't build one, then Russia wouldn't. But Sakharov indicates that Russia would have built one regardless. As Secretary of State Dean Acheson said, "How can you persuade a paranoid adversary to disarm by example?"[1]][2]

LaBerge: Did your husband write a paper on that subject?

J. Pitzer: He gave interviews and speeches. I believe he has been interviewed for this oral history about his views and this period.

[Professor Rudolph Peierls, who was head of the British group of scientists who came to Los Alamos to help develop the atom bomb, relates in his autobiography Bird of Passage that he visited Robert Oppenheimer in Princeton shortly before the latter's death. He says, "We agreed that he could have saved himself much trouble if he had resigned from the General Advisory Committee of the Atomic Energy Commission when the commission refused to act on the committee's advice about the hydrogen bomb. As chairman of the committee he was especially blamed for opposing the hydrogen bomb, and as a result his position became untenable."

This was a course of action that Ken had strongly urged Robert, as well as Conant and Du Bridge, who followed Robert's lead on this viewpoint, to take at that time. As he stated in a speech to the southern California section of the American Chemical Society on March 8, 1952, Ken felt strongly that, "The technical leadership should be in the hands of those who, in addition to their technical qualifications, also believe in the objectives that had been officially accepted as desirable. If

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[1]Holloway, David, Stalin and the Atomic Bomb. p. 301.

[2]Bracketed material was added by Mrs. Pitzer during the editing process.

any individual lacks enthusiasm for the objective, he should drop off the team that is trying to do the job and voice his objections from the outside. It was in this sense that I believe certain members of the GAC have been open to criticism."

Peierls further quotes Robert as saying, "You know, there is the attitude that says 'As long as I keep riding on this train, it won't go to the wrong destination.'"

To me, personally, this is an example of hubris, not wisdom.

Obviously, though we credited Robert for his many talents, neither Kenneth nor I were members of the Oppenheimer cult. I think <u>perhaps</u> Robert respected those who disagreed with him, but at the same time resented those who were not influenced by his charisma and whom he could not manipulate.

A few weeks before President Truman ordered the AEC to proceed to research and develop the hydrogen bomb, our family was invited one weekend to spend a night with the Oppenheimers at their home in Princeton. This invitation was, obviously, related to Robert's desire to have an opportunity to try to persuade Ken to oppose the research on the bomb. While the visit was otherwise pleasant, by the time we left the next day, neither was able to convince the other to change his position on the issue.][1]

LaBerge: It would be wonderful, maybe the next time, to hear from you what your husband's views on the interaction between science and society are.

J. Pitzer: I think he wrote articles about that. I can give you reprints.

LaBerge: Even a couple of sentences of your view of his views. Did you talk about that at home?

J. Pitzer: I suppose so. But he lived it.

LaBerge: I know when we talked before about his deciding to be an administrator at Berkeley and then at Rice [University], making that choice to be in administration and not just do his research--

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[1]Bracketed material was added by Mrs. Pitzer during the editing process.

J. Pitzer: Ken told me what he found attractive about the offer to be the president of Rice University was that he felt he could contribute--he felt he could make a difference--and also he would be able to continue to do his research. He enjoyed administration. But only as long as it didn't interfere with doing the science that he wanted to do. Both here when he was dean--I guess he resigned in 1960 as dean after ten years--and at Rice, there were other considerations that made him decide to resign, but essentially he felt that there was a cyclical occurrence of problems in administration. You would solve this same problem once and then a few years later it would come up again [laughter], with a different cast of characters. There were other important considerations in both positions that entered into it, but that was one reason that he resigned both places.

I have always been thankful that when we first took on the responsibilities of a presidency of a university, that our children were already grown. Our youngest, John, was a senior at UC Riverside in 1961. I think it would be extremely difficult if not impossible to have young children and to take on that job. However, I was always available for family emergencies. When Russ's wife Martha had to have surgery in 1967, and again in 1970, I took care of their three young children for a week or two, and when John's younger son was born I stayed with John and Claire's three-year-old son while Claire was in the hospital and afterwards.

Joel Hildebrand and G. N. Lewis's Academic Robe

[Interview 2: April 9, 1998] ##

LaBerge: We were just looking at photos, and there's a wonderful photo of Joel Hildebrand and your husband at the Greek Theatre.

J. Pitzer: This was when Joel retired--became emeritus--I think, like Ken, he never retired--and he received an honorary degree at commencement. Ken was then the dean of the College of Chemistry. They were sitting on the stage at the Greek Theatre when President [Robert Gordon] Sproul was awarding honorary degrees. When he announced in his very resonant voice [chuckles] "Joel Henry Hildebrand," the audience in the Greek Theatre just erupted in spontaneous applause, very long and enthusiastic applause. Ken was standing behind Hildebrand with an honorary hood that he was going to put over Hildebrand's head after Sproul conferred the degree, but the applause of the audience was so prolonged that President Sproul forgot to confer the degree [laughter]. So Ken had to whisper to President Sproul, "President Sproul, you forgot to confer the degree." So Sproul proceeded to do that, and then there was another long applause for Joel, because he was so admired and loved by his students.

We were saying something about the robe Ken was wearing on that occasion. It was a silk academic robe that had belonged to G. N. Lewis and had been given to my husband by Mrs. Lewis after Gilbert Lewis's death. He used it while he was dean and left it with the chemistry department when we went to Rice University. I don't know what happened to it then.

LaBerge: Maybe somebody there does know.

J. Pitzer: Maybe. I think Robert Connick succeeded him as dean. But it wouldn't fit him because he's so tall. It may be stored somewhere.

LaBerge: I didn't look at the list carefully of the academic genealogy [see Appendix], but was Professor Lewis your husband's professor too?

J. Pitzer: No, Professor Wendell Latimer.

LaBerge: But he must have had a wonderful relationship with G. N. Lewis for Mrs. Lewis to give him the robe.

Text on Thermodynamics

J. Pitzer: There was a very congenial family sense in the department at that time--there still is, but it was a much smaller department, of course, when we first came here. Ken was working in thermodynamics, which was G. N. Lewis's field. When it came time to revise the Lewis and Randall text on thermodynamics, Mrs. Lewis was very enthusiastic in wanting Ken to do it. Ken and Leo Brewer did do the second edition. The third revision was just published in 1995. Ken did that edition by himself. It is a classic. Ken was the founder of modern theoretical chemistry at Berkeley.

LaBerge: So it's still used.

J. Pitzer: Yes. A classic text. Although the third edition that Ken did just recently, he viewed as less as a text, I think, but more as a reference book for people in other fields as well as chemistry: the geological sciences, geochemistry, astronomy, environmental people, natural science, chemical engineers, biochemistry, and marine chemistry. The preface to that edition explains that very carefully. It is a much more rigorous book mathematically than either the first or second editions and emphasizes the use of thermodynamics in these other fields.

LaBerge: Did you help him doing editing or did you always read the things that he wrote?

J. Pitzer: All of the books that he wrote he wrote out in longhand first, and I would read that text for meaning. I wasn't competent with the chemistry or the equations, but I would read the text for meaning and sentence structure before he had it typed. Then after it was in proof we went over the proof together.

LaBerge: Same thing with speeches and things like that?

J. Pitzer: Yes. Frequently we would discuss beforehand a speech or an article that was nonscientific. Just general points.

LaBerge: Would he deliver it to you for practice?

J. Pitzer: Sometimes. After he would have it typed I would frequently go over it with him. I'm flattered to think that he valued my opinion. But he frequently gave some speeches just from notes. He was accustomed to speaking from notes. As an undergraduate at Caltech he was on the debating team. Their team won a national competition his senior year.

LaBerge: I know just from what he said, and I know from all the things that you talk about, that you were involved in, that you know more chemistry than you give yourself credit for [laughter].

J. Pitzer: I would never claim that.

Postwar: National Science Foundation and the NDRC

LaBerge: Last time, you gave me--and I have a copy of it now--the memorandum of William Golden. You talked a little bit about the National Science Foundation and just what that paper was about and what your husband's impression was of the idea of having science advisors to the [U.S.] president and everything. Could you talk a little bit about that?

J. Pitzer: Oh, he very much favored the establishment of the NSF and gave Golden some valuable advice about its organization--which Golden acknowledged. He thought it was very important for a full-time science advisor to be in the president's office and in frequent contact with the president.

LaBerge: At the time was it James D. Conant? When William Golden had that piece--there was some time when Conant was appointed--

J. Pitzer: Well, Conant was head of the NDRC [National Defense Research Council] during the war. He and Vannevar Bush. George Kistiakowsky, a chemist, was the first advisor--for President Eisenhower. He was very able.

LaBerge: I don't have this all clear. You said something about the chemists from the western states were sort of rebels [laughter].

J. Pitzer: Everybody in the scientific community during the war dropped their own personal research and did what they could to win the war--willingly and productively. Most of the administration of the NDRC was carried out in the East by eastern people like Conant and Bush, and most of their staff were people that they knew and were acquainted with. One of the difficulties of people out here in the West--or middle West, too, I believe--was that it took so long to get their ideas or an answer on their ideas through this bureaucracy that had been set up in the NDRC. It worked very well in general, but it just was sometimes frustrating that these second or third echelon scientists weren't as eminent scientists as, say, at least here at Berkeley or Chicago or Stanford or other places. They weren't as experienced scientists or as eminent scientists as the people who were trying to get answers through the bureaucracy. Most of the men here were members of the National Academy of Sciences and the men in the bureaucracy were not. Also Conant was still president of Harvard and did the NDRC work as a part-time administrator, which really wasn't adequate, as it was a full-time job.

I might mention here another area in which Ken influenced science policy in an organizational way. He was appointed co-chairman of a committee to revise the bylaws of the National Academy of Sciences. I don't remember the year, but I think it was during President Johnson's administration or possibly President Nixon's. I think that because I believe that the Congress had to approve or ratify the revision and Ken thought a man who had been identified and served with a Republican president should be co-chairman. So when Ken was asked to be chairman he asked that James Killian (I think) who was the former president of MIT be appointed co-chairman. I hope I remember this correctly, but I'm sure it is all detailed correctly in his file of professional papers which the Bancroft has.

At any rate, the committee to revise the bylaws of the NAS redefined and strengthened the position of the president of the NAS, making it a full-time job with a definite length of term, a Washington residence, and an adequate salary. The first president under the new by-laws was Frederick Seitz. Also I think the eligibility for membership in the NAS was extended to some of the social sciences--psychology and anthropology, for example. But, also, importantly, the National Academy of Engineers and the National Institute of Medicine were created and chartered. I believe the thinking was that the NAE was more professional than "pure scientists" in the NAS and should have their own organization, still under the umbrella of the NAS, as was the NIM. The NAE was chartered in 1964. But I may be wrong about that. Nineteen sixty-four would be during Johnson's administration.

Ken also served the NAS by being elected to the council at two different times and by being chairman of an ad hoc committee to nominate a new president. He also served on other committees.

LaBerge: Yes. I know Sally [Hughes] and your husband talked about what he did during the war, so we're not going to talk about that. What I'm really supposed to do is to find about Stanford and about your role as a president's wife and as a dean's wife--both at Rice and at Stanford.

XVIII UNIVERSITY PRESIDENT'S WIFE

Rice University, 1961-1968

LaBerge: Why don't we start with Rice because there were a couple of things that happened there too. How did your life change and what were your duties?

J. Pitzer: As a president's wife? Well, it was an extension, really, of what one does as a dean's wife. One does a lot of entertaining. Right here I would like to pay tribute to all wives of deans and chairmen of departments. I feel that they get very little recognition for the work they do, particularly arranging social events in the colleges and departments--often with little or no financial recompense. Ken agreed with me. When he was president at both Rice and Stanford, he earmarked funds for this. When eminent people come to visit, we always entertained guests.

LaBerge: For instance, you showed me the photo of President [John F.] Kennedy coming to Rice. What would you have done prior to that?

J. Pitzer: Kennedy's visit was handled by the White House almost exclusively, and the Democratic party [chuckles]. There was a ceremony when Kennedy gave a speech in the Rice stadium, in May or June 1963, when he announced plans to go to the moon. But his time otherwise was very full and was organized in a way that he spent most of his time with the local politicians. I think [Vice President] Lyndon Johnson helped in that planning; I'm not sure. There was a dinner at a large convention hall for him that same evening or the previous evening. There were one or two dinners that we went to, as we usually did. That sort of thing.

[I do remember the circumstances when I first heard of Kennedy's assassination. We had been to a large banquet in

Kennedy's and Johnson's honor the evening before in Houston. We had sat at a table just in front of the head table where Kennedy and Johnson and their wives had been. It was unbelievable to hear he had been shot.

It was at noontime. I was at home in the President's House. The phone rang and it was the officer in charge of the army ROTC unit at Rice. He asked to speak to my husband and I told him he was at lunch at the Faculty Club. He asked me to stay on the line while his call was transferred. While we were waiting for Ken to be called to the phone, he told me that Kennedy had been shot. When Ken came on the line, he asked Ken for permission to take the Rice ROTC students to Dallas to help patrol the streets, if necessary. He had been alerted to prepare to do this. Remember, there was at first a feeling that it was part of a wider conspiracy. Fortunately, in the end, the students did not have to go.

Another tense situation came earlier in 1962 during the Cuban missile crisis. I don't know how we received the warning that it was a serious situation. It may have been received because Ken was on the General Advisory Committee of the AEC, perhaps when he was chairman of the committee. However, I think anyone following the news became aware of an unusual and threatening situation.

At any rate, the warning was taken very seriously, since Houston was within range of the Soviet-Cuban missiles. Those students who were able to return home were advised to do so. For those who couldn't, the campus authorities stocked water, food, first aid supplies, et cetera, and selected sites on the campus as shelters. Fortunately the situation was defused.][1]

One of our main concerns when we first went to Rice was to get in touch with the faculty, to entertain the faculty, to get acquainted with them. We had frequent buffet dinners of fifty or so at the president's house in which various members of the faculty and the Board of Governors would all be present. I think it went very successfully. The Board of Governors were an exceptional group of men and women on the board, who had a very sincere interest in the university, starting with George Brown, who was chairman of the Board of Governors. He was president of Brown and Root, a large engineering firm, and extremely wealthy. He used his wealth wisely. He was generous to Rice especially, funding under Ken's guidance the Brown

[1]Bracketed material was added by Mrs. Pitzer during the editing process.

Awards in Teaching Excellence at Rice, and also several buildings. His wife, Alice, was a dear. She was very interested in art and music and served on several boards of museums. They were very cooperative in the things that my husband wanted to do for the university, and they were very interested in close contacts with the faculty. The Browns enjoyed entertaining the faculty also. So I think those parties helped.

LaBerge: How did you learn how to do that? Was there some kind of a guidebook for you?

J. Pitzer: No. There were of course traditional events that I was told about by the previous president's wife, who was still living in Houston. That was President and Mrs. William Houston. They were still in Houston, and they were very helpful. In fact, Ken had taken a course from President Houston when Houston was a professor at Caltech. He was a professor of physics at Caltech when Ken was there. And he was a member of the National Academy of Sciences, so we were acquainted with them.

LaBerge: Did you have a secretary who had been there years before and filled out the calendar and would work with you?

J. Pitzer: No. I filled out my calendar myself--in consultation with my husband's secretary. I don't think there was a social secretary at that time. But my husband's secretary and her staff were very helpful with invitations, et cetera, also the development office.

LaBerge: So you did your own planning and your own ordering of the food and all of that?

J. Pitzer: Yes. The director of food service at the Faculty Club catered for me. The President's House at Rice was located on the campus in a grove of live oak trees. I thought it very important to live on the campus. While we were still at Berkeley I had observed how the fact that neither President Kerr or then-Chancellor Glenn Seaborg lived on campus in the University House--contrary to President and Mrs. Sproul, who did live in the President's House, as the University House was then called--had contributed to the impersonal atmosphere on the campus, as Mario Savio later phrased so vividly.

The President's House at Rice was a very gracious home with large full-length windows and glass doors which opened out onto lovely terraces and a beautiful garden. [One of the wives of a member of the Board of Governors was a rose enthusiast and she and her husband built me a beautiful rose garden, which the

Rice students loved to walk through--and sit on the benches, but they didn't even pick the roses--at least not many! The enclosed garden at the rear of the house had beautiful azaleas lining the walls and swimming pool. The first few years there were also numerous gardenia plants on the campus but they gradually were replaced.][1]

I had a wonderful household staff at Rice--a wonderful cook and maid/housekeeper and gardener. I enjoyed doing most of my own flower arrangements when we entertained.

We always had a reception for all of the faculty and their wives and the Board of Governors at the beginning of the year at the Faculty Club. In addition to entertaining visiting notables and speakers, we had a box in the football stadium where we invited the presidents of the visiting university and also Houston friends of Rice. Frequently we had a dinner in our home before a night game.

Every year I would entertain the wives of new faculty at a morning coffee. I also had morning coffees for the Graduate Wives Club. At Easter the Faculty Women's Club would have an Easter egg hunt for their children in our garden. In the spring every year all the women students were invited to a reception in our home and garden, as well as visiting members of women students from other Texas universities, as part of an occasion known as the "Rondolet." I attended all meetings of the Faculty Women's Club, ROTC Review, et cetera. We also regularly had dinner with the students at the several residential colleges. I have probably forgotten a few events, but that is the general idea. We also entertained and interacted with Dr. and Mrs. Robert Gilruth. He was director of the Manned Space Center near Houston. At commencement we had a lunch at the house for all the Board of Governors and their wives.

[Also we participated in and attended events in Houston--at the museums, the ballet, the symphony and theater, the University of Houston, St. Thomas University, et cetera. I think one of the most obvious differences between the Rice Board of Governors and the Stanford Board of Trustees was the Rice board's sincere enjoyment of contact with the Rice faculty. Of course most, but not all, of the Rice board lived

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[1]Bracketed material was added by Mrs. Pitzer during the editing process.

in Houston, and the Stanford board mostly lived in San Francisco or elsewhere, which made it less convenient.][1]

Offer from MIT and Caltech

LaBerge: One thing we talked about but not on tape was that when your husband was president of Rice he had what you called an offer--but he didn't--from MIT. Could you tell me about that?

J. Pitzer: Yes. It was not a formal offer. He was asked to go up and interview for the Board of Trustees at MIT, just to consult with them about other candidates, they said. That was after President Jay Stratton resigned at MIT. We had been in Cambridge at MIT for a month or two during the spring semester just before we went to Rice. They had asked him to come for a whole semester to teach on Ken's sabbatical, but Ken didn't want to spend that much time there. So we just spent a few weeks there at MIT while the Strattons were still in office. Kay Stratton (Mrs. Stratton) gave me several very helpful pointers on being a president's wife.

After Stratton retired--I guess it was some months after we had been at Rice--I was awakened very early one Sunday morning by a member of the Board of Trustees at MIT who I think had forgotten the time change between the East Coast and Houston [laughter]. It was someone whom Ken knew, who had been on the faculty there in Cambridge. Actually it was James Fisk whom Ken had succeeded at the AEC as director of research. He wanted to talk to Ken. I told him that he was in a meeting in the White House of the President's Science Advisory Committee. So he phoned him there and asked him if he would be willing to come up and I suppose be interviewed, but I guess they put it "consult" on other candidates they were considering. He agreed to, and I forget whether he extended that trip and went from Washington to Cambridge or went up at another time. But he did go on and talk with them. They asked him if he would be interested in being a candidate, and he said no, under no circumstances, because the legal process to change the Rice--it wasn't the Rice original will; it was the Rice charter--to integrate Rice and to charge tuition had begun. The process had been started, and there was no way that he was going to leave at that time. He was committed to see that through. But

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[1]Bracketed material was added by Mrs. Pitzer during the editing process.

they asked him about various other people that he was acquainted with that they were interested in. He gave them his opinion, and we thought that was that.

But some weeks later they asked him to come again and he said he wouldn't be able to do that at that time. A week or so after that James Killian, who had been president before Stratton, I think, and was on the Board of Trustees at that time, came down to Rice unexpectedly for just a social visit for a day. He came over to the President's House and we had a cup of coffee, I had a luncheon engagement, and it was just a pleasant visit. Perhaps I'm wrong. Perhaps they asked him the second time to come for a visit.

LaBerge: After Killian's visit to you.

J. Pitzer: Yes. Ken said no, that he wouldn't be able to do that, and he wasn't a candidate for the presidency. And so then they appointed someone else.

LaBerge: Who did they appoint?

J. Pitzer: Howard Johnson, who had been dean of the Alfred P. Sloan School of Business. He was actually in the process of moving to Chicago, I think. He had accepted a job as head of the Federated Department Stores, I believe, and was actually with a moving van en route to Chicago when they called him back. So it was my impression that they really wanted Ken as a president, but he never took it seriously and had told them so.

I forgot to mention that later, in 1968 I believe, Ken had been out here being interviewed by the Stanford Board of Trustees. He told them that yes, he was interested and perhaps had accepted by that time--I don't know; this was in July at the Bohemian Grove encampment. One or two of the trustees from Caltech were there at that time and asked him if he would be interested in being a candidate for the Caltech presidency. Lee Du Bridge had just resigned, and Ken told him no. He had already accepted at Stanford.

LaBerge: Bringing up Lee Du Bridge, that's one of the things that we had talked about also off tape. When you were at Rice you had contacts with Texans who were prominent in the White House like Lee Du Bridge and others.

J. Pitzer: He was not a Texan. He was president of Caltech, formerly from the University of Rochester, and had been director of MIT's Radiation Laboratory during the war. He was also on the General Advisory Committee to the AEC.

LaBerge: Okay. Did he have something to do with the science advisors to the president?

J. Pitzer: Later, for a short period of time, he was science advisor to [President Richard] Nixon or [President Ronald] Reagan--I forget which. Neither Nixon nor Reagan used a science advisor very much, in contrast to Eisenhower, Kennedy, and Johnson, and to some extent Ford. Nixon abolished the President's Science Advisor in Februyar 1973 and also disbanded the President's Science Advisory Committee. That was when he left--I guess after he had retired as the Caltech president. He was on the Atomic Energy Science Advisory Committee at the same time that Oppenheimer and a good many people were. During the time that Ken was director of research, actually.

LaBerge: At the AEC.

J. Pitzer: Lyndon Johnson was vice president when we first went to Rice, and George Brown, president of the Rice Board of Governors, was LBJ's very good friend, and Alice Brown was Lady Bird Johnson's closest personal, confidential and trusted friend. I believe she confided in Alice more than anyone else, but Alice never spoke of it or her, never betrayed her confidence.

I had forgotten this--shortly after we went to Rice, President Kennedy's director of defense, Robert McNamara, wanted Ken to leave and go to Washington as assistant secretary of defense for science--I forget the exact title. Ken was totally uninterested. He made himself unavailable--I think he had an important trip somewhere and finally McNamara told George Brown that he got the message--that Ken wasn't interested. That position was filled by Herbert York and later Johnny Foster, I believe, both former directors of the Lawrence Livermore Laboratory.

The Vietnam War

LaBerge: Tell me how the Stanford presidency came about. Did that offer come out of the blue?

J. Pitzer: Yes [laughs]. Stanford had a selection committee, I believe, made up of the members of the board of trustees, faculty, alumni, and I think students. I'm quite sure. David Packard was an alumnus on the committee, and he just appeared one day at Rice and told him they were interested in him. It was tempting because it would be returning to California, although at that time, in 1968, it was of course a very critical time of the Vietnam war, and Ken was very convinced that we should get out--and he had been for some time.

As you recall, he had written letters to Johnson and to Don Hornig, the chairman of the President's Science Advisory Committee to Johnson. Hornig was also president of Brown University later. (And Ken wrote a letter later to Nixon.) In 1965 actually, he wrote a memorandum to George Brown, who was the president of the Rice Board of Trustees and a very good friend of Lyndon Johnson's. In fact, George was Johnson's primary advisor in Texas. The Browns and Johnsons were good friends. The Browns had a home in the hunt country in Virginia.

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LaBerge: I think some of that was cut off. It was the Browns' house in Virginia where Johnson had his heart attack when he was vice president.

J. Pitzer: Yes. This is digressing, but back in Johnson's administration George [Brown] respected my husband's opinion about things other than at Rice [chuckles]. This was just before Johnson was contemplating enlarging the military advisor role and sending over 200,000 troops. George Brown asked Ken what he thought of the situation. Ken wrote a memorandum and said he thought it was the wrong thing to do. He gave the reasons why he opposed it; I think you have a copy of that memorandum [see Appendix]. George, too, was worried about enlarging the war, but other advisors prevailed. But anyway, I just mentioned that to show that Ken had been convinced at a very early stage of the advisability to end the war as soon as possible. And in 1968 when David Packard came to see him he felt that unless the war were ended or at least unless the draft were ended, that every campus in the country would be essentially ungovernable soon.

LaBerge: Now Rice wasn't. Or were you beginning to have protests there?

J. Pitzer: No, and in fact the oral history that a Rice professor took not many years ago--

LaBerge: Which we have a copy of that we're going to put with this one.[1]

J. Pitzer: At the very end he mentions that and says he thinks that Ken should take credit for his policies that he had established,

[1]See interview with Harold M. Hyman in Appendix.

with consultation between faculty and trustees and students, that his policies were responsible for the cooperative attitude there at Rice. There was a real air of civility and responsibility on the campus, I thought, including among most of the students. But that was during Nixon's period in 1968 when Packard was on campus, and Nixon of course had said he had a plan. He said this before his election in 1968. He had a "plan" to get out of Vietnam and he was going to get out, and so on. One could believe that or not. Everyone hoped that he meant it.

I guess that was partially in his thought, but Ken said he might be interested in Stanford. He felt he had accomplished most of what he wanted to do at Rice, and George Brown had retired as chairman of the board. The new chairman wasn't as forward-looking--well, one thing was Ken wanted him to establish a business school at Rice. The new chairman wasn't very active in that. Ken didn't see that he would be accomplishing a great deal more at Rice, and he was ready for a new challenge. And it meant a return to California.

Stanford University, 1968-1970

LaBerge: What did you know about the atmosphere at Stanford before you got there?

J. Pitzer: We knew there had been protests, yes.

LaBerge: What was the welcome like there when you got to Stanford?

J. Pitzer: Very cordial. Except for the student body president [laughs], who was very self-righteous, I think. And except for the radical groups of students on campus, who got support from off-campus radical groups.

LaBerge: What happened with the student body president?

J. Pitzer: I don't know what he was thinking really, because he wasn't very consistent, but I think he felt he had to represent the student protest movement that was occurring on both the Berkeley and Stanford campuses in general at that time. That's amusing because somewhere in Ken's files --and I haven't found it--but Ken found a letter from this student body president named Hayes--Denis Hayes--written to him when Ken received the American Chemical Society Priestley Award. Ken gave a speech about science's obligation to consider the environment and so on, and be responsible for scientific discoveries and their impact on the culture and environment. And this boy, Hayes, wrote a very complimentary letter practically fawning, I guess [laughter], saying it was the greatest speech he had ever heard. That amused us. I wish I had found that for you, but maybe you'll find it in the files somewhere.

The faculty and trustees were very welcoming.

As far as my role at Stanford--again, I had a very competent, loyal, and devoted staff at the Hoover House. I inherited the Japanese houseman/butler (Paul) who had been employed by Mrs. Sterling. I hired a maid/housekeeper (Billie Lee Bell) who was excellent and a tower of strength. After we left Stanford she continued to be employed by Mrs. Lyman and Mrs. Kennedy. We also had a cook for a short time, but found that that wasn't necessary, as we rarely had dinner at home--almost every night we had dinner at a student residence hall, with a faculty group, an alumnus, or some other more formal function.

We had other help to come in for assistance in big parties. One person who helped one day a week and for parties was Essie. She was born in Ireland and emigrated to this country as a young woman, probably during the 1920s. She got a job at one of the large department stores in Washington, D.C. During the Christmas season she worked as a clerk selling Christmas wrappings and decorations. One day a man came in and gave a large order to be delivered to the White House. He was Herbert Hoover's valet--who became a rather legendary figure. He was a Belgian by birth, I think. One day when Hoover was in Belgium as the administrator of the Commission for Relief in Belgium--this was in the early years of World War I when Belgians were starving--he appeared at Hoover's headquarters in Brussels and announced he would be Hoover's valet and maintain his wardrobe, et cetera. He was Hoover's faithful servant until Hoover's death. He and Essie married and Essie was employed by the White House as a maid/companion to Mrs. Hoover.

Essie was a joy to have around. She never lost her Irish accent or Irish phrases or Irish humor. She told me a great deal about Mrs. Hoover. When we left Stanford she gave me a small brass bowl which had belonged to Mrs. Hoover and which she had given to Essie.

The crew from the buildings and grounds was also excellent. They took care of the large gardens and provided me with the flowers and greenery for my flower arrangements.

We also had a very nice graduate student, Paul Jeremiason, who had been a helicopter pilot in Vietnam. He lived in a downstairs apartment and helped with security as well as large parties.

Faculty Senate's Role

LaBerge: I have a couple of things written down that I know happened at Stanford, that your husband was instrumental in getting done, and I'd like to hear you talk about them. A greater role for the Faculty Senate, for instance.

J. Pitzer: Yes. Ken accepted the position at Stanford in July sometime, I think, but he felt he couldn't leave Rice until the first of the year. So an acting president was appointed: Dr. [Robert] Glaser, who was with the medical school. Ken was aware that the faculty had not been sufficiently consulted in previous years about policies.

I think that was a reflection of Frederick Terman's period as a provost. He was quite authoritarian. He was provost during [President Wallace] Sterling's period and probably also with President Tresidder before that. Then, Richard Lyman succeeded Terman as provost under Sterling, and he apparently continued the same authoritarian policies. That was perhaps why Lyman wasn't considered as president in 1968. That wasn't Ken's style. I think Lyman learned a lot of lessons during Ken's administration which helped him when he was eventually appointed as president in 1970.

Ken was aware that the faculty was rather restless about this, and he and Glaser both agreed that the Faculty Senate should be given a greater role, and Glaser consulted with him. Even before Ken arrived they instituted several measures to give the senate a greater role in the governance of the university.

LaBerge: How was it different from the Academic Senate at Berkeley?

J. Pitzer: Probably it was modeled on it, because Berkeley for years had had, and still has, one of the strongest and most powerful faculty Academic Senates in any university. Ken had been vice chairman of the Academic Senate during the period when President Sproul was the honorary chairman. In those days the vice chairman of the Academic Senate was equivalent to what is now the chairman. He had served on committees of the Academic

Senate here when he was in Berkeley, and the Committee on Committees, and the Committee on the Budget. And the Committee on Academic Freedom and the Committee on Tenure and all those sorts of things. That was before he was vice chairman, of course.

But he believed in a strong role of the Academic Senate, and he put many of those measures into effect at Rice and I think many of the measures that he instituted at Stanford were modeled on the Berkeley plan.

Stanford Research Institute

LaBerge: And what about the Stanford Research Institute [SRI]?

J. Pitzer: I'm not too familiar with the original organization of that. That was during Sterling's and Terman's time. I think it was started with several professors at Stanford--engineering and electronics. Well, they didn't have electronics to begin with, probably--not until Packard and [William] Hewlett established that. Engineering and applied science, I think. This institute was started a few blocks away, not on the Stanford campus as I recall, and there was some involvement with the university financially and they started to accept restricted research from the government, perhaps during the war.

Before Ken went to Stanford there were objections by the students to the conducting of restricted research on the campus. Ken very strongly agreed with that. He thought that any research carried out on the campus should be open, and the professors should be free to discuss their research with students in a professor/student relationship, and there should not be any secrecy of that sort in the academic world. He agreed with that. Stanford students had been involved in a disruptive protest at the Stanford Research Institute. I think this was before we arrived at Stanford. The students were protesting against Stanford's involvement in restricted military research in which Stanford professors participated.

But meanwhile, over the years, the structure had been inherited as it were, and developed, until the Stanford Research Institute was a very prosperous and competent institute which employed professors from Stanford at the same time they were teaching on campus who did do some work on some of the restricted research. The institute had their own board of trustees, I believe. And they got their own financing--

mostly from the government, I think. Anyway, it was a rather awkward situation; it had to be resolved. It was. The institute was separated from--Stanford was separated from any governance and responsibility for the research that--.

I don't know what was done about the policy of encouraging or discouraging professors from consulting with the Stanford Research Institute. I think there was probably a policy instituted restricting just how much research--but whether professors were prohibited from doing research there or not, I don't know. Certainly other professors consulted with other firms for certain periods of time. Of course if professors did use too much of their time gone from the campus consulting, that was objectionable. I don't know just what the ruling was on that.

Hoover Institute

LaBerge: What about the Hoover Institute? Was there something about that too?

J. Pitzer: Another rather touchy situation.

LaBerge: You just walked into everything touchy, didn't you?

J. Pitzer: Yes [laughter]. We inherited several very sensitive and critical issues from the previous administration. [U.S. President] Herbert Hoover, of course, had established the Hoover Institute to contain his World War I papers. As I understand it, when the agreement between Hoover and Stanford University was agreed upon, the Hoover Institute would have a board of trustees who would be approved by the Stanford Board of Trustees. That's my understanding. I believe they could go out and get their own financing to support their scholarly work if they wished to. The first few directors had been eminent historians, some from the Stanford history department, I believe. The director of the Hoover Institute was supposed to be approved by the Stanford Board of Trustees with consultation by the Stanford president, too. Those early appointments were made under those conditions.

But when Sterling was president [chuckles] he told us--I don't know whether it's in confidence or not, but I think it was generally known--that during his presidency the directorship of the Hoover Institute became vacant, and W. Glenn Campbell, as you probably are aware, and who I believe had been teaching at Toronto University, went down to New York where Herbert Hoover was living and interviewed him. He impressed Hoover so much that Hoover offered Campbell the directorship without consulting Sterling or the Stanford Board of Trustees. It was a fait accompli. Sterling was in a very awkward position.

LaBerge: He's still there, isn't he? Or emeritus?

J. Pitzer: Yes. He's no longer director. He's a very outspoken person [laughs] and later was on the Board of Regents of the University of California.

LaBerge: And was very outspoken.

J. Pitzer: Yes. That's another story [laughter]. He was also on the Board of Trustees at Stanford University at the same time he was appointed here. He and another man were on both boards, and when Kenneth arrived he told the Stanford Board of Trustees he thought that was a conflict of interest. They had been appointed by Governor Reagan to the UC Board of Regents. One resigned from the UC board, and I guess--

LaBerge: Glenn Campbell must have resigned from Stanford because he was--

J. Pitzer: I think that's right. I think he was on the Stanford board. Anyway, that's another story.

But to continue with the situation--when we arrived at Stanford, Glenn Campbell was very firmly in the director's seat. I don't think he and Sterling got along too well. But it had evolved into a similar situation that had evolved with the Stanford Research Institute, that members of the board of trustees of the Hoover Institute were being appointed without consultation with the Stanford board or with the Stanford president. The Hoover Institute board was getting financing--Campbell was very good at raising money for the Hoover Institute. They were acting as a separate entity without any input from the Stanford board, which had been the original understanding.

This sort of simmered along during my husband's presidency, and he brought it to the attention of the Stanford board, and they agreed it was a problem [chuckles]. Ken felt that the problem would become critical when Campbell reached retirement age and a new director had to be appointed. How would that appointment be made? They said, "Well, we don't want this to be another Stanford Research Institute situation."

Perhaps they did employ Stanford history professors and so on. Actually, Glenn Campbell was an economist, not a historian--as I recall, anyway. Nothing was really resolved, but the Stanford trustees were alerted and it was a continuing problem during my husband's administration. I think it simmered along during [President Richard] Lyman's administration, and I forget whether it was Lyman or [President Donald] Kennedy that finally came to grips with it. It was resolved, I mean. I think the thing that escalated the crisis in that problem was that Campbell got to be the same age as the Stanford professors' age of retirement. I think he wasn't inclined to retire. That's as I recall it. I think it was decided that that should be resolved so that people in the directorship retire at the same time as Stanford professors, but I don't know just how that was settled.

LaBerge: A researcher could find that someplace else. It's interesting to hear all the roots of it and that it was going along for so long.

J. Pitzer: Kennedy and Campbell came to blows, figuratively, more or less [laughs]. Not actually, but--. When Donald Kennedy's administration was being investigated by a committee in Washington--it was a Senate committee--Chairman John Dingell. It was a congressional committee investigating university overhead finances. I forget whether that was before or after Campbell had to resign. I think probably after. But you can see there was an element of difficulty there. Actually, at the time, Campbell was serving on a committee appointed by President Reagan to oversee some policy decisions.

However, Ken rather enjoyed jousting with Glenn Campbell. Campbell had a keen sense of humor and an acerbic tongue.

Renovation of the Hoover House, Stanford Campus

LaBerge: Just talking about the Hoovers--the president's house that you moved into was the Hoover house. Why don't you just tell us a little bit about the renovation and how you encouraged the architect to write up the plans?

J. Pitzer: The Hoover house had never been actually worked on since the Hoovers moved out. They never lived there for very long, unfortunately. It was Mrs. Hoover who planned the house and oversaw its construction, but they were never able to live there for very long. But she loved it there in Palo Alto. She hoped to make it a retirement home. I don't know that the house had ever been given formally to the university by Mr. Hoover until after her death, just before or at the time of his death. But he had offered it as a residence for two or three presidents, including the Sterlings. But Stanford didn't actually own it, so they didn't put a lot of money into its maintenance. It was in pretty bad shape. It needed a lot of renovating. The roof leaked. And the oak-paneled walls needed to be rubbed down and waxed. We had to bring in a special craftsman to repair some leaded glass windows. The house needed to be rewired electrically. A lot of things of that sort. It was not earthquake safe. The outer walls of the whole house were made of cement blocks with no reinforcements.

They were very fortunate in that the architect that had worked with Mrs. Hoover to plan the house and build it was Birge Clark, who was an alumnus and the son of a professor of art at Stanford. He was still living and practicing as an architect. He was delighted to work with the university to renovate it. He was a delightful man; I enjoyed working with him very much. The house had a very strong personality--Mrs. Hoover's personality, with some rather unique architectural features.

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J. Pitzer: It was a huge house and very original in its plan. Every bedroom had a balcony off of it because Mrs. Hoover liked to sleep outdoors during the summer. That was before air conditioning. They would move the beds out, she and the Hoover boys, and there was one outside the president's bedroom. The top of the house was a terrace where potted plants were. Stairs led up to it from the garden. It was made for entertaining; Mrs. Hoover loved to stage amateur theatricals. She was a very outgoing person, apparently.

That was one thing that I found interesting because when I was growing up in high school, Hoover was president. But Mrs. Hoover stayed in the background. Of all the first ladies who had been written up I knew less about her than other presidents' wives. I thought that the house represented her personality so strongly that I encouraged Birge Clark to tell me about why they did this and why they planned that and so on. Her personality came out very vividly. When she was out there during the summer, she would invite students in for amateur theatricals or, on the spur of the moment, for tea or something like that. She enjoyed having people in, and the house was built with that in mind. I asked Birge Clark so many questions, and he was able to answer them.

He said one day, "I have a lot of letters from Mrs. Hoover that she wrote to me from Washington from the White House during the course of our planning for this house." I said, "Birge, I wish you would write up your experiences about the planning because I think that Mrs. Hoover's personality deserves being illuminated." He said he would. Afterwards he told me he thoroughly enjoyed going over the letters. He had a great number of them, apparently. But he was a bit uncertain about publishing them in a form that would be accessible to the public. Her letters did reflect some of her life there in Washington as well as her life here in Palo Alto. So he got together a manuscript with some pictures and the house plan, and an appendix with a good many of her letters. The whole manuscript told about the planning and the interesting things about features in the house. He had several copies made of the manuscript, and he gave me two of them.

LaBerge: And you've given me one to put in The Bancroft Library.[1]

J. Pitzer: Yes. That was later. I was reading it over one day, and in the introduction he had written in conjunction with the renovation of the house, he said he had enjoyed working with me. The manuscript was left to my discretion as to what to do with it. I was reading it one day and I thought, "Why doesn't he copyright this if he is concerned about the use to which this manuscript may be put?" I phoned him and he thought that was a good idea. He did copyright it, and it's my understanding that it's now published and available in printed form.

LaBerge: Probably in the Stanford bookstore.

J. Pitzer: Probably.

LaBerge: When you were there were the renovations finished? Or were they continuing when you left?

J. Pitzer: All of those we had planned were finished. One thing I enjoyed about the Hoover house was that it was so large that I was able to use all of my art objects I had collected over the years. Several years later--after the 1989 earthquake they felt they had to earthquake-proof the house and do other repairs. When we first arrived we had to stay at the Faculty Club for a couple of months, I guess, while they were still doing the renovations--the rewiring and so on.

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[1]See Pitzer papers on deposit in The Bancroft Library.

LaBerge: But you moved in your own furniture.

J. Pitzer: Yes. We had a lot of our own furniture, and furniture we had bought while we were at Rice. My husband and I felt that any furniture we used for our personal use we should buy ourselves --both at Rice and Stanford. There were very few new pieces of furniture that we had to buy at Stanford, and those that we did we paid for--and which I am still using.

Before we moved to Stanford I asked Mrs. Sterling to provide me with a list of the traditional events which occurred at the President's House. This she did. They included many of the events that occurred at Rice--like entertaining the new freshman class and their parents at a reception, the annual tea of the Faculty Women's Club, lunch for the speakers and guests at commencement time as well as the Board of Trustees, a reception for senior students and their parents, a reception for the fiftieth year reunion of the Alumni Association. At Christmastime there was a reception for all of the women employees at Stanford. All of this was in addition to entertaining at lunches during the football season and also lunches after the monthly board meetings. Again I have probably forgotten things. But it was a continual program and sometimes became rather difficult--in between the student protests. Also, we had to take frequent trips away from the campus to speak to Stanford alumni groups all around the United States. We had just returned very late one night from a meeting with the Los Angeles Stanford Alumni--I think that was the first night that police were called on campus.

Student Protests at Stanford, 1970

LaBerge: Let's talk about the situation with the students. Give me some anecdotes about the student protests. There were a couple now probably that are funny and there are some that aren't very funny, but there was something about red paint.

J. Pitzer: Yes, that was a disappointment. Cars full of young people would come on campus, some of them were not even students at the university. Probably some high school students and off-campus people were involved--in fact, I'm sure of it. They were part of the group, I'm sure, that would come on campus at night and throw rocks and break windows and write graffiti. They threw paint on the walls of the president's house and Provost Lyman's house and on the university residences. There was an article published recently in the Stanford alumni magazine--the Stanford Record?[1] Is that the name of it?

LaBerge: I can look it up.

J. Pitzer: In which they reviewed this period. I think there's a statement by former President Lyman that said that my husband was reluctant to call police on the campus. That, I think, was a self-serving statement, because Ken recognized the necessity and did call in the police, but yes, he did regret that it was necessary. They were called on campus frequently: night after night. The problem was that there were very few police that were available. There was a small number of Stanford policemen and what few there were were poorly equipped [see article by Stanford retiring policeman in Appendix]. The campus was outside the city limits of Palo Alto, so we had to depend on the sheriff and sheriff's deputies. When they were called, these deputies came from all the cities around there: Palo Alto, San Mateo, even San Francisco. But they were called night after night and they became exhausted, of course.

It's such an open campus that it was very easy for these groups of students and nonstudents to evade them. The police were exhausted. There was a faculty committee that Ken had appointed to advise him on these disruptions, composed of Provost Lyman and members of the Academic Senate. Everybody was exhausted, of course. There was some pressure--of course alumni were very upset and I don't blame them. And the trustees were upset. There was some pressure to call in the National Guard. This my husband absolutely refused to do, especially after the Kent State University [in Ohio] massacre [spring 1970]. He had predicted before then that to call in the National Guard could only result in a tragedy similar to what happened later at Kent State.

LaBerge: One time you told me about hearing about that on the radio. Why don't you tell me for the tape? You were at home listening.

J. Pitzer: This must have been March of 1970. It was after the secret invasion of Cambodia, and there had been some serious rock throwing and damage on the campus as a result of that invasion. The Faculty Senate committee that advised Ken on such campus disruptions had scheduled a meeting. To go back, previous to the invasion of Cambodia, Ken had had some warning or some

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[1]See "Years of Hope, Days of Rage: Twenty-five Years Later," Stanford, September 1995.

information that this was what President Nixon was considering. He told me at the time that if that happened every campus in the country would explode--which did happen. And a lot of the violence on our campus resulted after that.

Then there was the Kent State shooting. That shocked everybody, of course. That morning when the Kent State massacre occurred, he told me at breakfast that there was a meeting scheduled of his committee on campus disruptions that morning, and he was concerned that there would be even more pressure to bring in the National Guard. Whether Provost Lyman wanted to do that, I don't know. Maybe. I just don't know what his opinion was on that. But my impression was that he was one of those who were in favor of calling in the guard. Of course Ken was not going to do that--wisely, I think, in view of what happened at Kent State. And it was Ken's courage in standing against that which probably prevented a similar disastrous tragedy at Stanford.

I knew that there was a meeting that morning, and about noon I was listening to the radio and heard the CBS program interrupted with a flash that there had been a shooting on the Kent State University campus in Ohio and there was an unconfirmed report that two students had been killed by the National Guard on the campus and many others seriously wounded. But it was unconfirmed and they would interrupt programs later when they had investigated and had the result of the investigation.

Knowing that this committee was meeting, I phoned his secretary and dictated a message for her and asked her to take it in to my husband at this meeting immediately, which she did. I guess the meeting dissolved so that they could--I don't know what happened to the meeting, but I think they then did everything they could to keep calm on the campus. It was only a short time later that confirmation on the radio was that there had been four students killed, two men and two girls, and many seriously injured. All the campuses were in a state of shock all over the country then. It was out of this committee of campus disruptions that Ken was meeting with that morning that the idea came to send Stanford students to Washington in coat and tie (and haircuts, probably!) to ask for appointments with Congress and the president and with David Packard.

LaBerge: What was David Packard in Washington?

J. Pitzer: He was assistant secretary of defense. Actually [Robert] McNamara was still secretary, but he was traveling a lot and Packard was really doing a great deal of his work. It was the idea to send students to Washington to interview Congress and the president and people in power to tell them how students felt about the war. They not only felt that Stanford students should go, but they sent messages around to other universities --many other universities sent students down in coats and ties, quite the opposite of the violent protests occurring on the campuses. They acted very politely but gave very strong opinions, of course. Stanford University was the first one to do this, and I think it was Professor Dornbush, who was a professor of sociology, that organized this. I don't know whose idea it was originally, but he helped. And other members of this committee did too. I think many of the faculty contributed their own money to the transportation fund for the students, as we did also.

LaBerge: All along before that, hadn't he met with student groups?

J. Pitzer: Continually. He had hoped that there would be some reasonable element of the student body that would eventually emerge to calm things down. This did happen later and the radicals lost support in the student body. Actually, if you remember, there had been a sit-in at the engineering building, I believe, previous to the Kent State massacre. I think it lasted for a week or so. Do you remember that?

LaBerge: I don't remember that.

J. Pitzer: I don't remember when it started--whether it had started before we arrived--I don't think so. I think it was also associated with the Stanford restricted research they were objecting to. Quite a large group of students sat in and took over the building so that classes couldn't continue. The policy of Ken's administration was they were free to come and go, go out and get food, come back, but there was always a group there to sit in. It lasted over a week, I guess. At all times Kenneth insisted at least some members of the Stanford police force be present. At least two or three, if not more, members of the Stanford police force were just in the building at all times to observe--there was no damage done inside, as I recall. It was a very quiet and orderly sit-in, but yes, they were protesting against the restricted research being conducted by some of the engineering professors.

The Stanford police were friendly and restrained people. At one time during the sit-in, the students talked them into leaving their guns--the police had guns with them. The students persuaded the police to leave their guns outside or not bring them in. Unfortunately, there were some restricted government papers stored in the building. I don't know how sensitive they were, but there was a lot of--I don't know if it was from the war [World War II] or postwar or what, but there was a lot of information there that was restricted and fairly sensitive. Ken told the police, "No, you must retain your guns." So they managed to rearm the policemen in a way that didn't disturb the students too much. I thought that was rather interesting, that they accomplished that. There was some pressure from the government agencies to send in the National Guard or the army to protect the papers. But the students were not harming them and Ken felt that to do that would radicalize the rest of the student body, as happened at Sproul Hall in Berkeley. He waited them out and after about ten days they gave up and ended the sit-in peacefully.

After the Kent State massacre and the students went to Washington and interviewed a good many people, including the president, I guess, and members of Congress and Secretary Packard, there was a period of calm. Shock and emotional calm, I think, following that. It was during that period that Ken wrote a memo to the board of trustees.

LaBerge: The memo that you gave me? That he didn't send?

J. Pitzer: Yes. I believe that was the memo to the board of trustees that he didn't send. He was exhausted physically. Not emotionally, but he was exhausted. He showed stress. It was a very stressful period, and he didn't enjoy all his time being spent being a policeman and not being able to accomplish some of the academic ideas he had thought were necessary and desirable. And Stanford faced the necessity for conducting a drive for capital funds.

LaBerge: Alumni were involved in that, right? In the capital funds drive?

J. Pitzer: Oh, they would have to be. Of course a good many of them still supported the war and David Packard was a very important alumnus. Without his backing a drive for capital funds wouldn't go very far. Kenneth never did enjoy raising money for that. He told the trustees at the time he came that he didn't want to take an active part in raising capital funds. They said, "No, that's our responsibility." With all the disruptions on campus, positions were hardening on both sides, including alumni and trustees. There were times when Ken felt that he could continue, and he eventually decided that he didn't want to. He predicted that the violence wouldn't end until the Vietnam War ended.

<u>More on the Vietnam War</u>

LaBerge: Did anyone ever say something to him like, "Please don't speak out your opinion on the war"?

J. Pitzer: Yes, definitely. He gave speeches to students, at both commencement and matriculation, saying that he felt the war was wrong.

LaBerge: What about the ad in the <u>Washington Post</u>? Tell us about that.

J. Pitzer: After Ken had expressed himself strongly against the war several of the trustees said, "You should keep your opinions to yourself," which he didn't agree with. He couldn't accept that. After all, it was a major element in campus governance. Senator Frank Church, the senator from Idaho, came out to the campus in the spring of 1970. April, I guess it was. He gave a speech--I believe his son was a student there at the time. His speech mostly had to do with the war and how it was being governed without much input from Congress--Congress hadn't actually yet declared war. I don't remember too clearly. He did quote an historian--I believe it was Carlyle, but I'm not sure, and I can't quote the phrase exactly, but it was that when the left and the right go to extremes the center cannot hold. [added later:] I have now found the quotation since I last talked to you. I had thought it was originally from an historian, but this quotation is from the poet W. B. Yeats, written in the aftermath of World War I: "Things fall apart; the center cannot hold/Mere anarchy is tossed upon the world." [end insert] I thought the atmosphere in the lecture hall--a small one--was vibrant with the feeling that that quotation expressed--that the structure of the university and the structure of our government were both under attack and under stress. The mostly adult audience was very silent, thoughtful, and sobered.

The mayor of Palo Alto was sitting next to me. He was a Democrat--

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J. Pitzer: --and [the mayor] was in entire agreement with Senator Church. Church was also a Democrat and a statesman. Wasn't he chairman of the Senate Foreign Affairs Committee? And he was very courageous.

LaBerge: He was chairman of something. I remember him being very outspoken.[1]

Ad in the *Washington Post*

J. Pitzer: I had come across an article just that morning before his speech that Church had written in a magazine discussing the different roles of Congress and the president in conducting a war. I took it over to Ken at his office because I was going to the speech, and Ken used the quotations from Church's article to introduce him. It was a very impressive, sober, and thoughtful occasion.

Anyway, the mayor of Palo Alto went back to his office and wrote a very brief but effective statement. It was later published in the Washington Post with signatures of citizens of Palo Alto and financed by citizens' contributions. The headline was "Congress, you must act for us," in big letters, and then this very simple, dignified statement of opinions about the war and the role Congress should take to end the war. Then he made this petition available to the citizens of Palo Alto, and thousands signed it. The first name on the petition was the mayor's, and the second was Kenneth's name--not, of course, identifying him as president of Stanford, just his name as a citizen. It was published in the Washington Post--I don't know how many pages, all six columns on each page. There were thousands of names.

LaBerge: Was it your name also or just his?

J. Pitzer: No, just his. I would have been glad to sign it but I knew nothing about it. It was at least five or six pages long of six columns each, just fine print. It was a very impressive statement. This was published, I believe, in the week of May in 1970. If you wanted to look it up in the newspaper file I'm sure you could find it. I had a copy that my son and daughter-in-law who lived in Washington sent me, but somehow it's been misplaced. The Washington Post the next day or soon thereafter wrote an editorial saying it was the most effective and reasoned demonstration of public opinion and public protest that had occurred. It was done in a dignified, respectful

[1]At the time, Senator Church was a member of the Senate Foreign Relations Committee. In 1979, he became chairman. He received a B.A. (1947) and LL.B. (1950) from Stanford University.

manner and was very effective. But neither President Nixon nor Secretary Packard appreciated it, I'm sure.

LaBerge: Did you have repercussions from that, do you feel?

J. Pitzer: I don't know. It's just the general atmosphere of some of the trustees thinking my husband should keep his opinions to himself, which he was not about to do. He felt that the minimum for the campus unrest to defuse--if Nixon kept his promise about getting out of the war was that he didn't need to continue the draft. At a minimum, that would certainly defuse protests on the campus. Ken reasoned that if Nixon really meant to end the war, there was no reason to draft and train thousands of more men.

Shortly after we first arrived on the campus, and that was the first of the year in 1969, just after Nixon had been elected in 1968 with the promise that he was going to get out of Vietnam. Nixon said something like, "I have a plan; I'm going to get out." We were both very doubtful that he really did have a plan, that it was just a slogan for the campaign. Shortly after we moved into the president's house, John Gardner, who was Nixon's Secretary of Health and Welfare in his cabinet and was also on the Stanford Board of Trustees, came to the President's House for a drink with us. He was very supportive of my husband but believed that Nixon really meant to get out of the war. Ken felt the very fabric of the nation was being torn and Nixon didn't really have a plan to stop the war. John Gardner was very hesitant, but I felt it had never occurred to him that that might be the case. I think he was quite shaken.

I think one of the most gratifying occasions in Ken's public service--gratifying to him, I mean--was during LBJ's final year as president. Ken was on the President's Science Advisory Committee (PSAC). The members of PSAC were convinced that the belief that we were winning the war in Vietnam was not correct, as opposed to the opinions being given to Johnson by other advisors. As I understood it, PSAC tried to convince McNamara of this but were unsuccessful--this was before Tet. But it may have contributed to McNamara's decision to resign after Tet. Clark Clifford was then appointed as secretary of defense and came to a meeting of PSAC. He grasped the significance of their arguments immediately, as I understand it, and undertook to convince Johnson.

That was probably a large factor in Johnson's decision not to run for office again, at least Ken felt that PSAC meeting with Clifford was significant. A picture of the PSAC members at this meeting was taken and distributed to the members. I believe I have given you a copy. I don't remember everyone who was on that committee, but I recognize the pictures of Charles Townes, William Hewlett, Jerome Wiesner, Donald Hornig, et cetera.

Another incident I recall was that we received a phone call from the San Francisco office of the Internal Revenue Service telling us they were sending an agent down to audit our personal income tax. This was a favorite technique of Nixon's staff to harass opponents of Nixon and the war. I don't think we ever officially made it onto Nixon's "enemies list," but this came close.

A very nice, very embarrassed IRS agent arrived and conducted a very short, very perfunctory review of our tax--not a real audit at all! Ken did not remember this but I do--vividly.

Also there were some puzzling incidents that made me think that our phone at the Hoover House was being tapped. That may have been my imagination but I could find no other explanation. Since we had no major concern about that, I did not make a fuss. I don't think Ken agreed with me that this happened.

LaBerge: Vietnam was behind everything, wasn't it?

J. Pitzer: Yes, and all the campuses and especially the administrators of the universities were like lightning rods.

ROTC

J. Pitzer: Another issue on the campus was whether to maintain the ROTC [Reserve Officers Training Corps]. That was another sore spot with students and faculty. I think they told him that my husband's statement was good. I'm not sure this issue was resolved. I think that at the time we arrived in Stanford the ROTC program was mainly controlled by the U.S. Department of Defense. Of course the army and navy conducted training on the campus, and I believe they more or less prescribed what general curriculum their ROTC members should take, which didn't sit well with my husband. He thought that the students should have more liberty to choose their own curriculum, and that the training should be auxiliary to academic and faculty requirements. He talked to the officers who were on campus who were in charge of ROTC--both army and navy--and they were agreeable to changing that. I think that was about as far as that problem went. It simmered along, too. Ken tried to get the faculty to get in on that too, and it just simmered along. Pluses and minuses. I don't know how it was finally resolved.

LaBerge: What else would you like to say about the Stanford time for the record? And then if we don't get it on tape and you want to add something--

J. Pitzer: I was glad that he decided to leave Stanford. I could see how stressed and fatigued he was. But I didn't realize until much later that I also was very stressed at the time.

[The years at Stanford were brief, intense, and exhausting. But Stanford came through those years in relatively good shape.

It was Ken's firm belief that the faculty was the core and the heart of a university. He had observed how the essential, basic fabric of other universities was torn by the deep division of the faculties--at Columbia, Harvard,[1] and, of course, at Berkeley. That did not happen at Stanford. Ken held the faculty together. This was the most important thing with which I think he should be credited. He consulted with the faculty on every issue, especially with the committee on campus disruptions. He informed the faculty of every proposed action, and they supported him--even gave him several standing ovations at faculty meetings.

Every university campus in America was totally unprepared for the legal ramifications arising from the clash between the traditional aloof, impartial, "ivory tower" atmosphere which conflicted with the student revolution and its attempt to politicize and control the campuses.

Ken appointed the first legal counsel to the president at Stanford. He consulted with faculty committees to establish standards so that when, later, in President Lyman's administration, a hearing was held on the dismissal of a tenured professor, it was done so by established rules consistent with the highest standard of academic freedom and in a calm and dispassionate atmosphere. I doubt if this hearing could have been held in such a dispassionate atmosphere if the faculty had been badly divided earlier on other issues.

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[1]See Coming Apart by Roger Rosenblatt.

He consulted with faculty, student body officers, and the Student Body Association Judicial Council to establish rules and regulations so that when, in President Kennedy's administration a group of students took over the president's office, rules and regulations for their discipline were in place.

Since that hearing and dismissal of the tenured professor during Lyman's administration has received extensive publicity, I might add a few remarks about it.

During the previous administration--I think when Lyman was provost--a rule had been made at Stanford that anyone who held a rally or demonstration at the home or on private grounds of a member of the administration or those of a member of the faculty was subject to dismissal. I believe this rule applied to both faculty and students. This rule was ambiguous at best, as I understood it, since the Hoover House was owned by the university and many faculty homes were built on land leased from the university.

The tenured professor mentioned above organized and led a large group--students and nonstudents probably--on a march to the Hoover House one night. There he addressed them in the gardens with a bullhorn. When we had first arrived at Stanford I had requested that flood lights be installed outside to brightly illuminate the extensive lawns and gardens of the Hoover House. We just turned on the flood lights and let him speak. One Stanford police car was parked unobtrusively on the other side of the house.

I did not listen to his speech, but my staff said it was full of demagoguery. Members of the faculty committee on campus disruption monitored the speech, of course. No doubt this professor hoped to provoke the administration to some repressive action which would radicalize and divide the campus. But after his speech the crowd dispersed peacefully.

As I understand it, Provost Lyman the next morning wanted to charge this professor with breaking that ambiguous rule, and hold a hearing for his dismissal at that time. But my husband thought it was too weak a case and would not hold up to the standards of the AAUP (American Association of University Professors). It wasn't until later that this professor apparently stepped over the line.

Many alumni and even a few members of the Stanford Board of Trustees could not understand why a tenured professor could not be dismissed arbitrarily, "like in a business." It is a very serious matter for the administration or the board of trustees of a university to be censured by the AAUP. I recall a case in which the board of trustees of a university was censured. The university lost a large proportion of its most valued senior faculty and found it was impossible to recruit young men who would accept appointments to replace them under those conditions. It was many years before that university recovered. Since my husband had been a member of the board of trustees of three different colleges, as well as a member of the board of directors of a large corporation, he understood all of the issues involved. He was a staunch defender of academic freedom.

There were a few amusing incidents that occurred during our time at Stanford--not many, but a few. One of the most amusing was an episode generated by the editor of the Stanford Daily, the student newspaper. The editor was Philip Taubman--I don't think I have that last name correctly. He later had a very fine career in journalism--I think with the New York Times or the Boston Globe. He had a sense of humor and a sense of proportion--characteristics which were not common to many editors of campus newspapers in that period.

One morning in April of 1970, I think it was, the Stanford Daily had a large picture on the front page. It was of an installation "hidden" in the hills back of the Stanford campus. The large round installation was surrounded by a high-security fence. The big accompanying headlines described this installation as a secret missile site or as a nuclear reactor or some such similar installation. The accompanying article had overtones of sinister secret research being conducted on the Stanford campus.

When my husband arrived at his office that morning at eight o'clock, the first thing he was shown was this issue of the Daily. He was told that the Los Angeles Times had a camera crew and reporters en route in flight to cover the story. I was told my husband roared with laughter. He told the director of communications to phone the editor of the Los Angeles Times and ask him if he didn't know what date it was. It was, of course, April 1--April Fool's Day. The Times crew was paged at SFO and told to return to L.A. by the next flight. The "sinister" installation was a covered water reservoir, one of the main sources of water for the campus and was visible from the highway. It had been there for many years.

The protests and necessity for police action continued on into the early 1970s into President Lyman's administration and did not diminish until Nixon finally decided to end the Vietnam

War in 1974, which was the major reason for the restoration of order on all of the campuses, including Stanford. Most universities have recovered. Fortunately they were strong enough to do so. Some ideas which arose out of the student revolution have been incorporated in the universities, but others were transitory and generational and not viable.

Ken did not relish spending all of his time on police and discipline matters. He had hoped to introduce measures to strengthen undergraduate education and did do that among other things. He did introduce the concept of alumni trustees on the Board of Trustees, among other more important things. Records and papers documenting Ken's policies and actions during his administration at Stanford are contained in the presidential files which he left in the president's office at Stanford and should be available there. But mostly he missed his research, for which he had no time.][1]

Invitation Back to Berkeley

LaBerge: He is one of the only presidents who went through that period who went back into academic life. Isn't that right?

J. Pitzer: That's right--productive academic life. There's only one other president that I can recall, the one at the University of Virginia--he was a professor of English or history who was productive after retirement. Most presidents retire and take on a job at a foundation or retire. They don't keep abreast of their own field or aren't able to. That was one reason why Ken decided to retire, because at Rice he was able to have some postdocs, and he did do quite a bit of productive work in chemistry at Rice. Of course, that all stopped with all this difficulty at Stanford.

LaBerge: Although his plan was to--

J. Pitzer: He had hoped to have some postdocs and do some research, as he had done at Rice. He was a professor of chemistry as well as president. When he resigned--well, he was offered some presidency of more than one foundation, but he didn't want to do that; he wanted to get back into chemistry. He was offered professorships at several universities. He could have stayed

[1]Bracketed material was added by Mrs. Pitzer during the editing process.

on at Stanford as a professor of chemistry. We talked it over and seriously considered it. Several days after he resigned at Stanford, the dean of the College of Chemistry here at Berkeley and the chairman of the department came down to see him. The dean was [David] Templeton, and the chairman was Bruce Mahan. They said they wanted him back, that they hoped we would come back to our friends here. They had already had a meeting of the faculty of the college. Ken told them that he was going to take a year's sabbatical before he decided, and we did. We traveled around the world to Samoa, New Zealand, Australia, and Greece, and ended up in Cambridge, England, for several months, associated with Cambridge University. He did some chemistry there and did a lot of reading in the library and so on [chuckles]. He also gave several lectures.

During our stay at Cambridge we lived in the Master's Lodge of Sidney Sussex College. This was arranged by Jack Linnett, professor of chemistry at Cambridge and head of chemistry at Cambridge, who had just been elected master, but had not yet moved in. Jack later served a term as vice chancellor of Cambridge University. Jack and Rae Linnett had spent a semester at Berkeley, during which Jack had taught Ken's classes in thermodynamics during a semester sabbatical which we spent in Europe and England--mostly at Leiden and Oxford. That was in 1955 or 1956 during Ken's Guggenheim Fellowship. During Jack's term in the 1970s as vice chancellor of Cambridge University, Jack had to deal with student protests and violence. He died of a heart attack due to the stress. If he had lived, Jack would probably have been knighted. Margaret Thatcher was Prime Minister when he was vice chancellor. Jack had been her tutor in chemistry at Oxford when he was a professor there.

Berkeley had said that there would be some--well, not difficulty but delay and red tape--to bring someone in at that level would be rather unprecedented [laughter]. But they would set it in motion and have it ready when he was ready to decide. They had to go to the Regents to get the approval to bring back a full professor [laughs], which they did. The Regents approved the appointment.

After we returned to Berkeley, the successive deans of the College of Chemistry have told me that they found Ken to be a wise counselor and source of valuable advice in their successful efforts to maintain the premier eminence of the college, frequently rated as the best department in the country. At one time Ken was asked to be the acting dean during a time when the current dean was on sabbatical leave. He thanked them but declined.

We were very happy to come back to Berkeley. We had kept our home here in Berkeley on Eagle Hill. It was wonderful to come back to our view of the bay from our windows. Ken was very productive in chemistry in those years; he taught until 1984 when he became emeritus. His graduate students and postdocs--he had postdocs up until the time of his death. I think he published over 200 papers in that period--about 400 papers in his lifetime, and several books, including the third revision of Thermodynamics.

[tape interruption]

LaBerge: You were just saying that he had done 400 papers, several books, the third revision of Thermodynamics.

J. Pitzer: Yes. And he was asked to get together a collection of his most important papers in a series of books on 20th century chemistry. His book Molecular Structure and Statistical Thermodynamics was volume I of that series.

He was asked to write an introduction to his most important papers, and they were published in 1993. He also edited a book titled Activity Coefficients in Electrolyte Solutions, second edition, published in 1991.

[Throughout his career my husband had opportunities offered to him to accept executive positions in foundations or industry. But he was devoted to the academic life. I think he imparted that devotion--his philosophy and idealism--to many of his graduate students, over half of whom accepted positions in universities instead of industry. Two of his graduate students, Professor William Gwinn and Professor George Pimentel, became valued members of the chemistry department in Berkeley.

There is a tradition in the Pitzer family to support education, starting with Ken's great-grandfather, Claiborne Pitzer, who donated part of his land on which to build a schoolhouse in pioneer Iowa in the 1830s. Ken's father helped to establish three of the Associated Colleges in Claremont, California, including Pitzer College. His total fortune, which was substantial, was given to these colleges. Ken was a trustee of three colleges during his career--Harvey Mudd College, Mills College, and Pitzer College.

Ken established the Flora Sanborn Pitzer Professorship in mathematics to honor his mother's memory at Pitzer College. Since Ken's death the Pitzer Family Foundation, which consists of myself and our three children as trustees, has funded the Kenneth S. Pitzer Distinguished Professorship in the Department of Chemistry at the University of California at Berkeley, as well as the Kenneth S. Pitzer Professorship in Science and the Jean M. Pitzer Professorship in Anthropology at Pitzer College. My three children also funded the Jean M. Pitzer Archaeological Laboratory at Pitzer College.

As far as I was concerned, personally, it was heaven to be back in our own home, with nobody intruding on our private life. In both the President's House at Rice and the Hoover House at Stanford there were always unannounced workmen wandering around--furnace repair men, electricians, et cetera, who might come to do some repair work. It was also delightful to be leisurely at breakfast with the morning newspaper after so many years of necessary orders to be given in the morning.

I also now had time to pursue my interest in archaeology. Professor Robert Heizer of the Department of Anthropology here at Berkeley had encouraged my interest in lithic technology. He had sent me a collection of lithic artifacts collected from the Santa Barbara Channel Islands to analyze while we were at Rice.

I hadn't had time to do this. But after our return to Berkeley, I worked as a volunteer on that collection in the anthropology department with Heizer and Tom Hester, one of his graduate students, currently professor of anthropology at the University of Texas-Austin and director of the Museum of Anthropology. There were several other graduate students working in the same laboratory room. I was delighted and pleased with their attitude toward me. They were respectful and helpful and treated me like just another student.

I gave a paper on that research at the annual meeting of the Society for American Archaeology when it met in San Francisco in 1974. The paper was later published and was the first paper on the Channel Islands' technology. I have also had two monographs and another paper on lithic technology which were published. I think I gave you copies of them.[1]

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[1]See Pitzer papers in The Bancroft Library.

I am currently working on a collection of artifacts I collected from the beach at our place at Clear Lake, which I think is quite important.][1]

Building and Sailing Boats

J. Pitzer: [Since boats were such a big part of Ken's life, I might add something here about our good times and our experiences in boats.

Before we were married, in fact when he had just graduated from high school, Ken built a sixteen- or eighteen-foot sloop designed by William Atkin, named the "Jean." Previous to that he had only built model boats--rather elaborate ones--and a rowboat.

During his college years he kept the "Jean" anchored off the home of his father and stepmother at Lido Isle in Newport Bay. We often sailed in it on weekends. During his college summers he sailed it, with a college friend, to Catalina Island and back. When we married he left it there as it was not easily trailered.

During the winter of 1935-1936, when we first arrived in the Bay Area, we bought an old motorboat hull about twenty-two feet long. Ken and my brother-in-law, Arthur Browne, spent weekends repairing it and recaulking it, and installing a secondhand two-cycle Fairbanks and Morse inboard marine gasoline engine, and then constructing an open shelter cabin, with a pipe berth that folded up.

Ken had always read about the beautiful waterways of the San Joaquin and Sacramento River Delta. We wanted to explore them. In July 1936 we loaded our sleeping bags, camp stove, food, cans of water, and gasoline et cetera on board the "Jakey," and left our berth in the Richmond Harbor intending to be gone ten days to two weeks. First we crossed San Pablo and Suisun Bays and then went down several sloughs to the San Joaquin River and then through the Stockton Channel to Stockton. We shared the channel with several big freighters.

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[1]Bracketed material was added by Mrs. Pitzer during the editing process.

After exploring the Stockton area, we went through Little Potato Slough and beautiful Georgiana Slough to Steamboat Slough and then the Sacramento River. At nighttime during this trip we would tie up to trees along the banks. When we spent our first night in the Sacramento River we were awakened twice by the beautiful stern wheel steamboats, the "Delta Queen" and "Delta King," when they passed us all lit up--making their nightly trips between San Francisco and Sacramento.

The next morning we started the engine and then it abruptly stopped. Ken took the engine apart and found that a crankshaft balance weight had broken off and the connecting rod bent. We paddled down the river to the little town of Courtland where a garage mechanic kindly lent us the necessary extra tools. We could not get any spare parts, we learned by phone call to San Francisco, because the engine was last built in 1910 and then the plans thrown away!

So Ken decided to reassemble the engine to operate on one cylinder instead of two. The broken piston was put back so as to block intake past the dead cylinder. The engine worked fine. We had spent about a week on the trip and decided to return home. We proceeded via Steamboat Slough to Rio Vista and anchored for shelter behind Decker Island.

Since I was six months pregnant, we decided to cross Suisun Bay at nighttime as the bay was quite rough going against the wind and the bow tended to pound on the waves. After dinner the waves calmed down and we crossed Suisun Bay and went through the Carquinez Straits, again sharing the channel with freighters.

In Suisun Bay we saw the old steamboat "Yale" which had been mothballed. She had operated for many years with her sister ship "Harvard" on overnight trips between San Francisco and Los Angeles. During the war I believe she was taken out of mothballs and served on the East Coast.

I was steering late at night in the Carquinez Straits, steering from navigation light to navigation light, when I was startled by signal blasts from a freighter overtaking us silently. We answered her signal with our little signal horn and scooted out of her way. We anchored behind the breakwater near Mare Island and leaving early the next morning before the San Pablo Bay became rough, went on to our home berth in Richmond. We also had to keep out of the way of the large auto ferry "Calistoga" on the run between Vallejo and San Francisco. She passed us several times before we reached Richmond as the tide was against us.

The next summer we again explored the Sacramento River area in the "Jakey." The engine continued to run beautifully on one cylinder during the remaining two or three years we owned the boat. It appeared to run just as fast and used only about half the gasoline consumed before.

We explored the Napa River and Petaluma Creek and took her to Palo Alto one summer. Also on calm moonlit nights in the summers we would take some friends or my sister and her husband and some supper and cruise across the bay to Tiburon, Strawberry Cove, Richardson Bay, et cetera. Then we owned a thirty-foot sailboat "The Ruby," a very fast, very wet San Francisco Bay type of an older vintage. We took our two children out on that.

The next boat was the "Xmas," a ten-foot sailing dinghy--so named by Ken because it was green inside, red on the outside, and abbreviated. We built that in the garage on Avon Road and our two older children helped by putting in screws in the hull for Ken to fasten. We sailed that in the Richmond Channel--occasionally in the Bay, but mostly at Clear Lake after we bought that place in 1945. We still have it.

We had wanted a place which had warm weather, dependable northwesterly winds for sailing, and warm water for swimming. Clear Lake provided all that. During our years in Houston we would return to Berkeley every July. Ken would go to the Bohemian Club encampment. I visited my sister Mildred Glacken and also would consult Professor Heizer in anthropology about my archaeology projects. Then we would go to Clear Lake for a couple of weeks. We would also maintain contacts with our friends in Berkeley then.

When we were in Washington during the war, in the summer of 1944, we chartered a lovely old Friendship sloop which was based at Annapolis. We had always believed in taking our children sailing with us. The owner of the Friendship was aghast that we planned to take three young children--John was only three--with us. With another couple to help sail the boat, we explored Chesapeake Bay and the rivers and harbors of the Eastern Shore. Everything went beautifully. The children helped to sail the boat and enjoyed swimming in their life preservers over the side of the boat.

During the summer of 1946 we borrowed a Jr. Clipper Class sailboat which belonged to some cousins of Ken's and we took our three children on a week's cruise up the Steamboat Slough and Sacramento River area. By that time Ann and Russ were old enough to be a very competent crew. Ann was ten and Russ was eight.

During the winter of 1947 Ken started to build the "Jean II," a sharpie design twenty-eight-foot sailboat he designed with a centerboard and water ballast tanks so it could be trailered. We took the hull to Clear Lake in the summer of 1948 and he finished it there.

It was fortunate it was a trailerable boat because we went back to Washington at the end of 1948 for the AEC job and took it with us. We kept it most of the time at a small shipyard at the mouth of the Rhode River in Maryland, just off the Chesapeake Bay. We sailed it on the Chesapeake the summers of 1949 and 1950, but trailered it back to Clear Lake in 1950 because we expected to spend a year in Oxford, England, after we left Washington in 1951. The Korean War and the first test of an atomic bomb by the Russians interfered with those plans, delaying our departure until the end of summer 1951, when we returned to Berkeley.

We sailed the "Jean II" on Clear Lake for a number of years and then bought a Highlander Class sloop, designed by Douglas. It was a fast planing boat which required lively action by the crew in gusts of wind. We trailered the Highlander--I don't think we ever gave it a name--to Houston to sail on Galveston Bay which we did for a summer or two.

After that we bought, with our friends the Gordons of Rice University, a Rhodes 27 sloop which was more suitable for Galveston Bay. We gave our share of the boat to Rice University when we left Houston. Bill Gordon was an electronic engineer, dean of engineering at Rice. Ken brought him to Rice from Cornell University. He designed and built the "Big Dish" at Arecibo, P.R., used by radio astronomers. He was a member of and later the foreign secretary of the National Academy of Sciences.

There was very little sailing time during our years at Stanford--even at Clear Lake. When we returned to Berkeley Ken immediately started testing designs for his next boat, which turned out to be a sixteen-foot open sloop with a keel, named the "Susan" after our granddaughter. It was a fast, comfortable boat. That was our last boat except a very lightweight, transparent dinghy, mostly plastic strips, which was named the "Ann E" or "Annie" after our daughter. We had previously owned a wooden dinghy in the Chesapeake which she had helped to build, also named after her.

The summer of 1967 we chartered a thirty-six-foot L36 class boat out of Vancouver, B.C., and explored the Straits of Georgia and various inlets of the area north of Vancouver. Connie and Art and Russ were with us on that cruise. I believe it was 1969 when we chartered the same boat and sailed with our friends the Gordons and Ann in the same general area. We have also chartered a sailboat in the Mediterranean and the Bahamas, both times with the Gordons and Ann.

Ken and I have also sailed a boat out of St. Thomas in the Virgin Islands.

Oh yes, we also owned a fast ski boat during our children's high school and college years so they and their friends could waterski.

There was also an experimental boat that was named the "Little Dipper." Ken experimented with the design of a rigid sail with it. But since the lightweight, strong materials for mast and sail were not available then, the sail was not a success. But the theory was correct.

I could also say a great deal about our adventures in our camper--an eight-foot Alaskan camper on a four-wheel-drive pickup. We had lots of adventures in it all over the U.S., including Alaska and in Canada. Ken enjoyed taking it on remote roads in mountainous areas--it seemed that the more remote and difficult the road, the more he enjoyed it. At the end of these roads he would go on long hikes, but I would stay in the camper, knit, and enjoy the view.][1]

Family

J. Pitzer: Finally, I have said very little about our children and our tremendous pride and satisfaction in their very successful careers and also in our pride and love of and for our five grandchildren.

Our children have given Ken and me a depth and meaning to family life and our relationship to one another. Our son-in-law and both daughters-in-law have enriched our family greatly.

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[1]Bracketed material was added by Mrs. Pitzer during the editing process.

LaBerge: Thank you very much for spending the time doing this.

Transcribed by Shannon Page, Gary Varney, and Caroline Sears
Final Typed by Mary Mead and Shannon Page

TAPE GUIDE--Kenneth and Jean Pitzer

INTERVIEW WITH JEAN MOSHER PITZER

APPENDIX

KENNETH S. PITZER

Personal

January 6, 1914	Born, Pomona, California
July 7, 1935	Married Jean Mosher
Children	Ann, Russell, and John

Education

1935 B.S.	California Institute of Technology, Pasadena, CA
1937 Ph.D.	University of California, Berkeley, CA
1962 D.Sc. (hon.)	Wesleyan University, Middletown, CT
1963 LL.D. (hon.)	University of California, Berkeley, CA
1969 LL.D. (hon.)	Mills College, Oakland, CA

Positions Held

1937-1961	Instructor through Professor of Chemistry, University of California, Berkeley
1943-1944	Technical Director, Maryland Research Laboratory, Washington, D.C. (on leave from U.C.)
1947-1948	Assistant Dean, College of Letters & Science, University of California, Berkeley
1949-1951	Director of Research, Atomic Energy Commission, Washington, D.C. (on leave from U.C.)
1951-1960	Dean, College of Chemistry, University of California, Berkeley
1961-1968	President and Professor of Chemistry, Rice University, Houston, TX
1968-1971	President and Professor of Chemistry, Stanford University, Stanford, CA
1971-	Professor of Chemistry, University of California, Berkeley

Other Offices Held

1956-1961	Harvey Mudd College, Trustee
	National Academy of Sciences:
1958-1961	Chairman, Chemistry Section
1964-1967	Council, also 1973-1976

1958-1961	Mills College, Trustee
1958-1965	Atomic Energy Commission, General Advisory Committee (Chairman 1960-1962)
1962-1972	The RAND Corporation, Trustee
1964-1965	NASA Science and Technology Advisory Committee
1965-1968	Houston Chamber of Commerce, Director at Large
1965-1968	Federal Reserve Bank of Dallas, Director
1965-1968	President's Science Advisory Committee, Member
1965-1971	Universities Research Association Council of Presidents (Chairman 1967)
1966-	Pitzer College, Trustee
1966-1971	Carnegie Foundation for Advancement of Teaching, Trustee
1967-1971	American Council on Education, Board of Directors
1967-1986	Owens-Illinois, Board of Directors

Honors and Awards

1943	American Chemical Society Award in Pure Chemistry
1949	Precision Scientific Co. Award in Petroleum Chemistry of the ACS
1950	U.S. Jr. Chamber of Commerce Award as One of the Ten Outstanding Young Men in the Nation
1951	Alumnus of the Year Award, University of California,Berkeley
1951	Guggenheim Fellowship
1958	Clayton Prize, Institution of Mechanical Engineers (London)
1963	Priestley Memorial Award (Dickinson College) Carlisle, PA
1965	Gilbert Newton Lewis Medal (California A.C.S.)
1966	Alumni Distinguished Service Award, California Institute of Technology, Pasadena, CA
1969	Priestley Medal, American Chemical Society
1975	National Medal of Science (U.S.A.)
1976	Gold Medal of The American Institute of Chemists
1976	Willard Gibbs Medal (Chicago Section, American Chemical Society)
1978	Centenary Lecturer, Chemical Society (Great Britain)
1984	Berkeley Citation (University of California)
1984	Robert A. Welch Award in Chemistry
1986	Honorary Fellow in the Indian Academy of Sciences
1986	Mack Award, Ohio State University, Columbus, OH

1987	Pitzer Lecture, Department of Chemistry, U. of California, Berkeley
1988	Rossini Lecture, 10th IUPAC Conference on Chemical Thermodynamics, Prague, Czechoslovakia
1991	Clark Kerr Award (University of California, Berkeley)
1994	Gold Medal Award (Association of Rice University Alumni)
1994	Hall of Fame of Alpha Chi Sigma

Professional Organizations

National Academy of Sciences (Counselor 1964-67, 1973-76)
American Philosophical Society
American Chemical Society (Counselor)
American Academy of Arts and Sciences, Fellow
American Physical Society, Fellow
American Nuclear Society, Fellow
American Institute of Chemists, Fellow
Faraday Society; Chemical Society, London
Geochemical Society

Social Organizations

Bohemian Club, San Francisco
Chemists Club, New York
Cosmos Club, Washington, D.C.

Publications

1947	"Selected Values of Physical and Thermodynamic Properties of Hydrocarbons and Related Compounds," several co-authors, 2nd Edition 1953, Carnegie Press.
1953	"Quantum Chemistry," Prentice-Hall, Inc.
1961	"Thermodynamics", with Leo Brewer, Revision of Lewis and Randall's book, McGraw-Hill Book Co., Inc.
1992	"Activity Coefficients in Electrolyte Solutions," editor and chapter author, CRC Press, Boca Raton.
1993	"Molecular Structure and Statistical Thermodynamics,"(comprising selected papers of Kenneth S. Pitzer with added comments), World Scientific.
1995	"Thermodynamics," 3 ed., McGraw-Hill Book Co., Inc., New York, 1995. (ISBN 0-07-050221-8)

Also numerous articles (385+) in scientific journals and publications on physical, geological, and theoretical chemistry; research policy; and university governance.

PUBLICATIONS

Kenneth Sanborn Pitzer

1. Argentic Salts in Acid Solution. I. The Oxidation and Reduction Reactions. *J. Am. Chem. Soc.*, **57**, 1221 (1935). (With A. A. Noyes and J. L. Hoard.)

2. Argentic Salts in Acid Solution. II. The Oxidation-State of Argentic Salts. *J. Am. Chem. Soc.*, **57**, 1229 (1935). (With A. A. Noyes and C. L. Dunn.)

3. The Crystal Structure of Tetramminocadmium Perrhenate, $Cd(NH_3)_4(ReO_4)_2$. *Zeit. f. Krist.*, (A), **92**, 131 (1935).

4. Hindered Rotation of Methyl Groups in Ethane. *J. Chem. Phys.*, **4**, 749 (1936). (With J. D. Kemp.)

5. The Entropy of Ethane and the Third Law of Thermodynamics. Hindered Rotation of Methyl Groups. *J. Am. Chem. Soc.*, **59**, 276 (1937). (With J. D. Kemp.)

6. Thermodynamic Functions for Molecules Having Restricted Internal Rotations. *J. Chem. Phys.*, **5**, 469 (1937).

7. Thermodynamics of Gaseous Hydrocarbons: Ethane, Ethylene, Propane, Propylene, **n**-Butane, Isobutane, 1-Butene, **Cis** and **Trans** 2-Butenes, Isobutene, and Neopentane (Tetramethylmethane). *J. Chem. Phys.*, **5**, 473 (1937). Errata: *J. Chem. Phys.*, **5**, 752 (1937).

8. The Heat Capacity and Entropy of Silver Nitrate from 15 to 300°K. The Heat and Free Energy of Solution in Water and Dilute Aqueous Ammonia. The Entropy of Silver Ammonia Complex Ion. *J. Am. Chem. Soc.*, **59**, 1213 (1937). (With W. V. Smith and O. L. I. Brown.)

9. The Heats of Ionization of Water, Ammonium Hydroxide, Carbonic, Phosphoric, and Sulfuric Acids. The Variation of Ionization Constants with Temperature and the Entropy Change with Ionization. *J. Am. Chem. Soc.*, **59**, 2365 (1937).

10. Silver Oxide: Heat Capacity from 13 to 300°K, Entropy, Heat of Solution, and Heat and Free Energy of Formation. The Heat of Formation and Entropy of Silver Ion. *J. Am. Chem. Soc.*, **59**, 2633 (1937). (With W. V. Smith.)

11. Silver Chlorite: Its Heat Capacity from 15 to 300°K, Free Energy and Heat of Solution and Entropy. The Entropy of Chlorite Ion. *J. Am. Chem. Soc.*, **59**, 2640 (1937). (With W.V. Smith and W. M. Latimer.)

12. Silver Chromate: Its Heat Capacity, Entropy and Free Energy of Formation. The Entropy and Free Energy of Formation of Chromate Ion. *J. Am. Chem. Soc.*, **59**, 2642 (1937). (With W. V. Smith and W. M. Latimer.)

13. The Heat Capacity of Diamond from 70 to 300°K. *J. Chem. Phys.*, **6**, 68 (1938).

14. Restricted Internal Rotation in Hydrocarbons. *J. Am. Chem. Soc.*, **60**, 1515 (1938). (With J. D. Kemp.)

15. The Heat Capacities, Entropies, and Heats of Solution of Anhydrous Sodium Sulfate and of Sodium Sulfate Decahydrate. The Application of the Third Law of Thermodynamics to Hydrated Crystals. *J. Am. Chem. Soc.*, **60**, 1310 (1938). (With L. V. Coulter.)

16. The Heats of Solution of Cesium Perchlorate, Rubidium Perchlorate, Rubidium Chlorate, and Lead Phosphate. *J. Am. Chem. Soc.*, **60**, 1828 (1938).

17. The Heat Capacity and Entropy of Barium Fluoride, Cesium Perchlorate and Lead Phosphate. *J. Am. Chem. Soc.*, **60**, 1826 (1938). (With W. V. Smith and W. M. Latimer.)

18. The Entropies of Aqueous Ions. *J. Am. Chem. Soc.*, **60**, 1829 (1938). (With W. M. Latimer and W. V. Smith.)

19. The Free Energy of Hydration of Gaseous Ions, and the Absolute Potential of the Normal Calomel Electrode. *J. Chem. Phys.*, **7**, 108 (1939). (With W. M. Latimer and C. M. Slansky.)

20. The Symmetry Number and Thermodynamic Functions for Molecules Having Double Minimum Vibrations. *J. Chem. Phys.*, **7**, 251 (1939).

21. Corresponding States for Perfect Liquids. *J. Chem. Phys.*, **7**, 583 (1939).

22. The Heat Capacities, Heats of Transition and Fusion, and Entropies of Ethylene Dichloride and Ethylene Dibromide. *J. Am. Chem. Soc.*, **62**, 331 (1940).

23. The Thermodynamics of **n**-heptane and 2,2,4-Trimethylpentane, Including Heat Capacities, Heats of Fusion and Vaporization and Entropies. *J. Am. Chem. Soc.*, **62**, 1224 (1940)

24. BOOK REVIEW. Thermodynamics and Chemistry, by F. H. MacDougall. *J. Phys. Chem.*, **44**, 825 (1940).

25. The Vibration Frequencies and Thermodynamic Functions of Long Chain Hydrocarbons. *J. Chem. Phys.*, **8**, 711 (1940).

26. Chemical Equilibria, Free Energies, and Heat Contents for Gaseous Hydrocarbons. *Chem. Rev.*, **27**, 39 (1940).

27. The Entropies of Large Ions. The Heat Capacity, Entropy and Heat of Solution of Potassium Chloroplatinate, Tetramethylammonium Iodide and Uranyl Nitrate Hexahydrate. *J. Am. Chem. Soc.*, **62**, 2845 (1940). (With W. M. Latimer and L. V. Coulter.)

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29. The Heat Capacity and Entropy of Silver Iodide and their Interpretation in Terms of Structure. *J. Am. Chem. Soc.*, **63**, 516 (1941).

30. Thermodynamic Functions for Molecules with Internal Rotation. *J. Chem. Phys.*, **9**, 485 (1941). (With W. D. Gwinn.)

31. Thermodynamic Properties of the Crystalline Forms of Silica. *J. Am. Chem. Soc.*, **63**, 2348 (1941). (With M. A. Mosesman.)

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33. The Thermodynamics of Branched-Chain Paraffins. The Heat Capacity, Heat of Fusion and Vaporization, and Entropy of 2,3,4-Trimethylpentane. *J. Am. Chem. Soc.*, **63**, 2419 (1941). (With D. W. Scott.)

34. Color and Bond Character. *J. Am. Chem. Soc.*, **63**, 2472 (1941). (With Joel H. Hildebrand.)

35. Nitromethane: The Heat Capacity of the Gas, the Vapor Density, the Barrier to Internal Rotation. *J. Am. Chem. Soc.*, **63**, 3313 (1941). (With W. D. Gwinn.)

36. Free Energies and Equilibria of Isomerization of the Butanes, Pentanes, Hexanes, and Heptanes. *J. Res. Nat. Bur. Stand.*, **27**, 529 (1941), RP 1440. (With F. D. Rossini and E. J. R. Prosen.)

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40. Thermodynamics of Styrene (Phenylethylene), Including Equilibrium of Formation from Ethyl Benzene. *J. Am. Chem. Soc.*, **65**, 1246 (1943). (With L. Guttman and E. F. Westrum, Jr.)

41. The Molecular Structure and Thermodynamics of Propane. The Vibration Frequencies, Barrier to Internal Rotation, Entropy, and Heat Capacity. *J. Chem. Phys.*, **12**, 310 (1944).

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46. Heats and Free Energies of Formation of the Paraffin Hydrocarbons, in the Gaseous State, to 1500°K. *J. Res. Nat. Bur. Stand.*, **34**, 403 (1945). (With E. J. Prosen and F. D. Rossini.)

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51. Energy Levels and Thermodynamic Functions for Molecules and Internal Rotation. II. Unsymmetrical Tops Attached to a Rigid Frame. *J. Chem. Phys.*, **14**, 239 (1946).

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53. Bending Force Constants for Halogenated Ethylenes. *J. Chem. Phys.*, **14**, 586 (1946). (With N. K. Freeman.)

54. Electron Deficient Molecules. II. Aluminum Alkyls. *J. Am. Chem. Soc.*, **68**, 2204 (1946). (With H. S. Gutowsky.)

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56. The Thermodynamics of Styrene and its Methyl Derivatives. *J. Am. Chem. Soc.*, **68**, 2213 (1946). (With C. W. Beckett.)

57. Heats, Equilibrium Constants and Free Energies of Formation of the Acetylene Hydrocarbons through the Pentynes to 1500°K. *J. Res. Nat. Bur. Stands.*, **35**, 467 (1945), RP 1682. (With D. D. Wagman, J. E. Kilpatrick, and F. D. Rossini.)

58. Heats, Equilibrium Constants and Free Energies of Formation of the Mono-olefin Hydrocarbons. *J. Res. Nat. Bur. Stands.*, **36**, 559 (1946), RP 1722. (With J. E. Kilpatrick, E. J. Prosen, and F. D. Rossini.)

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60. Heat Content, Free Energy Function, Entropy and Heat Capacity of Ethylene, Propylene and the Four Butenes to 1500°K. *J. Res. Nat. Bur. Stands.*, **37**, 163 (1946), RP 1738. (With J. E. Kilpatrick.)

61. The Entropies and Related Properties of Branched Paraffin Hydrocarbons. *Chem. Rev.*, **39**, 435 (1946). (With J. E. Kilpatrick.)

62. The Heat Capacity of Gaseous Cyclopentane, Cyclohexane and Methylcyclohexane. *J. Am. Chem. Soc.*, **68**, 2537 (1946). (With R. Spitzer.)

63. Pancake Effect in Gas Clouds. OSRR No. 1176. Publication Board 15620. (With W. M. Latimer and W. D. Gwinn.)

64. Vibrational Frequencies of Semi-rigid Molecules: A General Method and Values for Ethylbenzene. *J. Res. Nat. Bur. Stands.*, **38**, 1 (1947), RP 1758. (With W. J. Taylor.)

65. Normal Coordinate Analysis of the Vibrational Frequencies of Ethylene, Propylene, **cis**-2-Butene, **trans**-2-Butene, and Isobutene. *J. Res. Nat. Bur. Stand.*, **38**, 191 (1947), RP 1768. (With J. E. Kilpatrick.)

66. Electron Deficient Molecules. III. The Entropy of Diborane. *J. Am. Chem. Soc.*, **69**, 184 (1947).

67. Tautomerism in Cyclohexane Derivatives; Reassignment of Configuration of the 1,3-Dimethylcyclohexanes. *J. Am. Chem. Soc.*, **69**, 977 (1947). (With C. W. Beckett.)

68. Relabeling of the **Cis** and **Trans** Isomers of 1,3-Dimethylcyclohexane. *Science*, **105**, 647 (1947). (With F. D. Rossini.)

69. The Nature of the Hydrogen Bond in KHF_2. *J. Chem. Phys.*, **15**, 526 (1947). (With E. F. Westrum, Jr.)

70. The Thermodynamics and Molecular Structure of Cyclopentane. *J. Am. Chem. Soc.*, **69**, 2483 (1947). (With J. E. Kilpatrick and R. Spitzer.)

71. The Thermodynamic Properties and Molecular Structure of Cyclohexane, Methylcyclohexane, Ethylcyclohexane and the Seven Dimethylcyclohexanes. *J. Am. Chem. Soc.*, **69**, 2488 (1947). (With C. W. Beckett and R. Spitzer.)

72. Heats, Equilibrium Constants, and Free Energies of Formation of the Alkylcyclopentanes and Alkylcyclohexanes. *J. Res. Nat. Bur. Stands.*, **39**, 523 (1947), RP 1845. (With J. E. Kilpatrick, H. G. Werner, C. W. Beckett, and F. D. Rossini.)

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75. Gas Heat Capacity and Internal Rotation in 1,2-Dichloroethane and 1,2-Dibromoethane. *J. Chem. Phys.*, **16**, 303 (1948). (With W. D. Gwinn.)

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77. The Thermodynamic Properties and Molecular Structure of Cyclopentene and Cyclohexene. *J. Am. Chem. Soc.*, **70**, 4227 (1948). (With C. W. Beckett and N. K. Freeman.)

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81. Thermodynamics and Vibrational Spectrum of Acetaldehyde. *J. Am. Chem. Soc.*, **71**, 2842 (1949). (With W. Weltner, Jr.)

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86. Energy Levels and Thermodynamic Functions for Molecules with Internal Rotation. III. Compound Rotation. *J. Chem. Phys.*, **17**, 1064 (1949). (With J. E. Kilpatrick).

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89. The Infrared Spectra and Structures of the Iron Carbonyls. *J. Am. Chem. Soc.*, **72**, 1107 (1950). (With R. K. Sheline.)

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99. Methylchloroform; the Infrared Spectrum from 130-430 cm^{-1}, the Energy Levels and Potential for Internal Rotation and the Thermodynamic Properties. *J. Am. Chem. Soc.*, **75**, 2219 (1953). (With J. L. Hollenberg.)

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Letter from Kenneth Pitzer to Jean Mosher

T.A.T. Maddux Air Lines, Western Division

In-flight

July 9, 1930

Dear Jean:

Please excuse my writing as the plane is pitching around quite a bit.

We left at 9:30 from Glendale and were over the top of Baldy at 9:45--going some! Could see Camp Baldy, Ice House, Sierra Club Cabins, Devils Back Bone and the other things nearby. Lake Arrowhead stood out beautifully. All morning we flew at about 12,000 ft. altitude, often over clouds, while after noon we came down to about 7,000 ft., flying under clouds and often in the rain. This last part was like a 2 hour's roller coast ride. We arrived at Winslow, Arizona an hour ahead of time and stopped for a few minute. Lunch was served on the plane. We are approaching more rain and I think the pilot is going South of the storm. The noise is bad but hardly as bad as I expected. One can just hear what is said if you shout or speak close to the ear. The pilot just said we were going back to Winslow to avoid a storm ahead. The pilot opened up the motors to full speed so he must be in a hurry. The storm is coming as fast as we are going away. It is getting too rough to write so I shall finish later.

We have had quite a time. The storm became worse and we have had to take a train for the night. This will delay us 24 hours but it is better than taking a chance with the storm. It has rained almost continually since we got out of the plane. The dispatcher at the landing field said this was the first time that had happened since April. We are on a slow train now. If it moved as much as it stopped it might be a fast train. It seems terribly slow after the airplane. Write me at the following address please--

Kenneth Pitzer
Millbrook NY
c/o Geo. A. McGonegal

I will be there about July 19 or 20.

Hope you are having as good a time as I, probably better, though you'll have to go some.

With lots of love,

Kenneth

Note from Jean Pitzer, 3/19/98: This trip was taken following junior year in high school. Also to visit family of his stepmother. He went with his father and stepmother. The plan was a metal Ford tri-motor.

The envelope is addressed to Jean Mosher, 1294 N. Park Ave., Pomona, Calif. The envelope has a printed return address of

Santa Fe
Ealing House and Dining Car System
Fred Harvey, Manager

The postmark is Clovis, N.M., July 10, 1930, 11:00 a.m.

PARACELSUS (1493-1541)
|
Jacques Gohory (Leo Suavius) (1520-1576)
|
Jean Robin (1550-1629)
|
Jean Herouard (?-1627) and Guy de la Brosse (c.1586-1641)
|
William Davison (1593-c.1669)
|
Nicaise Le Febvre (c.1610-1669)
|
Christophle Glaser (c.1615-1672?)
|
Nicolas Lemery (1645-1715)
|
J. G. Spitzley (?-?)
|
Guillaume-Francois Rouelle (1703-1770)
|
Hilaire-Marin Rouelle (1718-1779)
|
Jean Baptiste Michel Bucquet (1746-1780)
|
Antoine Francois Fourcroy (1755-1809)
|
Louis N. Vauquelin (1763-1829)
|
F. Strohmeyer (1776-1835)
|
Eilhard Mitscherlich (1791-1863)
|
Johannes Müller (1801-1858)
|
Hermann von Helmholtz (1821-1894)
|
Otto Lummer (1860-1925)
|
George E. Gibson (1884-1959)
|
Wendell M. Latimer (1893-1955)
|
Kenneth S. Pitzer (1914-)

University Integrity

Kenneth S. Pitzer

At the moment, there seems to be special need to discuss the internal logic of the university—the relations between its students, faculty, governing board, and administrative officers, and especially the factors which are essential to the university's integrity as an institution. The trials of Columbia University have been all too prominent in recent weeks. But many other American universities have suffered. And, as we look around the world, we note the troubles of one of the oldest and most prominent universities, the University of Paris. While the pressures leading toward disruption are not the same everywhere, it is true that some universities have been able to contend with these factors much better than others have. The problems here in Houston seem not to have been as severe as those in some other locations, but anyone who is sensitive to the thinking of various individuals can detect the presence of the same ideas, objectives, and frustrations.

In commenting on these problems I want to distinguish carefully between those cases where the institution suffered a real breakdown—where the educational activities were substantially disrupted—and those in which an expression of student opinion got slightly out of hand. So long as students respect the rights and privileges of others who may hold differing views or who may merely be uninterested in a particular topic, they certainly have the right to express their views on the public issues of the day. In some cases overenthusiastic picketing has been conducted in a manner that has somewhat infringed upon the rights of others, but the institution has been able to handle the situation with an appropriate firmness and compassion and then has been able to continue with no loss of integrity.

The Pressures

What are the pressures that are especially great today? What do the activists want? Some of you undoubtedly know better than I, but I hope you will accept the following brief summary. There is deep student concern over certain issues confronting our society, especially race relations and the war in Vietnam. This concern is combined with knowledge on the part of certain older students who have seen the technique of civil rights demonstrations yield the fruit of favorable congressional action. Recently the population in general and the governmental leadership have found these techniques less convincing. As a result there is, in these active student groups, a sense of frustration. Many students have shifted their activities to the political sphere by supporting their favorite candidate for the Presidency; this is most commendable. But a small hard core of extremists—those with the greatest arrogance and the least faith in their country—have escalated their demonstrations from the legal range to the level of kidnap and blackmail. Unfortunately, in a few cases substantial numbers of other students and faculty have supported these extremists or have opposed the use of feasible methods of dealing with them.

Joseph Shoben of the American Council on Education puts it in these terms (*1*):

(1) Like a great many other citizens of our republic, students in large numbers are sufficiently frustrated and distraught by the nature and entailments of the war and by the unhappy state of our race relations to act on their discontent. (2) Because they are primarily in contact with colleges and universities as institutional agencies of society, students are especially sensitive to ways in which the campus may appear to mirror what they regard as the American malaise of our time. As a consequence, they strike against the target that is most available to them whenever they *believe* they have cause to strike. (3) These attitudes of students—and it is crucial that we remember that students are not alone in the attitudinal positions that they assume—are shared in sufficient numbers to define a reality that cannot be ignored in the development of academic policies and practices.

It would be true but hardly adequate to say simply that the university is not the appropriate target for these frustrations since the university does not and should not have the authority to deal with matters of this type. We must go much further and emphasize what the proper role of the university is, what its proper response in situations such as these should be, and, very particularly, why the university as a corporate body should not seek political power.

Proper Role of the University

The primary function of the university is the transfer of the intellectual treasure of mankind to the next generation. In addition, universities seek to add to existing knowledge, to solve presently unsolved problems, and to assist their communities in applying this intellectual heritage to problems of current concern. Also, universities seek to provide a wholesome environment for the growth and development of their students as individuals; this responsibility is especially heavy for residential colleges and universities with respect to their undergraduates. But the role of transferring knowledge—the teaching role—is central, and, while in many cases it does not require all of a faculty member's time, it should have first priority for the time it does require. At the same time students should remember that the university is intended for those who want to learn from its faculty. There are some things which can be learned better in other places—in hospitals, in the ghetto, in artists' studios, in factories or business offices, and particularly in churches or in the home—and there are some important truths that come only with experience in life. It is not the purpose of our colleges to duplicate these forms of learning. Our colleges and universities, with their libraries and their laboratories and particularly through the guidance of the members of their faculties, offer a particular and very important opportunity for learning, but it is a special opportunity and it confers a special status both of privilege and of responsibility upon the faculty members. Students and professors are equal

The author is president of Rice University, Houston, Texas, and president-elect of Stanford University, Stanford, California. This article is adapted from a commencement address delivered 17 May 1968 at the University of St. Thomas, Houston.

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as citizens in the eyes of the law, but they are not equals within the framework of the university. The assumption is that the faculty knows more than the students and that there is an apprentice relationship between student and teacher.

While the student has the obligation, while learning, to respect the superior knowledge of his teacher, at the same time the teacher is obligated to listen as well as to lecture, to understand the interests and enthusiasms of his students, and to appropriately recognize these factors in his teaching. Also, professors must take the time to know their students as individuals, to discuss the current concerns of students. If changes are needed in the university the faculty should make those which lie within its authority and should urge administrative approval of any others. Professors should explain to students the proper role of the university and the nature of academic freedom and the way in which this relates to the citizen's freedom under the Bill of Rights. I believe that, in the United States at least, a real breakdown has occurred in a university only when most of the faculty have failed to talk with their students in this way—either because they have been diverted by excessive emphasis on research or professional activities or because they have failed to recognize this as one of their responsibilities. And it is the same faculty members who have failed in these responsibilities to their students who have, possibly from a sense of guilt, also failed to insist that students obey the law and respect the rights of others in expressing their opinions.

Proper Response of the University

If faculty members are to have a major role in university decision-making, as they should have, then they must accept a corresponding responsibility for institutional welfare. In particular, they should make every effort to prevent organized student groups from exceeding legal bounds in their efforts to influence either university or governmental authorities.

Another important principle, both on the campus and in the community at large, is that of tolerance and respect for an individual who honestly holds a contrary opinion. Faculty members and all others involved in university leadership should be spokesmen for freedom of speech and the tolerance of differing opinions which necessarily accompanies that freedom. While academic people have been quick to condemn those outside the campus who would limit freedom of expression, there have been a number of unfortunate episodes on college campuses in which unpopular speakers have been denied this freedom of expression and have been badly treated. For example, both Secretary Rusk and former Secretary McNamara have been denied a fair hearing at some very distinguished institutions. President Wallis of the University of Rochester has gone so far as to say, "concerning the freedom to present controversial views on campus, . . . on few campuses in America today does such freedom truly exist." I would not go that far; indeed, I think there is real freedom to present controversial views on many campuses, but this freedom needs to be reemphasized. And those colleges and universities where this freedom is not really present are suffering from a serious disease which deserves urgent attention.

At its most recent meeting the American Association of University Professors recognized these problems and the obligation of faculty members to play a major role in dealing with them. The resolutions of this association included the following:

> In view of some recent events, the 54th Annual Meeting deems it important to state its conviction that action by individuals or groups to prevent speakers invited to the campus from speaking, to disrupt the educational operations of the institutions in the course of demonstrations, or to obstruct and restrain other members of the academic community and campus visitors by physical force is destructive of the pursuit of learning and of a free society. All components of the academic community are under a strong obligation to protect its processes from these tactics.

The University and Political Power

In recent years it has been seriously advocated, not only by some student groups but also by an occasional faculty member and by other adults, that universities should take official positions on controversial subjects and campaign actively for their adoption by governmental authorities. Such action would inevitably destroy academic freedom. For example, a professor or a student of economics would no longer be really free to advocate his solution to the gold problem if his university were to adopt officially a different position on this question. And a donor to the university could very legitimately object if his gift, intended for education and the search for truth, were used in an active campaign on a public policy question contrary to his viewpoint. Universities can advocate honesty, tolerance, freedom, and other ethical qualities both by proclamation and by example, but, if they are to defend these qualities and are to offer freedom to their members to discuss matters of current controversy, universities as corporate bodies must not seek political power.

Throughout history, universities have suffered whenever and wherever they became tools of political or ideological power. In voluntary or enforced betrayal of their central teaching role, these institutions ultimately helped undermine and even destroy the intellectual heritage they were designed to preserve and enlarge. In Europe and Asia in the 1930's and during World War II, many universities allowed themselves—either willingly or under dictatorial coercion—to become important tools of political power.

Fortunately, American higher education, so far, has been spared this supreme test of its integrity. However, this fact does not preclude the need for careful review of our principles and, where needed, even revision of our priorities. But neither review nor revision should ever affect the integrity of our colleges and universities.

Conclusion

Now you may ask what the individual citizen can do to help colleges and universities maintain their integrity. Whether or not you are professionally involved in education, you can encourage a proper emphasis on the teaching-learning function and the responsibility to recognize students as individuals. You can also help by understanding and defending academic freedom and by insisting, outside as well as within the campus, on tolerance for differing viewpoints. Finally, you can help maintain the institutional integrity of our colleges and universities through understanding, aid, and support.

Reference

1. J. Shoben, American Council on Education, unpublished report.

How Much Research?

Kenneth S. Pitzer

How Much Research?

The educational aspect is crucial in justifying further growth in research.

Kenneth S. Pitzer

The American people, through the national government, have given remarkably strong support for scientific research throughout the period since the second world war. There are very few peace-time activities that have received as strong support as basic research in universities, which was given a 25 percent per year increase in funds each year over the 5-year period 1958–63 after already having grown at a very rapid rate over the preceding decade 1948–58. For the next 3 years, 1963–66, the growth still continued at the relatively high rate of 15 percent per year increase. The total rate of federal expenditure for research in universities was now well over $1 billion per year, and it is not surprising that questions were asked and that congressional committees made special studies of research activities.

Both the Elliot Committee and the Daddario Committee handled their assignments in a most constructive and responsible manner, and their reports were generally favorable. Nevertheless, the question remained unanswered about how much further growth of basic research was really justified. Recently several committees of scientists have struggled with this question. The report, "Basic Research and National Goals," prepared by the Committee on Science and Public Policy of the National Academy of Sciences, is directed primarily to this question.

The requirement most frequently suggested by scientists is one calling for a continued 15 percent annual increase, and this figure is justified on the basis of an 8- to 10- percent annual increase in the number of research students, and a 5- to 7-percent annual increase in cost because of price rises and increased sophistication of instrumentation. But the gross national product and the total federal budget grow at a much slower rate, approximately 6 percent per year. Consequently, it was easy to see that funds for academic research could not continue to grow at 15 percent per year for many more years without becoming an absurd proportion of the federal budget. Since a schedule for a leveling off of the research budget has not been forthcoming from the scientific community, budgetary officials in Congress and in the Executive branch have been forced to make their own decisions, and the result was a reduction to a 10 percent growth from 1966 to the 1967 fiscal year and the prospect of not over a 6 percent growth next year. Even these last figures indicate a strong underlying support for research in view of the budget pressures of the Vietnam War.

I believe it is very important, however, for scientists to continue a discussion among themselves and with governmental leaders in an effort to work out generally acceptable principles for determining how much research. These comments are intended as a contribution to that discussion.

Diminishing Returns

Next let us put aside for a moment the discussion of federal dollars and consider the nature of scientific research as it is today. I believe it is easy to see in the current situation factors supporting a concept of diminishing returns. At least three factors apply here.

First, we see that further growth brings less able people into research. All of the most creative scientists now have little difficulty in finding good positions, and it is quite clear that the contribution of those who are added by further growth in research will be less, per person, then the present average.

Second, there is the enormous increase in published literature which makes communication of really important discoveries more difficult. It is neither feasible nor desirable to prevent the publication of competent but relatively pedestrian research results; nevertheless, the increasing volume of such papers makes it harder for a scientist to learn of the unexpected result which would suggest a new idea for his own work. I am sure that improvements can be made in our publication system, but the fact will remain that the net value of additional research of mediocre quality is diminished by the burden that it places on scientific communications.

A third factor, which is closely related to the second, is the tendency toward over-specialization. As the population of research scientists grows, there is a tendency to split up into narrower fields of specialization. But major discoveries frequently arise from the interaction in an investigator's mind of concepts developed in other fields of science. Excess specialization will decrease the range of science which will be interacting in the minds of creative individuals.

I conclude from these three factors that in many scientific areas the argument for further growth as a means to an increased rate of major discoveries is not very convincing. There are convincing arguments for growth, but these relate to research training, and I shall return to them presently.

Arguments Favoring Science

If we now look outside the research laboratories, we find strong arguments favoring science, but these do not uniformly favor further growth.

Science is an important part of our intellectual heritage; it is a response to our curiosity about nature, about ourselves, and the things we see about us. Consequently, science has an essential position in our education system, and reports of advances in science are of interest to citizens generally. I find it difficult to argue, however, that we need to increase further our research effort in all areas in order to have more discoveries to report to the community generally at a time when the public is interested in only a small fraction of the present research output.

The importance of science and, more

The author is president of Rice University, Houston, Texas. This article is based on the William Albert Noyes Lecture delivered at the University of Illinois on 12 April 1967 and on a subsequent address delivered at the University of Florida.

explicitly, of developments based upon scientific research to economic progress is widely accepted. Certain economic studies indicate that about half of the recent increase of production in this country may be attributed to advance in technology with additions of capital and labor contributing the other half. Since the cost of the additions of capital and labor is much greater than that of the expenditure for advanced education, research, and development, the latter seems very well justified. This is a value to the public that, in my view, does justify further growth in research. In some areas, such as medicine, the value is best discussed as public welfare more broadly than dollar income, but, here, also, I believe additional research can be justified. But if we accept these justifying factors we must accept also certain implications concerning the type of research, its geographical location, and its relationship to education.

Research is more likely to contribute to economic development and public welfare if it is in a field of science related to technology, or to medicine, or agriculture, or is at least relevant to other scientific disciplines from which important practical developments have arisen. Also the nation has a right to expect the benefits of technology to be equally available in all geographical regions, and this is one justification of the demand that advanced education and research be as uniformly distributed over the nation as is feasible.

One of the best methods of encouraging useful developments based upon new scientific discoveries is to bring students who have participated in the scientific work into the development laboratories. In any event, the staff recruits for development, as well as for management of technologically advanced activities, need to be familiar with the latest science and with the nature of research. And the best way to accomplish this is through research activity in the universities in which these recruits receive their most advanced education. Thus, one can build a much stronger case for additional research which is associated with graduate education than for the research alone.

Educational opportunity is given great importance in our society, not only for the welfare of the society generally, but also because we value the individual most of all. In accordance with this principle, we believe that gifted individuals should be able to pursue their education to the most advanced level if they wish. An increasing number of brilliant and creative students are choosing to seek the Ph.D. with the research experience that it implies. This research opportunity should be provided, at least in fields of modest cost, by the necessary expansion of academic research.

We must also recognize those areas of research involving major supporting facilities, such as particle accelerators in the billion-volt range, large telescopes, oceanographic research vessels, and space probes and satellites. These facilities make possible unique experiments that open up whole new areas of science. This nation must maintain a leading role in these fields during the pioneering phase, at least. We cannot afford not to be in the forefront during the exploration of totally new territory. On the other hand, the cost of the work per scientist is so high that it is not reasonable to expect to provide research opportunity for every competent investigator who wishes to work in these fields. Rather, the magnitude of our program should be judged in terms of the importance of the field and the facilities necessary to support a vigorous effort.

"Little Science"

Those who are familiar with recent discussions of these questions will recognize that I have arrived at the definition of "big science" as contrasted with "little science." I am not going to say more about big science; decisions concerning these major national facilities and programs must be made as they are now being made on a case-by-case basis in the government. It is to little science that I now return; the typical unit is a university professor with several graduate students. Instruments are used, but their cost, per year, is small in comparison with the cost for personnel and operating expenses.

Such little science is also carried out in industry and in private and government research institutes, as well as in universities. Indeed, such research may be of great value in support of the pursuit of the industrial or programmatic objectives of such organizations, and should then be supported on that basis. But I have indicated earlier, and I want to emphasize now, that there is much greater public-welfare justification for additional basic research which is associated with education, than for the additional research alone. Thus, I prefer such terms as "research training" or "academic science" to "little science," because I believe the educational aspect is crucial.

Many scientists have argued that every scientist with real research talent should have his program supported if it falls in the range of little science. I maintained this position myself during the years 1949–51 when I was director of research for the U.S. Atomic Energy Commission; at that time it seemed clear that a wider diffusion of research fundamentally relevant to atomic energy was clearly in the national interest. But, the growth in research since 1951 has been enormous, and criteria which were adequate then may be inappropriate now. In fact, it is not clear to me that one can any longer justify support for all competent applicants in the little science area unless their research is an essential part of the training of students in research. But I do believe that one can still justify further growth of the academic science which constitutes Ph.D. level research training because of its relevance to both the development of the talent of individuals and to the progress of technology in terms of both economic growth and public welfare.

Adequate Federal Support

Let us examine more precisely the federal funding that this policy implies. I believe it is possible to have very good academic research in the little science area for a group comprising one professor and four or five students with government support of $50,000 per year. This includes student stipends. I believe it is essential that the federal government continue to carry at least its present proportion of the cost of this type of academic science. There should be, on the average, one Ph.D. per year awarded from this group; hence, we can take $50,000 per Ph.D. as the basis for government funding. This amount is somewhat larger than the estimate of the Westheimer Committee, which was $30,000 per Ph.D. in chemistry. Since approximately 8000 Ph.D.'s are awarded annually in science and engineering, the total expenditure currently required is $400 million per annum if we take the $50,000 estimate.

Let us now recommend that this annual expenditure level of $400 million be increased as required by the growth in numbers of doctoral students and in the cost of research of this type. From this point of departure, an increase of 15 percent per year until 1975 would raise the expenditure level to approximately $1 billion, which would be well justified in my view.

As one looks beyond 1975 it seems very likely that the growth in the number of doctoral students will be slower because the population of the appropriate age group will level off. Also the growth in the proportion of students seeking the Ph.D. may decrease. Hence, I believe one can justify a policy of adequate federal research support of this type, for all qualified doctoral students, as far into the future as is meaningful.

Although this is my primary conclusion, two other matters require attention. One concerns the additional support of research in universities in excess of the level just considered for Ph.D. training. The other concerns the allocation of funds for Ph.D. training among disciplines and among universities, and between institutional as compared to project grants.

The total federal funding for research in universities this year is approximately $1.5 billion, or $1.1 billion more than the $400 million which I attributed to research training for the Ph.D. in little science. This indicates that there is a large amount of intermediate level science in universities which involves substantial instruments, as well as postdoctoral and other professional research personnel in addition to professors and students. Examples include nuclear physics programs involving small cyclotrons or Van de Graaff accelerators. Even a program in chemistry including postdoctoral fellows, and possibly a mass spectrometer, would contribute to this additional cost. There is a large expenditure in universities for medical research, but relatively few Ph.D. degrees arise from this area. I do not intend to discuss this component of cost in detail; I shall only say that it is important; indeed, it is essential to American leadership in science; but I do not believe one can justify its increase in proportion to growth in number of Ph.D. students. Reports such as that of the Westheimer Committee show the importance of these additional costs for better instruments. The need for growth in number of postdoctoral appointments is, in my opinion, an open question which needs prompt study. Certainly the present level of expenditure should be maintained, but I believe it is more important to provide the basic level of research support for additional doctoral students and their professors than it is to increase all of these other categories of research expenditure.

New Core Grants

Finally, I wish to urge a new pattern of grants for part of the basic level of Federal support, which I estimated as $50,000 per Ph.D. Support for the basic costs of any worthwhile but relatively inexpensive research in a given field—for the chemicals, vacuum pumps, oscilloscopes, and similar items—should come through relatively flexible core grants to the university. The size of these grants should be related primarily to the number of Ph.D. degrees awarded in various scientific disciplines.

Project grants for basic academic research were originally intended to provide only the extra support for unusually expensive experiments, but project grants must now cover these basic costs in most laboratories. This is a clumsy method; it is expensive in administrative time and disastrous when a misjudgment denies a good scientist and his students even this basic level of support. The proposed core grants would take over this basic support and allow project grants to resume their original and appropriate role.

A careful study should be made in order to choose the best method for administering the core grants. If they were based simply upon the number of Ph.D. awards in science, a very careful check upon the quality of students and programs would be required to avoid the temptation to lower standards. Also, special consideration would be needed for new programs or for those growing very rapidly. But market forces should be allowed to control the distribution among fields of study and among institutions through student choice influenced by employment opportunities, as well as the intrinsic interest in each subject, and by the attractiveness of each university's program.

Probably the core grants should be allocated primarily on a departmental basis with appropriate consideration of research costs in various fields, but universities should be free to make reasonable adjustments between departments and be able to meet necessary costs outside of, as well as within, departmental budgets.

Funds for student stipends would continue to flow through grants for fellowships or traineeships, as they are presently allocated to universities for award to students. These grants should be increased gradually to replace student stipends in project grants, and then further increased in proportion to the number of Ph.D. degrees granted after appropriate consideration of quality and any other relevant factors.

The new core grants, together with the traineeship grants, would provide the basic cost for research training and would be increased from year to year in proportion to the increase in doctoral theses completed; these funds should not be in competition with the project grants, which would provide additional funds above this minimum level of research training expenditure.

Summary

In conclusion, I believe the components which I have discussed constitute an outline of a sound program for federal support of science in universities, which provides first, a basic minimum of funding proportional to the growth of the research student population, and second, a pattern of grants based upon justified need and individual merit for more costly instruments, postdoctoral appointments, and other factors that allow our best scientists to be more productive. In addition, there is, of course, the array of major national facilities and programs, each judged individually, in fields requiring very costly equipment.

This proposal is based upon my belief that people are more important than machines. While elaborate instruments are important, we should give first priority to those programs which provide the opportunity for an initial experience in research for all our able and creative young minds. We can afford to keep the door open to all these gifted young people; let us be sure to do so.

Effecting National Priorities for Science

Kenneth S. Pitzer

For 20 years after the close of the Second World War, science received unprecedented and in some respects unquestioned support in this country. Annual increases of 25% in science budgets were not uncommon. The Federal Government encouraged and financed a major expansion of graduate education and research in science in the universities. The Government expended even larger sums for research and the development of new technologies in industry and in nonprofit research institutes.

The growth of federal research and development expenditures contributed significantly to our national economic progress and prosperity in the postwar period. Its benefits to mankind may be seen in telecommunication satellites, heart transplants, and higher crop yields, to cite but a few examples. The flight of Apollo 8 is symbolic of the international leadership America has achieved through these expenditures.

This period of increasing and largely unquestioning support has ended; some of you have received this message with particular vividness by having your research grants reduced or not renewed. Also, the net effect of the results of science and technology on the quality of human life is being questioned—sometimes irrationally but nevertheless actively.

Don Price in his address as the retiring president of the American Association for the Advancement of Science describes this as a two-front attack—"a political reaction and a new kind of rebellion." I will return to discussion of the rebellion later.

The reaction was to be expected. Budgets for any program, no matter how worthwhile, cannot increase by 25% per year indefinitely. The war in Vietnam undoubtedly accentuated the suddenness of the change, but it would be wishful thinking to assume that the trend of the decade 1955–65 could have continued much longer. In the future, growth in support of science cannot be expected to be much greater than the growth in our total national productivity.

In leveling off appropriations for research, business leaders and Congressmen are now evaluating science more critically than previously by the same criteria they use in making decisions about other matters. They see a level of research and development beyond which there is a diminishing return for additional expenditures, and they ask whether we are not at that level now. The businessmen must justify industrial research in terms of new and profitable processes and products and, while the record is reasonably good, it apparently fails to justify large additions to the present level of research activity. Congressmen must consider not only the national interest as they can best judge it, but also the views of their constituents; and they, too, are asking sharper questions and voting smaller increases in appropriations.

We must be more explicit about the contributions of scientific research to particular national goals. We must discuss recent research which has been valuable as well as the degree of relevance of various basic fields to particular practical problems. I do not mean that we should abandon the more esoteric subjects. But the desire of more and more able people to work in a given field is no longer an adequate reason for indefinite expansion of federal support; more convincing criteria must be devised to justify the magnitude of effort in each discipline.

Furthermore, these improved and more specific justifications of scientific research should be discussed widely. In the end, Congress will make the major decisions; consequently, your arguments should be addressed to your own Congressman.

This halt in the growth of funding for science threatens to undermine an important program initiated a few years ago to add new centers of excellence in research. There is now a blatant contradiction between the federal programs encouraging the development of additional major centers of academic science and the absence of additional funds to finance these centers on a continuing basis. I refer particularly to the University Science Development Program of the National Science Foundation. Thirty grants in the range of \$4 million each have been made to enable universities with promising programs to expand and improve these activities in the hope of joining the 15 or 20 leading centers of academic science. The continued support and improvement of these 30 centers will require an increase of at least \$50 million in the federal grants to universities each year. This is not an enormous sum, but it is an increase. And in many science appropriations for this year there was no increase at all, while in other cases the increase was less than the rise in price level.

We should recognize what lay behind the Science Development Program. First was the assumption that more science was unquestionably desirable; indeed that we were far below the optimum level. The second idea arose from the remarkable development of innovative industry around certain major university centers of advanced scientific and engineering research—particularly the complex around MIT and Harvard and that around Stanford.

The substantial flow of federal funds into these two complexes and the economic prosperity of their immediate environs have attracted many imitators and excited the pork barrel instincts of Congressmen. Contrary to widespread popular belief, federal research grants do not automatically trigger local economic growth. Neither does the presence of a strong, graduate-level university program. What counts most is the presence of both scientific and financial entrepreneurs—men who are willing to take new ideas, work on their practical development, and other men who will provide enough seed money to bring these innovations to the production stage.

Now we are faced with the collapse of the assumption that more science is unquestionably desirable, and we must

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American Chemical Society

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face the problem of how broadly to distribute a limited amount of financial support for research in universities. If it is spread too thinly, we will destroy the excellence of our present centers of real distinction and great productivity. But I do not believe it will be politically acceptable to "pull the rug" from under those new centers that have really made great progress under their NSF Science Development grants.

In my opinion, we should make it abundantly clear that we now have enough or more than enough centers for doctoral study and research and that no encouragement will be given from federal sources to new centers or to those presently of marginal quality. The states should be urged, through their individual coordinating mechanisms, to control the number of state colleges and universities that are authorized to offer the Ph.D. degree. Such federal-state cooperation should make possible a compromise pattern of distribution of federal support for academic research which will both maintain the quality of our best universities and allow a reasonable number of additional universities to continue their progress toward comparable excellence.

In addition to the particular problem I have just discussed, active consideration is being given to new mechanisms for federal support to universities. Congressional debate on these proposals should generate a more cogent policy and a firmer commitment concerning the federal role in higher education. It will be an improvement if somewhat more of the federal support of universities comes under the banner of graduate education and somewhat less under research. Furthermore, many decisions allocating support among programs within a single department which are now being made in Washington might better be made locally. Thus, I favor an appropriate program of institutional grants. It is not clear at this point whether this type of new program will be limited to science or will relate to higher education generally.

Now I want to turn to a different type of attack on science which Price has called "a new kind of rebellion." This attack comes, not from the leaders of business and government, but rather from student activists and literary and philosophical spokesmen. It is worldwide in scope. The attack is primarily directed against the impersonality of our technological society and the power of the so-called military-industrial complex.

André Malraux says that "the most basic problem of our civilization is that it is a civilization of machines."

The housewife shares this feeling when she is unable to get human attention to the error in a computer-prepared bill. There are now probably fewer errors than with precomputer methods, but there is more frustration when errors do occur.

Most people limit their opposition to the particular application of technology which has annoyed them, but certain intellectuals charge that science is the cause of it all. Thus Malraux writes: "We, for the first time, have a knowledge of matter and a knowledge of the universe which . . . suppresses man."

These charges are echoed by students and faculty on the campuses and have had more effect so far in making scientists re-examine their own philosophy than on general public attitudes. But it is well for scientists to take the lead in this study. The convocations held last March 4 on many campuses demonstrated the great interest of many scientists in a new evaluation of the effects of science on society.

The area of defense-related government activities is one of particular importance, both because of their destructive nature and because of the secrecy of their administration. Many examples are familiar. Concern about the safety of large underground nuclear tests has recently intensified and for good reason, in my opinion. The Atomic Energy Commission had considered carefully various hazards, but there was little release of information and, therefore, little public discussion. Then it was discovered that numerous small earthquakes followed closely after one or more of the larger tests. There arose a new concern that even larger test explosions might trigger damaging earthquakes. Since AEC plans still larger tests in the future and expects to fire them at new sites located in areas with a history of damaging earthquakes, this hazard cannot be ignored.

I am not a geophysicist and cannot estimate this earthquake hazard as well as those expert in that science. But when I was asked to look into this situation a few months ago, I was struck by the fact that there was no real need for secrecy in discussing this problem. The details of the explosive devices were irrelevant. All of the essential information was unclassified, or ought to be. Hence I urged, as did others, that this problem be discussed openly. It has now received some attention at a recent AEC-sponsored conference on Off-Site Safety Programs for Underground Nuclear Detonations held at Las Vegas. This initial report, while desirable in opening the subject for public discussion, is inadequate. In particular, this subject should be studied by scientists who have no affiliation with AEC. This is a matter of judgment as well as expertise; consequently, conflict of interest is an appropriate consideration.

I believe the risk that a damaging earthquake might be triggered deserves a much more substantial public hearing before large tests are held at the new sites in central Nevada and the Aleutian Islands, which are seismically active areas. Then Congressmen, governors, and other responsible officials as well as the interested public can form their own judgment, balancing this and any other risks against the need for the tests or the extra costs of moving to a nonseismic location.

The problem in this case is not that the risk is completely ignored; rather that it has been examined primarily in closed circles with the effective judgment rendered by officials committed to the test program. To be sure, the President makes the final decision on a nuclear test, but by that time all preparations have been made and there is enormous pressure on him to go ahead. This sort of problem should be considered at an earlier date by an impartial judge and jury.

Let us turn now from military to civilian applications. Ever since the time of Francis Bacon we have held a sort of laissez-faire theory that scientific knowledge would automatically yield economic and social progress. In contrast to economic laissez-faire, there must be a source of financial support for the basic scientific research. But after the basic discoveries were made it was assumed that practical inventions would ensue and that economic forces would lead to the implementation of the useful and desirable developments. To a considerable extent this theory has been confirmed, and we do enjoy many benefits indirectly arising from scientific discoveries.

But economic laissez-faire was found to be unsatisfactory, in particular because of the boom-bust instability. Businessmen came to recognize the desirability of government intervention to stabilize the economy at a prosperous level. Likewise, scientists are coming to realize more fully that some practical applications of science can be extremely dangerous to the world. The atomic bomb constituted a shattering example, but we are now observing that the cumulative degradation of our environment from many less spectacular causes can, in the end, be very serious.

Furthermore, it is important to note that most major new technologies are influenced or regulated by the Government in some way. For example, television, air transportation, drugs, pesticides, and offshore oil drilling are all regulated in one way or another. Thus, it would not necessarily extend the range of government control to ask that this influence on, or regulation of, new technologies be more sensitive to humanistic factors.

Consider, for example, the proposed supersonic transport airplane, which is a government-financed project. It has been well known that a highly annoying sonic boom will necessarily accompany each plane in supersonic flight. But much of the earlier planning for this project assumed that our people would acquiesce in this annoyance, and only economic and technical feasibility factors were considered. In my view, however, the top priority should be given to the desires of the majority of people, who do not want to be annoyed by sonic booms. The convenience of faster travel for a few people should be strictly secondary. This should have been recognized before a major project was undertaken with thousands of people employed. Now there are political and economic pressures to continue the project, and there will be human hardships if it is terminated. My point is that we should have had a more humanistically oriented set of priorities for the early decisions.

A supersonic transport is more acceptable for transoceanic flights, and I would see no objection if it were economical on that basis. But I do not find a relatively small saving in time for a few people to be sufficient justification for very large public expenditures when in competition with our other needs today.

It will not be easy to foresee all of the possibly damaging effects of a new product or machine, but we should try to do so. In the past we have usually assumed that deleterious side effects of a new technology would be negligible or could be remedied by subsequent action. In many cases that was correct. But when it was not correct, the problems became severe. Once the new technology is established there is a strong pressure for its continued operation. It is far harder to stop an operation than not to start it at all. Also, a modified technology may be possible which accomplishes the purpose and avoids the damage, but the change is a lot easier at the design stage than after the plant is built.

How can we effectuate a more humanistic set of priorities? The extremists say to stop all scientific work, but I doubt that they really mean it. I am sure that most people want to retain the advantages of science and technology. In that case basic science must move ahead substantially as at present. It is impossible to predict the practical consequences of truly basic research. But as soon as applications can be visualized, the process of judging their desirability should begin. From that point onward in any proposed practical development, one should ask not only, "Is it possible?" but also, "Is it desirable?" And the desirability should be judged from a humanistic as well as an economic basis. The market place is a good measure of the usefulness of a new product to the users, but it gives no measure of the damage it does to others and to our environment. We need to assess that damage in advance, if possible, in order to invoke the proper corrections or even to stop the entire development when necessary.

Since scientists are peculiarly able to visualize possible applications of new scientific knowledge and their effects, scientists must play a major role in this judgment process. But other citizens who are sensitive to individual and community attitudes should also participate and help apply the humanistic value test.

In most cases the decision should not be to stop the development of a potentially useful technology; rather the new feature in decision-making would be a much more active study from the very beginning of all possible damaging side-effects and the means to avoid them.

How is this broader judgment of desirability to be made? Do we need a new technology review council somewhat like the Council of Economic Advisors? Probably some central group close to the President is needed to deal with special cases and to promote this viewpoint throughout the government. But primarily we need greater sensitivity and more active attention to these questions in all government agencies. The problem is especially critical in agencies such as the Atomic Energy Commission and the Department of Transportation which are the promoters of new technology as well as regulators of it. Unless some higher authority keeps emphasizing questions of risk and damage. the promotional side of the agency is likely to dominate.

Much of the study of the desirability of new technologies in this broader sense can be done outside of government. Professional organizations such as the American Chemical Society could provide a forum for anticipating the environmental problems likely to arise from new processes and products. Universities include the various types of people, scientists, engineers, lawyers, and humanists, needed for fruitful attack on this general problem. I believe one or more university groups should propose and analyze new decision-making mechanisms which could better deal with these problems.

In summary, there is now both a reaction—a more critical questioning of support for science by leaders in government and industry, and a rebellion—an outright attack by some students and writers on science because in their view it allows machines to dominate people. Furthermore, the dangers and difficulties arising from new technologies, ranging from possible nuclear war to major pollution of water and air, are forcing us to abandon the laissez-faire viewpoint that the natural result of scientific discoveries will be desirable improvements in our conditions of life. A new approach is urgently needed. We must adopt a more active role in (1) justifying specific areas of research deserving increased support; (2) developing a better pattern for federal support of graduate education; and (3) judging the desirability of possible new technologies on a broad humanistic basis. Each of us must accept these obligations, either as a scientist or as a citizen.

Basic Ideas and Beliefs

Handwritten by Kenneth S. Pitzer – May 25, 1958
Copied by Jean M. Pitzer

Science is good, but not a complete basis

1. Comprises a structure of truth external to ourselves—true whether men exist or not. Science promotes humility for open mindedness, fairness, etc.
2. Science promotes human welfare by showing what is possible with respect to nature. Proper use of science will avoid trying the impossible or the use of known wrong methods. Science is incomplete, however, and doesn't always yield an answer.
3. Science is not a complete basis for personal or for national life. One needs principles and beliefs towards which one strives, which cannot be established scientifically. Example—is mere biological continuance of human life a pre-eminent good (Pauling, Russell, etc. apparently so believe)? [Note: I believe this reference is to the philosopher Bertrand Russell—J.M.P.] Or are conditions, such government responsive to the will of its citizens more important?

Religion is good in so far as it leads men to control certain impulses and to live lives which are satisfying and happy in the long run. Religion can be misused to lead men to action, which is generally destructive, although possibly pleasing to one person or a small group. The mystery is used to lead men to do what they could not be convinced to do rationally. There is a risk that scientists may acquire similar power to invoke the mystery arising from general ignorance of science.

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Interview with Kenneth Pitzer

by David Ridgway

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Interview with Kenneth Pitzer

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Ridgway: *As we have had the pleasure of talking with each of our outstanding interviewees, Dr. Pitzer, we find very little commonality, if there is such a word, in early influences in directing each of you into science. Were there special factors, in your case, as a youth?*

Pitzer: *I'm sure that there were. The strongest were really a combination of parents and other close relatives. My mother, though she died when I was thirteen, nevertheless had a very substantial influence until that time. She was at one time a mathematics teacher. She and her whole family were very much interested in educational and general intellectual things. Though father was not particularly interested in science, he had a very keen intellect. He respected people with professional attainments of any type. I had two uncles who were very much interested in things mechanical and scientific. Discussions with them and their encouragement had a great deal to do with focusing my interest in the direction of science or engineering.*

Ridgway: *Did the actual geographic environment in which you were brought up have any influence on your interest in science or more specifically in chemistry?*

Pitzer: *Pomona College in Claremont was six miles away. This isn't very far but even then the college itself wasn't so close that one just casually participated in college activities. Cal Tech was 25 miles away and probably had as much influence in this respect. I would emphasize that I seldom got onto the Cal Tech campus. The existence of both institutions had considerable influence on the general activities of my community. Robert Milliken's strong flair for publicity about science contributed a great deal, I'm sure.*

Ridgway: *What, if any, were the components of your scientific instruction in the years prior to your higher education that you thought were important?*

Pitzer: *The things that I've been saying about my family and the community in a broader sense were really probably more important than high school or lower school as such. The situation in the schools was probably not atypical for relatively small town systems of that time. The science and mathematics teachers ranged from fair to good, but I don't think any of them were really superlative. I would not credit them with having any major positive influence in this regard. The fact that many of them were good teachers certainly contributed. If there hadn't been reasonably good instruction in science and mathematics, I'm sure this would have had a very negative effect. The physics teacher in high school was, at the time, also managing the local athletic league and was out of the classroom lining up the referees or bus transportation almost as often as he was in the classroom. Even here the net effect may have been positive since I gained experience helping other students when the teacher was away. One aspect of both this physics teacher and of the chemistry teacher, I think should be stated more positively than I have so far. They both encouraged reading, questions, and even some work in the laboratory, over and beyond the regular course of study. I'm sure that this had a very positive influence as far as I was concerned.*

Ridgway: *Were there any overriding factors in the choice of a particular institution for higher education including graduate work?*

Pitzer: *In southern California at that time, in my opinion, the attractiveness of Cal Tech to someone with interest and with facility in things mathematical and scientific was very great indeed. At that time it had just recently developed to national leadership, and Dr. Milliken, particularly, had a great capacity to communicate the standing and quality of the institution through the public press and through other public means of communication. The first and very important influence was that of A. A. Noyes, who was then approaching retirement. In that period he was taking a great interest in freshmen. He worked hard to make chemistry an active and growing subject through modest research activities beginning right at the freshman level. Earlier, my interests were not in any sense specifically focused on chemistry; that is, I could have as well as not gone*

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into physics or some form of engineering. But Noyes' interest in freshmen had a very great influence right from the beginning at Cal Tech. In later years there were comparable positive contributions from both Don Yost and Linus Pauling, but Noyes' influence was the one that took effect right in the beginning. One of the ways that it manifested itself was in the summer programs that he carried on essentially on his own. He had encouraged the Institute to buy a building down in the Newport-Balboa-Beach area that had become available. Remember this was depression time. It was for a potential marine laboratory. He owned a house right next door, personally. Incidentally, it was quite a castle on a cliff that looked out over the entrance to the Newport Harbor with a beautiful view. He took over one room in the Institute building as a chemical laboratory and would invite a few people for the summer for research activity and find them some means of minimal livelihood. In the mid 30's one didn't ask for anything more. He invited me down the summer after my freshman year. It was really an exciting experience. The work during that summer, together with a little done at the end of the freshman year and a little later in the sophomore year, led to a series of two or three papers on the higher oxidation states of silver. This was really significant scientific work, done under very stimulating conditions.

When it came to choosing the graduate school there were several considered. The attractiveness of the situation at Berkeley may have been in part due to many similarities to Cal Tech. In part because of the relative isolation from eastern universities, there was at that time a very close relation between the Berkeley and Cal Tech departments, much visiting back and forth, stimulating discussions and a great deal of mutual respect. I was influenced by Professor W. F. Giauque who was a very strong figure with very positive, definite, ideas as to how things ought to be done in low-temperature research. Sometimes I agreed and sometimes I didn't, but the mere fact of having to occasionally argue with such a strong figure was, I'm sure, also a positive influence. I would be remiss if I did not.

Ridgway: *We find it illuminating and informative while talking to people who have made an impact on science, to have them do a bit of self-evaluation. Of your many contributions to the literature, which do you feel have had the greatest impact—either on fellow scientists or the community at large?*

Pitzer: *One has to discuss this question in terms of several contributions and in terms of the approach and method of investigation rather than trying to list each contribution. I have, of course, my own views about which papers were most important, but now there is a publication known as the "Citation Index" which allows you to check up, by inference, on whether your ideas are right as well as used and referred to by others. I took a look at that not too long ago and it tends to confirm my own views in most cases. There are several specific contributions that have been the basis for a fairly wide level of activities by others. The first one, historically, was that concerning internal rotation about the single bond in ethane. This presented a great puzzle at the time. I was a graduate student and had been studying quantum mechanics more thoroughly than most. This enabled me to contribute the theoretical part in collaboration with Dr. Kemp who had just made thermodynamic measurements.*

Since many organic molecules have groups subject to internal rotation about single bonds, I turned next to a more general theory with W. D. Gwinn, a student of mine, to the analysis of available data on other molecules. We generalized the appropriate theory from the very simple molecule ethane, to cover most organic molecules that have rotations about single bonds, and we went out of our way to present the results in a form that would be convenient for other people to use.

After the work on internal rotation about single bonds, I turned to the more general question of unusual motions in organic molecules, particularly ring molecules. I will mention just two examples. The geometrical structures of five- and six-membered ring molecules are very different and are really two different topics. The concept of pseudorotation in a five-membered ring and the method of transferring vibrational force constant data from simpler molecules to predict the molecular potential for the ring motion in cyclopentane constituted procedures that were important for various five-membered ring molecules.

In the six-membered ring the balance of force pattern is quite different, and it leads, not to a pseudorotational situation, but to an equilibrium between chair and boat forms. In this respect my work was essentially an approach to the same problem from a somewhat different background than that of Hassel in Norway and some others. Out of the combination of our efforts the whole field of conformational analysis of ring compounds arose. In contrast to the five-membered ring case where my contribution, with collaborators, stands alone, my contribution in the six-membered ring area was, I think, significant and substantial but was only one of several contributions in various parts of the world.

A contribution used particularly by chemical engineers concerned the description of the fluid state. I use the word fluid to emphasize that we're considering both gases and liquids, and the full range of pressure and temperature. The puzzle in this area was that, although basic principles were reasonably well understood, there was no satisfactory equation of state that represented the observed facts within anything like experimental accuracy. By defining a new variable which I called the "acentric factor," and by freeing myself from the limitation of conventional mathematical functions, I was able to represent the volumetric and the various thermal properties of fluids over essentially the full volume, pressure, temperature range.

I'm currently engaged in an investigation somewhat similar to that on fluids but related to the properties of electrolyte solutions both aqueous and otherwise.

In the work on fluids and in a number of other areas, such as the thermodynamic properties of large molecules, the investigations have involved the bringing together of knowledge of spectroscopy and, of course, the underlying quantum theory, together with that of statistical mechanics and then the skill, or art, if you wish, of making approximations which simplified the problem to one of tractable proportions, without emasculating it by the elimination of some essential feature.

Ridgway: *Now, what was the state of the "art" in your field when you first decided to bend your energies in this direction?*

Pitzer: *I have been trying to refine the state of knowledge in a territory that had already been explored to some degree. Nonetheless, I think it is important to note that in the middle 1930's quantum mechanics had attained a form that was quantitatively valid and generally applicable. This was really new, and although quantum mechanics had already been applied successfully to a few problems, it was really quite new as a general tool for the physical chemist or theoretical chemist. The ability to use quantum mechanics effectively without the electronic computer was very much a matter of finding approximations which retained the essence of a problem and yet simplified it enough to make it tractable so you could get an answer.*

Ridgway: This style and ability would seem difficult to define or quantify. It sounds as if it might be sort of an art.

Pitzer: Yes, and many of the artists weren't all that successful. There was an adage during the early quantum mechanical years that if the nth approximation fitted the experimental results the n-plus-one-th approximation would not. And this means that the nth approximation had fitted by accident rather than by having been really a skillful approximation that retained the essence of the problem and somehow sloughed off detail that wasn't important.

Ridgway: A major issue facing all of us is that of so called technical obsolescence. This, you are not! But obviously you have developed new tools, techniques and skills. Do you have suggestions on how a scientist can change with the times?

Pitzer: In general, I've felt that one can learn a great deal by individual study, and I suppose this is one of the most important things I try to impart to a graduate student, that he doesn't have to take a course to learn everything. He can learn by individual study, and I insist that he do enough of it so he will have acquired confidence. That doesn't mean that the contact with other people in groups, as well as individually, is not very important.

Ridgway: Did funding for equipment, supplies, technical assistance represent a problem over the years?

Pitzer: Of course! Back in the period we're talking about everything was done practically with baling wire and a pair of pliers and your own hands, figuratively speaking. Until after World War II, funding was very, very modest but even that modest funding was very essential. The fact that the State of California provided, through the University, some limited funds for research and the fact that G. N. Lewis administered those funds in a fashion that made them accessible to junior members as well as senior members of the department, were all very important during the pre-World War II years. In the post-war years when external funding was a substantial part of the picture, I've never had any particular problems.

Ridgway: What do you see as the future in your area of investigation?

Pitzer: I think that in terms of investigations that would follow this general style, there certainly are a continuing and attractive set of opportunities. That doesn't mean they will concern the precise subjects that I've been involved with in the past, because I think some of those are pretty well solved. However, there are other subjects that could be clarified and generalized by investigations of a similar pattern. On the other hand, I think there are many contributions that could be made by a chemist in the future in overlapping areas of science that frequently carry some other name. I suppose the biggest one of these will be biological. In other words, chemists are looking at biology on the molecular level, and it has a chemical complexity such that I think is going to require the chemist's, including the physical chemist's, point of view and training to contribute successfully to it. To a significant degree, I think the same sort of thing can be said, for example, with respect to the earth sciences. That is, the whole surface of the earth, its history and so forth, is again a chemical problem.

Ridgway: Dr. Pitzer, my next question should come as no surprise since our readers are basically interested in "Education." Training or educating the graduate student in research is as much a part of the overall process as is the more generally thought of lecture-textbook business. What do you look for in a young predoctoral student who wishes to work with you?

Pitzer: I suppose my views there are not particularly unusual. Obviously you want someone that has a high mental capacity, who is bright and intelligent. You want somebody who is enthusiastic, who is really interested in what he is doing. In some cases, one may want someone who has acquired or clearly has the background to acquire rapidly skills at some particular type of investigation. It just depends on what one hopes to accomplish. In other words if you are looking for, say, spectroscopic investigation as a critical part of your research, you may want somebody who has already had some experience with that general type of instrumentation. One of his important roles may be to convey these skills to graduate students in a more intimate way than the professor can possibly do. On the other hand, on other occasions you may not care about such particular skills. Experience with electronic computing is another element that may be desirable. Often it doesn't really matter what specifically one has done in the past as long as it's generally related to the investigations planned for the future. Let me emphasize again a spontaneous interest in science. He ought to be thinking of science as puzzle solving that involves a lot of fun, not think of it just as a job.

Ridgway: Have there been any marked changes over the years in these traits which you would suggest reflect better or perhaps not as good training in the sciences?

Pitzer: I think that students are better prepared, at any given stage along the way, now, in their capacity to deal with fairly sophisticated mathematical things than they were in corresponding stages in the past. On the other hand, you are now more likely to find enormous gaps in factual chemical knowledge, than you used to. The cause is partly the change in the freshman college courses in chemistry; these used to be largely nonmathematical or have a low level in mathematical requirements but used to cover the factual chemistry of the more important elements and compounds in a systematic way. It's not merely the facts acquired at that time but the concept that a chemist ought to know a certain body of facts just as direct experimental knowledge, that is now missing. Recently much more time is given to theories and, insofar as these are really sound, effective chemical theories, this is all to be commended. However, the net result is that the student nowadays may seldom experience instruction in which there is an expectation that he will remember a large body of qualitative systemized facts. I think this is a natural development, by and large, and probably commendable, but I think it may well have been over done at times, because much is known experimentally that has not been reduced to any sort of quantitative theory. Therefore, if one is going to be fully effective in chemistry, one must carry a certain amount of factual knowledge in one's mind.

Ridgway: Do you have strong feelings about our present educational process?

Pitzer: I don't know whether I should say I have strong feelings. I'm certainly very much interested in, and concerned about, our educational processes at various levels, but I'm not one that thinks they are either horrible or perfect. I am very much concerned that they be both of high quality and sufficiently responsive to, and understood by, the general public and that they're properly supported, too.

Ridgway: Your attitudes and comments are especially valuable because of the two presidencies of major universities held by yourself. Priority decisions on time devoted to research and that devoted to fund raising is a problem we all have, but not to the same degree as you have had. A third parameter is that of the time and energy devoted to so called classroom teaching. Do you see a dichotomy (should I say a "trichotomy?") in the research and teaching processes?

Pitzer: I have found the combination of teaching and re-

search to be very satisfying. They are mutually supportive. The result is that I don't see the teaching versus research as an antagonism except in the purely mechanical sense that the time you spend directly engaged in one you can't spend directly engaged in the other. But some of your time is spent indirectly in support of both. My attitude, which is very strongly in support of the dual activity and the synergism of support of teaching for research and research for teaching is, I think, influenced by the very satisfactory relationships that have been developed in this department here. In chemistry at Berkeley I have had colleagues who had similar attitudes and handled their full range of obligations not only responsibly but also enthusiastically. As I've gotten wider experience in later years, I've come to recognize that this is not necessarily a typical or broadly generalizable condition. Not all people who are successful and effective in research also enjoy teaching. Not all people who are successful and effective in research are capable teachers for one reason or another. Likewise there are certainly lots of people who are capable of good teaching but probably would make a trivial contribution to research, if any and, after realizing the triviality of their research contributions or the frustrations of having them refused publication, would find research of no constructive effect in relation to their teaching. Thus, as the years have gone on, I've been more and more inclined to recognize that there are probably only a limited number of departments in the very best universities that can really be staffed almost entirely by people who are outstanding in both teaching and research. Even then, there may need to be more recognition of diversities than there has been in some cases.

In terms of higher education, generally, I think we have to accept a pattern of diversity in which we will always encourage the duality of equal excellence in teaching and research, but we should be sure that we get excellent teaching even if it means including, with full respect and full standing, in the faculty people whose research contributions may be relatively minor, but who are able to do, and interested in doing, a really strong job in teaching through the years. I think, as one gets away from the top level of the universities, the chances of obtaining faculty members who are excellent in both teaching and research drops rapidly and can no longer be the dominant pattern of staffing; you get a situation where that is the exception rather than the rule.

There was a concept a few years ago in which research was to be encouraged widely in undergraduate teaching institutions as a means of continual retraining or refreshment so that the teaching would be kept up to date. I think that this is an impossibly expensive way of approaching the problem. The cost in time transferred from teaching to research, the cost of the research-supporting personnel and the other research costs makes this impossibly expensive as a general pattern. That doesn't mean research ought not to be encouraged in a few undergraduate institutions. There are a few examples where there are unusual capacities and opportunities, but it's just not a pattern that can be adopted generally.

Ridgway: *From time to time in our discussion you have mentioned the word "satisfaction." What kind of satisfaction have you gotten from research and what kind of satisfaction, a good feeling inside, have you gotten from your other activities?*

Pitzer: *As far as I'm concerned, I take an inner personal satisfaction in just solving a problem that seemed challengingly complicated and of some significance. In other words, while I enjoy solving a pure puzzle that has no external significance, I take much more satisfaction in solving a problem or puzzle that seems to have applicability on a broader range. One has, certainly in the social sense, a satisfaction in recognition by colleagues, by people you respect, of your own attainments, and I think scientists are particularly fortunate in that this sort of recognition is international. It gives you points of contact all around the world in a very pleasant way. Likewise, on the teaching side, the essence of having enthusiasm in teaching is to have real personal interest in students and to feel a sense of satisfaction yourself in their accomplishments, not only when they are in your class, but also subsequently. I've certainly been fortunate in that regard.*

Ridgway: *Are there special responsibilities that the scientist should bear toward society other than those normally encountered in the life of the community (those responsibilities as a citizen, irrespective of his profession)?*

Pitzer: *I think any professional or well-educated person has a somewhat greater citizen's responsibility than the usual one for taking an intelligent interest in the problems of the community and reaching his own best conclusions. Whether he wants to publicize his conclusions is up to him, but at least he ought to have reached them and implemented them by his own voting and his discussions with other individuals. Some of the problems facing the community have a scientific aspect. Then I think his responsibility is a more specific one which involves his using his best efforts to see that the society is evaluating the problem with a correct estimate of the scientific aspects. That doesn't mean that the general community needs necessarily to understand the science. But, if the problem is being presented for public discussion with false estimates of the scientific input, then I think a scientist has a real responsibility to try to correct that falsehood and convert the approach to an evaluation of the other aspects of the problem with scientific inputs within the range of current knowledge.*

Ridgway: *In the present era, one in which the scientist has obviously had his halo tainted if not corroded, do you see a greater necessity than in the past of critical assessment of the scientist's discoveries and results as they might affect the whole community?*

Pitzer: *This certainly is a problem that has come in for a lot of discussion recently. I think, in the very broad sense, the ideas that have come to be called technological assessment are good ideas. It seems to me the scientist, when he's developing something that is likely to have one or more practical applications, ought to speculate in his mind and in his discussion with his colleagues, including colleagues in other professional areas, what these applications might be and what their social consequence might be. He can call risks to the attention of people. He may be able to build up interest in more constructive or more responsible utilization of his discoveries rather than leaving it more or less to chance. This is a new field, this business of trying to foresee all the possible implications of some new technology; and it must be done primarily in later stages after the scientist's discoveries have been published and engineering development is proceeding. But technology assessment is an area in which scientists can contribute, and I think scientific careers are going to become, and should become, more diverse. I think there should be a real interest of scientists, a willingness of a certain number of scientists to devote full careers or substantial parts of careers to this technology assessment process.*

Ridgway: *What are the major activities that you feel have helped to enrich your life outside of the professional sphere?*

Pitzer: *This has involved many things. I've had a very happy* family situation—a wife who has been both capable and devoted and three children who have been, on the whole, a great pleasure and a great satisfaction. It doesn't mean one can't find individual times of distress along the way, but relatively very few. I've always found the University communities to be very stimulating places, in terms of human contacts outside one's own immediate professional field.

I like to travel and visit places of interest whether in terms of natural phenomena or scenery or in terms of human developments both recent and ancient, and a career, such as mine, gives opportunities for travel without undue interference with other obligations. Also the international character of science tends to make it pleasant because one frequently finds personal contacts in various places.

One of my hobbies, a rather serious one through the years, is sailing and boat design and building. While chemistry has very little to do with it, my scientific background makes it feasible and interesting to learn the basic physical principles underlying my interests in boat design and in sailing and to apply these principles in this activity. Since I enjoy working with my hands, I enjoy occasionally building boats which I have designed.

Ridgway: Have your interests and accomplishments had an effect on the educational or professional accomplishments of your own children?

Pitzer: I'm sure they have had an influence. Each of the children was a serious student and came to realize that children of professors were expected to excel in school. I judged that the pressure of this expectation was more than adequate and sought to moderate this pressure rather than to accentuate it. I think this worked out very well. All of the children went at least far enough in chemistry and related sciences to prove to them, and to me, and to anyone else who was interested, that they were capable and successful in science, and then felt quite free to go their own way and emphasize other things as they chose. It turns out that one did follow a career in the theoretical side of chemistry remarkably similar in its focus to my own and is now at Ohio State. The other two have gone into things that involve a significant mathematical or scientific basis (my daughter at the Salk Institute and another son in economics). They have no close relation to chemistry but both are having substantial success with their own particular objectives and activities.

Ridgway: Now we take a look at the future. Do you see areas of our science or related ones that are being neglected?

Pitzer: I suppose in a formal sense you can always say yes, although I think that would be rather misleading. There is always a certain amount of rushing from one area of science to another in accordance with the opportunities that new instrumentation or a new concept provides. Sometimes this leads to a neglect of other areas of science in which important things can still be accomplished. I think chemists are less inclined to do this than scientists in some other areas so that I don't really have any particular complaint.

I should say something about interdisciplinary areas and about dividing lines between disciplines. I think one can easily be unrealistic concerning interdisciplinary areas. Any human organization and any human activity has to be subdivided in some fashion to make it tractable and operable. You've got to break them down into smaller units, whether they are to be managed hierarchically, in the sense of the military organization, or whether they are to manage themselves democratically, in the sense that a university department handles most of its own affairs. I don't see any objection to using traditonal disciplines as compared to non-traditional ones for this organizational pattern, provided there is enough flexibility in the boundaries. In any case, it should be easy enough to cross boundaries so that new fields, which do not fall conveniently within a single discipline, get the attention that they deserve. I do not believe that disciplinary boundaries have been unduly inhibitory. The physics/chemistry line is a broad-based one and illustrates my point. It has always been soft enough so that there wasn't any very great difficulty there. It's always been the pattern that the chemistry students take a certain number of courses in physics. There has been a lot of research collaboration between physicists and chemists at Berkeley and in many other locations. This always involves some extra complications but it's still feasible.

Ridgway: We have asked this question of each of our interviewees and obtained some very interesting answers, not always expected ones. If you were just completing a degree program in the year 1975, would you launch a career that parallels that which you did follow, or might there be some new and perhaps quite different career?

Pitzer: I think a person with the same interests that I had at the time, the same satisfaction in problem solving and the same interest in natural phenomena, should seek a career somewhere within the range of physical science or the more quantitatively oriented biological science or applied science, including engineering. I'm not necessarily predicting that, if I were deciding today, I would choose chemistry or the same subdivision of chemistry. I think there are many opportunities in physical chemistry, somewhat informally defined as I've always thought of it. But there are probably enhanced opportunities now in the area in which one is focusing on problems of biological importance. There are many comparable opportunities in other areas that are based upon physical science with more or less emphasis on application. I think the whole field is a challenging one, an interesting one in which a young person would certainly find satisfaction and I would certainly include physical chemistry among that menu of opportunities.

A person who has had in his early career a lot of satisfaction in mathematics as well as in experimental measurements would be interested in an area where the state of advancement of knowledge is such that one is dealing with things quantitatively and numerically rather than just qualitatively. I think it is a somewhat different type of person, in terms of his interest and aptitudes, who goes into a purely descriptive, qualitative, classificatory type of science.

Interview with Kenneth Pitzer
Conducted by Harold M. Hyman of Rice University
August 1, 1995, after revision 1997 by Kenneth S. Pitzer
[videotape of this interview is deposited in The Bancroft Library]

Pitzer: I have somewhat of an outline here of the major situation as I saw it at the time. Shall I just go ahead with that, or do you want to--

Hyman: This major situation is the reason or the reasons you came to Rice?

Pitzer: Well, in part, yes. One of the attractions in going to Rice was that it seemed like it was a place where it would be feasible for me to maintain some scientific work, in research and with young postdoctoral scientists. That proved very feasible, by the way, and we accomplished quite a little science during that period.

I knew about Rice also because two of my own doctoral students were then on the Rice faculty, and in particular, most importantly, Robert Curl, who has since been chairman of the department recently and has really had a very distinguished career in science; [indeed, he received the Nobel Prize in late 1996].

Hyman: And is a very nice person.

Pitzer: Indeed, [laughs] he is a very nice person.

It was very clear, however, that there were two major problems at Rice College that were identified in conversation right from the beginning. The two questions were, up to that date, the exclusion of black students, and the fact that they charged no tuition for students. The admission of blacks was essentially a moral question, in the context of the society of now as compared to when Rice was founded, and the tuition question concerned income. Universities need money, but it's not just the loss of the actual income from tuition, it's the signal that it gives other people that Rice had been apparently so wealthy and so well funded that it didn't need this money. And that essentially led to virtually no consideration for Rice from national sources, large corporations, major foundations, and similar sources of financial funding for the university.

The third thing that came to my attention later that I thought was also important--and I want to mention right at the beginning--there had never been a development of a tenure system for the Rice faculty. I thought that needed to be dealt with, not so much that there had been any serious question about the dismissal of anyone for some emotional or questionable single action. The problem was more that in selecting faculty for permanent status, there was no clear-cut single point of decision. It was sort of a gradual, much softer consideration over several years, and could lead to ambiguities and also lead to lower standards.

The trustees were well aware of the first two questions, the need for action, and it seemed to me to be prepared to act. I made it clear that prompt action was essential, and I was only interested in it [the presidency] if they were committed to some reasonably prompt action, but that I was quite flexible as to the detailed procedure, the detailed mechanism. I was not a legal expert, and I was not expert on what you might call the public relations and alumni relations in the Houston and Texas community, and these all had to be considered as to just how one was going to go about this process.

The legal process is to sue the attorney general under the cy près doctrine, which is an old English doctrine--

Hyman: Would you spell that for the purpose of people using this doctrine?

Pitzer: Yes. The doctrine essentially says that for a donated public service, charitable, or educational entity, that if in later years the primary purpose is impeded by some secondary condition, then the secondary criteria or restrictions can be removed or readjusted. As I said, this had a basis in longtime British common law and was carried into the Texas legal system.

Hyman: You consulted attorneys?

Pitzer: Oh, indeed. One of the best legal firms in Texas was very much involved, Baker Botts. That was a firm, of course, which also had a long time with Rice in that the Baker of the naming of the firm had been one of the original trustees selected by Mr. Rice personally, and although he was no longer living, there was a close tie with that firm and a close commitment with that firm to Rice, and then Malcolm Lovett, a son of the first president, was also a member of the firm and a trustee. So that was very closely tied together.

Questions were raised as to whether there was any need for a legal action, whether the trustees might not just go ahead and take these steps, with appropriate declarations, but the majority felt that that just might lead to a lawsuit and a legal wrangle that would be less favorable to the university than if the university took the initiative.

Hyman: Was there opposition among the trustees to the step--

Pitzer: Not really opposition. It was just discussion of alternatives. I don't recall that anyone--well, was uneasy with the final decision to sue the--the mechanism then under cy près was to sue the state attorney general, as the representative of the public interest. The attorney general at that time--let's see, was--[laughs] I'll fill that in. Wagoner Carr.

Hyman: Oh, Wagoner Carr.

Pitzer: C-A-R-R, yes. A preliminary discussion with him told presumably George Brown or one of the lawyers that he would not personally oppose it. On the other hand, he would not stand in the way of anyone who had a legitimate interest in the Rice situation taking his position, essentially, in court, to oppose the suit under this doctrine of cy près. And a couple or possibly more alumni did, under the first name of John Coffee--

Hyman: Would you spell that?

Pitzer: It's just the kind of coffee you drink. He--and I've forgotten right now the second name involved, and I don't know whether there really were more than two, they hired a lawyer and did oppose the suit, and it was tried in the--I guess you'd call it the superior court of Harris County, Houston. They asked for a jury trial, and there was a little debate about whether that should be opposed, but the decision was to go ahead.

The lawyers that were actually handling the case, the senior man was Dillon Anderson, who was a noted top-flight and experienced lawyer, not just in Houston and his firm but he had a major office in Washington, D.C., and the national scene and so on. But the one who handled it in detail was Tom Martin Davis, a very able lawyer who was helpful. And then Malcolm Lovett, who was both a trustee, son of the first president of Rice University, and a member of the same law firm, didn't actually operate on the case within the firm but was the liaison to the trustees, to me, and to others in the Rice and Houston community.

The jury was accepted, and I actually participated in the legal discussion as to what challenges should be entered and so forth on the Rice side. And of course, I testified and helped arrange testimony from the presidents and chancellors of several of the leading universities, both in Texas and then--Logan Wilson, who was from Texas and then president of the American Council of

Education, I believe it was, had a national position in the higher education world, and he was very helpful.

So the trial went forward. I was surprised that we had to get a unanimous decision on a civil case of this sort, but we did, and we got it.

Hyman: I have to check up on the Texas law; I'm surprised also.

Pitzer: Yes. In California law, in a civil case, a nine-to-three vote is sufficient. It was appealed. The appellate part was essentially in the hands of the lawyers; I didn't have very much to do with that, except being impatient about the delay that this imposed in terms of implementing the favorable decision. But the appellate court, a three-man court, issued the same decision, wrote a very good sustaining statement that to me was so excellent, so convincing from the point of the view of the public and the alumni generally that I arranged to have it printed up in, whatever, 20,000 copies or whatever was appropriate. It was distributed under a preamble from the head of the alumni association, which I thought was more appropriate, but I was really at least partially the instigator of that.

Hyman: You suggest that there was no serious split among the alums on this matter?

Pitzer: My sense was that very few were in the Coffee camp, as it were, in opposition. There were a lot of people that were a little bit uneasy about it, and I thought this appellate court decision was just the right sort of thing to reassure them that all the proper procedures had been followed and that the public interest and the Rice interest was really being served. In other words, well, we made our case, that in order to fulfill Rice's original intent for the institution, these provisions that--it wasn't even clear that they were requirements of his, as compared to sort of customs of the time and the situation initially, but nonetheless, if they were, it was appropriate to make the changes. There was clearly no opposition on the campus whatsoever. Indeed, the view of the campus was such that I had to control the impatience over the delay in implementing the admission of the black students until it was legally permissible. And there was a little problem in a single case there, but we worked through it without any real big difficulty.

The implementation of tuition involved more consideration and detail in various aspects. I recommended tuition only for newly-admitted students; in other words, not retroactively on any existing students. At least those that were going through on regular schedule; I've forgotten actually what we did about the student who had been admitted and then took an extended leave of absence or something like that. And I favored starting at a fairly modest level and increasing it gradually through the years. There was some discussion in the board as to whether it ought to be applied immediately to all students, but again, there was no real argument about it. The board of trustees went along with this aspect very agreeably. Everyone agreed that there should be a very strong, generous scholarship program for cases of financial need, probably more generous than was typical on the national scene, but still based on the same need criteria as per usual in other universities.

The idea at the time was that the tuition level would be increased, again on each incoming class, year by year, upward, and I don't know that there was any very clear record as to just how high it was supposed to go in the long run, but my thoughts, and I'm fairly clear on this, was that I would have gone up to about 80 percent of what you might call the national top prestige level of Harvard, MIT, Stanford, Columbia, and places like that. My impression is that in fact, it leveled off at a somewhere lower percentage, and one can argue the merits one way or the other on that. But that was after my time. In other words, we were on the schedule that I had in mind at the time I left.

Hyman: May I interrupt you to ask how do you account for the ease of breaking the racial exclusion barrier at Rice as compared to what was going on elsewhere on campuses, in analogous situations?

Pitzer: Well, I suppose essentially no one had any objection. The great majority felt that an injustice was being removed, but the Rice students were a self-confident lot in terms of their academic credentials and all that sort of thing, and their future employment possibilities, and so no one felt threatened. Now, there was no affirmative action under discussion; affirmative action hadn't been thought of at that time. [laughs] It was assumed that anyone, any black student would come with full qualifications, and the first one was a mathematics graduate student who was obviously very good. There were relatively few undergraduates, but they all met to the full qualifications, so that no one was being threatened by--

Hyman: A novel point you recognized--

Pitzer: That was one of the things that I never worried about in the slightest. There was overwhelming support in the faculty and student body, and if there was any defection on that, it would be taken care of easily and locally, without probably ever coming to my attention even.

So back to the third thing I mentioned: tenure to the faculty. I regarded it as important, both to be in line with standard, first-line college and university practice nationally, and to be certain that there's no ambiguity among the junior faculty members about their status as to continuing at Rice or having to seek something somewhere else. I had to give a little explanation to the trustees, but it wasn't a serious problem at all. There was little complication for two or three faculty that had been around about four or five years and might have thought they were already permanent, and maybe I didn't think they were permanent. I asked for a new review on these cases, but that was not a big problem.

The three things were accomplished. The next two closely related things that had to follow after the three, although--at least the first two--although they had been contemplated and I discussed with the trustees. First was a general capital fund campaign, and secondly was a plan for future development, and all this is sort of underlying--all this is sort of implementing the idea that Rice might in the future become more nearly what Edgar Odell Lovett contemplated in 1917, that it would take its place among the very top level universities both of the United States and of the world.

Now, when you're just opening the doors, it's one thing to contemplate something like this. It's entirely understandable that you don't get there immediately, and anyone that lived through the 1930s understands why not much progress could be made then. But still, what had actually developed was an excellent undergraduate college, with a few small but very good graduate programs, and a few other graduate programs, valuable locally but not at the top level of leading private universities with distinguished graduate programs more or less across the board. But it seemed to me that Rice, with no counterpart institution in that general part of the world, that there was an opportunity for Rice to attain the sort of stature that Edgar Odell Lovett proposed in the beginning. But that would take more money, and it would take more money not just sort of piecemeal to build this building or hire that professor, but a higher level of funding generally. One ought to have an evaluation of the various requirements recorded in a paper and a specific plan, at least for a decade ahead.

So these two aspects came together, came along simultaneously, I should say, and this was the first campaign of that type in Rice's history. I was intending to ask you ahead of time, but I don't think there's been one since, has there?

Hyman: That's right. The circumstance is formally in George Rupp's decision of having to entertain thoughts of leaving Rice, that is reported centered around the question of the present board's unwillingness to have a second capital campaign.

Pitzer: That is consistent with my information. When I visited Rice, what was it, about a year and a half ago, something like that, after Malcolm Gillis's inauguration, on his invitation, I talked to Frank Ryan, who was the vice president for development and so on at the time. That was the indication that I had gotten, although it was a little bit different. I'm somewhat hesitant to say this, but I don't see any reason for not saying it.

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Pitzer: The question was not just, "Should there be a campaign, a second campaign," but whether it should be initiated without having a consulting firm come in and evaluate and and plan it. I was told that the board was unwilling to go ahead with the campaign without having this sort of planning. In fact, we did have that planning; we did have a firm that had helped plan capital campaigns at Northwestern [University] or other places, and they participated right through the campaign, in a limited way. Didn't cost very much, took some time. They did interviews with major donors as to what might be anticipated, and to me it was valuable and appropriate. Thus, if that was the point of difference, and as I say, it's second-hand, but it seems to have been an unfortunate difference on that particular aspect of the problem.

Well, anyway, our campaign goal was $33 million. It actually raised $43 million. In my view, it was the strongest message that Rice needed additional money and would continue to need more in the future. The ten-year plan, which is in the record of course, was written for the education of the Rice community, even to some extent on campus, the trustees and the like, as to how Rice really compared with the top-line universities in the country and how, while its quality was excellent, the size of the operations was limiting in many respects.

And I reinforced that aspect with a number of confidential memoranda to the trustees with various comparisons, Rice and Caltech, rather than MIT, for example, whereas Caltech is not much bigger than Rice; it just had more money per student, if you wish, just comparing the science and engineering with Rice.

Another thing that was carried out toward the end of my time at Rice was the set of awards for teaching, teaching awards for faculty. George Brown was interested in this very much personally, and I was too. I think it's very important to strengthen teaching, particularly when you're also doing other things that give greater emphasis to research, don't let teaching be forgotten. And indeed, the awards give real concrete, substantial encouragement and credit for it. I suggested the idea of voting by recent alumni rather than current students. I thought it was important to avoid faculty just sort of playing to the applause of the audience of current students. Recent alumni have a mature judgment about how effective the teaching really was. George was quite happy with that aspect. He provided the money, and we jointly decided on how many awards would fit in of two different levels, I think it was, in--

Hyman: Some faculty express a concern, but it is not mine--

Pitzer: Yes, sure.

Hyman: That the teaching awards favor the faculty colleagues involved in teaching the large, introductory courses, because by definition, all students take those courses. Did you consider that with Mr. Brown when you established the--

Pitzer: Well, probably, but I think that's something you accept. This is one of the problems, one of the situations. We have it here [at UC Berkeley], and we've been very successful even without any such award system of getting a faculty member who is truly outstanding in their research to take the time, at least now and then, to teach a class far larger than any at Rice, and to do a very good job of it. But of course, there are some people that are just not adapted particularly to teaching a large class, and they, I think --you shouldn't be pushing them into doing it if they're not going to do it very well anyway, but I think they can live on their recognition from excellence in research. Then let those who both can and are willing to teach with great skill a large class essentially get the recognition for it.

I recognize that for a concern, but I don't think the awards were completely that way. As I recall, at Rice there were some who were people that had not taught the very large classes. Of course, with some growth opportunities in the faculty, there were selections to be made, including even Professor Hyman [laughs]--

Hyman: I'm certainly glad that you hired me.

Pitzer: This was really fun, as far as I was concerned, looking at various qualifications, discussing them with the recommending people. They would bring candidates, hoping that for at least some of them, this would be an attractive opportunity, and it worked out that way. That last aspect, of course, was the final reality of improving the stature of Rice and the whole university scene.

I should say a few words about student life. Residential colleges had been established, but just before I was there. I give great credit to my predecessor, William Houston. I should have mentioned that I had known him slightly when I was an undergraduate at Caltech in my senior year. I took a course from him in mathematical physics, and had maintained at least a little contact. At least I knew who he was, and we had a great many good, favorable contacts with one another. Although I don't think he'd done any research while he was at Rice, he had maintained contact with his scientific work, his scientific world, and I planned to do even more in essentially maintaining some research.

While I am distracted on that, I might just add a word about my way I operated on this. I found that once my mind was involved with some personnel or administrative problem, I was not going to think any more good thoughts about my chemistry problems. So I did the chemistry the first thing in the morning. I had a separate office and two or three young men there, postdoctorals, and I would work with them and a couple of faculty. Bob Curl, who was a former student and was on the faculty, and I published one joint paper at least during this period, and I think more than one.

And then about ten-thirty, I would go over to the president's office, and my secretary was always free to call me if there were something that really needed urgent attention, but most any problem of that sort can wait until ten-thirty in the morning. And then the rest of the day was for that. And of course, sometimes I was out of town, sometimes other obligations took the time completely.

Hyman: You mentioned that the legal problem was solved with what seems to have been relative ease, and that the matter of initiating tuition from students was initiated, again with relative lack of great strain. And now you're going to the residential college system, and I'd like to know your perceptions about the degree of ease with which the coeducational colleges were initiated, coeducational habitation of the colleges.

Pitzer: That's after my time.

Hyman: I know that, I know that. But it's the degree of ease that I'm inquiring about, your sense of the seriousness of maintaining the unisex colleges.

Pitzer: The only thing I can say about that is that, as I say, I could sense that that was coming. And I remember saying to Norm Hackerman, my successor, one time, probably about '69 or '70 or something like that, when he was in office at Rice, "You're going to have this question of mixing the sexes in the residential colleges." But I said the situation at Rice is enough different certainly than it was at Stanford or here that you make your own decisions about that on the basis of the local situation.

But regardless of that, I thought the residential colleges were a great idea, and I was glad to do everything I could to strengthen the situation, and to get additional colleges built in accordance with the growth intended in the ten-year plan. And so we got the Brown College very promptly, and the Sid Richardson was all signed up when I was still there. I remember walking over the campus looking at the location, potential location, with George Brown and Perry Bass, who was the Richardson Foundation top trustee who was controlling the gift. Although George Brown was, of course, negotiating this thing, I was present and included. So we just took advantage of what had been done there and carried it along.

Now, you've invited comments about the people, and I'll make some comments on my initiative about three very important trustees, and I'm going to leave it to you to raise further questions. The most important one, of course, as far as I was concerned, was George Brown. He had the money, his own and his brother's, and his brother died rather early in the time I was there. The Brown Foundation money was essentially his brother's money, but there was every indication that he was using it wisely and consistent with his brother's probable intent when he was alive.

Hyman: Did you visit the Rice Hotel suite, 7F and 8F? The Brown family's two residential suites.

Pitzer: I don't remember whether I did or not.

Hyman: I raise that because those two suites at the Rice Hotel were well known as the place where major deals were made.

Pitzer: [laughs] Well, I may well have. George and his wife, Alice Brown, were most cordial hosts. We got really very well acquainted with them and had great affection for both of them. And I think it was mutual. We were out in their west Texas ranch and doing things out there and so on.

But, to come back to the really important aspects of this, George Brown knew people in government at the national and statewide level intimately, and he had a very close relationship with Lyndon Johnson, who was then vice president initially, and then president. And with business people nationally. If he wanted to check up on what I was saying as to what was the national pattern or expectation at Yale or Harvard or MIT or someplace like that, all he had to do was call up one of his friends who was a trustee there and say, "I'm getting this sort of a story; is this correct?" Which made it very easy to work with him, and it meant that he could exert a great deal of influence on things.

One of the things in which I played only a somewhat peripheral role, but it fits in with this pattern, was the location of the manned spacecraft center out there halfway to Galveston, and he was negotiating that in part with funders. It turned out that Congressman Albert Thomas was in a key position. I had testified before him when I was director of research for the Atomic Energy Commission, so we knew one another. Thomas knew that I had national standing scientifically in both the government world and in science itself. How much of a role that played is not clear, I'm sure it's only very much a secondary part of it, probably.

But anyway, we were able to build in a number of details and put that project across, which was only helpful to Rice itself in a rather secondary manner, but it did help, and it gave prominence. Just before you came, there was an event in which President [John F.] Kennedy gave a talk in the Rice stadium about the space program, and that probably would never have happened without this other connection. That was the biggest audience I've ever presided over, probably--I'm sure it was.

George Brown unquestionably favored the advancement of Rice to a higher level, provided undergraduate quality was protected. He still was very anxious to keep the quality of the undergraduate Rice engineering program particularly, and of course, the engineering area was one in which Rice already had good graduate programs. So that he knew where I wanted to go and was in general agreement with it, except for possible slight details.

Newton Rayzor was the one who first approached me, was greatly devoted to Rice, and had a lot of contacts with faculty, so that he knew what was going on on the campus, what faculty were thinking, and so on. He had enough money of his own to be sort of an equal among trustees, although it wasn't at the George Brown level. Again, also, he and his wife were most friendly, a pleasure to be with personally, and went out of their way to make us at home. On the other hand, he did not have a national acquaintanceship and the access to sort of national contacts, national information and so on as compared to George Brown.

Malcolm Lovett represents sort of a special case. He was the son of the first president of Rice, was a great help as an intermediary between the lawyers handling the Rice suit on the changes and as between them and the other trustees and with me. However, he was really not acquainted nationally in the sense that George Brown was.

This led to an ambivalence with respect to the question of strengthening Rice toward a higher level of university status and effectiveness. He realized intellectually that his father's aims had been at a very high level, but his actual experience with Rice had been in later years in which his father had essentially acquiesced in going along with what could be done with the relatively limited amount of money that had come from the original Rice gift, plus a few relatively minor additions. I'm sure he'd gone along quite happily with the moderate additions during the Houston period, in which Harry Hanzen and a few others had made fairly substantial gifts, although the biggest financial improvement of that period was the Rincon Oil Field that George Brown really engineered; the getting of some funds to buy this oil field that was legally entangled so that it was not attractive to a private purchaser, but apparently was attractive to a nonprofit institution.

Anyway, it turned out the oil field had a lot of oil in it, and the resources were handled very conservatively. In other words, the income from the oil field was reinvested, and only income from those securities were the additions to the operating income of the university.

But to go back to Malcolm Lovett, he remained nostalgic in a sense for the Rice that he had known, as compared to the Rice that his father had contemplated and declared to be an aim for the longer range future.

That then is what I thought would be worth saying about the three trustees that were most important as far as I was concerned. I could answer questions about others if you wish, but I don't think I'd add to that a great deal.

I'll make just a few remarks about the situation in 1968. George Brown is now retired as chairman. I've forgotten whether there was any definite age rule on that or whether he just decided that that was long enough. Malcolm Lovett was now chairman. The major changes contemplated in 1962 had all been accomplished, and a number of other advances had been made that seemed worthwhile and we could take a great deal of satisfaction and pride in. In the absence of any attractive, persuasive offer elsewhere, I would undoubtedly have continued at Rice quite happily for a number of more years.

I sought special funding for, among other things, a graduate school of business, which was indeed obtained within a few years thereafter. There were a number of other special projects that might have been funded and brought into existence.

Hyman: Was the music school situation satisfactory to you--

Pitzer: No, that was an opportunity--it was a very marginal--I would say good but very marginal-sized operation in my time, and I would have welcomed an expansion. But I can't say that it was quite as specifically on my agenda as the graduate school of business was. In fact, it was a great development. [laughs] The present situation is very impressive.

But offers, several offers, were at least tentatively being presented to me, and the Stanford one was the one that seemed to fit best. Then it became a real offer. I suppose I should say that, while I still had some California ties, we had been very happy in Texas, but we also had some connections still in California. And I must say that I anticipated in due time a probable tension with Malcolm Lovett when it came time to design a second ten-year plan, which would imply a second capital fund campaign, if we were going to move Rice seriously toward top-level status among private universities. So I decided to accept the Stanford offer.

As you can see, I am sort of working from some notes here. That's the end of the notes. I'll be glad to comment on other issues you might raise.

Hyman: I'm sure you've anticipated the question I will pose now: that is, upon leaving Rice for Stanford, you possessed the most advantageous position of anyone to reflect on the events at Rice following your departure. I'm sure you paid close attention.

Pitzer: Oh, yes.

Hyman: Again, considering the purpose of this interview to recall all we can about what happened at Rice many years after you, I'd be grateful if you would offer your reflections on what was done right and wrong, or in between, in the events following your departure.

Pitzer: Well, I have not made any effort to specifically refresh my memory. I didn't keep any particular records considering what happened post my departure. There was the offer to Masterson.

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Hyman: How did you feel about that, just to start up your memory again?

Pitzer: Well, I thought it was an inappropriate offer. He was not of that stature. I had a perfectly friendly relationship with William Masterson, but I didn't think that he was, shall we say, of the national stature in any respect. Then mechanistically--

Hyman: Well, before we leave that point, was Mr. Lovett in favor of Mr. Masterson becoming president?

Pitzer: Oh, he must have been, yes. There wasn't sufficient consultation with faculty. Even without putting them formally on the selection committee, something, some indications of this sort would have certainly come to maybe other members of the board. I had no first or even secondhand knowledge of the details of what happened in terms of the board. I received several calls from various people. I think I did make one telephone call to Malcolm Lovett, urging that he give second thoughts or have caution or something like that, but I have no even very clear memory of that. It might even be a false memory, although I think there was one telephone call.

Hyman: You used the word "mechanistically"--you were going to follow it with a suggestion about the quality of the machinery that resulted in Mr. Masterson being named to succeed you.

Pitzer: Oh, by that time in the university world generally, it was pretty well accepted that, while the trustees made the selection, they had substantial consultation with the faculty leadership, and frequently with alumni or maybe even student leadership as to what the evaluations of these very important constituencies would be of the individual. These various mechanisms by which this is brought about. As nearly as I could tell, there wasn't any of it at Rice. Not to have any consultation or communication of at least a semi-formal sort is, I think, just a bad procedure. It's just wrong mechanistically.

Hyman: So how do you account for the lack of sensitivity on this point?

Pitzer: I don't know that I account for it. See, Newton Rayzor, was he still on the board?

Hyman: I think so.

Pitzer: I am surprised that he didn't arrange to take sufficient action to bring some caution into that. I'd have to really look down the list of board members to speculate as to where the caution might have been brought in. Frankly, I was quite puzzled by this going-on. It didn't seem likely in the Rice that I had known. Now, Frank Vandiver, on the other hand, was--although with virtually no administrative experience, was a figure of national stature and had personal qualities such that there was no reason why he couldn't handle at least an interim appointment.

Hyman: I suppose perhaps it's worth noting that with respect to the choice of Mr. Masterson, Frank Vandiver was a proponent of that choice.

Pitzer: I can understand that, because Frank had very strong personal feelings, and I think he and Masterson did have a pretty close personal relationship. But I think Frank Vandiver thought that he'd be the power behind the throne supporter if Masterson were the president, and he could be guiding him on the important decisions, and letting Masterson do the details. Now, that's just Kenneth Pitzer's speculation, but--

Hyman: It's a very interesting one, very interesting one indeed. Well, you certainly had an interesting set of experiences after you left Rice, but that's a subject for another interview.

Pitzer: That's another--

Hyman: That's another subject.

Pitzer: [laughing] There are lots of questions as to why I ever did it, because it wasn't that I wasn't at least somewhat warned, but on the other hand, I completely underestimated the degree to which Stanford would be a sort of national focus for the student demonstrations as compared to just being a local point of sensitivity.

Hyman: Let me on that point offer a tribute to you, and a sense when the disturbances at Rice picked up momentum after your departure, those upsets assumed a remarkable form, as I'm sure you know. That is, the several segments of the Rice community--faculty, students, alums--from all parts of the campus coalesced specifically with very few exceptions, and the resistance expressed to the appointment of Dr. Masterson as president was expressed with almost no destruction of property or abuse, physical or other, again with minor exceptions. Many of us had reflected on the difference between what you ran into at Stanford and what occurred at Rice, and I must say, there was a consensus that the relatively benign atmosphere that prevailed at Rice was very much a tribute to what you had accomplished during your tenure there.

Pitzer: Well, thank you very much. I appreciate your giving me whatever credit is due there, but Rice was different in many ways.

Hyman: Oh, yes.

Pitzer: Well, I think this business--the worst actors at Stanford in many cases were not even Stanford people. But there was a so much larger community out of which small numbers could be recruited. Well, those were remarkable times.

Hyman: Remarkable times indeed. Let me on behalf of President Gillis, and on my own behalf as historian, thank you for this opportunity to record your reflections on your Rice career, your own career, and to hope that future researchers find profit, as I'm sure they will. And again, I'll end it at this point.

Pitzer: Very good.

Interviewee: Dr. Kenneth Pitzer

Date: March 22, 1994

Place :Conference Room at Rice University

Interviewer: Louis J. Marchiafava
John Boles

Dr. Kenneth Pitzer

Interview - Tape 1 Side 1

LJM: Today is March 22, 1994. This is an interview with Dr. Kenneth S. Pitzer. Participating in the interview are Dr. John Boles and Dr. Louis Marchiafava. This is part of the continuing Rice History Project Oral History Series. Beginning the interview, Dr. Boles.

JB: Dr. Pitzer, I have a series of questions to ask that will sort of chronologically go through your involvement with Rice, from the very beginning of the Search Committee or the Board of Trustees approaching you, to your retirement. And some of the questions I am asking, historians always do things in retrospect, so I am asking questions in some sense because I know things happened later and I am asking them for those reasons. So I would like to begin with the question of your, the board's initial choice of you. I mean, what kind of selection process was involved, how were you first contacted?

KP: Well, I was first contacted at least in person by J. Newton Rayzor, who was a very active trustee at the time. He was active particularly with respect to campus and faculty activities, but also with the board generally. I presume there was a telephone call, because I don't think he just walked into my office, without anything before. But there is an interesting coincidence in this connection that came up just last night at dinner at President Gillis'. The person I met in the hall was James Kinsey, who had just graduated from Rice in chemistry and had a post-doctoral fellowship at Berkeley and happened to be standing in the hall when Newton Rayzor walked in and directed him to where my office was. At least that is Jim Kinsey's memory. Of course, I didn't even know who directed him. Anyway, Mr. Rayzor and I had a very friendly conversation, and he explained the situation at some length here. He asked if I would be interested and I indicated that I was not generally interested in a university presidency because I did want to keep my hand in on research and advanced scientific work, but I have known Dr. Houston, my predecessor here at Rice, from taking a course with him at Cal Tech when I was a senior undergraduate.I had kept in some contact with him through the years. I knew that he had continued scientific activity here, although it was somewhat different in character, in detail, than what I would want to do. I knew that he had had enough time to be able to maintain his activity in physics generally, so it appeared to me that it would be a feasible thing. So I told Mr. Rayzor that I would be interested on that basis - that it could be done with enough spare time so far as the presidency was concerned, to have a continued activity in science. He thought that that was feasible, and as I say, we had a friendly conversation generally. I don't think he said too much in terms of the selection process and I don't know that I quizzed him particularly about that. I assumed that this was just an initial approach, and that I might not hear anything more of it. I think the next I heard was actually a telephone call saying that George Brown would be in San Francisco and could I see him. And, as I recall, I went over to San Francisco and met him, probably at the Bohemian Club, I don't know. I was a member and he had been a guest at times. It's conceivable that he even came to Berkeley, but I think that I went over to San Francisco to see him, and we had a quite general conversation about the character of Rice and his thoughts as to what was important, not only in the short range but in the longer range. At that time George Brown was interested in how to get what's now the Johnson Spacecraft Center to Houston. It didn't have Johnson's name on it in the immediate future, and it turned out that a figure in this negotiation would be the congressman from Houston named Albert Thomas. And I knew Albert Thomas and he knew me. I had been Director of Research for the Atomic Energy Commission and I had testified before Albert Thomas's committee, which had jurisdiction, for three years so that we knew one another pretty well. That was just interesting information, but it helped for us to get acquainted with one another in a more relaxed fashion than if we hadn't had some mutual connection. Of course, he knew that I knew President Houston too, and I am sure we had other mutual acquaintances. Again, I don't think he went into great detail about just what the selection process would be or how many other people were being considered, but by this time itseemed pretty clear that this was serious. In the course of these conversations I am sure I made it quite clear that the racial restriction situation would have to be dealt with, but I didn't have any detailed criteria as to just how fast or by what mechanism. If there was any real question whether it had to be eliminated, then they had just better look elsewhere, since I would not be interested. I presume the timing question was discussed too, but I don't have any clear memory about how much at those very early stages. It came up more seriously a little later. If I remember the schedule, I went off on a trip to Taiwan and Japan for about a month, butwhether that was between the Rayzor visit and Brown visit or after both of them, I dont recall.

JB: Was this in late 1960 or early '61?

KP: Early '61. I think it was after both of those visits, and if so, I said I would be away. This had been long committed, and was a very interesting experience as a matter of fact. But anyway, by, as soon as I got back from that trip, which must have been maybe in May, something like that. Does that make sense time wise? I have not tried to check this.

JB: No, you didn't, right.

KP: My wife and I were invited to come, and I guess it was dinner and discussion afterwards at the Bayou Club and then substantial visits on the campus. William Masterson was sort of guide through the campus visit. On the other hand I knew people in chemistry quite well, both Bob Curl, who is now Chemistry Chairman and who had been a research student of mine, and John Kilpatrick who is now retired, had been a research student of mine. I knew Richard Turner, who was a very distinguished organic chemist. I knew him quite well. He was here. And of course I knew President Houston quite well, so I had many contacts on the campus and then there were other interviews arranged to get acquainted with others, so quite clearly it was a serious exploration. Then after the substantial discussion at the Bayou Club. I think it was the Bayou Club. It was not a terribly large group. It must have been mostly the regular trustees.

JB: Before we go on, in the very preliminary discussions with Mr. Rayzor and Mr. Brown, you raised questions about the desegregation of Rice and possibly about tuition. So you knew Rice well enough in that initial conversation to be aware of the racial restriction?

KP: This was something we were aware of at that time. Rice wasn't in the newspaper but heavens, anyone that was conscious of this problem, would realize it had to be a problem. I had very much been interested in this thing. I had gone out of my way at Berkeley to encourage particularly our first black graduate student. We call it "black" graduate student now, and he went on for quite a career. A very interesting young man and I followed his career. Also I had appointed, I guess, our first black faculty member and followed him, watched him get so involved in causes in which he was involved because of his race, that it really distracted him from his chemistry. Although he stayed in academia, as I recall, he just couldn't compete at the research level. And I had many other connections so that I was totally aware of the problem for an advanced level of academic institutions, so to continue racial restrictions was just ridiculous.

JB: So you made it very clear, in other words, that both for personal and moral reasons, you would not be involved with a university with racial restrictions and that if Rice had ambitions, in order to meet those ambitions, it would have to do away with that.

KP: Yes.

JB: And what was the reaction of Brown and Rayzor? Did they completely agree or?

KP: Yes, they agreed. Now, to what extent they would have balancedthose two reasons, I really don't want to try to say. I may have had an opinion then, and I might remember it accurately or inaccurately, but I don't think that I want to try to put that on record. I would say that our discussions indicated that this was no longer a serious matter of controversy with the board. I don't say that they necessarily had a unanimous formal vote, but that Rayzor and Brown knew their colleagues, at least the trustee member colleagues, well enough and some of the others. There was no question in their mind, that once the questions of legalities and of , shall we say, alumni relations, community relation, procedure, and so forth, were sorted out, that they would move. I made it clear that I didn't pretend to dictate these details. I said it was for them to choose. They knew the community and they knew the law. They had lawyers, Malcolm Lovett who presumably knew the law, or at least his colleagues did. I was perfectly willing to follow any detailed procedure as long as it wasn't unduly delayed.

JB: You came to Rice July 1, 1961.

KP: It was sort of a transition period, because I had an obligation to a special lectureship at MIT, which I cut short but did fulfill. And I had to do a little tapering off of my obligations at Berkeley, so we were in and out of the Warwick Hotel for a while. There were some renovations in what was then called the President's House and so I wasn't really here full time until maybe October except for that MIT lectureship and then maybe November.

JB: I know that in September, mid-September of 1961, The Thresher polled the students, that it was a very high turnout, and 65% of the students voted in favor of ending the racial restriction. And the faculty voted about the same, I think it was 8 to 1, so it is of interest to me that you came here with that commitment and that Rayzor and Brown had suggested they agreed in the spring of 1961 and there was that evidence that alumni, students, and the faculty thought that the racial restriction should be changed. The board did not really make the formal decision until September, 1962. Is that right? About a year before ...

KP: Yes. There was a long discussion about several aspects of it. The first question, well one question, I won't say the first question, one question was, "Is there any need for any formal, legal action with respect to the racial restriction?" The word white appears only in the context, white residents in Houston and Texas. The board named as individuals by Mr. Rice himself, were all, or at least the great majority of the governing board when the Institute was implemented. And in the first class they admitted non-Texas residents, let alone non-Houston residents. Therefore, you could easily read that as being just an indication of the primary beneficiary and in no sense a restriction. Houston was a segregated educational society at that time. White just meant it was intended for whites, not Negroes. This was argued seriously. Why sue the Attorney General over this question? Why not just admit Negroes next year? But the other question about the tuition, that was different in the sense that, it may not have been any more clear cut in the charter, but it was clear cut in the history, in that no tuition had been charged for all these years from the beginning until the present. And therefore there was no indication that it was a non-restrictive criterion.

JB: In the 1930's the board had talked about charging tuition.

KP: I don't think I was told that at the time. I may have been, I don't remember. But at least it was something the board had talked about, certainly thought about. They had had legal advise as to procedure - to sue the Attorney General on the English common law doctrine of cy pres. That the primary intention of Mr. Rice was for a distinguished learning institution and that these were secondary conditions that seemed appropriate at the time and that you should, if need be, change the peripheral aspects in order to meet the donor's primary objective. And of course, the irony in part was, that Rice wanted to obtain financial support from nationally oriented corporations, nationally oriented foundations, who took the position that if, a private university, didn't avail themselves of a normal source of private university income, namely tuition, why should they divert funds, that could be used elsewhere, to help Rice. I think this is the essence of the argument at the time, and I think that George Brown had already had some discussions with the Ford Foundation and had gotten that response from them. He probably had some discussion with what was then Standard of New Jersey, rather than Exxon, and Texaco. They would say, "Well now, if it's a petroleum engineering related project, we will be glad to support you, but in terms of a new Humanities building, we are not much interested." That's maybe an over statement but I think that George Brown was aware of that attitude. I didn't have to instruct him. But I had to encourage him to explain it to others.

JB: Who on the board was the one who persuaded the board that the Attorney General had to be sued?

KP: Well, this was a matter of legal advice. They, after all, had contact with The Baker and Botts Law Firm. Malcolm Lovett was sitting there, but I don't think Malcolm was the primary expert on this point of view. The record will show who the key people were. I am terrible on names.

JB: I just wondered if Mr. Lovett was the one who, in some sense, felt that the more conservative approach should be taken, and that in some sense Baker and Botts backed up his legal opinion.

KP: I think there is some truth in what you just said. Malcolm Lovett tended to be conservative in this sort of matter and wanted to be sure that he wasn't associated with anything that anybody could criticize as possibly not in complete compliance with the law.

JB: Several times before that, when the board first contemplated in the 1930's investing in stocks, they wanted to get a legal agreement from the Attorney General that that was OK to do. And before they were willing to buy the Rincon Oil Field, they actually wanted to get a legal agreement that that was a proper use of some of Rice's funds. So there seemed to be a sort of a tradition of legal caution and Mr. Lovett seemed to represent that viewpoint.

KP: I think you are right. Although I would not have said all of that on my initiative, of course, I wouldn't have known about it. At least, if I was told about this earlier history, I don't remember it. I just leave it as a confirmation of your view there. But in the discussion around the board table, it's clear that there was a minority that felt that we might just as well go ahead on the racial side, but it was a minority and it was not an argumentative minority. In fact, I was on that side. But I didn't press it. I said, "We want to get it done. We want to get it done and keep everybody as happy as we can."

JB: I would like to go back to before, when we were first talking about when Mr. Rayzor and Mr. Brown came to you and asked you about Rice and we quickly went on and then talked about matters of race. I would like to know, what was it about Rice that piqued your interest? Did you understand that here was a, was it your viewthat here was a small regional university that had a potential to become a national university, that there was tremendous promise here? I mean, what was it that attracted you? I know you expected to have time to do research here. It was small enough to allow you to do research, but it had to be something more than just research.

KP: Yes, you are correct. I was fairly familiar with Rice. You see, Robert Curl was a Rice student and I had had another Rice undergraduate, a woman, Judith Brown. She is on the Wellesley faculty right now. I had know Professor Houston and so I had followed him in his career some. I had these two students who were research students and I knew how well Rice students did at Berkeley. The University of California kept track of what we called the grade point differential between transfer students as to how well they did as compared to their grade averages elsewhere, and Rice students always jumped out. They were always graded harder at Rice than the later. It's true also that they always jumped up from Rice to Stanford. Stanford is an easy grading school compared to Rice. But more seriously, I knew Rice had a remarkably able undergraduate student body. I knew that it had some very high quality research. I knew about Bonner's work, for example, in physics. I knew Houston, I knew the character of an institution that he would be associated with or would mold. And I knew that it was very well off in terms of its endowment. I knew all these things. If I didn't know them ahead of time, I could find them out in half an hour, once I was really interested.

Certainly while I undoubtedly confirmed all this with Rayzor and probably didn't spend much time on itwith George Brown. I made it clear that that aspect of things I was already pretty well informed about. But there were things to be done. Although Rice had some very distinguished individual programs, on the national scene, it was a relatively small, minor element, as compared to some other fairly small institutions, such as Princeton, which was of course the model that Mr. Lovett brought with him to Houston, and of course, my undergraduate alma mater, CalTech. CalTech is even smaller. Princeton is bigger but not all that much bigger than Rice and yet they were, more or less, on the first line on the national scene, at least in the disciplines that I knew best. So it seemed to me that here was opportunity The Houston community was prosperous enough and there is enough private wealth in Texas as well as interest in Texas really that substantial developments were potentially possible here.

LJM: In determining whether you might accept the appointment here, did you have a hierarchy of items that you wanted to accomplish? Did you formulate a plan or a series of things that you wanted to do, if you accepted the position? Did that work out before you even arrived?

KP: In any real formal sense, I am sure the answer is no. In an informal mental sense that you think over in the middle of the night when you don't fall asleep right away or something like that, certainly yes. But the two things that we have talked about already, in other words, the racial restriction question and the full ability to commend financial support from all types of legitimate sources, as well as augmenting the income in a direct source in terms of asking students who have the ability to pay to make some reasonable tuition payment. Another thing that I certainly never found out about until I got fairly closely connected. This was that there was no faculty tenure system here. That didn't seem to me to be the right way to do things. But it seemed to me to be the sort of thing that one could deal with. I didn't quite understand why President Houston hadn't dealt with it, but we could worry about that one if and when. Since I have mentioned it, I might say a few more words about it. It seemed to me that universities had learned through the years that with young faculty candidates you ought to have a clean cut, definite decision making process as to whether they are career faculty members or they are, shall we say, apprentice faculty members, that may have good qualities but don't fit into the career pattern and needs of the institution. I did not favor the extreme pattern wherein the assistant professor didn't even have a special presumption of being considered for the associate professorship. We didn't do that at Berkeley. The assistant professor there or at Caltech, if he was doing very well, was not placed in competition with others. If he or she was of highly superior merit one went ahead and made the promotion, or maybe an accelerated promotion.

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KP: I was well aware of the AAUP rules in this regard. I didn't regard them the holy writ, but they were indications of a practice that was acceptable in the academic profession and that had experience behind it. I never quite understood why Rice hadn't faced this before, but I decided that we should. I found most of the faculty agreeable to this. They felt that it was a good idea, so I approached it to the Board, not on the basis that the career faculty needed more assurance. Rice had not inappropriately discharged anybody in its history so far as I could find out. The point was, they would put off decisions on young people until they had, in effect, kept someone of less than top quality too long and it was no longer fair to them, to tell them it was time to go elsewhere. Some of the Board had some trouble digesting this idea. They thought that the Board should not make this commitment of tenure. But I didn't have any trouble getting a unanimous vote eventually. I got support from Newton Rayzor in understanding the reasons locally and talking to other people on the campus and from George Brown who knew the national scene. When he talked to anybody from any other major university, he found out that I was talking sense, so he supported it. As I say, there was no real difficulty, but it had to be explained carefully.

JB: Let me ask you about a note in the tenure report. There is a little sentence in there about Rice's ambition should be a national university, not set by regional standards. And in your final presidential report, again I think you concluded with a sentence to the affect that Rice's should measure itself in terms of national, not regional ambitions. And in your letter of resignation to Malcolm Lovett, you listed a series of things - the state hadn't come through with the benefits and so forth, and you said that the plans had not been going along as rapidly as possible for a graduate school of administration. Does this suggest that there were people who still had merely regional ambitions for Rice? When you came here, did you see that as one of your roles, that you in some sense had to raise the goals of Rice? Did you have to raise Rice's ambition?

KP: I had to raise Rice's ambition in terms of some people. I am sure there were plenty of faculty that wanted that. They were certainly recognized on the national scene and so on. My impression was that George Brown was certainly in favor of that and he knew what it meant. Newton Rayzor may not have been as well informed about it, but he was in favor of it as it was explained to him, so there is no question about there. Other members of the board it was not quite so clear, but there were certainly elements elsewhere on the board, elsewhere in the alumni community and so on that thought that Rice was an awfully nice institution and had been attractive to them but in terms of more or less regional criteria rather than national criteria. I thought I made it clear at the beginning, if that was really the criterion that the majority had in mind, well they had better look for somebody else. And in the course of leaving, I guess I should just out and say it, that after George Brown's retirement, it was less clear that that vision or leadership was still as strong as I hoped. But, if I could reinforce it with some memoranda, why not?

JB: It was clear too that, before you, Rice had been content primarily to being an undergraduate teaching college. And that if you look back at your eight years, in terms of increasing graduate students, increasing graduate orders, increasing graduate programs, that one of your goals was to, in some sense, lead Rice to become a center for research and scholarship. Did you find that there was a good bit of support in recognition of that on the part of the board or was that again where you were having to educate the board?

KP: It depends on who you are talking about. That there were some members of the board that wanted that and only needed guidance as to what it really meant. There were others that thought it was sort of nice to have a generously funded regional institution that didn't have to go out and ask for any additional money unless it was for some particular project that some wealthy friend or donor would just love to finance. There had been some of that in the past years.

JB: This point puzzles me. I mean, it seems to me from what I have read that what you have just said very accurately and nicely summarizes a viewpoint that existed. But it seems odd, here in this place. When Mr. Lovett planned Rice and when Mr. Lovett eagerly laid out his vision of Rice, he made it absolutely clear that his ambition was to make Rice a university of a first class.

KP: That's right.

JB: And it is puzzling to me that people had such reverence for Mr. Lovett and yet somehow missed the essential point.

KP: You stated it beautifully.

JB: Even people who often had known Mr. Lovett extremely well.

KP: Such as, one may be very candid, such as his son.

JB: Yes, it puzzles me. Do you have any sense how there could be that gap between reverence for Mr. Lovett and his famous speech "The Meaning New Institution" and that willingness to be content with essentially a Texas, small liberal arts college?

KP: You put it in extremely bold terms, but you certainly put your finger on the situation. There was that element of it and as you say, Mr. Lovett initially in the beginning and in the early years, was clearly following the national and international distinction course. I assumed that and hoped and sought to regain it. Well, we made quite a little progress in that period. Of course, I knew the depression very well. I was a student right through it and saw all sorts of people that had all sorts of financial difficulties in the universities. CalTech had severe financial difficulties when I was an undergraduate. So it is entirely understandable that with pressures of the depression, even though Rice was relatively very well funded, that they were inclined to draw back on their ambitions and if that continued, not only for the decade of the 30's but on into the decades to some extent of the 40's and 50's, although during Houston's period there certainly was a move in the more ambitious direction. It is clear that what you stated was a real phenomenon. You loved these wonderful words but you sort of went off in a different direction.

LJM: I wonder if we might, if you could for us, identify those members of the board who were supportive of a broader, national scope for Rice?

KP: Well, I think I have done it to a considerable degree. I am a little reluctant to get too specific because my memory is less than perfect and these points of view are less than fairly expressed at times. But by far George Brown was a person who knew the world, not just the university world, but the political world, the financial world, the engineering world, nationally and internationally. And if he wanted to check on what was a national standard in something, he knew somebody to call up in New York or somebody like that and have a conversation or with someone in Washington and so on. He was the great strength in that direction. Now there were various intermediate people, Newton Rayzor was completely supportive, but didn't have necessarily the arguments or the stature to convince somebody else. Now, I don't want to be unduly critical of Malcolm Lovett, but he was one that was awfully comfortable with the intermediate, more modest pattern.

JB: Hugh Liedke, would he have been in agreement with George Brown?

KP: Who?

JB: Hugh Liedke.

KP: Hugh. Oh, Liedke. Yes, that's right, you mentioned his name. He would have been the George Brown type,and I had respect for him, but he came on the board relatively late, and was not really too strongly interacting with other board members, at least I was not aware that he was. I never had the feeling that he was devoting himself to Rice to anywhere near, even remotely to the degree that George Brown was. And there were others too. Mrs. Hobby was on the board and she of course, knew the world. She was pretty committed to Rice. She was less fully involved with things than Mr. Brown, but more so, I would say than Hugh Liedke was. Herbert Allen is an interesting case. He was really very supportive, and he, to a lesser extent did know the national scene. He had his own business, active in Europe in a significant way, in England and Scotland. Not to the same extent as George Brown but very much in the same pattern. We can mention a few more maybe.

LJM: It is interesting to see the interplay between the board members. That's why I asked the question.

KP: John Ivy I didn't know all so well. He seemed to be most interested in managing the oil investments at Rice and less interested in it otherwise. William Kirkland was really quite supportive of a national point of view. He was a Yale graduate, wasn't he, I think? Maybe it was Princeton. It was one of the Ivy League. He was very supportive of a national point of view. Wortham intermediately so, I would say. Gardiner Symonds came on before Liedke but relatively late. He was a Stanford man. He knew the national scene very well. Among the others, Charles Duncan was very supportive as long as he was here. But then they sold their coffee business to Coca Cola, and he went off to Europe to manage Coca Cola, so he wasn't here too much of the time. I'll try to say words about a few others.

JB: I am very interested in George Brown, though in a few minutes I want to ask you about the ten year plan and the capital fund campaign. It seems to me that's where you really pushed Rice into recovering that sense of the original Mr. Lovett's vision. But you know, I am a southern historian, and if you were a historian and you knew nothing about Rice, what you would read about George Brown in his relationship to Lyndon B. Johnson, for example, if you read the Caro biographies, George Brown comes across as crass, vulgar, and manipulative, bribing all the people to get his government contracts. If you read the LBJ literature, George Brown does not come off at all as a person of sophistication or vision. He seems like just a Texas wheeler and dealer. Now, everything that you read about Rice, on the other hand, suggests that in contrast to that George Brown, there is another George Brown who is a man of vision, who is a man of some sophistication, he is a man who understands. Could you elaborate on the Rice version of George Brown?

KP: You stated it very beautifully. George Brown did have this other aspect. If the purpose to be accomplished was best to be accomplished by wheeler, dealer methods, he was very skilful at it, perfectly ready to do it. I mentioned right at the beginning the matter of NASA and the Spacecraft Center and that was partly wheeler, dealer. I think that the fact that I could contribute constructively to this, knowing Albert Thomas and knowing the Washington scene somewhat, at least let us get off to a very warm personal relationship and established it quicker and more completely. Of course my methods would be quite different from h,s but I didn't think that he was doing anything that was dishonorable in the framework of that sort of negotiation. But I would let him do that side of it. Now if you were coming from some other place that was trying to compete for the manned Spacecraft Center, you would not necessarily be happy with the maneuvering that went on there. There is no question that what George and as long as he lived, his brother Herman had remarkably good connections with political people, with business people in various places. He manipulated real estate, things like that. I didn't hear too much about that. And of course he was getting contracts for Brown and Root by various means that presumably followed the custom of the world that he was operating in. I don't think he, as I observed him, maybe I was unduly tolerant of it, but it seemed to me, he was just playing the various games according to the rules of those games, and when he was in the higher education game, well, he was perfectly willing to seek the best for Rice in accordance with the way that I would guide as the appropriate way to go about it. He didn't try to bribe the Ford Foundation into giving us a grant. We applied for it and he may have talked to a trustee, you know, to have gotten a favorable pushy, but there is no reason why others wouldn't do the same thing, so why not?

JB: Well, I take it too, he had quite a broad vision about Rice. I mean, under his leadership, Rice became much more of a university. You point out in several of the documents how the humanities and social sciences increased very significantly. You called for improving the fine art environment on the campus and so forth. So he clearly supported those humanistic kinds of things as well as engineering and sciences.

KP: Give Alice (Brown) credit for that. She was a wonderful woman. We thought the world of her. Jean and I thought the world of both of them. And Alice had a real role in that side. They were in the Houston fine art community to a considerable degree, and if he didn't spend much time in it, she did. As Miss Ima Hogg and various other people did.

JB: Maybe this would be the time to move into that ten year plan. I understand that the ten year plan was an outgrowth of the normal re-accreditation procedure.

KP: No.

JB: It wasn't that?

KP: No, these were separate affairs. The ten year plan was my effort to put before the board and after the board, the Rice community and the general community what would represent the first ten years of realistic and feasible movement toward a national stature and a research university, a graduate as well as undergraduate university status for Rice. So far as I was concerned, the accreditation was something that Rice was obviously entitled to and you satisfied the accreditation association gracefully and fully and insofar as you could generate some useful discussion as to improvement in instruction or management or anything like that, well use it. In other words, go through it sincerely, but it was to me that success was a foregone conclusion and if you could use it for constructive purposes and be a good citizen in the accreditation world, you did, but it was not a mechanism in the sense of the development at Rice in this more ambitious character. I would communicate piece by piece things that I thought Rice ought to do in the terms of more ambitious programs. And then I thought that after a few years it was sensible to put together a package of this sort. But the thing the ten year plan it did interlock with was the capital fund campaign. In other words, the fund campaign was part of the essence of making the ten year plan development feasible. And you see, this was a matter, not just of money that you got in the fund campaign. It was the matter of conveying to the outside world that Rice wasn't so completely, fully endowed but that it deserved consideration for further gifts and a self-consistent pattern included the tuition aspect, and included the plans of what you were going to do with all this.

JB: In the preparation of this ten year plan, according to the introduction, and you mention people who helped in the writing, Chancellor Carey Croneis, Professor Franz Brotzen, and so forth. What was your role? Did you lay out the kind of thing that you had in mind, or did you have a very heavy hand, a creative hand, at the right places in development of this plan?

KP: Well, I had a fair heavy hand, but I made clear with what I thought we ought to be doing and I asked them to, in the first place, examine the package, think about it and then fill in some additional details and put it in a more complete form. I had a pretty strong hand in it.

LJM: In preparing the ten year plan, and the committee that worked with you on it, how deep did the enquiry go into the faculty or into the department heads in order to come up with the specifics that appear in this plan?

KP: The simple answer to that is, I don't remember very well. I am sure it was discussed with a number of members of the faculty, particularly senior members, department chairman, or people that had national standing within their fields. Some of the people are acknowledged or in the committee structure, but probably a good deal was done individually or informally. But in terms of greater detail, I just have to admit, I just don't remember.

LJM: The reason why I bring that up is that you emphasized better representation for the faculty and opening lines of communication that apparently, you felt, had not really existed sufficiently before.

KP: Well, I think my style was somewhat different in being more open and welcoming feedback from faculty on various matters rather than a more elaborate committee structure, but I think I was more formal, for example, in questions of faculty promotions, and particularly promotions into tenure status. I think I went through more formal review procedures than had been the practice before, but I am hesitant to say how much of a change it was because I don't remember. I do remember that we had an elected faculty committee, that went over the final departmental and dean's recommendations. I actually presided, as I recall, but didn't vote. But I made it clear that I reserved the presidential decision. But I didn't want to be too heavy handed in the discussion because I wanted their views, not to just get them to rubber stamp my views. But without going back into the records as to whether it had been the practice before, I am not sure how much of a change this was.

JB: When this was being prepared, obviously the board read itand discussed it and so forth. Did you find that it was an easy sell to the board or was this, did Mr. Brown just simply pick this up and ....

KP: No, no. As I said, this was somewhat simultaneous with the fund drive. The fund drive sequence involved one decision to employ an outside advisory or consultant group to sample opinion of alumni and other potential donors and report back to the trustees whether such a drive is feasible, and if so, is it feasible for thirty million or fifty million or only fifteen or what have you. Multiply everything by ten for the present day situation. Although I had never directly been through this thing from the same position, I knew that this was the normal way to do it. I don't think that I selected the group that did it. I think again, George Brown had contacts. He called somebody at somewhere else and said, "You had a big fund drive recently, how did you get started on it? Who did you hire?" Then, to come back more precisely to the ten year plan, the thing we had a little exchange of communication about, what I originally presented to the board was that addition that you said you found that has tables and numbers instead of those graphs. The board essentially accepted it in principal but said for public purposes, they wanted less detail. Probably they were later frank enough to say they wanted less detail that somebody could point out to them as being a discrepancy.

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JB: Dr. Pitzer, I would like to go back and get very clear in my mind the sequence here. Because I wondered if, for example, the ten year plan came first and then that was the engine that drove the decision to have a fund drive, or if it was understood that Rice, for propriety reasons, needed a fund drive and in some sense this was a case statement of that need. Or was the work absolutely sort of simultaneously?

KP: My memory is that they were more or less simultaneous and I'm afraid I can't sharpen that fuller other than to say that I think it would be wrong to say it was this and then that, as to compared to it, a reasonable degree of simultaneity Now, with respect to the actual late stages on this, I did look this up. It was a July 29th meeting." I presented the near final draft which had been distributed earlier. General approval was given but some revisions were requested by the board. Also requested was the division in the two versions, one omitting detailed charts, tables and schedules."

JB: This is 1964?

KP: 1964, July 29th. August 19th, special meeting." Revised ten year plan in two editions presented and approved subject to some further revisions." And these were quite minor. September 30th, 1964. "Final editions approved and procedures for public release of the simplified version were formulated." That's the simplified version, and you have ....

JB: The longer version.

KP: Yes, the longer version, and you had the press release that went. And there was a press conference and so on. That press conference, as I recall, involved the fund drive, didn't it?

JB: Yes.

KP: I think so.

JB: Yes. In part because some of the numbers that were listed in here for the achievement of certain kind of buildings and so forth, exactly matched the 33 million dollar ...

KP: Well, sure. They knew it would be involved together.

JB: Now, of course Rice has a history of controversy over capital campaigns, including a recent controversy. The capital campaign that you were involved in, the 33 million dollar campaign, was Rice's first really significant entry into modern fund raising.

KP: That's right.

JB: The development office was really developed and it was the first time Rice more or less publicly told the world it needed money. Was that a very difficult decision for the board to make? Or was it kind of an obvious thing, "We truly have got to do this to achieve our ambitions?"

KP: This was intermediate in between the two extremes that you stated. And it would vary between different members of the board. If Rice had the more ambitious objective, it clearly ought to seek funds from all normal legitimate sources for a graduate level university. It's special history would mean that the relative amounts of money from different sources would probably be quite different. But to forego completely any of the major sources of funds that Stanford or Chicago or Princeton or some other university seeks, would be just being less ambitious, less well funded then you had a potential to be. Furthermore, unless you create a climate in which the public generally, at least the well informed public, generally recognizes that you have this ambition and that you are seeking funds in this pattern, they are inclined to give money to somebody else. They think "you don't need it"

JB: Was there an explicit relationship or pattern between the decision to charge tuition and the decision to have a fund ...

KP: When you say explicit that's ...

JB: The connection was clear though?

KP: The connection with the underlying rational was, I'm sure, clear to the principal most active members of the board and I don't mean just George Brown and Newton Rayzor. I mean Kirkland and Lovett and the rest of them. This was clear but it wasn't a step by step relationship. It was just a generally discussed relationship.

JB: Thinking back to that whole situation, you know I was an undergraduate here then, and I remember the kind of excitement about Rice's court challenge to revise the charter, to be able to charge tuition, the ten year plan being announced, the decision to have a fund campaign. It was clear things were happening. Those were things that were not without controversy.

KP: Yes.

JB: Thinking back to the challenge to Rice's decision to be desegregated, what could you say about the kind of opposition in the community, in the alumni community, in the larger community, to those efforts to move Rice forward?

KP: Well, the opposition of Mr. Coffee and whoever the other man was, whose name I forget, was one thing. This was a definite thing that was going to require a court trial and it had to go through the steps there. The question of opposition more generally, was something that had to be considered in terms of the general relations of the university to its community, but since it was essentially a move from a very unusual and abnormal situation to what any person in the higher education world, would recognize as the normal relationship. I didn't worry very much about it. My feeling was that as long as we were reasonably discreet, reasonably courteous and skillful about it, that that would go all right. And it did. I probably mentioned this, maybe I already put it on the tape: after the Appellate Court decision was rendered, I read it, I thought that this was a beautiful statement of the whole case and had it reproduced in a number of copies for all Rice alumni and friends and anybody that the development office had on the mailing list. And the Alumni Association signed a cover letter and sent it to the alumni and we distributed otherwise. Well, here were three totally independent, very well respected people looking at the Rice situation saying that it was legal and that it was the reasonable thing to do. I didn't detect any serious opposition. There were probably letters in some file I left here that some alumnus wrote or some other citizen wrote and said, "I won't even buy a ticket to a football game now because of this." But it was such a minor element that it just didn't seem to be worth spending much time about.

JB: I had a student a few years ago who did a master's thesis on the decision to desegregate and charge tuition. And according to her research, out of 14,000 living alumni, six alumni wrote letters attacking the decision. Six out of 14,000.

KP: The only sensitive thing of that you haven't raised is that the mathematics department wanted to jump the gun a little bit, and we had to do some pretty sensitive maneuvering there. But it all came out all right, so I don't see any point in burdening the departmental record.

JB: If we could back just a minute, I know we began talking earlier about developing the strategy of the decision to change the charter. I believe a man named Tom Eubanks was very involved and as a lawyer at Baker & Botts developed that strategy. How much were you involved. Very much in the strategy or did you simply leave it to Baker & Botts to figure out a way to ...

KP: Let's put it this way, I got to know Tom Eubanks very well, but more than that. I got to know him so well I used him for some personal legal work too, even after I left Rice. He was a great friend of mine. For a while I lost contact with him, but I noticed a letter so I stuck it in the file here. And I have asked about him and I have been told that he retired somewhat early from Baker & Botts and I guess he is still practicing. I have been so busy this visit, I haven't tried to get in touch with him. But it wasn't just Tom Eubanks and I am sorry I can't think of the names of the other's there was one very distinguished ...

JB: Tom Davis, is that who it was?

K{: Tom Davis was the active person, but there was another even more senior person, but Tom Davis is the one that I talked to quite a little and he is the one on the day to day basis handled it with Eubanks as an assistant. But the final presentation to the jury was given by a man who had been in Washington as maybe what would be know as the National Security Adviser to the President or something like that, very high level person on the national scene. It's ridiculous, I can't think of his name. The final argument to the jury, the final presentation to the jury, Tom Davis, as I recall, gave the initial part and then the opposition had their ...

JB: Coffee and Billups. Billups was the other one.

KP: Billups was the other name, yes, Coffee and Billups. Their lawyer gave the middle presentation and then this man, whose name I can't think of, gave the final sort of peroration on the Rice side and it was beautifully done.

JB: I know part of Rice's decision about the timing of this case had to do with the change of Texas Attorney General.

KP: That certainly had to be considered. You had to deal with a real human being. And, I don't remember the details, but I do remember that that was.

JB: It was assumed that the new Attorney General would be more willing to make the kind of decision that Rice wanted than the one retiring?

KP: You are probably right but I don't remember for sure. It could have been just that the retiring Attorney General might well just say, "Look, I'm almost out of office, I don't want to stick my neck out on something like this."

JB: Somewhere in this thesis there is a listing of all the lawyers who were involved, Dillon Anderson?

KP: That's right.

JB: Tom Morton Davis, O. Don Shapiton???

KP: I don't remember him.

JB: Melvin Ellesley???

KP: That was the ...

JB: Dillon Anderson.

KP: Dillon Anderson was the one who gave the final presentation. He didn't spend very much time on the case, but he was clearly the person who had been more experienced on the national scene, had greater prestige. And as I say, his was a remarkable presentation, not just the words he said but the way he said it, the vigor and obvious deep commitment to it.

JB: I take it that neither the Rice administration nor the Baker & Botts' lawyers had any idea that Coffee and Billups were going to file a counter suit? You expected this to fly through, essentially unchallenged, and was then taken aback by their ...

KP: No, I wouldn't go quite that far. We certainly hoped it would fly through. But I'm quite sure either Tom Davis or Malcolm Lovett actually said, "Until whatever the time required has elapsed, we don't know. Somebody may oppose it." Probably more could be said about what you just raised, but I don't remember it. I can't say it now.

JB: But I take it that people were quite frustrated by that counter suit. This was sort of an unanticipated fly in the ointment?

KP: We were certainly disappointed to have it. It delayed the whole operation. Actually we were disappointed again when they decided to appeal it in spite of the unanimous verdict. Our lawyers weren't worried about losing the appeal, it was just a nuisance and delay of it, although you never can tell about a thing like that. There are three human beings who are going to vote, but they had every confidence about winning the appeal. The problem of getting a unanimous verdict in the beginning was something that concerned them, where they were much less certain about it.

LJM: Did the appeal have an adverse affect on fund raising, grants.

KP: Well, you see, until we got this passed, we didn't really go out very generally to any community, other than a community that Rice had been dealing with all along. I probably have overstated that a bit. We were talking to the Ford Foundation and some other groups from time to time about a proposal of a more general character than they would have considered without Rice making these general changes. But they were willing to discuss and if these changes were committed to and well on the way to being executed, but it was also a lot easier to discuss with them after, the primary decision was made, and still easier after the appellate decision was made because that meant it had been really tested to the ultimate and stood up. And then, well, I think that is enough on that.

JB: In 1960, if you look at the distribution of the faculty and the distribution of the graduate students and so forth, Rice was overwhelmingly a technical university.

KP: Yes. In fact, the outside world in science and technology often thought it was the Rice Institute of Technology, which it never was. And I am sure this had a good deal to do with the decision to change the name to university and that, of course, occurred before I came. But in the period I was here, a lot of people elsewhere were still thinking of it as an Institute of Technology.

JB: To some degree there still is that ...

KP: And to some degree it still is.

JB: But anyway, by the time you left in 1968, I would think that if a person just looked back at your seven years, I guess that the most remarkable thing might have been the development in additions to the graduate program, but the really remarkable development, in terms of faculty and programs, was in the humanities and social sciences.

KP: We brought in some awfully good, young people in that period, and a few senior people too.

JB: You had been at a great university, Berkeley, that had quality straight across the disciplines? What role did you play in nudging Rice to become a full fledged university? Don't be overly modest here.

KP: I had the ambition for Rice to be superior on the national rather than a regional scale. It ought to be a reasonably general university. Now, I had the view, for example, Rice doesn't need to have a medical school. There is a medical school that has the Baylor name. It's right across the street. Medical schools are expensive. They are complicated and they are burdensome. Rice has no ambition to have a medical school. Princeton doesn't have a medical school. Berkeley doesn't have a medical school. We don't need a medical school. On the other hand, a university must have at least the central humanities departments and the central social science departments. Also economics, for example, is getting more and more mathematical and quantitative and to the lesser extent, sociology and some other fields are becoming more quantitative, so that the synergism that's potential there if you develop these fields in a fashion that takes advantage of it. But I also just assumed that they would never have changed the name to "university" if they hadn't wanted to be a university. So I assumed that this wasn't really an issue with members of the Board and people like that. Occasionally an executive of an elective utility claimed they couldn't hire anybody as engineers and I would have to point out to them that the reason they couldn't hire a Rice engineer was that they were trying to pay salaries below the national standards so people could go into the electronics world instead. If IBM was offering more money than the local power company, well, what did one expect the Rice graduate to do?

JB: You clearly argued against having a medical school. I guess there was preliminary discussion of a law school, but that was scotched. But you did make a case on behalf of the development of a graduate program in administration?

KP: Yes.

JB: And you indicate in your letter to Malcolm Lovett when you resigned that the inability to move forward on that was one of the frustrating things.

KP: Yes.

JB: Now, ultimately, that program was established. What was the hesitation?

KP: I guess it was that I had not adequately cultivated the Jesse Jones related interest. Maybe I had a higherfinancial entry level that I wanted to see before committing to it. I don't know.

JB: Higher entry level? You wanted to have more funds up front before you started?

KP: More funds, not necessarily in cash but in commitment. In other words, I didn't want this to be in any sense a drag on the rest of the university. A management school ought to be able, in the Harvard terminology, to stand on its own bottom without having to be subsidized elsewhere. And I didn't see any point to having a management school unless there was a reasonable prospect of its being among the top 20-30 nationally, and maybe even better in the long run. But of course it takes time to get there. In fact, there is little more that can be said about that. I had brought a young man by the name of Ferdinand Levy to Rice. I had gotten on to him from a variety of ways. He was too young to be a potential dean of a management school then but he could fit in to the economics department. At least he could keep me informed as to what was what in this world. I was able to get in touch with him. He actually was at Stanford at the time. I had no, of course, special connection at that time with Stanford, but he had worked for Rand and I was trustee of the Rand Corporation so I could get a line on him there. He seemed like he would be valuable and Edwards, who was chairman of economics at the time, agreed. So I brought him in, You probably remember the college bowl. Levy managed the Rice team that won the national record there.

JB: Set all time records in their scores.

KP: "All time record" ... (laughter) and so on. He was a Tulane graduate and we kept in quite close touch and he really wanted to go in management school direction and went to Georgia Tech, where he was dean of their management school after a certain number of years. He is now in Hong Kong on a several year appointment setting up a management school in Hong Kong. We had made some starts in the direction of a management school that would really be visible on the national horizon on a very early date. I thought we needed more money than I was able to get. Now, Norman Hackerman may have gotten the amount of money that I was thinking about or maybe not. I know him well, but I have never asked him that question. Of course, I didn't have a definite number. I would just make calculations and judgments about it. Now, this must have meant that I was somewhat disappointed in the backing that then Board, minus George Brown by this time, had given. I don't mean that George without interest in this thing, but after all he had worked so hard for all these years. And if this didn't particularly appeal to him, I didn't want to push him on it. Look, I am reconstructing a lot of this. I may be getting some of it wrong, but I think I have given you a reasonable story there.

JB: How would you characterize your relationship to students and with the Thresher. We were earlier laughing about the Thresher strike. Can you say something about those two topics?

KP: Well, I was well aware that the students' newspaper could be rather prickly to deal with at times. And on the other hand, as long as you maintained a good natured relationship, it usually worked out pretty well. We had somewhat tense times and of course, the one that was most tense was after the dean of students, S. W. Higginbotham, found some technical reason for displacing the Thresher editor. Then I had the problem of keeping things within bounds and making it clear that we were not censuring the Thresher - at least I wasn't censuring the Thresher. On the other hand, I wasn't prepared to cancel the appointment of the dean of students over a thing like this even though I thought he had been foolish. He had a point and he probably needed to do something but he could have thought of a better way of doing it. But it all worked out. It was not a major problem. We had more fun with the beer bike race then ...

JB: Which is this weekend.

KP: I heard that.

JB: It's now out on the other side of the stadium.

KP: I was in the colleges for meals quite a lot and we had good relations with various students. I recall one in particular. I think he was a student president, if not one of the higher student officials. He had come from New Mexico. I got pretty well acquainted with him. And I learned about the relationships between the old Spanish descendent cattle ranchers in northwestern New Mexico, but not in the Navajo Indian Reservation territory. This is strange. You can throw this away. I just tucked that in the back of my mind and years later, a very good friend who was a colleague on the board of directors of Owens, Illinois corporation, also had some other business. This fellow member had a cattle ranch in the Dakotas, and he wanted to give that for a State Park and a nature conservation and set up another cattle ranch. He was going to look in New Mexico. So I warned him about this. Next time, a month later or two months later we had a board meeting and he Bob Levis is his name said, "Thank you for that warning about New Mexico. I had a local inquiry. I didn't want to get tangled up with that. Maybe an outsider it might work out but I'll look somewhere else." So I had some pretty good relations with students.

LJM: I need to turn the tape over.

Continued interview Tape 2 Side 4

LJM: I would like just to ask you some background on the establishment of the Space, Science and Technology enterprise with NASA and the involvement of representative Albert Thomas. Can you provide some background on how that developed?

KP: Well, NASA...

LJM: There was a sizeable amount of money involved.

KP: Yes. This was a major initiative of President Kennedy, to put a man on the moon. And we wanted to be part of this and if we could be a part of it in one way, we wanted to take advantage of it in other respects. I have already talked about the maneuvering to get the manned Spacecraft Center here, that George Brown was right in the middle of, and that I happened to be able to have a bigger play because I knew Albert Thomas ahead of time. I knew him quite well. As a part of that whole picture, it seemed as if Rice ought to be able to get a special activity in the science related to space. This would be appropriate to Rice University with strong science and engineering, but we didn't want the Manned Space Craftcenter here. We didn't want to be unduly distracted by this. We wanted something that was appropriate. I have forgotten the detailed mechanism by which we got the money for the building. I did decide to set up a department under the name Space Science, which I thought would have some special appeal because there weren't any other space science departments. This was the first one that came into existence and I located Alex Dessler as the first Chairman. He was primarily involved in recruiting Curt Michel and a few others who were the early members of the department and the program that involved some NASA launched, unmanned projects. Also they developed contacts with the manned space people. And I remember astronaut Glenn, who was later senator of Ohio. He came over and talked to student groups every once in a while. He was a very inspiring speaker and great to be in contact with. Some of the others were less so, but also very positive. Who was the first director of the manned Space Craft Center? I got to know him quite well. I used to take him sailing. He was a boating person too. I can't think of his name now. But we developed a multitude of contacts there and among other things we got the building. And it was perfectly reasonable as time went on that they thought that Space Physics was a better name for it. It turns out that Smalley's lab is in the building although it doesn't have any direct relationship.

JB: We now have the NASA archive, so all the archival data and all the telemetry, everything that had to do with Project Mercuty and so forth is in the Rice library. So it has come full circle.

LJM: Your relationship with Representative Thomas would seem to be a crucial factor in Rice obtaining this.

KP: It surely helped. George Brown's relations with Lyndon Johnson undoubtedly were more important, or the total of George Brown's relations with Lyndon Johnson and other people that Lyndon Johnson would put him in contact with, but of course, he had relations with Albert Thomas. But the fact that he could say that I was here, and that Albert Thomas already knew me and had seen me in thee national position as Director of Research for the Atomic Energy Commission, at a very important time for that organization, gave me credentials with respect to Albert Thomas and for that matter also with respect to Johnson, that not many other people would have had.

LJM: How important was achieving this in your mind? Do you consider that an important achievement in your administration?

KP: It was important. If you try to put things in numerical order, it would not be number one, maybe number two, but it is in sort of a separate category off to one side. It was a special opportunity, for heaven's sake take advantage of it. Since it is of constructive it is all synergistic with respect to other things, we took full advantage as long as it was not out of proportion to the rest of the university. And we kept it, I think, in proportion.

JB: After a quarter of a century has passed, thinking back, being as philosophical as you want to be, what would you say wereyour major accomplishments at Rice? And what were your major disappointments?

KP: I think this general matter of encouraging and helping and participating in Rice moving toward a national and international standing in the university world, and hopefully for the benefit, not only of Rice, but Houston and the whole community, would be number one of general accomplishments in whichI have a great deal of satisfaction. I suppose the disappointment is just that one didn't do more of the same. But I should acknowledge another characteristic I have; it is that I am not too tolerant of repetitive routines that are demanding on and on and on. Preparing, whether it is a departmental budget or a university budget, presenting it to somebody and getting it approved, it is exciting the first time. It's interesting and very worth while for several times because it takes several times to really accomplish what you think you have an opportunity to accomplish. After you have done it six or seven times, it has a sameness about it. I had this same characteristic in research and in teaching. I don't like to teach the same course too many times. I'll come back to it after ten years, but I don't want to do it repeatedly year after year. In research I had the same characteristic. I worked in a given special sub area for a few years and even made a major discovery or made a secondary discovery, or maybe didn't discover anything. But after a few years, I'll take up something else. I may come back to it after some years later. But I just don't understand former students of mine who spend their whole career to their retirement for age in, essentially in the same field as their Ph. D. thesis. I had one student at the University of Michigan who did that. He is almost as old as I am. He was among my first students and until his retirement day he was making essentially the same sort of measurement, just on a different substance. I couldn't do that.

LJM: One of the things that came to mind, and this came about as I was researching for the interview. I went though all the old newspapers when you resigned from Rice and is it fair to say, or has it any validity at all, when you left the environment at Rice was, at least among the student and the faculty, it was a quiet environment more or less. Is it true?

KP: Yes. In fact, I think I gave a speech or two that the Stanford trustees liked.

LJM: The newspaper articles sort of imply, as they do, that coming from such a quiet university, that you were sort of taken by surprise by the radicalism that you ran into at Stanford.

KP: Well, I underestimated it. Another one of my characteristics. I am not much inclined to worry about spilled milk or whatever. I don't get depressed about that. But I realized there were going to be problems. At Stanford as I got more committed I began to realize how serious the problem might be, but it was a little embarrassing to back off. Well, it wasn't that I wasn't warned. I knew there were complications. But it was going to be different. That was true. But then after a relatively short period of time, it appeared to me that this was going to go on for a while. Somebody ought to take that position who, if not professionally trained in crisis management, was at least willing to devote a piece of his life in essentially a professional level of crisis management. And I wasn't that person.

JB: In reading those newspaper articles about Stanford, it made me think, what an almost impossible task you had. But before you go off to Stanford, I'd like to come back just a minute to your decision to leave Rice. I mean, it's clear that you made this clear in your letter to the Rice board and then newspaper stories that you were a native Californian, that you had spent practically half a century there, and Stanford, of course, was a great university, and I think that you had even said when you came to Rice that you had hoped to sort of make Rice like Stanford. So in some sense this seemed to be the very obvious next step for your career. You have also just said here that a sense of fatigue or boredom sets in after six or seven or eight years. I would like to just explore that a little bit. I mean, had you worked out this sort scheme or schematic idea that any kind of administrator has a really creative, useful term of five or eight to ten years, before in some sense fatigue sets in for most people? Was it that sense of fatigue and boredom combined with the aspirations of going to Stanford? Or to what extent were the frustrations with Rice, the cause of you leaving? I mean, you have a very eloquent and understated sentence in your letter of resignation, when you talk about this sense that there are still people in this region who have only regional aspirations for Rice. I took this as a sentence that said a great deal. I don't want to put words in your mouth, butis it fair to say that it is sort of a combination of coming home to Stanford. Stanford is a great place to go to - and you have done

Rice for seven years, boredom and so forth has begun to set in. You have made real accomplishments here. There is a sense on the part of the board that we have been energetic for a while and now it's time to kind of coast. Is that a kind of accurate summary of the kinds of things that went into your decision to leave?

KP: Yes, all of them did. I don't think it is really very useful to try to go into more detail. If something very attractive and challenging had not come along, I would have stayed on at Rice perfectly happily. I was approached actually at the same time with respect to the presidency at MIT. I didn't really want to move to Massachusetts. I rather enjoyed Texas. I like to sail and Galveston Bay is a good place to sail. I had gotten very close relations with Bill Gordon and we enjoyed the sailing and other things. There were many things that we were happy about here, but on the other hands we had many home ties of one sort or another in California, not at Stanford primarily. Many friends are in Berkeley and we have many California connections otherwise. So that when there was some talk about both MIT and Stanford, I turned off the MIT very abruptly, but I did tell the Stanford people, "I will consider it." As I say, it turned out I was grabbing a tiger by the tail But I am not inclinedto spend much time looking back on things. My wife and I have had, on the whole, a very happy existence. I accomplished a great deal actually, professionally. It's not only my honors such as the Welch Foundation Award, the National Medal of Science and things like that. But also the ones that occurred in the late 1970's and some even in the 1980's; recognition of things I did when back at Berkeley in the 1970's. So the Stanford offer was tempting. I realized there was a danger in it, but the temptation was enough. I took it. I didn't think that Rice was going to be very different for the next two or three years whether I stayed or not. The way I sensed things with the Board, they wanted to sort of carry on with more modest efforts. I could push the Management School harder if I wanted to but in terms of anything else we probably just would go along. That's all right.

JB: Is this the first time you have been back to Rice since ...

KP: No, no. It has been several years now, but I was back two or three times fairly soon after I left. And then there was the special lecture that I gave mid-1970's, I think. I remember one time I went to the Texas Philosophical Society meeting. Then there was a time that I received the Welch Foundation Award. I know that time I stayed around quite a little. And then probably the last time I was here but I didn't come to Rice much was when my former student, George Pimentel. He received the Welch Foundation Award. He was also a Berkeley faculty member, and was at one time Deputy Director of the National Science Foundation As a former awardee, I was especially invited back, and Jean and I came. But I think at that time we were under a fairly tight time schedule. I barely came on the campus to maybe see one or two people, but not very much. But I did see some people at the Welch Foundation dinner that time including Charles Duncan. He usually attended those affairs. And of course the Welch trustees, Norman Hackerman, and so on. I have seen Norman in other connections through the years, really, frequently.

JB: I don't mean to ask you in any way to comment on your successors, but I would like to know, after you have come back to the Rice campus and have been here a couple of days. This is a quarter of a century after you left and in your ten year report you call for, you projected a student population of about 4,000 and now it is about 4,100. You call for a faculty in the neighborhood of 400. Now it is in the neighborhood of 450. You call for an enhancement of Fine Arts and now we have beautiful music school building and so forth. When you walk around the campus and you see the buildings, and you see the extensions, do you have a sense of gratification? How do you feel when you walk around and see the place, that in some sense has become what you laid out?

KP: It is a sense of satisfaction. My acquaintanceship and relationship with the three presidents in between have been quite different. Of course, I had known Hackerman before, professionally. He was president of the campus in Austin when I was here so I dealt with him in one role or the other. I dealt with him in all sorts of different relationships, very constructively, most of them non-Rice. In other words, not that we never talked about Rice, but that we had so many other things to talk about. I had a nice early visit with George Rupp, very friendly. He was out in California.Then on one of my visits here I remember I saw him here in his office here, maybe more than once but not many times. I must say that my early acquaintance with Malcolm. Gillis has been very cordial. He is a very much easier person to get acquainted with than George Rupp. We met on one of his initial visits in California and last night at dinner in a the discussion this afternoon, I would say we hit it off very well. If Rice in 1994 is not too different than what I was projecting for 1975, it's clear that he is pushing it in the direction that I thought it ought to go and at least now is on the level that I was contemplating then. And I say, more power to him, and if I can help in any way, I'll be glad to.

JB: Seeing the place 25-30 years later, do you have a sense of optimism? Do you think that Rice's potential is as promising as it once was, or are we in some sense faltering?

KP: Well, in one sense the size and level of activity for a leading position on a national and international scene, is now, higher than I contemplated in the ten year plan. But it was only a ten year plan. And there is lots of territory in that direction for Rice to progress into and I would think it has a very good probability of moving that way. But I am very pleased to see that it has gone as far as it has. In terms of immediate questions, one concerns Professor Smalley. He is here. He is staying here. He used his leverage nationally to obtain greater support. He told me in Berkeley, when he was discussing these things, "Rice has treated me very well, but I want to be a part of a first line chemistry and physics program. I am going to use my leverage, not so much to get something for me as to get something to be sure that physics and chemistry at Rice keep up with the world and expand and have better physical quarters." He played that game and won and I say, more power to him. It's great, great for Rice, it pleased Smalley. Smalley is happy. I congratulate Malcolm Gilles and anybody else that should be congratulated in that connection. It's great.

JB: Let me ask you one other quick question. You may just want to dismiss this. One of the issues that plagued George Rupp and that Malcolm Gillis is having to work with very energetically now is the issue is of big time sports. Of course, you were the president the last time Rice had a winning football season, I think it was in 1961. Do you have any thought or any words of wisdom about the continuation of big time sports or do you have any particular difficulties with it? In those days there was a special shelter program for athletes. It doesn't exist any more.

KP: Well, we had a lot of discussion at dinner with Gillis last night and a few others. It got on to that subject. As I have said to people, I was very fortunate in this respect. Jess Neely was a very fine man as well as an excellent football coach. He had been trained as a lawyer, you remember, at Vanderbilt. He hung out his shingle and nobody came, and that sounded familiar to me. My father was trained as a lawyer. He hung out his shingle, nobody came, and he became a farmer in the sense of raising oranges and grapefruit instead. He did very well by it, never pined over the fact that he hadn't used his law degree. But he did use it, actually, in business. He knew his way around in a way that he wouldn't otherwise. I was fortunate at Rice in having an athletic program that handled itself in an honorable way and if the coaches who weren't familiar with something that needed attention, somebody like Allen Chapman, pointed it out to them. I didn't have to worry about it other than to make it clear that it should be that way. There was no argument. Now it's true that we didn't win too many games. We won some. We won enough and when we beat Texas or we beat LSU, it was such an upset that everybody was so happy about it, even if it didn't happen very often. It was fine. But I realized myself that this wasn't going to go on forever. I did a little thinking about what could be done in terms of a new affiliation with Tulane and Vanderbilt and Duke or something like that, but there wasn't any need to explore it seriously because it wouldn't have any advantage unless things fell apart and now something is falling apart. So I must say, Malcolm proceeds with his own thinking and the advice he has gotten, and I understand he has gotten at least one of the trustees to really spend some time on this. He has found out a lot about what the problems are. There isn't any simple solution and no self-evident solution. But Rice's stature in the world doesn't depend on its athletic program. Malcolm and others should be skillful and thoughtful and careful in handling it, but realize that as long as they don't do something silly or ridiculous, it is not tragic for Rice. Rice will prosper in any case. Incidently, I had a very interesting discussion with Frank Ryan.

Continued interview Tape 3 Side 5

KP: As I just said, I had a very interesting discussion with Frank Ryan. I don't intend to go into that discussion, just to say that he is a remarkable person and that's in a sense something that was remarkable at Rice and wouldn't happen at practically any other place. Here is a top line quarterback and a Ph.D mathematician and now with a career that has continues on the mathematics and then in a sense builds on the reputation and contacts and credibility in a broad community that he has from his athletic career. If Rice can figure out how to be attractive to young men of that category, they ought not give it up easily, but maybe it's completely impractical. Well, I have enjoyed this opportunity to reminisce about these things very much. In your questions you clearly indicate that you have a very good comprehension of the various factors and that were involved in the decisions, plans, and actions that were made during my period here as president. A great deal of what I have been able to say has been confirming things that were apparently reasonably clear to some of you and others who have been on the scene. But again, from this visit it is clear that Rice is doing well. It could still do more and better but it is doing very well, made a lot of progress. It is a remarkably fine institution. I certainly have enjoyed having had a contact with it and wish it the very best for the future.

JB: Thank you very much. Since being here as a student when you were the president, I have a sense that you were the creator of the modern Rice. For all of us, thanks very much.

LJM: It has been a pleasure and I thank you.

N.Y. Times - April 19, 1970

Students End Sit-In at Stanford As President Gets New Power

By The Associated Press

About 600 militant students at Stanford University ended a nine-day sit-in at an electronics research laboratory yesterday after the Stanford judicial council voted to give the university president power to suspend the occupiers.

The occupation ended after the president, Kenneth Pitzer, was reported to have agreed to keep the building closed to all but maintenance personnel for a week. The demonstrators had occupied the building to force Stanford to end cooperation with the Federal Government on military projects.

Atlanta Door Chained

Mr. Pitzer had said he would not ca'l in outside force if the sit-in stayed peaceful. But both he and the judicial council, a student-faculty group that handles campus discipline, had warned that Federal troops might be sent to the laboratory without university sanction to protect secret Government files.

Protesting Negro students at Atlanta University chained shut the door of a room where the school's board of trustees was meeting with 10 student representatives.

The rebellious students demanded the resignation of 18 trustees and the merger of the six-college university complex into one school oo be renamed Dr. Martin Luther King university.

At Kent, Ohio, 1,500 of Kent State University's 18,000 students agred to ask the presidnt Robert I. White, t, to reinstate 40 members of students for a Democratic-society who were suspended from school after a riotous demonstration. Wednesday. They planned to vote Monday on whether to stage a student strike if the suspensions were not lifted.

Trustees of Michigan State University agreed to expand the school's new Urban Affairs Cenger and increased its budget for the next fiscal year from $250,000 to $1.5-million. They acted after 60 Negro students compained that the center was a "miserable failure."

Bomb at Miami U.

MIAMI, April 18 (Reuters)—A small homemade bomb exploded in the office of the dean of men at the University of Miami last night. The police said the office was left a shambies but there were no injuries.

Professor Pitzer's notes for uncovered topics for the oral history interviews (handwritten in pencil)

National Academy of Sciences

Elected April 1949 (age 35 - rare but not unprecedented)

Meeting of April 1950

Election of Bronk as president instead of the committee nominee Conant. (see special file of 1967-68)

Role of Latimer & Hildebrand

Basis of Conant's!

1- Lack of NAS-NRC activity

2- War time record of consulting Harvard friends rather than national authorities

(K.S.P. experience: Chadwell + ?

3- Presumed small allocation of time while President of Harvard

Publicity of 1967-68 (special file)

In my later contacts with Conant he showed no resentment.

Chairman of Chemistry Section 1958-61. no detailed comment

Misc. Special topics, NAS

AEC Fellowship program:

Established:

Loyalty + security investigation required by Congress.

NRC to recommend candidates + administer

NAS withdraws

Special Committee to nominate NAS ~~candidates~~ who performed secret work in World War I of great merit

appointed Sept 1950, Wetmore, Chm., KSP member

In file, no detailed oral comments.

Nominating Committee for officers + Councilors 1962 -

Houston, Chm., KSP member

At end of Bronk's term, select new President.

Discuss need for full time.

Nominate Fred Seitz on old, "part time" basis.

Reactivated for 1963 to consider shift of presidency to "full time" with appropriate changes in By laws, etc. Considered length of term, 6 yr. max, number, 2 max., but not to extend past age of 70.

Also, provision for nomination of competing candidate by petition of 50.

Nominating Committee for 1964-65, KSP chairman. Nominate anew for president - now on full-time basis

Seitz nominated.

Also for Vice President + 2 Council members.

1964-65 Committee "Survey of Chemistry. Westheimer Chaired, I assisted but was relatively inactive. I was "NAS liason member".

IAS -

1968 - Chaired special committee concerning a Lunar Science Institute to be financed by NASA + located at or near the Manned Spacecraft Center near Houston.

Harvey Mudd College:
Informal contact – suggested Joe Platt for President – –
Trustee 1956-61

National Academy of Sciences
Elected April 1949 when Conant was committee nominee but Bronk ele[illegible]
Meeting of 19[illegible]
Chairman, Chem. Sect. 1956-61 – no detailed comment
Council 1964-67 and 1973-76, [illegible] 1973-76
Chairman Committee on Nations [illegible]
Advised NAS President & Council on "external" matters related to the Government [illegible] and the public generally. Also nominations for the Public [illegible] Committee [illegible] Science & [illegible] [illegible]
[illegible] most important [illegible] 1973-76
P[illegible]
was [illegible]
with the NAS including the joint operation [illegible]

Misc. Board Memberships

1957-61 Stiles Hall (U. of C. YMCA) Adv. Board.
a useful organization but I made no special contribution

Miscellaneous (Other Kelly Committees, Etc.)

1. President, Sigma Xi Chapter 1945–47. Interesting items concerning joint meeting with Sigma Xi Club at Shell Development when the President Jewett of the NAS was to speak & then withdrew because of matters in Washington.

2. Am. Chem. Soc. Committee on New Activities. 50% of members to be under age 35. 1943–44
   Reflects resistance to dominance of Charles Parsons, general secretary (general manager) of the ACS & "out of touch" [illegible] [illegible] was at Maryland Park [illegible]
   Note that World War II restricted time & travel,
   Recommended "[illegible] [illegible]" [illegible]
   Publication Committee [illegible] [illegible]
   Interesting contrast in volume of research [illegible] [illegible] "pure chemistry"
   One journal ACS adequate [illegible], several needed now –
   [illegible] [illegible] [illegible] [illegible]

3. Am. Chem. Soc. Committee on New Publications 1946–47
   a. Considered several proposals but no strong recommendations for new journals
   b. Board of Directors adopted a "policy statement" essentially as recommended by the Committee

IUPAC + Int'l. Scientific Activities, etc

1951-53 IUPAC, Comm. on Chem. Thermodynamics
meeting NYC + Wash. DC 1951
Recommendation of new definition of temperature
with just T = 273.15 for ice point

1953-59 Several meetings + topics including more on.

1959 Symposium at Wattens, Austria

1962-68 Several meetings + topics
Culminating in 1967 meeting at Prague where I was a
U.S. representative on the IUPAC Council

1973-74 "Killian Comm." Also the "Blue Ribbon Committee on Scientific Advice to the Federal Government."
James Killian, former Pres. MIT + first Presidential Science Adv. to Eisenhower was Chm.
KSP was vice Ch. as was Emmanuel Piore - Retired Chief Scientist + V.P. IBM

After [illegible] Pres. Nixon abolished the President's Science Advisor and Pres. Sci. Adv. Committee [illegible] when Nixon
NAS decided to [illegible] a [illegible] 1974
[illegible]

This [illegible] re-established the
[illegible] 1974 date, V.P. Ford [illegible]
[illegible] was Guyford Stever, then Director of the NSF
and former [illegible] of Carnegie-[illegible] Univ. [illegible]
[illegible] made public. Stories [illegible] Editorial July 8, "

Report presented [illegible] Congress [illegible] participate

NAS

Council (2nd time) 1973–76

Advisory Committee on Academy Membership

1976–78

Question:
annually
(1) [illegible] increase in total number elected
(2) [illegible] of older members not acquainted with recent science

Interim Report, March 1977, recommended on [illegible] total number [illegible] ([illegible] per year)
[illegible] – [illegible] in total [illegible]
(3) [illegible] action by the Council + by Special Nomination [illegible] for candidates from new or neglected fields
Groups [illegible] fields [illegible]

Final Report [illegible] March 1978
[illegible] April [illegible] 1978
Discussed [illegible] Council action [illegible] recommended
[illegible], Council action [illegible]

1981 Nominating Comm. [illegible] Chairman
[illegible] in May to → Frank Press nominated for President
question whether his service as Sci Advisor to Pres. Carter
made him too "partisan" + "a Democrat" vs Republican
[illegible] "professional" + [illegible] position
[illegible] "[illegible] [illegible]" Wiesner with John Kennedy)
+ hence ok.

[illegible] + Council cand's
[illegible] V.P. ([illegible] cand.) + Council cand's

Chm. Chem. Sect. 1958-62 July '58 - June '62, 3 yrs.; my secy. Betty North, handled details after I was at Rice

Council 1964-67 and 1973-76

1950 - Election of Detlev Bronk as president by nomination from the floor in competition with Jas. Conant committee nominee, Conant withdrew after losing a voted to make it unanimous.

~~[illegible]~~

1950

[illegible] one or more [illegible] second involved was research. Recommend:

Mark K. [illegible], [illegible] Lab.

[illegible]

[illegible] Committee on [illegible]

[illegible] - [illegible]

~~[illegible]~~ also Executive Comm. '64-67. Most [illegible] in '66-'67 was "[illegible]". [illegible] proposed [illegible] [illegible] Class II (biological sciences) & [illegible] "medical sciences" or "human [illegible]". [illegible] 67. major [illegible] NAE [illegible] - [illegible] full-time president

Creation of [illegible], Res. Assn. Inc URA

Nominating Committee for President NAS

1962, Wm. Houston was Chm. [illegible] [illegible] 1950. Nominated Fred Seitz, elected in 1962

Reactivated to consider changes in operation incl. full-time president

1964,5 I was chairman. With shift to full-time basis, a new election was needed. Seitz was chosen. Other officers & councilors were also nominated in 1965

?

AEC Fellowship [illegible]

Congress imposed FBI investigations as to loyalty & security

NAS - NRC declined to evaluate candidates, but urged that

the program go ahead. I arranged for Oak Ridge [illegible] to administer

MEMORIAL SPEECH by John R. Thomas

January 25, 1998

Kenneth Pitzer was such an exceptional person and had such diversified interests that I am going to restrict my comments on his life to the time I knew him best. That was during a two-year period when I worked as assistant chief of the Chemistry Branch of the Atomic Energy Commission in Washington when Ken was director of research.

I first met Ken in freshman chemistry when I entered Berkeley in January 1940. In the spring of 1943 when I finished undergraduate work, I joined the chemical warfare project headed by Ken and Samuel Ruben and maintained contact with him until he was called to Washington to head another war-related project.

In early 1949, Ken went to Washington as director of research of the relatively new Atomic Energy Commission and when he asked me to join him and work with Spofford G. English, Berkeley chemist, student and Glenn Seaborg and chief of the Chemistry Branch, I could not refuse.

When Ken accepted the job with the Atomic Energy Commission, his goals were to strengthen and enhance basic science in the government labs, to use the prestige of the commission and its standing with the Congress to strengthen physical sciences in American universities, to engage universities in the commission's government laboratories work, and to use these resources for aiding the early development of the peaceful use of atomic energy.

With these goals in mind, Ken recruited additional people for the Physics, Chemistry and Metallurgy Branches of the Research Division. To enhance the level of basic science being done in American universities, Ken initiated a grants program in the physical sciences. The grants were given in response to research proposals which were relevant to atomic energy in the broadest sense. Those of us in the branches reviewed the proposals and passed judgment on their merit and relevance, and recommended funding.

To ensure objectivity in the program, Ken assembled advisory committees of distinguished scientists in the various fields. I attended lunch for the chemistry advisors on the only day that I am aware of that they formally met, and although there were five members, I am sorry to say that the only name I can recall, and be sure of, is George Kistiakowsky, a famous chemist from Harvard, who made major contributions to the chemical explosive component of the atomic bomb. I believe that if a historian ever conducts an audit of the effectiveness of the grants program and of Ken's plan to stimulate university interest in the government laboratories programs, the result will be very favorable. Today's national labs owe some of their success to Ken's vision. The audit will also show that a large number of distinguished scientists were materially helped in their careers by the grants program.

On August 29, 1949, Ken's role at the AEC was vastly changed. That was the day the Soviets detonated their first atomic bomb. The American government learned of this on September 3, when a WB-29 weather reconnaissance plane on a routine flight from Japan to Alaska discovered nuclear fission products from the Soviet test. Spof English had been involved in setting up the surveillance program and evaluating the results. He was sent to notify David Lilenthal, then chairman of the Atomic Energy Commission, whom he found about nine o'clock at night on an eastern beach. For the rest of his stay in Washington, Ken and his staff were influenced by the consequences.

The following things happened:

An immediate program involving armed service groups, meteorological experts and the Research Division was established to evaluate Soviet plutonium production by sampling the earth's atmosphere for gaseous fission products released when reactor fuel elements are dissolved to recover the plutonium.

A second item, which was destined to lead an intense debate, was the question of starting a program to build a superbomb. Although American scientists, headed primarily by Edward Teller, had given considerable thought to a bomb design involving fusionable materials to produce such a bomb, there was no immediate agreement that these ideas would work. Ken participated in the ensuing months-long debate between J. Robert Oppenheimer and his AEC General Advisory Committee who did not want to initiate a program to develop such a bomb for technical and moral reasons, and Ernest Lawrence, Edward Teller, John Wheeler, and others who did, feeling, correctly it turned out, that the Soviets were already embarked on such a program. On March 10, 1950, the debate was settled when President Truman ordered a crash program to do so.

An immediate need was facilities to produce tritium and the Research Division participated in the discussion to select a contractor to build new reactor facilities at a new site for its production. In a meeting lasting fifteen hours, a recommendation was drafted and made to Carroll Wilson, then chairman of the AEC, that Du Pont, who had been the original contractor for Hanford, be asked to reassemble their team to do this. President Truman called Crawford Greenwald, chairman of Du Pont, to request that they do so in the national interest. Greenwald agreed and the program to build facilities at Savannah River was initiated.

The Raw Materials Division of the AEC, which ran the uranium procurement program with abnormal secrecy, doubted that their one supplier, South Africa, could supply the needed uranium.

In response, Lawrence and the group at Berkeley proposed a high current production linear accelerator which when targeted on depleted uranium, which was available, would generate enough neutrons to produce tritium and plutonium. Ken and the Research Division carried the proposal through the AEC and won approval for $100 million to build such a device. Lawrence and Pitzer solicited Standard Oil of California as an industrial partner for the Berkeley Radiation Lab. They also selected the abandoned naval air training base at Livermore as the site for development work. In time this became the Lawrence Livermore National Laboratory.

The Raw Materials Division revised their uranium procurement. They raised, and published a price for uranium, and offered a financial incentive to prospectors who found significant domestic deposits. Individual prospectors responded, finding ample deposits in the American West. Ken, with his wonderful dry sense of humor, used to enjoy telling how the accelerator project so dramatically changed the outlook for uranium reserves. The large accelerator was never built, although the project was not terminated until about mid-1952.

Concern that the Soviets were ahead in the super bomb race triggered an urgent study of highly radioactive fission products from the plutonium program as a radiological warfare weapon as a deterrent to the Soviets. Professor Albert Noyes, from the University of Rochester, who had participated in the direction of chemical warfare research, then completed, headed the study, in which the Research Division actively participated. It was concluded that the idea was impractical and considerations of radiological weapons were dropped.

To complete the story, although Ken left the AEC in 1951, the U.S. exploded a superbomb device, of Teller's original design, 800 times the strength of the Hiroshima bomb on November 1, 1952. However, it was not a deliverable weapon. On August 12, 1953, the Soviets exploded their first superbomb, which was deliverable, with a strength of 30 Hiroshima bombs. On March 1, 1954, the U.S. exploded a deliverable superbomb which incorporated lithium deuteride as a fusionable explosive, which a strength of 1,300 Hiroshima bombs. On November 23, 1955, the Soviets exploded a similar device and parity in superbomb design was achieved.

Ken's management style made him an extremely effective leader. He made it clear what he wanted to accomplish, and while he was always willing to discuss the details as to how it might be done, he delegated authority liberally and supported his staff whenever they needed it. Ken Pitzer was a brilliant scientist, an accomplished manager and administrator, and had the rare talent of inspiring people associated with him to perform at their highest possible level.

John R. Thomas

MEMORIAL REMARKS FOR KENNETH SANBORN PITZER

Pitzer College, February 23, 1998
Joseph B. Platt

I first met Ken Pitzer in 1949. He was at that time professor of chemistry and Dean of the College of Chemistry at Berkeley, taking a two years' leave of absence to become Director of Research for the recently established Atomic Energy Commission. I was at that time associate professor of physics at the University of Rochester. Ken invited me to become chief of the section of the Research Division which managed the Commission's research investments in physics and mathematics. I accepted. Accordingly he was my boss for two years, and we worked together closely.

The military uses of nuclear energy had been developed by the Manhattan District of the Army Corps of Engineers during the Second World War. After the war the Congress decided to transfer government subsidized nuclear work to the control of a civilian agency, so that the peacetime benefits of nuclear energy and nuclear research could be openly developed. The Research Division was charged with supporting research, both in government laboratories and elsewhere, to extend our knowledge of nuclear processes. That was an exhilarating two years' assignment, which took both of us to many universities, government laboratories, and corporate laboratories. Those years began much of the understanding we now have of nuclear processes. We at the Atomic Energy Commission were involved in the building of the postwar "atom smashers" such as synchrocyclotrons, which led to our present understanding of nuclear forces. We helped develop a number of the early electronic computers, and much else. Ken had a significant job, and he did it well. We became good friends as well as colleagues.

Ken was a very quick study. He was an excellent chemist, and he also knew a great deal about the sciences in general. He understood how research is done and how it can usefully be applied. He was an excellent teacher, and had a sure sense of academic quality. It was a pleasure to work with him.

I am here in Claremont because of Ken Pitzer. When Harvey Mudd College was established in 1955, the trustees of the new College asked Ken for nominations for its presidency. I do not know how many candidates Ken suggested, but I do know he nominated Art Campbell and me. Art decided he was not interested in the presidency of any college, but that he might be interested in chairing the department of chemistry of this new college. I *was* invited to the presidency and I accepted. When I started looking for a chemistry department chairman I asked Ken for suggestions. Ken remembered that Art Campbell was interested, and Art did come. Hence Ken Pitzer was responsible for the appointment of the first president of Harvey Mudd College, and also for our first faculty appointment, a professor of chemistry.

Ken's parents, for whom this college is named, were also major figures in the founding of Harvey Mudd College. Mr. and Mrs. Russell K. Pitzer had been benefactors of Claremont McKenna College. When the next college was proposed, they volunteered to give a building to CMC with the understanding that the new college could use the building for a decade, or until we had our own space, whichever came first. The senior Pitzers also required that the new college secure certain other gifts. These conditions were met, and Harvey Mudd College was housed in Pitzer Hall North, on the CMC campus, for our first five years.

Ken Pitzer continued to be of help to Harvey Mudd College. I suggested that he might be a helpful trustee. He was elected to our Board and served until he became president of Rice University. It really is helpful to a college to have on its Board a respected faculty member from another institution; the Board does not need to depend on the college president for all its information about academic matters. Ken helped our Board to understand the relationship between teaching and research, how faculty appointments are made, and why tenure exists. He also gave an independent estimate of how well we were doing.

Ken helped introduce the Platt family to Southern California. When we were first here, he took the four of us to the Pomona Fair, with particular attention to the huge model train system that was on view there. Ken knew the man who had designed that system. Ken came often to the Pomona area while his father was living, and each visit usually included some time with and for the Platts. Ken introduced us to the old Taix's restaurant, a great place to eat in downtown Los Angeles, then not too far from Union Station.

We continued to finds ways to see each other in later years. Both of us occasionally took some summer time on the Russian River. We had an annual routine of paddling a canoe up the river and back, while we brought each other down to date on our family news, on our views of the academic world, and on the world in general. We shared a love of the outdoors and of the water.

Accordingly, I join with all the Pitzer College family in remembering with warmth and respect the many happy and productive hours we have shared with Ken Pitzer and with Jean. And, as a former president of two other Claremont Colleges, I can report that Ken Pitzer has done his full share in making The Claremont Colleges what we now are.

In the years before World War II, J. Robert Oppenheimer, the "father of the atomic bomb," made California and UC Berkeley a world center for physics. He and his wife Kitty lived here on and off for several years as the drama of exploring the limits of atomic energy unfolded. Now, during the fiftieth anniversary of the explosion of the atomic bomb at Hiroshima, here is part of their story, based on some new research and interviews with people who knew them well. It is a story about a compelling, headstrong woman who lived at a time when a woman's pre-eminent role was to be a wife.

J. ROBERT OPPENHEIMER MEMORIAL COMMITTEE

Every generation produces women like Kitty Oppenheimer: handsome, wealthy, exceedingly bright, sexually aggressive—women who break out early, run hard, challenge rules, and live with an intensity that burns so bright it eventually self-destructs. No age was quite like the one she found herself thrust into—the turbulent political years leading into the second world war—and no woman in history has ever had her vantage as wife of the central figure in the making of the atomic bomb at the dawning of the nuclear age.

A contemporary feminist might dismiss Kitty as a woman who just happened to marry a famous man. Kitty did have ambitions, but they were hobbled—not so much by the times or tradition as by her need for a man to be central to her life and by her capacity to fall deeply and, twice, profoundly in love. She was married four times; two of her husbands would play portentous roles in the upheavals of their times.

In the decades since their deaths, Robert Oppenheimer has achieved a kind of martyrdom while Kitty has been dismissed as a drunk and a misfit, another of the crosses he had to bear. Yet her detractors knew little about the woman or about the union that seemed so improbable to many in his world. In fact, it was a formidable marriage, a love story played out in a time of sound and fury.

In the fall of 1941, J. Robert Oppenheimer bought the rambling house perched at the top of Eagle Hill near North Berkeley for his wife Kitty and their infant son Peter. The young family lived in this aerie, with its commanding views of San Francisco Bay, for a year before leaving for Los Alamos, where the Manhattan Project would become the focus of their lives. After the numbing blow of Hiroshima and the end of the war, a weary Oppenheimer took his young family home again to Eagle Hill. They stayed less than two years. For Kitty and Robert Oppenheimer, One Eagle Hill was the setting for some of the happiest years of their lives.

More than one person who knew Kitty remembers that "she could be totally *imperious*," but few knew how apt that word was. Katherine Puening was born in Germany in 1911 to aristocratic parents. According to Robert Serber, an emeritus professor of physics at Columbia University who was an intimate friend of both Oppenheimers, her father was a German princeling and her mother one of Queen Victoria's many granddaughters. The Puenings moved to the United States when Katherine was two. Her father renounced his title and asked his daughter not to speak of hers; yet her mother took Kitty to Europe every summer to stay in the houses of titled relatives. A second cousin was said to be General Wilhelm Keitel, chief of the Nazi Supreme Command, who would be sentenced to death at Nuremberg.

Kitty was "handsome, lively and commanding...vivacious and irresistible, charming when she chose to be," those who knew her said. She was attractive in the sultry, sexual manner of a Jeanne Moreau. Nobel Laureate Glenn Seaborg, a young post-doctoral fellow at Berkeley before the war, remembers being "awestruck" by Robert Oppenheimer, but he chooses his words carefully when he describes Kitty: "She was very as-

KITTY OPPENHEIMER

FIRST ATOMIC WIFE

by Shirley Streshinsky

sertive, straightforward, plain-spoken. She was not interested in small talk. She was acerbic. I can see why she might not have been popular."

Much later Robert Oppenheimer, pressed for details about his wife, would tell security investigators: "I believe she had an early marriage, which was annulled. A very nasty fellow. She has told me very little about it, but I think he was quite talented a musician." In fact, her first husband, whom she married just out of high school, was a homosexual, and the marriage was short-lived. Her parents saw to the annulment, had the records expunged, and packed her off to Europe.

She returned to the University of Wisconsin to study chemistry, math, and biology. In Pittsburgh for the Christmas holidays in 1933, she was invited to a party to meet "a real Communist"—Joe Dallet, tall and ruggedly good-looking, with a longshoreman's accent that belied his background. His father was a wealthy New York investment banker, and Joe had gone to Dartmouth.

The attraction was electric. They were married two months later, and she joined him in Youngstown, Ohio, where he was organizing for the Communist Party in the steel mills. Kitty's father, an engineer at Bethlehem Steel, could not have been pleased. For the next two years she lived a life of poverty and political commitment, selling the *Daily Worker* on the street, passing out leaflets at the steel mill, and becoming a member of the Party. Finally, in June of 1936 "although Joe and I continued to be very much in love, the poverty became more and more depressing to me," she would say. She went home to her parents, now living in England. When, by chance, she learned that her mother had been intercepting Joe's letters, Kitty was furious. She wrote Dallet that she wanted to come back. He answered that he was on his way to fight with the Abraham Lincoln brigade in the Spanish Civil War. In March of 1937 she was at the docks at Cherbourg when the *Queen Mary* arrived with Dallet aboard.

For ten days, she would remember, "We walked around and looked at Paris, went to restaurants, the sort of thing one does in Paris." A month later he wrote to her from Spain: "I was overjoyed by your desire to come here and work... I love you." In October, Dallet scrawled, "Writing this in an olive grove by candlelight with artillery and avion bombing rumbling in the distance... By the time you get this we'll be in action... Until we meet." At dawn on October 13, 1937, while leading his men into battle near the town of Fuentos de Ebro, Dallet took a bullet through the brain and died.

Kitty was in Paris, on her way to join him. "For a little while I had some notion of going on to Spain anyway," Kitty would later testify, saying she felt emotionally involved in the Spanish cause. Instead she returned to the States. She was 27 years old, a widow with a radical past that was to haunt her for the rest of her life.

She enrolled at the University of Pennsylvania, majoring in biology. The next year she married ("out of loneliness," she would say) Richard Stewart Harrison, a physician she had met in England. When he set off for Southern California to do cancer research in the new field of radiation, Kitty stayed behind to finish her degree. After graduating with honors in June of 1939, she joined Harrison in Pasadena and started post-graduate work in biology at the University of California. She seemed determined to get a doctorate and do her own work.

Not long after her arrival, the Harrisons were invited to a garden party at the home of experimental physicist Charles Lauritsen. There they were introduced to the guest of honor, the awe-inspiring J. Robert Oppenheimer, who taught at Cal Tech part of each year. Kitty Harrison took him by storm. By the end of the afternoon she was, for the last time, overwhelmingly in love. He was equally intrigued by her and called her "golden."

At the time, Oppenheimer was involved in a long-running affair with beautiful, brilliant Jean Tatlock. She lived in San Francisco, had a doctorate in psychology, was a member of the Communist Party, championed left-wing causes, and had politicized the formerly apolitical Robert. She also suffered from debilitating bouts of depression. "Their time together was stormy," says Dr. Serber, who was Oppenheimer's research assistant at the time. "He wanted her but he didn't."

In the summer of 1940 Oppenheimer invited Kitty to the cabin in the mountains of New Mexico that he owned with his brother, Frank, also a physicist. Summers at the ranch were legendary. Oppenheimer's students came, visiting physicists from around the world gathered there, and everybody was young and robust and followed Oppenheimer's daring lead. On nights of the full moon, they would take off on horseback with whiskey and graham crackers in their saddlebags and ride for hours along mountain trails that Oppenheimer seemed to have memorized.

An expert horsewoman, Kitty could keep the pace and up the ante. Everyone saw the fascination on Oppenheimer's face. By the end of the visit, Kitty was pregnant. She went to Reno for the necessary six weeks and got a divorce on the morning of November 1, 1940. That afternoon she and Robert Oppenheimer were married by a justice of the peace. Her Nazi cousin would have been aghast. Of the German-born princess's four husbands, one was gay, one was a Communist, and one, Oppenheimer, was a Jew.

Peter was born in May, and the family moved into One Eagle Hill that fall. The soaring two-story living room, with its beamed ceiling and great stone fireplace, offered the perfect background for Oppenheimer's collection of fine art.

The Oppenheimers entertained often and well. Martinis were stirred, not shaken; the couple had sophisticated palates, and Kitty was an accomplished cook. But Oppenheimer's colleagues and their wives weren't so quick to embrace the woman who had managed to marry their adored "Oppie." Kitty hated the nickname and always called him "Robert."

Many of their closest friends were members of the Communist Party, including Oppenheimer's brother Frank, his wife Jackie, and Haakon Chevalier, a professor of Romance languages at Berkeley. Robert and Kitty contributed to left-wing causes and attended meetings to raise money for refugees from the Spanish Civil War.

Oppenheimer's office on the third floor of LeConte Hall on the UC campus became a nucleus of intense activity. By now physicists knew that theoretically it was possible to create a bomb of almost incomprehensible force, enough to bring any enemy to its knees. They believed the German physicists were on their way to creating such a weapon. Robert Oppenheimer was given the job of making sure the Allies got there first.

Kitty settled into Eagle Hill, planted a vegetable garden and the flowers that she loved—botany was her enduring pastime—and enrolled at UC Berkeley to continue work on her doctorate. Soon, however, her lab was doing war work, making preservatives for foods, and her academic career was put on hold once more.

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Robert and Charlotte Serber moved into the apartment over the garage at One Eagle Hill and became part of the Oppenheimer household. "Kitty and Robert got along almost always," Serber recalls. "Robert had a nasty tongue and a quick temper, which he had to learn to curb."

In agreeing to lead the Allied team that would enter the atomic race, Oppenheimer had made a Faustian bargain: knowledge and power in exchange for his soul. He would have the chance to do physics on the grand scale—with all the money and government support it would require. To do this, he would bring together a dazzling array of the world's most renowned physicists, chemists, and mathematicians, many of them Jewish refugees.

In the spring of 1943 they began to arrive at Los Alamos, legends in the world of physics working alongside America's most promising graduate students. The average age was twenty-seven. They were young couples, for the most part, products of middle-class America, thrilled to be part of the adventure. The empty New Mexico mesa became a top-secret boomtown.

One of the young wives would write, "It was not a casteless society. Lines were drawn principally not on wealth, family, or even age, but on the position one's husband held in the laboratory." Kitty Oppenheimer was first in line.

To help ease a serious labor shortage in the newly created city, the wives were pressed into service. A few refused, but for a while Kitty worked half-time in the lab at the doctor's office and found time to teach one young wife how to cook. She also made regular trips to a nearby farm to bring fresh produce and chickens to the mesa to be distributed to families.

Until drought put an end to all frivolous uses of water, she managed to grow flowers in front of her house. She was expected to entertain a steady stream of VIPs who came to the secret installation, including the notoriously difficult General Leslie Groves, who was running the show for the military. To everyone's surprise, both Oppenheimers were able to charm the surly Groves. Robert seemed miraculously to have brought his temper under control and surprised everyone with his newfound aptitude for administration. Soon Kitty joined the ranks of young wives who found themselves pregnant. Daughter Katherine Tyne—called Toni—was born during the first year at Los Alamos. Now she had two young children to care for, as well as all her extra duties.

Oppenheimer's secretary, Priscilla Duffield, says about Kitty, "She was a very intense, very intelligent, vita kind of person, and I think there's nc question at all that she was difficult to handle." There were persistent rumors about Kitty's drinking. Two women who saw her often during that period, Duffield and Elino

Hemplemann, agree that she drank no more than anyone else. Serber, who was always part of the inner group, adds, "Kitty suffered from pancreatitis, which could be very painful. She took the drug Demerol, and at times it gave the impression she was drunk."

Jealousy was another charge brought against Kitty. The one woman she had reason to be jealous of was Jean Tatlock, her husband's former fiancée. On one of his periodic trips to the Bay Area, he went to Tatlock's Telegraph Hill apartment, evidently not aware that FBI agents were following him. The agents watched the lights go out in her living room and on in the bedroom, then followed the couple when they emerged the next morning, until she kissed him goodbye.

Six months later Jean Tatlock would fill the bathtub in her apartment, gulp down a large number of pills, and write, "To those who loved and helped me, all love and courage. I wanted to live and to give and I got paralyzed somehow." When he learned of her death, Oppenheimer was at Los Alamos. Shaken, he walked off into the woods alone.

During the Los Alamos years, Kitty watched her six-foot husband go from a reed-like 140 pounds to a gaunt 115 as he undertook the gargantuan task of pulling an atomic weapon out of the dazzling display of brilliance he had assembled in the New Mexico wilderness.

On Monday, July 16, the first atomic device was exploded. Soon after, the war was over, and Oppenheimer became the man of the moment. Offers came flooding in from universities. Profoundly shaken by the implications of his Faustian bargain and physically exhausted by the superhuman effort, he decided to come home to Berkeley and Eagle Hill. Now, however, he was a world figure, a celebrity on the cover of *Time*. He was "the father of the atomic bomb," an American hero. He felt an enormous responsibility to see that the malevolent force he had helped to set loose on the world would be used in the quest for peace.

While Robert traveled back and forth to Washington, Kitty stayed at Eagle Hill with the children, started a garden, and took classes again. Then he was offered the directorship of the prestigious Institute for Advanced Study at Princeton, a title formerly held by Albert Einstein. There he would be close to Washington, where he was already serving as chairman of the General Advisory Committee, which advised the newly founded Atomic Energy Commission on scientific and technical matters.

Kitty did not hesitate. She intended, she told Serber, to guide her husband

The "father of the A-bomb" was suddenly suspect. A new law had been passed stating that a person could not receive a security clearance if it could be proved his spouse had been a member of the Communist Party.

through "the paths of power." The family left Eagle Hill for the 18-room director's house, Olden Manor, on the Princeton campus.

World leaders found their way to their home, and so did old friends from the Los Alamos days. "There was a loving, close bond between them," Elinor Hemplemann says. "He was amused to death by her." At Olden Manor, Kitty turned a sunroom into a kind of greenhouse where she could grow orchids.

The Oppenheimers bought a house on an isolated beach on St. John in the Virgin Islands, where the family spent holidays sailing. Like the cabin in the mountains of New Mexico, the beach house was simple, spartan—and friends flocked in. They were years full of a tenuous promise.

All that changed when, in the summer of 1949, the Russians exploded an atomic weapon, and the arms race, which Oppenheimer had tried so desperately to prevent, was on. A few months later, Klaus Fuchs, one of the British scientists at Los Alamos, confessed to having passed information to the Russians throughout the war. Words like "Communist" and "fellow traveler" were used like hammers to batter anyone who before the war had been involved in left-leaning politics. Frank Oppenheimer would lose his teaching position, and so would many other young scientists who had labored honorably on the Manhattan Project.

In the summer of 1950, Robert and Kitty took the children to the cabin in New Mexico. Robert's back was hurting and Kitty was ailing. There would be no moonlight horseback treks along mountain trails; the old verve was gone. At night they played poker with Peter and Toni. During the day Kitty worked on a watercolor of the mountains. They walked the high pastures with the children, searching for four-leaf clovers. Peter, sensitive and shy, had spent almost all of his nine years in the shadow of the bomb. With so much going on, both children had been neglected, Serber believes. And things weren't getting any better.

The "father of the A-bomb," recently adored by an American public who credited him with ending the war, was suddenly, amazingly suspect. A new security law had been passed stating that a person could not receive a security clearance if it could be proved that his spouse had been a member of the Communist Party. On December 3, 1953, President Eisenhower refused Oppenheimer any further access to secret information until charges against him had been investigated. Removing his security clearance would deny him a voice in the nation's nuclear policy. The Personnel Security Board called for a hearing "In the Matter of J. Robert Oppenheimer." It was tantamount to a trial for treason. Clearly, Kitty was on trial as well.

The Army major in charge of security in the atomic bomb project, obviously fascinated by Kitty Oppenheimer, had reported: "She struck me as a curious personality, at once frail and very strong. I felt she'd go to any lengths for what she believed in... I was sure she'd been a Communist and not sure her abstract opinions had ever changed much. But feelings were her source of belief. I got the impression of a woman who'd craved some sort of quality or distinction of character she could attach herself to, who'd had to find it in order to live. She didn't care how much I knew of what she'd done before she met Oppenheimer or how it looked to me. Gradually I began to see that nothing in her past and nothing in her other husbands meant anything to her compared with him."

On the morning of April 12, 1954, a clear, bright Monday, they filed into the Atomic Energy Commission. Lloyd Garrison, Oppenheimer's lawyer, would note that they "made a pretty bedraggled kind of spectacle. Kitty had had the misfortune to fall down some stairs just before, and she had her leg in a cast and was on crutches, and her appearance didn't add much to the smoothness of things."

When called to testify, she was characteristically blunt. Asked how one went about leaving the Communist Party, she answered, "I think that varies from person to person. Some people do the bump, like that, and even write an article about it. Other people do it quite slowly. I left the Communist Party. I did not leave my past, the friendships, just like that. Some continued for a while."

Their lawyer remembers, "A man's life was at stake. It was like a murder trial...in which the evidence was murky and half-known... Robert was in the most overwrought state imaginable. So was Kitty—but Robert even more so. He would pace his bedroom floor at night...he was just an anguished man."

With a few exceptions, the scientific community was enraged by this attack on a man they revered. For four weeks a glittering array of scientists trooped into the room as witnesses, most to rail against the committee and praise Oppenheimer. It must have seemed as if he were witnessing his own funeral. In a way, he was.

The board voted to remove Oppenheimer's security clearance. The hearings, his close friends and colleagues agree, destroyed him. He had been banished from the halls of power.

Defeated, the couple returned to Princeton. Speaking invitations poured in from all over the world, and Robert and Kitty traveled widely. But illness plagued them both. Kitty's pancreatitis was causing her problems, and the pain killers slurred her speech and caused raised eyebrows. Robert was once more down to 115 pounds. He smoked incessantly and would not give up his stiff martinis before dinner. Rumors about their drinking, hers particularly, blossomed again. His detractors called their house "Bourbon Manor."

It must have seemed as if he were witnessing his own funeral. The board voted to remove Oppenheimer's security clearance. The hearings, friends and colleagues agree, destroyed him. He had been banished.

By the 1960s, the political mood of the country had changed enough for the Oppenheimers to be invited to the Kennedy White House. In 1963 Oppenheimer, his family gathered around him, received the Fermi Award from President Johnson. Some saw this as Oppenheimer's "rehabilitation," but nothing was changed as long as his security clearance remained blocked.

That same year Kitty and Robert returned to Berkeley. It was a sentimental homecoming, with a crowd of 11,000 cheering him wildly. Oppenheimer phoned the owner of One Eagle Hill to ask if he might come by privately. She left the door open for him—so he could sit in the garden alone and remember.

Plagued by throat cancer, Robert Oppenheimer died in February of 1967. He was 62. Kitty scattered her husband's ashes in the clear, warm waters around the Virgin Islands. Then she and Toni moved into the little cottage on St. John. Gentle, loyal Robert Serber, whose wife had died some months before, became Kitty's confidant and companion. Peter, at odds with his mother, was living in the West. Kitty, Serber says, "had directed all of her energies into her husband's career. She felt an intense love and loyalty for Robert, and she felt it for her children as well." But it was her husband who consumed her. According to Serber, "After Robert's death, she had no will to live."

It became clear to Serber that Kitty was coming apart. She went off a mountain road on St. John and crashed her jeep down the hillside. The accident battered her face enough to require plastic surgery.

The only thing that kept Kitty going, Serber believes, was her concern for Toni, who had recently married a man of whom Kitty disapproved. In 1972, at Kitty's request, Serber bought a 58-foot ketch and hired a crew, and they set sail from St. John. Kitty was deeply depressed and drinking hard. They got as far as Panama when she became desperately ill. He took her to the hospital where, a week later, Kitty Oppenheimer died. "I think she drank herself to death, that it was deliberate," Serber says. She was 61.

In 1977 Toni committed suicide. Only Peter survives. He lives a quiet life in New Mexico and does not give interviews. He maintains friendships with some of his parents' old friends from the Berkeley and Los Alamos days. He is a gentle man, they report, and a good father to his own children. ▲

Shirley Streshinsky's most recent book is a biography of John James Audubon. Last summer she interviewed Dr. Robert Serber, confidant to both Oppenheimers, in New York City. Her search for One Eagle Hill ended only a few blocks from her own home in the Berkeley area.

ALONG THE EL CAMINO

Bill Workman

On Retirement Eve, Stanford Cop Reflects on Career

Campus police have evolved to professional status during 25 years

When Raoul Niemeyer took his job with Stanford University police nearly 25 years ago, the campus cops were viewed as little more than night watchmen who went on patrol in mud-brown former taxicabs.

At the time, Stanford was still recovering from the anti-Vietnam War turmoil of student unrest that had overwhelmed the ill-trained and poorly equipped campus police.

Niemeyer, a veteran San Jose cop with an outstanding record for training officers, was the first hire in newly appointed Stanford Police Chief Marv Herrington's efforts to modernize and improve the public safety department.

One of the first things Neimeyer did after signing on as captain was to order a white paint job, Stanford logo and red and blue emergency lights – replacing the old taxi-yellow ones – for the department's patrol cars.

His next move was to develop a recruiting and training program, still in place today, that routinely brings to the campus bright, eager deputies. By the time they have been assigned regular duties, they are well on their way to commanding the respect of the university community.

"One of the challenges has always been to get quality candidates who are able to communicate effectively with faculty, students and staff but also able to handle a hardened criminal from outside the campus when things get tough," recalls Niemeyer, 60, a wavy-haired man with the firm handshake of someone who spends his spare time working on and racing stock cars.

Niemeyer, who retires this month, reminisced the other day about his career and the changing demands of policing Stanford over the past quarter-century. The 8,000-acre campus is a city in its own right with a police force of 35 sworn officers.

Born in Berkeley and raised in Hawaii, Niemeyer said he had dealt with his share of homicide cases as a San Jose cop. Yet he was unprepared for the shock that awaited him and Stanford little more than a month after he went to work there.

Pulled out of bed by a dispatcher's predawn phone call the morning of Oct. 13, 1974, he went to Stanford Memorial Church where the half-nude body of 19-year-old Arlis Perry, recently married wife of a Stanford student, had been violated by altar candles.

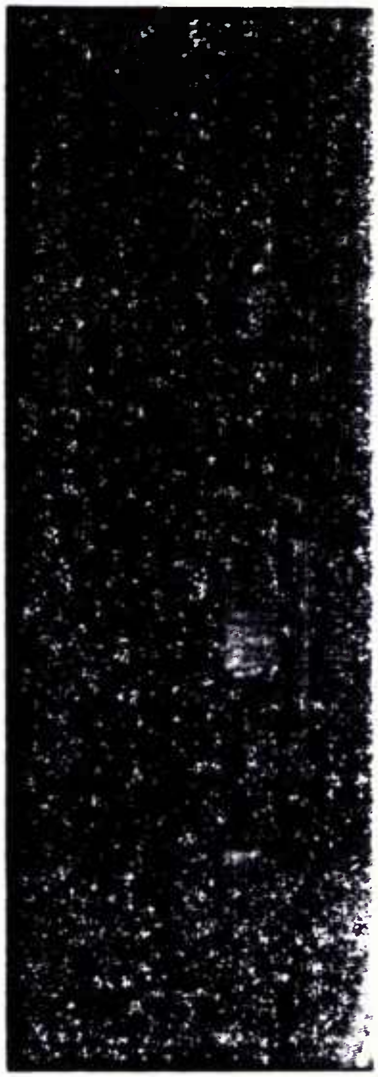
Perry had been stabbed in the head and strangled after she had gone alone to the historic church to pray and meditate the night before. She had apparently been accidentally locked inside with her killer.

"It was an awful thing. I was really upset and we became almost obsessed with trying to solve the murder," said Niemeyer.

Unfortunately, the brutal and bizarre slaying remains unsolved.

Slayings are rare at Stanford, but as the university's chief investigator most of his career, Niemeyer has played a key role in helping the campus community cope with the shattering effects of more than a half-dozen.

Perhaps the most stressful for faculty, he said, was the case of Theodore Streleski, a mathematics student for 19 years who in 1978 bludgeoned to death Professor Karel deLeeuw in revenge for wha



Captain Raoul Niemeyer of Stanfor

Of the many world figures he's met as a cop, Niemeyer said, one of the most interesting was the Dalai Lama.

MICHAEL MACOR / The Chronicle

I University's police department, who is retiring soon, reflected on his quarter-century career on the campus.

viewed as Stanford's unfair treatment of graduate students.

When Streleski was released from prison in 1985, Niemeyer was kept busy for months, he said, taking measures to ensure the safety of other math professors in the event Streleski returned to campus with more mayhem in mind.

On one wall of Niemeyer's office hangs a huge map of Stanford and surrounding communities that was once used in the prosecution of Robert Lee O'Connor, the so-called "jogging bandit" convicted in 1983 of 21 counts of burglary and suspected of ripping off about 500 Peninsula homes. The map's many dots represent burglary sites.

Niemeyer, who led the two-year Peninsula investigation of O'Connor, is taking the map home as a memento when he retires.

Because of its tradition-steeped setting, an otherwise minor disturbance at Stanford can often bring the media running, said Niemeyer, who has also been the department's press officer for years.

For example, there was the "kidnapping" in early November of the Stanford Tree's googly-eyed costume after a break-in at the Stanford Band's since-demolished building. The mascot theft came shortly before the annual Big Game with the University of California at Berkeley.

Bay Area media had a lot of fun with the story before the costume was finally returned by its unidentified Cal student captors. However, Niemeyer takes credit for bluffing the thieves into bringing it back by repeatedly posing the threat of possible criminal prosecution, although authorities apparently never intended to take the pranksters to court.

A more routine function for campus police has been providing security for visiting dignitaries, said Niemeyer.

Of the many world figures he met as a cop, Niemeyer said, one of the most interesting was the Dalai Lama. At the end of his latest Stanford visit a few years ago, the Tibetan religious leader gave Niemeyer an autographed copy of a book of his writings, and in return, Niemeyer fastened a Stanford police pin to the Dalai Lama's robe, he recalled.

As they were leaving for a news conference, Niemeyer said his VIP companion grasped his hand and the two men strolled out of the building hand in hand.

"I'm glad no one got any of it on film," said Niemeyer, recounting his momentary embarrassment at the gesture. "No man had held me by the hand since my dad when I was a kid."

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Weapon of Choice

After Nearly 30 Years, Sidewinder Missile Is Still Potent, Reliable

It Is Also Inexpensive, but It Almost Didn't Get Made; The Russians' Imitation

Spare Parts From Junkyards

By JOHN J. FIALKA
Staff Reporter of THE WALL STREET JOURNAL

In the early 1950s, one of the nation's most potent, most reliable and simplest weapons was born under a cover of deepest secrecy at a Navy test facility in California's Mojave desert.

Known by the code name "Local Project 612," the Sidewinder missile-development program was so secret that its very existence was kept from meddlesome Navy and Pentagon officials in Washington who were financing it.

And that, Sidewinder enthusiasts insist, is what made all the difference.

In an era when the Defense Department is plagued with criticism for buying $7,600 coffee pots, $400 hammers and a number of trouble-prone, multibillion-dollar weapons systems, the story of the Sidewinder is peculiar.

Next year the heat-seeking, air-to-air missile will experience its 30th year in the U.S. combat arsenal. Some military experts maintain that an updated version of the Sidewinder continues to be the nation's most effective and successful weapon. While many more expensive weapons systems have come and gone, the Sidewinder remains the fighter pilots' weapon of choice in the U.S. and in 19 other air forces around the world. The Sidewinder has been used in recent years by U.S. and allied forces to shoot down Libyan, Syrian and Argentinian fighter jets. The Russians have developed their own version of the missile.

Sidewinder missiles currently cost around $70,000, making them by far the cheapest air-to-air missiles in the Pentagon's inventory. (By comparison, Phoenix air-to-air missiles cost about $1 million each.) The Sidewinder's price is kept down by the fact that 14 companies, led by Raytheon Co. and Ford Aerospace & Communications Corp., compete to make its various parts.

But the Sidewinder represents a weapons-development adventure that probably can't be repeated, says Walter B. Laberge, one of its Navy developers. "I know

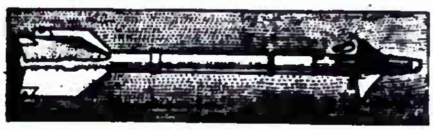
who now is in charge of research for Lockheed Corp.'s missile and space program.

Junkyard Scavenging

The Sidewinder was designed by a small team of men at the Navy's Naval Weapons Center at China Lake, Calif. The team, which included Mr. Laberge, was led by a physicist, the late William B. McLean. He scrounged money from other projects, scavenged spare parts from Pasadena junkyards and was driven by a passion for making cheap, simple things that work.

Current weapons-development programs, Mr. Laberge says, are rigidly generated from specifications set by the military—specifications, he says, that are "almost always asking for too much.

"The way the world works," he adds, "once you get the specification, you win the contract only by selling the customer



A Sidewinder Missile

what he wants. We set ourselves up by presuming we know the answer before we've done it."

That was not the way of Bill McLean, who sheltered the Sidewinder program from its critics and eventually forced the missile on a reluctant Pentagon. He ignored specifications and concentrated on concepts that his experiments told him would work.

The Sidewinder saga began in the late 1940s, when the Air Force and the Navy set the specifications for a new kind of missile. It had to perform in all weather conditions and had to be able to kill enemy aircraft by striking them head on.

Those specifications pumped billions of dollars into two major missile-development programs, the Navy's Sparrow and the Air Force's Falcon. They were to be highly complex, almost miniaturized fighter planes that carried their own radar beacons.

'Miserable Mathematician'

Mr. McLean, who arrived at China Lake in 1945, was sure the Sparrow-Falcon approach wouldn't work. "He (McLean) was a miserable mathematician, but he had an incredible feeling for how things should work," recalls Thomas S. Amlie, an assistant on the project who later became the director at China Lake.

What would work, Mr. McLean concluded, was a simple, rocket-propelled bomb with a heat-detecting device built into its nose. It would thus automatically home in on the intense heat of jet-engine exhausts.

Working part of the time in his garage, Mr. McLean designed about 85% of the missile himself. It had a total of nine moving parts and its "brain" consisted of seven radio tubes. It used gas pressure from burning rocket fuel to move its control fins and to generate a small amount of electricity.

There were no heavy batteries to wear down, no hydraulics systems to leak or

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Weapon of Choice: After 30 Years, Sidewinder Is Still a Potent Missile

Continued From First Page
freeze, no precision-machined tolerances. "It had the mechanical complexity of a small washing machine combined with a table radio," recalls Howard A. Wilcox, Mr. McLean's principal assistant at the time.

The first problem Mr. McLean ran into was money. China Lake, at the time, was operated by the Navy's Bureau of Ordnance. The Navy's Bureau of Aeronautics in Washington was in charge of all missile development, and it was devoting all of its money to the Sparrow.

China Lake had a small amount of discretionary funds; and when those were exhausted, Mr. McLean's superiors in the Bureau of Ordnance—which had an intense rivalry with the Bureau of Aeronautics—found ways to siphon money from other programs to keep Sidewinder going.

For a while Sidewinder existed as "Feasibility Study 567." Then a Defense Department official touring China Lake discovered the project and decreed that it be canceled because it would never work. After that Sidewinder went on as "Local Project 612."

Because of much tighter cost controls, "Today you couldn't hide a program like that," asserts Mr. Wilcox, who estimates that the total development cost of the Sidewinder came to $30 million.

Mr. McLean's next problem was testing. The first 13 shots of the Sidewinder failed. That, according to Mr. McLean's co-workers, would have also doomed a major development program. Officially, however, Sidewinder didn't exist. So Mr. McLean took what he had learned from the failures (caused by vibrations of the rocket interfering with electrical guidance) and applied that knowledge to a 14th shot, on Sept. 11, 1953. It hit the target.

Once the Sidewinder team demonstrated the missile could hit targets, the Navy's official interest was soon aroused. The Air Force, though, was resistant.

Then Mr. McLean worked out a deal with an assistant secretary of the Air Force: If the Sidewinder could beat the Falcon in a shoot-out, the Air Force would consider buying some.

There are many stories about the shoot-out, which was staged by the Air Force at Holloman Air Force Base in New Mexico in 1956. Mr. McLean and a few people he had brought with him from China Lake found themselves up against a team of technicians from Hughes Aircraft Co., which was developing the Falcon.

According to Mr. Wilcox, the Navy was given the corner of one hangar, an aging F-86 fighter and a few wrenches. The Air Force assigned itself a newer fighter and tons of Hughes test equipment to monitor the more complex and delicate Falcons.

Whether any of this bothered Mr. McLean was hard to tell. "He was an easy man to overlook in a crowd, although people never did because he was a rather nervous individual," recalls Mr. Wilcox. "He was always twitching slightly. His brain was always going at a mile a minute."

At one point, Mr. Amlie recalls, Mr. McLean decided to rattle the opposition by demanding that he be given some test equipment. What more did the Navy want? the Air Force asked. A stepladder and a flashlight, Mr. McLean responded.

Toss of a Coin

After a toss of a coin, the plane carrying the Sidewinder went up and knocked down the first target drone with one shot. For six straight days the Hughes team struggled with the Falcon, but it refused to leave the launcher. Then, out of desperation, the Air Force ordered another Sidewinder shot. Another drone was destroyed.

"Their missile was so damn complicated and expensive that they couldn't fire it unless it was perfect," Mr. Wilcox says. "Our philosophy was when the pilot hits the pickle (pilot slang for trigger), the missile goes."

This aspect of the Sidewinder wasn't lost on the U.S. pilots who later went into combat over North Vietnam, where the Sidewinder proved to be three to four times more effective than other missiles in dogfights. (The Pentagon also purchased and continues to use both the Sparrow and Falcon missiles.)

One combat veteran, Navy Cmdr. Charles M. deGruy, recalls that on the instrument panel of his F-4 Phantom there was a switch that selected the type of missile to fire:

"One said Radar (the Sparrow) and the other one was Heat (Sidewinder). Because of the relative confidence you had in the Sidewinder, most of the guys who crossed the beach went in there with their switch in the Heat position."

Russian Version

At about that time, the Russians unveiled the Atoll, a missile believed to be copied from stolen plans of an early version of Sidewinder. Atolls have since been sold to 25 other countries.

During the later 1970s, the Navy figured out several ways to improve the Sidewinder. For one, the heat-seeking element in the missile's nose was cooled by a bottle of liquid nitrogen, making it supersensitive.

When Sidewinders sense heat, they emit a low growling noise that is heard in the pilot's earphones. It means the missile has found a target. The older Sidewinder wouldn't growl until pilots approached the rear of an enemy plane.

With the more sensitive nose, Sidewinders can attack from the side and, in some cases, from the front, explains Capt. Lawrence E. Blose, the Navy's current Sidewinder program manager. "The fact that you can do that does wonderful things to the enemy's psyche," he says.

The first combat test of the improved missile came in August 1981 when two Navy F-14s encountered two Soviet-built Libyan fighters over the Gulf of Sidra in the Mediterranean. The Libyans fired their Atolls first and missed. The F-14s fired two Sidewinders. Both hit.

In June 1982, the Israeli air force shot down about 85 Syrian MiGs over the Bekaa Valley, losing only two of their own jets, both to groundfire. Although the specifics of this fighting are still secret, U.S. sources have been told that the Sidewinder was the major weapon used and that the ratio of its kills to missiles fired was over 80%. A spokesman for the Israeli air force won't confirm these figures. "But you can say," he adds, "that the Sidewinder is a good missile."

At about the same time, the Argentine air force was discovering just how true this was. According to Jeffrey Ethel, the co-author of a new book—"Air War South Atlantic"—that is based on interviews with both Argentine and British pilots who fought over the Falklands, British Sidewinders destroyed 19 airplanes out of 23 attempts.

The book notes that the Argentine pilots didn't help themselves any by assuming their best defense was to turn tail and try to outrun the slower British Harriers, a maneuver that set off Sidewinder growls in the British pilots' ears.

For his efforts on the Sidewinder, Mr. McLean received a $25,000 award from the government and a plaque from President Eisenhower. He was later transferred from China Lake to the Navy's submarine-warfare research center in San Diego, where he served as technical director until retiring in 1974. He died four years later.

To the end, he remained a maverick within the system. The Pentagon, he once complained before a Senate committee, has forgotten the need to experiment with prototypes.

"The total acquisition process," he said, "rewards the design of complex and expensive systems and penalizes work on simpler and therefore less expensive ones."

A Slight Memorandum of My Soldiering in the

2nd. Neb. Vol. Cav.

I joined the Neb. 2nd (Company C) on the 23rd. of Apr 1863 at Omaha N.T. Came home on the 24th, returned on the 26th and drew our clothing and part of our, arms , sabres and revolvers and on the 27th we started for Soiux city Iowa which place we reached on the 5th of May. Our march up here was pleasant and agreeable to most all. We got Hay and Corn from the farmers along the road for our horses and mules. The camping places were good- one of the boys of our Comp (S0) was left sick at Seargents Bluffs - who afterwards died with the Rheumatism and was buried at the same place. The country between Council Bluffs and Soux city is very good Though Timber is scarce. We crossed Soux river on the 5th of May went in to camp (at Camp Cook) on the little strip of Decota which runs down between the two rivers — —(?) here we remained for six weeks outfitting our Brigade for a long expedition up the river. WE found the Iowa 6th Cav here which came up a few days before us _ here we recd our guns (Enfield rifles) and other equipage - During our stay here we were paid off we drilled from 2 to 4 hours a day most of the time - our guard duty was heavy here . Sometimes we would serve on gard every other day (the distance from Council Bluffs to Siux City 110 mi.) The Col. (Furnace)here commenced putting on military power to the utmost extent- One morning the details for gard rebelled and said they would not stand so much gard duty which was entirely unnecessary - and it was quite a while before the officers could persuade the boys to come out to gard mount. The boys got so embittered at the Col. tyranical orders - many of them I dare say would have taken his life if they could have had a good chance - While we were here Gen Cook was removed and Gen Sully put in his stead. On the 20th of June the remainder of us left Camp Cook for our northern march- The 1st and 2nd Battalions of the Iowa 6th and one Batt of our Reg had already gone - The Gen. deemed it best for us to go up by Battalions until the grass would get of sufficient growth for our horses to subsist on in part at least, Vermillion is a small town 95 (?) mi above Soux city . On our arrival here we were saluted with the roar of the cannon Yancton (the capitol of Decota) is on the Mi.-river in a very nice place for a town if it only had the country to back it but the country is almost barren and worthless, it has a few dozen houses in it one store 2 or 3 shops is about all We got to Fort RAndall on the 28th of June (distance from Soux city to Ft.Randall 150 miles) The surrounding country is very broken and worthless for anything no timber only a little cotinwood along the river.

On the 29th it rained most all the day the ground we were camped on was rather low and it got very muddy so we pulled up stakes and moved up on the —— —(?) a very beautiful place. Here we remained until the 5th of July. Here we reloaded our wagons and made ready for another march. The 4th day of July was a day on which nothing was going on in camp to distinguish it from any other day save a few rounds of our artillary. WE left Ft. Randall Sunday 5th of July Traveled 12 miles road poor, Co. S's Commissary wagon turned over on a hillside and emptied its contents down the hill. Several guns were broken sabres bent and things torn up in general. The grass was very poor today.

The 6th we traveled 16 miles, camped on Pratt creek,the country very broken,grass very poor almost dry enough to burn. I went fishing caught a couple of gars(?) One of the boats ran on a sand bar and had to unload,another one 12 mi aboveunloaded 300 or 400 sacks of corn. The weather very hot. Co. S (G?) was left to gard the freight that was unloaded. Tuesday we laid still Wednesday 8th travelled 22 mi the weather very warm. grass poor , several Antelope were seen , THe 9th traveled 25 mi. grass alittle better, the hotest day we had, some of the boys killed some Foxes

Friday, 10th travelled 10 mi and camped on Crow Creek, plenty of wood and water but no grass hardly. The grass was most all burnt off by the Indians.

Saturday 11th. Traveled 12 mi and camped on the Missouri River, plenty of wood water and grass, two of the wagons turned over today. WE unloaded all of the wagons in the evening. We camped two miles above the agency,nice place.

Sunday 12th nothing of importance transpired , we laid still, stacked up some grain.

Monday 13th Co. K(Neb.) and the Iowa boys came up with the other trane. We had to lay up here for a spell waiting for supplies

Tuesday 14th All quiet - the trane went back where the boat unloaded --

Wednesday 15th The Indians had awar dance in front of the Colonel's tent.-- These were the Winnebago Indians : They were removed from Minesota here the spring (1860) The Government is braking Prairie for them.-They seem dis-satisfied, say they won't stay here.Government is also building a Fort by the name of Lookout, the Indians number 2,500 -- -- ---- --- ---- ----

Thursday17th 7 companies of the Neb. 2nd were paid off. The wind blew very hard again all day and night. About 10 o'clock at night we had a rain storm

Saturday 18th Got the news-news-- that Vicksburg was taken. the band was round at 12 o'clock at night, giving music in honor of its fall.

Sunday 19th- I worked all day stacking grain on the river bank

Monday 20th- Company G and the trane came up

Tuesday 21st- all quiet

Wednesday 22nd- the same

Thursday 23rd- there was 5 Buffalos across the river

Friday 24th- there was two boats came down the river, the Shreveport and the Robert Cambel they were going to go by without stoping until a shot from a 6 pounder brought the Shreveport to shore. the Robert Cambel did not stop for the first shot so aball was sent across her bow which brought her too. The Bell Peora came up. Bob Campbel went on down in the evening. Nothing of importance transpired until the night of the 29th a man by the name of ------ (of Co. F) shot at another man and missed him and hit two more men in bed asleep. He was taken to Ft. Lookout and placed under gard.

July 30th- I went down to the Ft. Got me a pr. of boots, the place is improving very fast. I sent $40.00 home on the 29th July

- Distance from Ft. Randall to Ft. Lookout 97 miles-

We left Camp no. 13 on the 31st of July. We left about 100 of the boys that were not able to go, we traveled 8 mi and camped in a little creek, grass and water very poor

Aug. 1st-traveled 30 mi and camped on another little creek. Plenty of wood grass and water. During the day we passed thousands of Indian graves. Each grave was surrounded with a ring of stones laid on top of the ground.

Aug. 2nd- traveled 11 mi and camped on the Missouri river. We got our dinners and Co. L, K, H, recd orders to go with the trane 7 mi further. Some of the grass was burnt off. We camped on the river again.

Aug 3rd-the rest of the Reg. came. We moved up the river 5 mi where the grass was good and wood and water handy. This (camp no. 17) 1 mi below FT. Pierre

- Distance from Lookout to Pierre 83 mi-

The General made us a visit at night. We had a storm at night but very little rain, the wind would blow from one direction and then from another in kind of whirls, it was the hotest wind I ever felt. The ground was very dry and dusty at first. The wind blew our tentsmost all down and the dust was almost enough to strangle a person. Finally part of our horses made a stampede, the boys broke for the wagons to keep from being run o ver by the horses. The wind carried away lots of the boys hats, but most all were found next morning.

Aug. 4th-Nothing of importance transpired, a lot of the officers went up to the fort. some of the boys caught a young deer.

Aug.5th- in the evening a fire broke out about 3 mi down the river,a detail was sent down which soon put it out.

Aug.6th- I went out in the bluff and got some/~~cherries~~/~~and~~/ plums and choke cherries

Aug.7th- in the evening we had an awful hard storm of wind,but very little rain The first dash of ~~/ ~~/ wind flattened our tents to the ground and the dust blew so we couldnt ~~~~ see 5 steps part of the time, men were running in every direction in search of hats the wind had blown off, and putting out fires, some of the tents caught afire and almost burnt up. The fire got out in the bottoms and the men rushed down the hill with sacks and anything they could get most to beat it out and it was soon extinguished. At roll call we elected a new Commissary(Mr. Haring) for our Co., the Co. became dissatisfied with the old one (A. J. Comstock)

Aug. 8th- the boats went up . All quiet

Sunday 9th- we had preaching at 11 o'clock A.M and at 3 P.M. by L.M. Smith

Aug. 10th-Nothing happened worth mentioning

Aug 11th- I went out on a hunt ,the country looks desolate and dreary, some of the hills are covered with rocks others are the regular black hill no grass on them

Aug 12th- we took up our march northward again, traveled 9 mi and camped on the hig-h bluff opposite Ft. Pierre. We hauled wood and water from the river.

Aug.13th- traveled 15 mi and camped on a little creek (name Beaver), grass poor Here the whole command got together, our co. was on picket gard at night,seen some little game

FRiday 14th- traveled 25 mi. grass and water very poor . seen but little game. we used Buffalo chips for wood. The prairie we marched over today w as mostly level.

Saturday 15th- traveled 16 mi and camped at the mouth of Cheyenne creek . The country more broken

Sunday 16th- moved up the river 2 mi for better grass , a boat un loaded a lot of corn for us a mile above

Monday 17th- We laid still. The Col. made us aspeech at night

18th- nothing of importance transpired

19th- the boat came up

Thursday 20th- we had an awful hard hail storm. our horses broke for the river and ran into it. some swam to the other side,it rained very hard too.lightening and thundering almost incessant. The storm was terrific indeed. Soon the ground was flooded with water,after the storm was over Whiskey was issued to all the Soldiers. This was one of the sickening scenes to me, some of the men were soon staggering about in the mud and water cursing and swearing at dreadful rate. It's a wonder to me how such beastly beings live as long as they do. Surely this world would be much better off if they were somewhere else. I think Whiskey sinks a person deeper in the regoins of despair than any other evil on earth. There was several fights in the evening caused by the Whiskey A lot of cowards and sick boys went back to Ft. Pierre from here to wait for our return from the north .

The Iowa 6th had just got in their saddles to march when the storm came up. Some of the teams ran off and upset the wagons in the cre k. the water was soon rushing down over them. We sent our baggage an' mess boys down to Ft. Pierre on the boat.

Friday 21st- the Iowa 6th and Brigade trane went out 8 mi.

Saturday 22nd- We left camp no. 22 traveled 24 mi. in a N.E. direction leaving the river, as we got out on the broad level plain the grass got a great deal better. It was quite good.

Sunday 23rd- traveled 16 mi and camped on a little creek 4 mi from Swan Lake Mc Cormack(the Regimental quatbtermaster) drove a cripled Buffalo into camp or close to camp and the whole brigade almost ran with guns and revolvers to help kill the monster. The work was soon done and now to get him to camp was the next job. We soon had Picket roaps tied together and a hundred men hold of them brought him in quite easily. This was our first Buffaloo. The weather was quite cool. We had considerable ice. We camped in three mi of some Indians 300 in number that night. I believe the General made a treaty with them the next day.

Monday 24th- traveled 16 mi camped on another little creek.Grass very good not much game,a few buffalo.

25th- traveled 24 mi , splendid grass, seen lots of buffalo and antelope Killed 8 or 10 buffalo, had agay after them , no timber to be seen, the weather still very cool

Wednesday 26th- traveled 40 mi and camped on Beaver creek. We got into camp about 10 o'clock at night- seen thousands of Buffalo. Killed 50 or 60. the General let out a hunting party each morning of two men from each Co., but not withstanding his strict orders forbidding than these going out of the lines, it seemed it was too tempting for the boys to see so many of these animals and it was such good sport, they would slip out whenever a half of chance would present itself. There were four boys out by themselves seen a herd of buffalo comeing by so they dismounted and staked down their horses and thought they would shoot on foot and so the hrrd came by the horses jerked their picket-ropes in two and away they went with the herd leaving the boys afoot several miles from camp. Two of the horses was got the next day without saddles. I presume the Indians got the other two, this is some of the fruits of disobeying orders. We captured several Indians today.

They looked worse than the Buffalo such nasty dirty looking wretches I never beheld before.

Thursday 27th-traveled 5mi. camped on another little creek. Weather very cool Had to walk with our overcoats on to keep warm.

28th- Traveled 30 mi-camped on a slough. seen a good many buffalo . Killed quite a number, a buffalo calf ran inside of the command and was caught alive about half of the command got after it beating it over the head and back with their sabres. So mixed up was the crowd the General was obliged to call a halt until things could be straightened out. We captured a crippled Indian. He said (so I understand) Gen. Sibly shot part of his foot off. He said also there were 15,000 Indians not far ahead of us.

Saturday 29th- our battalli ~~n~~ Companies E & G of the Iowa 6th went out on a scout to the river, scouted the bluffs along the river for several miles -went down to the river and camped.

Sunday 30th- wentup the river a few miles to a grove of timber where Capt. Bayne said he seen some Indians the evening before. We formed in line at the edge of the grove and sent in scouts that scoured the woods through and through but narry Ingine could be found. Up the river a little farther we found an old wagon wheel supposed to be one that Gen. ~~Kearney~~ (Harny?)lost in his expedition several years ago. My Conerad (Zack) left camp left camp the day very unwell had now become very sick so we fell behind to avoid the dust- he kept getting worse all day and I began to think it very doubtful whether we should be able to get into camp which was 50 or 60 miles from where we started in the morning. Our march was slow on account of him being so sick. we would have to stop very often to let him rest. The main difficulty was in getting water, there was none between the river and camp. I of course reserved what I had in my canteen for him, and so we made out to get back to the camp we left the morning before. The remainder of the command had moved 30 miles, so I and Zack and 9 others stoped until morning 30 miles from the command. The Indians we took prisoner afterwards told us they seen us there that night but was afraid to kill us for fear they would be the worse for it. In the morning Zack was quite better and we started on after the command. After we had got a few miles we met an ambulance coming after Zack so we were all right now. We had for our supper and breakfast fresh buffalo meat broiled on the coles, and I tell you it was well relished by all except Zack for we had had nothing to eat since morning and very little then. We passed Long Lake (a very beautiful lake) in the fore noon stopped at White Lake (a very beautiful lake) for dinner and to rest our horses. This was 30 mi from where we started this morning, also where the command had stayed all night. After dinner we resumed our march, traveled 10 mi where the command camped, the grass was very good, the land more hilly, good spring water to drink. We passed a good many lakestoday. Long lake is about 15 mi long. Buffalo not so plenty . the last ttn mi was mostly sand hills.

Sept 1st- traveled 28 miles and camped by a lake, passed quite a number of lakes through the day. The country is more hilly and rocky. The water in most of the lakes is a little salty. Scouting parties are out every but see no Indians

Wednesday 2nd- traveled 25 miles and camped by another lake.,seen where the Indians had killed lots of Buffalo . About two hours after we got into camp the news came of several thousannd of Indians close by. We were just getting our grub ready to eat (some were more lucky than others having got through with supper) everthing was dropped , in about 15 minutes we were in full speed after the red man. In our dash some of the boys lost their hats, but could'nt take time to stop and get them and away they went bareheaded. Once in a while a horse would stumble and fall over the rocks (which were very abundant), pitching the rider probably a rod or two, but as far as I know no one was hurt. About two hours brought us up in line of battle, we (Neb. 2nd) waited a few minutes for the Iowa 6th and Gen. Sully which could not keep up. We (Neb 2nd) formed on the right, Iowa 6th on the left. Our battallion ~~ba~~ dismounted and fought on foot. We fired one round at them about three hundred yds. off, then marched ~~d/~~ down the hill to within about 50 yds. of them and opened our fire. It was now nearly sundown. The Indians had formed in a hollow square, women, children ,and ponies in sid, Warriors out sid . Just before the fight commenced the noise was terrific , women children were crying ,ponies neighing, Indians singing their war songs, and their dogs (which numbered about 1500) seemed to be all howling. The noise was almost deafening, in a few minutes however nothing was heard save the roar of our rifles and our revolvers. The balls from the enemy would whizz by our ears on our right and on our left thick and fast which told us that many of our brother Soldiers - - - - - -

Samuel Collins Fitzer

VOLUNTEER ENLISTMENT.

TERRITORY OF NEBRASKA.

TOWN OF Omaha

I, Samuel C Pitzer born in Magoupin Co in the State of Illinois aged Twenty two years, and by occupation a Farmer Do hereby acknowledge to have volunteered this Twenty Third day of April 1863, to serve as a **Soldier** in the **Army of the United States of America**, for the period of NINE MONTHS, unless sooner discharged by proper authority: Do also agree to accept such bounty, pay, rations, and clothing, as are, or may be, established by law for volunteers. And I, Samuel C Pitzer do solemnly swear, that I will bear true faith and allegiance to the **United States of America**, and that I will serve them honestly and faithfully against all their enemies or opposers whomsoever; and that I will observe and obey the orders of the President of the United States, and the orders of the officers appointed over me, according to the Rules and Articles of War.

Sworn and subscribed to at Omaha N.T.
this 23rd day of April 1863.

BEFORE

I CERTIFY, ON HONOR, That I have carefully examined the above named Volunteer, agreeably to the General Regulations of the Army, and that in my opinion he is free from all bodily defects and mental infirmity, which would, in any way, disqualify him from performing the duties of a soldier.

EXAMINING SURGEON.

I CERTIFY, ON HONOR, That I have minutely inspected the Volunteer, Samuel C Pitzer previously to his enlistment, and that he was entirely sober when enlisted; that, to the best of my judgment and belief, he is of lawful age; and that, in accepting him as duly qualified to perform the duties of an able-bodied soldier, I have strictly observed the Regulations which govern the recruiting service.

This soldier has Blue eyes, Brown hair, Dark complexion, is five feet five inches high.

2 Regiment of Neb. Cav Volunteers.

DECLARATION OF RECRUIT.

I, Samuel C Pilzer desiring to *VOLUNTEER as a Soldier in the* **ARMY OF THE UNITED STATES,** *for the term of NINE MONTHS, DO DECLARE,* That I am Twenty Two *years and* two *months of age; that I have never been discharged from the United States service on account of disability or by sentence of a court-martial, or by order before the expiration of a term of enlistment; and I know of no impediment to my serving honestly and faithfully as a soldier for nine months.*

GIVEN at Omaha Neb. Ter

The 23rd day of April 1863

Witness:

Samuel C Pilzer

Volunteered at Omaha N.T.

April 23rd 1863.

By

2d Regiment of Nebraska Cavalry.

enlistment; last served in Company ( )

Reg't of

Discharged

18

CONSENT IN CASE OF MINOR.

I, Do CERTIFY, *That I am the*

of *that the sai*[illegible]

years of age; and I do hereby freely give my CONSENT *to his volunteering as a* SOLDIER *in the* ARMY OF THE UNITED STATES *for the period of* NINE MONTHS.

GIVEN at

To All whom it may Concern.

Know ye, That Saml. C. Titus a private of [illegible] Company [illegible] Regiment of Neb. Vol. Cav [illegible] was enrolled on the 23d day of [illegible] one thousand eight hundred and [illegible] to serve [illegible] Months is hereby Discharged from the service of the United States this 24th day of Dec 186[illegible] at Falls City Neb by reason of term expired.

(No objection to his being re-enlisted is known to exist.)*

Said Saml. C. Titus was born in McGoupin Co, in the State of Illinois is 22 years of age, 5 feet 5 inches high Light complexion, Blue eyes, Brown hair, and by occupation, when enrolled, a farmer.

Given at Falls City this 24th day of December 1863

* This sentence will be erased should there be any thing in the conduct or physical condition of the soldier rendering him unfit for the Army.

J W Pearman

Major 2nd Neb. Cav. Com'd'g.

Advertiser Print, Brownville, N. T.

OATH OF IDENTITY

Of

County of

day of in the
eight hundred and sixty- , personally appeared before me
a Justice of the Peace for the County and
who being duly sworn accord
res that he is the identical
in the company commanded by Capta
in the regiment
that he enliste
day of for the term
and was discharged at
day of
of by reaso

Sworn and subscribed before me the day and year above written.

I certify that before whom the abo
affidavit purports to have been made is a Justice of the Peace duly authorized
administer oaths, and that the above is his signature.

IN WITNESS WHEREOF I have hereunto set my han
L. S. and affixed my official seal, this day of
in the year
at in the State of

Clerk of the

INDEX--Kenneth and Jean Pitzer

Sally Smith Hughes

Graduated from the University of California, Berkeley, in 1963 with an A.B. degree in zoology, and from the University of California, San Francisco, in 1966 with an M.A. degree in anatomy. She received a Ph.D. degree in the history of science and medicine from the Royal Postgraduate Medical School, University of London, in 1972.

Postgraduate Research Histologist, the Cardiovascular Research Institute, University of California, San Francisco, 1966-1969; science historian for the History of Science and Technology Program, The Bancroft Library, 1978-1980.

Presently Research Historian and Principal Editor on medical and scientific topics for the Regional Oral History Office, University of California, Berkeley. Author of *The Virus: A History of the Concept*, Sally Smith Hughes is currently interviewing and writing in the fields of AIDS and molecular biology/biotechnology.

GERMAINE LaBERGE

B.A. in European History, 1970, Manhattanville College
Purchase, New York

M.A. in Education, 1971, Marygrove College
Detroit, Michigan

Law Office Study, 1974-1978
Member, State Bar of California since 1979 (inactive status)

Elementary School Teacher in Michigan and California, 1971-1975

Legal research and writing, drafting legal documents, 1978-1987

Volunteer in drug education and hunger programs,
Oakland and Berkeley, California

Interviewer/Editor in the Regional Oral History Office in fields of business, law, social activism, water resources, and University history, 1987 to present. Project Director, East Bay Municipal Utility District Water Rights Project