CONNECTIONS

The EERI Oral History Series

Joseph Penzien

Stanley Scott
Robert Reitherman
Interviewers

CONNECTIONS

The EERI Oral History Series

Joseph Penzien

CONNECTIONS

The EERI Oral History Series

Joseph Penzien

Robert Reitherman and
Stanley Scott, Interviewers

Earthquake Engineering Research Institute

Editor: Gail H. Shea, Albany, California
Cover and book design: Laura H. Moger, Moorpark, California

Published by the Earthquake Engineering Research Institute
499 14th Street, Suite 320
Oakland, CA 94612-1934
Tel: (510) 451-0905 Fax: (510) 451-5411
E-mail: eeri@eeri.org
Web site: http://www.eeri.org

EERI Publication Number: OHS-11
ISBN (pbk.): 0-932884-01-7
Library of Congress Cataloging-in-Publication Data
Penzien, Joseph.
Joseph Penzien / Robert Reitherman and Stanley Scott, interviewers.
p. cm. -- (Connections: the EERI oral history series)
Includes bibliographical references and index.
ISBN 1-932884-01-7 (pbk.)
1. Penzien, Joseph. 2. Civil engineers--United States--Biography. I. Reitherman, Robert, 1950- II. Scott, Stanley, 1921- III. Earthquake Engineering Research Institute. IV. Title. V. Series.
TA140.P37A3 2004
624'.092--dc22

2004020444

Printed in the United States of America

1 2 3 4 5 6 7 8 10 09 08 07 06 05 04

Acknowledgments

The help, encouragement, and editorial feedback of EERI Executive Director Susan K. Tubbesing and the EERI Board of Directors were instrumental in both establishing *Connections: The EERI Oral History Series* and in bringing this volume to publication.

EERI gratefully acknowledges partial funding of this project by the Federal Emergency Management Agency and the National Science Foundation.

Table of Contents

The EERI Oral History Series

This is the eleventh volume in *Connections: The EERI Oral History Series.* The Earthquake Engineering Research Institute initiated this series to preserve the recollections of some of those who have had pioneering careers in the field of earthquake engineering. Significant, even revolutionary, changes have occurred in earthquake engineering since individuals first began thinking in modern, scientific ways about how to protect construction from earthquakes. The *Connections* series helps document this important history.

Connections: The EERI Oral History Series is a vehicle for transmitting the fascinating accounts of individuals who were present at the beginning of important developments in the field, transmitting sometimes little-known facts about this history, and recording their impressions, judgments, and experiences from a more personal standpoint. These reminiscences are themselves a vital contribution to our understanding of where our current state of knowledge came from and how the overall goal of reducing earthquake losses has been advanced. The Earthquake Engineering Research Institute, founded in 1949 as a nonprofit organization to provide an institutional base for the then-young field of earthquake engineering, is proud to help tell the story of the development of earthquake engineering through these *Connections* volumes. EERI has grown from a few dozen individuals in a field that lacked any significant research funding to an organization with nearly 3,000 members. The organization is still devoted to its original goal of investigating the effects of destructive earthquakes and publishing the results through its reconnaissance report series. EERI brings researchers and practitioners together to exchange information at its annual meetings, and via a now-extensive calendar of conferences and workshops provides a forum through which individuals and organizations of various disciplinary backgrounds can work together for increased seismic safety.

The EERI Oral History Series was initiated by Stanley Scott (1921-2002). The first nine volumes were published during his lifetime, and the tenth was issued shortly thereafter. Scott's work summed to hundreds of hours of taped interview sessions and thousands of pages of transcripts. Were it not for him, valuable facts and recollections would have already been lost. Scott was a research political scientist at the Institute of Governmental Studies at the University of California at Berkeley. He was active in developing seismic safety policy for many years, and was a member of the California Seismic Safety Commission from 1975 to 1993. Partly for that work, he received the Alfred E. Alquist Award from the Earthquake Safety Foundation in 1990.

Recognizing the historical importance of the work of those whose careers in structural engineering, geology, and other disciplines extended back to the first half of the twentieth century, Scott began recording oral history interviews in 1984 with his first subject, Henry Degenkolb. Based on the success of those recorded conversations, Scott embarked on an expanded effort, which matured into EERI's Oral History Series. He consulted Willa Baum, director of the University of California at Berkeley Regional Oral History Office, a division of the Bancroft Library. Since its inception in 1954, the Regional Oral History Office has carried out and otherwise promoted oral history interviews on a wide range of subjects, including science and technology, natural resources and the environment, politics and government, law and jurisprudence, and in many other areas. The Regional Oral History Office approved an unfunded interview project on earthquake engineering and seismic safety, and Scott was encouraged to proceed. Following his retirement from the University in 1989, Scott continued to pursue the oral history project. For a time, some expenses were paid from a small grant from the National Science Foundation, but Scott did most of the work pro bono. This work included not only the obvious effort of preparing for and conducting the interviews themselves, but also the equally time-consuming task of reviewing and editing transcripts.

In his oral history research, Scott included a selection of senior earthquake engineers who were present at the beginning of the modern era of earthquake engineering. The term "earthquake engineering" as used here has the same meaning as in the name of EERI—the broadly construed set of disciplines, including geosciences and social sciences as well as engineering itself, that together form a related body of knowledge and collection of individuals. The events described in these oral histories span research, design projects, public policy, and education.

Connections: The EERI Oral History Series is currently pursuing an ambitious program of publishing selected past interviews begun or completed by Scott, and bringing new interview subjects into the series.

Published volumes in *Connections: The EERI Oral History Series*

Henry J. Degenkolb	1994
John A. Blume	1994
Michael V. Pregnoff and John E. Rinne	1996
George W. Housner	1997
William W. Moore	1998
Robert E. Wallace	1999
Nicholas F. Forell	2000
Henry J. Brunnier and Charles De Maria	2001
Egor P. Popov	2001
Clarence R. Allen	2002
Joseph Penzien	2004

EERI Oral History Committee

Robert Reitherman, Chair
William Anderson
Roger Borcherdt
Gregg Brandow
Ricardo Dobry
Robert Hanson
Loring A. Wyllie, Jr.

Foreword

The task of continuing the EERI Oral History Series after the death of Stanley Scott in January 2002 was difficult because of the high quality and original touch with which Scott marked the program. But in another way, the task was easy. It was obvious that what was called for was simply to continue and extend his work, not to meddle with his proven pattern of success. This was especially true with respect to this particular volume, which is a combination of the interviews Scott conducted in 1993 with Joseph Penzien and the interviews I conducted with Dr. Penzien a decade later.

Stanley Scott was a political scientist at the University of California at Berkeley Institute of Governmental Studies, and was its director for three decades (1958-1988). He began his tape-recorded interviews, which would later mature into the EERI Oral History Series, in the mid-1980s. Scott's involvement in earthquake engineering goes back to well before the February 9, 1971, San Fernando earthquake in the Los Angeles area. It was the San Fernando earthquake disaster that, along with the 1964 Alaska earthquake, caused a sudden step-up in funding, research, and public attention. Just as those few who traveled to California prior to the Gold Rush of 1849 were in the vanguard of other pioneers who followed, so it is that those who devoted their careers to earthquake matters prior to 1971 in the earthquake engineering field—especially from a social science perspective such as Scott's—can be considered early explorers. Why did Scott veer from the political science mainstream into the realm of earthquake engineering?

In 1968, the Institute of Governmental Relations, where Scott was director, published Karl Steinbrugge's classic *Earthquake Hazard in the San Francisco Bay Area: A Continuing Problem in Public Policy.* This was a technically sound book that was also written and illustrated to be accessible to those who were not earth scientists or engineers. It was sparked by conversations between the two men. Steinbrugge was a structural engineer, the head of the earthquake underwriting department of the Pacific Fire Rating Bureau (later Insurance Services Office, or ISO), and a structural engineering professor in the architecture department at the University of California at Berkeley. Steinbrugge investigated significant earthquakes from the early 1950s through the 1980s, and was the lead author of many important field investigation reports. The major theme of earthquakes in Scott's career, as well as significant events in the wider world of seismic safety, hinged around Steinbrugge's 1968 book. In Scott's words:

> The ideas in Steinbrugge's report were immediately picked up by Bay Area state Senator Alfred Alquist, who got a joint legislative committee going on a shoestring budget in 1969. The topic's urgency was highlighted by the 1971 San Fernando earthquake, giving Alquist's effort a much-needed push. Thus began the state's great involvement in the earthquake problem. It had the unexpected personal payoff of launching me on a parallel career in seismic safety and earthquake engineering policy, which included 18 years on the Seismic Safety Commission.[a]

Steinbrugge was, like Scott, a pioneer in the way his career extended into another discipline. In Steinbrugge's case, he branched off into public policy, as one can tell from the subtitle of his 1968 book. One can also judge the extent of his influence in public policy from the fact that he played the leading role as the chair of important federal earthquake policy studies,[b] was the engineering mastermind behind all the first-generation federal earthquake loss estimation studies,[c] was a core member of the group who produced the State of California's influential *Meeting the Earthquake Challenge: Final Report of the Joint Committee on Seismic Safety*,[d] was the first chair of the California Seismic Safety Commission, and, like Scott, was instrumental in the development of the seismic safety policies of a number of other agencies.

---

a. "IGS's Early Work Helped Shape Bay Area Institutions," available from the website of the University of California at Berkeley, Institute of Governmental Relations: http://www.igs.berkeley.edu.

b. Steinbrugge, Karl V., editor and chair. *Task Force on Earthquake Hazard Reduction: Program Priorities*. Washington, DC: U.S. Government Printing Office, 1970.

c. Algermissen, S. T. et al., *A Study of Earthquake Losses in the San Francisco Bay Area: Data and Analysis*. Washington, DC: National Oceanic and Atmospheric Administration, 1972.

d. California Legislature, Sacramento, California, January 1974.

This oral history with Joseph Penzien began with interviews conducted by Stanley Scott in 1993. Those interviews covered Penzien's life and career up to roughly the 1989 Loma Prieta earthquake, but major research and consulting engineering themes in his career, such as the seismic analysis of bridges or of strong ground motions, had not yet entered the conversation. In picking up with the new interviews where those from 1993 left off, there was a great deal of Joe's career still to be covered up through 1993, plus the passage of an additional ten years. This introduced the editorial challenge of combining two sets of interviews that occurred a decade apart, while trying to handle the transitions so as not to interrupt the reader's feeling of sitting in on one continuous conversation.

My interviews with Joe Penzien took place in 2002 and 2003. The method used to compile this oral history is essentially the same as that used by Stanley Scott. What is printed here is accurate and can stand up to scholarly scrutiny, but it is not a transcript. The reader will appreciate the fact that digressions such as "Excuse me Joe, but I have to go put more money in my parking meter" have been edited out. Joe and I revised some wording on the transcripts after interviews were completed to improve clarity. A common case was to add the first or last names of individuals or to insert a phrase to improve the conversational flow and form a more an orderly presentation. To this end, some sentences or paragraphs were rearranged so as to group like material together. I also exercised an editor's selective function, as did Scott, and condensed material in some places. From the first generation interview transcripts, which are relatively voluminous, versions were produced that edited out repetitive material. After review by Joe Penzien, the more concise versions became the chapters presented here. In this process of condensation, nothing of historical significance has been left out.

Leah Radke, CUREE administrative assistant, did a careful job of transcribing the taped interviews. In producing a document that progresses through various revisions, typographical and other errors inevitably creep in. Obvious misspellings are easy to find, but the more insidious editorial errors camouflage themselves by their plausibility upon first reading, or require technical background to see that something is amiss. Joe Penzien himself was the primary quality control filter through which these pages passed. The thorough review of the manuscript by Robert Hanson, member of the EERI Oral History Committee, who spotted several subtle-but-significant errors that would otherwise have escaped detection, is gratefully acknowledged. The equally thorough editorial review by Anil Chopra was also essential to the process of getting the details right. In addition, Professor Chopra wrote the Personal Introduction, which the reader will find to be an interesting and engaging way to begin this volume's conversation with Joseph Penzien. As with the previous *Connections* volumes, editor Gail Shea thoroughly scrutinized the final manuscript and improved its quality.

Robert Reitherman
Editor and Interviewer
Executive Director
Consortium of Universities for Research in Earthquake Engineering
February 2004

Personal Introduction

I am delighted to contribute in a small way to Joseph Penzien's oral history, for he was my mentor. I took his graduate classes, had the privilege of doing research under his supervision, and worked with him as an academic colleague and professional collaborator. Almost my guardian angel, if you will, he offered opportunities and encouragement at precisely the most critical junctures in my career.

I first met Joe in the spring of 1962 at the University of California at Berkeley, when I took the graduate course in Dynamics of Structures. Ray Clough was the instructor for the first half of the course. He then left for a UNESCO seismological mission to the Mediterranean and the Middle East, and Joe taught the latter half. The following year, under Joe's supervision, my research for the master's degree addressed a problem suggested by John Blume's consulting office. They were designing a small but important addition to the top of an existing building in San Francisco. Neither Joe nor I can recall the nature of this addition, but I do remember that we found that the natural vibration frequency of this light appendage was close to the fundamental frequency of the building. We realized that we'd better be careful. We computed what seemed like unbelievably large forces in the light appendage when it was tuned to the building. These results were vindicated a year later when the great Alaska earthquake of 1964 destroyed penthouses of buildings only lightly damaged. I single out this research experience among our many collaborations because this project with Joe got me interested in earthquake engineering and launched my life-long interest in this subject.

Joe was appointed Assistant Professor of Civil Engineering at the University of California, Berkeley in the fall of 1953, and retired in the summer of 1988. Over his 35-year academic career, Joe taught several different courses in structural engineering and structural mechanics. He, together with Ray Clough and Vitelmo Bertero, developed the teaching program in structural dynamics and earthquake engineering at Berkeley, which many considered to be the best in the world. Although he taught all of these courses at one time or another, Joe's unique contribution was the course on random vibrations, perhaps the earliest course on this subject offered in a civil engineering department in the United States. Joe instituted this course in 1961 after he returned from a sabbatical leave at MIT.

Most professors on sabbatical use their leave time to recharge their batteries. Not Joe. On his sabbatical, he studied every day until midnight and took six courses per semester. To those who know him, it is just another example of Joe's unbelievable dedication, which is so fundamental a part of his personality. This grueling course of study at MIT included Steve Crandall's first offering of his famous course on random vibrations. At U.C. Berkeley, Joe's course on random vibrations became legendary for how tough it was and enhanced his already-existing reputation of teaching difficult subjects.

As a researcher, Joe has always been ready to tackle whatever problem comes his way, whether it be analytical or experimental. He works intensely at it, becomes engrossed by it, and cannot stop thinking about it until he has solved the problem. These attributes of tenacity and hard work have enabled him to contribute in an important way to many aspects of earthquake engineering, from characterization of ground motions (including his brilliant concept of their principal axes), inelastic response of buildings (in the 1950s he was one of the early researchers in this area), soil-structure interaction, stochastic response of structures, and earthquake engineering for tunnels, offshore oil-drilling platforms, and bridges. His development, in collaboration with Dixon Rea, of the shaking table at the university's Earthquake Engineering Research Center (EERC) was profoundly significant to our ability to advance seismic design. Dedicated in 1972, it was the first modern servo-controlled shaking table of significant size, and was the forerunner of the many shaking tables now in various countries. Interestingly, the Berkeley shaking table is what launched MTS Corporation into the shaking table business.

Joe was the founding director of EERC at U.C. Berkeley, featured prominently in this volume. More than any other individual, Joe was responsible for EERC's rapid rise to eminence. He created a research environment that put EERC on the map as the foremost institution for earthquake engineering research in the world. As director of EERC, Joe's tenure was characterized by ground-breaking research on a wide variety of earthquake engineering topics, in large part because of the tremendous spirit of collaboration he fostered. Joe encouraged and welcomed all faculty members to pursue their research goals. Those were magical days compared to these rough and tumble days of academia where funding is scarce and competition fierce.

Another unique aspect of Joe's career is his special rapport with Japanese earthquake engineers, which started with the year (1964-1965) he spent as a UNESCO expert at the International Institute for Seismology and Earthquake Engineering in Tokyo. After the 1968 Tokachi-Oki earthquake, he proposed a cooperative research program between Japan and the United States, a concept that led to the highly successful U.S.-Japan Cooperative Earthquake Engineering Research Program Utilizing Large-Scale Testing Facilities. This research program ended in 2002, after a successful 30-year collaboration.

During that 30-year period, a steady stream of Japanese visitors came to Berkeley to work with Joe, who was the U.S. leader of the testing program. His special relationship with the Japanese was also instrumental in starting the CUREE-Kajima Joint Research Program, which is ongoing today. I believe that a great deal of Joe's special relationship with Japanese colleagues is based on mutual respect for each other's vast expertise and also Joe's warm and modest personality that makes people from other cultures comfortable. I also suspect, however, that part of this relationship is based on his unbelievable stamina for lively conversation and drink until the early hours of the morning with these same Japanese colleagues!

Joe chaired the Steering Committee for the Eighth World Conference on Earthquake Engineering, which was held in 1984 in San Francisco. Paul Jennings, then EERI President, asked Joe to chair the committee when Fritz Matthiesen, the original chair, passed away while the conference was still in the very early stages of planning. Joe accepted the assignment with the condition that he be allowed to add one member to the existing committee—me. I welcomed the chance to work with him on this exciting project. Joe asked me to chair the Budget Subcommittee. If I had had the freedom to choose my assignment, finances would have been at the bottom of my list. In fact, it wouldn't have been on my list at all. I must add that the conference was a major success by any measure because of Joe's leadership and the dedicated work of members of the Committee and the EERI staff. I believe this was a principal factor in EERI conferring an Honorary Membership on Joe in 1986! Many know this success story, but few are aware that Joe accomplished it under perhaps the most trying circumstances he had encountered since the daunting challenges he had faced in his childhood and youth. His wife, Jeanne Penzien, was diagnosed with cancer during those years and died shortly after the conference ended.

In addition to Joe's many significant contributions to earthquake engineering research and practice, I do wish to underscore the importance of his book, co-authored with Ray Clough, *Dynamics of Structures* (McGraw-Hill 1975). It was a landmark book in terms of its broad scope, comprehensive coverage, and philosophy. Several generations of students and engineers in the United States and abroad learned the subject from this very book. It has been translated into Bahasa Indonesia, Chinese, Greek, French, Japanese, and Russian. This book was a major influence on subsequent textbooks on that subject, including my own. Incidentally, my book came about almost by accident. Over the years, my teaching had gradually drifted away from the Clough-Penzien book, however, I had not thought seriously of writing such a book of my own. You can imagine how hesitant I was to even contemplate such a project with Joe and Ray having been such critical mentors in my career; trespassing on the same subject seemed like blasphemy. Several years after Ray and Joe retired from the university, my wife and daughter persuaded me to work on a book that reflected my teaching philosophy. As yet another testament to Joe's extraordinary nature, his help was invaluable even in my book-writing project.

Joe's generosity and helpfulness knows no bounds. He helped me and other younger colleagues in too many ways to enumerate here. One instance, however, exemplifies Joe Penzien. When I decided to dedicate the 1993-94 academic year to writing a book, I also decided to temporarily phase out all of my other responsibilities, including being one of the two editors of the *Journal of Earthquake Engineering and Structural Dynamics*. Knowing that at this stage of his career—he had retired from the University and was devoting all of his time to International Civil Engineering Consultants (the company he started in 1990 with Wen S. Tseng)—Joe had many demands on his time and no reason to serve as an editor of the *Journal*, other than as a favor to me. He agreed to carry on the tradition of conscientious, professional editors, as established by Ray Clough, for a year while I wrote my book. To this day, I marvel at this gesture of generosity and friendship.

My admiration for him led me to nominate him for several ASCE and EERI awards. While each of these nominations was successful, I live with one regret about an effort that backfired. In 1985, several California universities decided to prepare a proposal in response to the first national competition for a National Science Foundation (NSF)-sponsored earthquake engineering research center. Joe and other leaders of the effort had designated me to coordinate preparation of the proposal, to be identified as the Principal Investigator, and by implication serve as director of the center if we were successful. I saw the proposal to completion, but thought that Joe should be the director of such a center, not me. In addition to the obvious fact that he was the most qualified for the job, I felt as a friend that a new challenge would be personally beneficial for him, as he was close to retiring from the University. I remember proposing this idea to Ray Clough, George Housner, and Mihran Agbabian. Naturally, everyone was most enthusiastic. Joe agreed and the proposal was submitted with him as the Principal Investigator. Unfortunately, the proposal was unsuccessful for a variety of reasons.[e] Instead of providing Joe with a new challenge, I brought him disappointment near the end of his outstanding academic career.

After retiring from the University in 1988, Joe launched a new career. He and Wen Tseng started International Civil Engineering Consultants, Inc. (ICEC), a consulting engineering practice in Berkeley, California. Joe's expertise in advanced analytical techniques and research, combined with Wen's professional experience at Bechtel, has made this company unique, bringing consulting engineering to new heights. ICEC has been called upon to do the most challenging engineering analyses for some of the most important projects of the past decade. These include several bridges—the San Francisco-Oakland Bay Bridge, the Golden Gate Bridge, and the new Tacoma Narrows

---

e. The NSF's decisionmaking process was formally investigated by the General Accounting Office. The NSF Earthquake Center decision has been the subject of at least two publications that I [Chopra] know of: (1) Bell, Robert, *Impure Science*. John Wiley, New York, 1992, 301 pp. Chapter 2 (pp. 37-71) entitled "Handing Out the Big Money: Neither Science nor Sense" is devoted to the behind-the-scenes history of NSF's decision. (2) Olson, Robert, Assoc., Inc., "To Save Lives and Protect Property: A Policy Assessment of Federal Earthquake Activities, 1964-1987, Final Report to FEMA, November 1, 1998." Appendix C (pp. 183-206) entitled *The NSF Earthquake Engineering Research Center, A Tale of Two States* is another assessment of the factors influencing NSF's decision on the earthquake center.

Bridge—and high-speed transit systems in Korea and Taiwan. During this period, many of Joe's long-time colleagues discovered a new dimension to his professional expertise—a rigorous researcher also had the talent to be a professional engineer. During my participation on the Caltrans Seismic Advisory Board (which Joe chaired for several years), Alex Scordelis remarked to me that he was unaware of Joe's vast engineering talents until recently, although the two had had neighboring offices in Davis Hall for over 20 years!

From a tarpaper shack near the badlands of South Dakota, to an elegant home in Lafayette, California; from a high school student in Idaho who did not have the funds for college, to a doctoral degree at MIT in two and one-half years; from a farming family where his father did not trust educated people and he was the only child of eight to go to college, to an eminent professor at the University of California, Berkeley. In his life travels, Joe Penzien has come a very long way, indeed. This oral history is an amazing story of a remarkable man, whom I am proud to call my friend.

Anil K. Chopra
University of California, Berkeley
April 2004

CONNECTIONS

Joseph Penzien

Chapter 1

Homesteading in South Dakota

They built a sod house. Those were tough pioneer days!

Penzien: I will start with my parents. My father, John Chris Henry Penzien, was born of German immigrants in Lapier County, Michigan in 1881. His parents, my grandparents, had emigrated from somewhere in northeastern Germany, near the Baltic Sea. They came over and settled in Michigan. Like many other immigrants in those days, they were farmers.

Scott: Do you know when your paternal grandparents came to this country?

Penzien: About all I can say is that my father was born in 1881, and his parents had immigrated sometime before that. They both died when he was just a young boy, and he stayed with an uncle and survived. He only went through school up to the sixth grade, and then he went out to work.

When he reached the age of 18 or so, he filed for a homestead. He filed when he was still in Michigan, but when the homestead was assigned to him, it was out on the plains of South Dakota, just north of the Badlands. It was terrible country, but that was what was available to him, so he left Michigan and went out to homestead. He was single then. Of course, it was raw land with nothing—no improvements at all.

Meanwhile my mother, Ella May Stebbins Penzien, was born in 1883 in eastern South Dakota, in Elkpoint. Her first husband died of tuberculosis when they had three small children, two boys and a girl. They were just little kids when her husband died of TB. There was a lot of it in those days.

So she filed for a homestead, and the government assigned her a homestead out in western South Dakota, right adjacent to my father's. That's how they met. When my father married her, he started with three children. They built a sod house. Those were tough pioneer days! They dug the sod out of the buffalo grass, and stacked it up, and that was their house. Anyway, after they got married, the young Penziens started to come along. There were five of us Penzien children.

Scott: There were three children in the first group, and five in the second?

Penzien: Yes. I had two half-brothers and a half-sister. Then I had two full brothers and two full sisters, who were all born out on the plains of South Dakota. I am next to the youngest.

I was born on the farm, near Philip, South Dakota, in 1924. Of course I was born out in the country, and my mother had a midwife or something like that. In those days a mother having a child did not go to the hospital. It was always just out on the farm. I was a twin, by the way, but my twin died at birth.

One-Room Grade School

Penzien: I was the only one of the eight who went to college. When my brothers finished grade school out in a little country school, they would have to work with my dad before they could go to high school. In fact, my two older half-brothers did not go to high school. The two older Penzien brothers did go on to high school, but only after staying out of school for two years while they worked.

The grade school had one room and one teacher who taught all eight grades. Today you would hardly believe that was possible. But that's the way it was, all eight grades in one room. Of course there weren't many students, out in the country like that. There were 17 students total in 1930 when I was in the first grade. Some grades might not even have a student. There were three of us in my own grade, two girls and myself. By the way, one of my brothers through correspondence located the teacher of that one-room schoolhouse, who now lives in Escondido, California. Her name is Esther Naramore Sain. I recently [October 2003] visited her, the teacher who taught me in first, second, and third grades, having not seen nor heard of her for 70 years until my brother put me in touch with her. She's now 93.

A one-room schoolhouse seems like kind of a strange start for someone who finally went on through college, became a university professor and all that. But that's how I started.

Scott: Well, nevertheless, you can get the beginnings of an education that way, can't you?

Penzien: It seems to me I got all the basics that children get today—reading, writing, and arithmetic. Also geography. And writing—I write quite clearly, so my script is easy to read. Nowadays they do not teach that.

Scott: Penmanship was emphasized back then.

Penzien: Penmanship was stressed, using the Palmer Method. We worked at it. So that was the start of my schooling.

Depression, Drought, and Dust

Penzien: Then, remember 1932 and the Depression? It really hit us hard. My father had about 480 acres of farmland, where we had Holstein cows, and pigs and chickens, and about two sections of range—grassland—where we had Hereford cattle. During the 1920s, we lived in an old tarpaper shack, which was terrible when you think of it today. Even the houses we now see in the slums look good by comparison. But when my father was making pretty good money in the 20s—taking cattle and hogs back east to Sioux City, Iowa or maybe going to Chicago with them—whatever money he got he would use to buy more land. Rather than building a better house to improve our living conditions, he would get more land. We had the old outhouse, and no running water in our home.

But then the Depression hit us in 1932, and the big drought hit at the same time. We got a double whammy. The drought brought on the grasshoppers. The crops would burn up, year after year. My dad would plow the farmland, seed it down, the crop would sprout and start growing, but then about the first of July it would dry up. Then the wind started the dust storms. We were right in the middle of the Dust Bowl. The dirt would drift on our farm.

We Finally Gave Up

Penzien: After five years we finally gave up. My father would have to seed down the farm in the spring, using money borrowed from the government, and it would all dry up. Then we would have to take out loans in the fall to buy cottonseed meal to feed the cattle, to get them through the winter, hoping that next year would be better. Then the next spring he would have to take out another seed loan.

That went on until 1937. There were no crops during those five years. We gave up. My dad told the tax collector, "All that land is yours." We just walked away from it.

Chapter 2

Relocating to Idaho

We loaded up a trailer behind our car, hired a semi-trailer truck to take some farm machinery and milk cows, and headed west.

Scott: You just walked away from the farm your mother and father had homesteaded and worked so hard to build up and add to?

Penzien: We just walked away from it. And my dad was not a young man any more. He was in his mid-50s. Boy that was tough! We decided to leave. We loaded up a trailer behind our car, hired a semi-trailer truck to take some farm machinery and milk cows, and headed west.

Scott: You headed west, like so many others in that era.

Penzien: We headed west, without really knowing where we were going to stop. It was not quite like *The Grapes of Wrath*, but still, we were one of the Oakies—people from the Dakotas, Nebraska, Kansas, and Oklahoma, who were giving up, leaving the farms and heading west. They called them "the Oakies" and we were among them. There are so many stories about that trip, but I don't want to get into all of that.

Scott: But include some of them, so readers will get a feel for how it was on the road.

Penzien: Well, first off, there was Old Shep. Farmers often had a shepherd dog to help herd the cattle. We were taking Old Shep with us, but when we started up and got about a mile away from the house, he broke out of the box and went back. He wasn't about to leave, and he went back home. I don't know what happened to Old Shep.

We got stuck in Rock Springs, Wyoming. There was a rainstorm, the truck got bogged down in the mud, and we had to let the milk cows out. We had to milk those cows night and morning. We had to let them out in a little garden area where the people allowed us to put them. We milked them and gave the milk to the people.

We got stopped by the police at the border of Idaho. They did not want to let us in. "Where are your permits?" We were bringing cattle and things, but had no permits. These were the kinds of problems the Oakies were facing in those days.

Scott: What did you do about the problems at the border?

Penzien: We sat there for about a day, and then I guess my father finally got it cleared up so we could go through. They just held us up for about a day.

It took us about three days to get from South Dakota into Idaho. We got into Nampa, Idaho in Boise Valley. Nampa is only about 20 miles west of Boise, the state capital. My dad looked around there, and it looked pretty good.

Scott: A good deal greener than South Dakota, probably.

Penzien: Oh, pretty green—irrigated country. Dad said, "I guess we'll stay here." He located a sales barn, where they sold livestock, and was able to board our cattle there. He then went out looking, and in a few days found a little five-acre plot with a small house on it that he rented.

Surviving By Hard Work

Penzien: Having got us settled, my dad went back to South Dakota to bring out a second load of farm machinery and a team of horses. When he got back to Idaho, he hired out cutting corn. We had a corn binder, and he was pretty busy cutting corn for various people in the area. We did all sorts of things, and survived.

Scott: Was the hired-out corn cutting and doing other farm operations what your dad did then in Idaho?

Penzien: He wanted his own farm, like he had had in South Dakota, but he couldn't get a farm. They just were not available, until after a year or so he was able to get a farm on a sharecrop basis. But those farms weren't the best. He kept sharecropping. He didn't own a farm, of course, because he didn't have anything.

Contemplating College

Penzien: Then the war came on at the end of 1941, and I graduated from high school in 1942. If it had not been for World War II, I would never have gone to college, and would have never ended up where I did. I was totally dedicated to my father. As long as my father was on the farm, I was staying with him.

First of all, my father did not much believe in higher education. He thought the educated people—he called them “city slickers”—were just out to live off the farmers.

Scott: He didn’t like educated people?

Penzien: Well, let’s say he didn’t trust them very much. If he stayed on the farm, I would have stayed too, because he would have wanted me to stay. But the war came on, and it was pretty tough sharecropping in Idaho, so he decided to work at a mine up in the mountains in Idaho. He did not work in the mine, but as a teamster, delivering goods around the mining camp. So my father no longer needed me, and I was free. I stayed down in Nampa. Having graduated in 1942, I was ready for college, and I was free to go if I could find a way of getting the tuition and supporting myself. I could not ask my parents because first of all they hardly had anything, and even if they had, I wouldn’t have asked my dad, anyway.

Scott: Because he was not in favor of higher education?

Penzien: Right.

Chapter 3

College

I had worked in the onion fields a few years before for 10 cents an hour. If it hadn't been for the scholarship, I could not have gone to college.

Scott: With your parents at the mining camp, you were free to go to college, and wanted to go, if you could manage it financially?

Penzien: Yes. I had a brother-in-law who had gone to the College of Idaho in Caldwell, and he helped me. I had been a good student in high school, and in those days the College of Idaho offered an examination to all high school seniors in the state. They would award two full scholarships to the highest achieving boy and girl. I took the exam, and came in second among the boy students, statewide.

Now the top girl and boy got a full scholarship, but the second place boy and girl just got a trophy—a gold cup. So I got a gold cup, but that did not help me much. Interestingly, three of the four statewide winners came out of the same Nampa high school. I got the cup, but had no money.

First Year: A Scholarship and Free Room and Board

Penzien: My brother-in-law, who had graduated from the college, said, "Joe, I'm going to go over and talk to the college admissions officer and see if I can't get them to do something." He was able to get them to give me a full-year scholarship. Know how much that was?

Scott: Probably not a lot in today's terms.

Penzien: It was $80 per semester—$160 total for the full year. That was a lot of money back then. I had worked in the onion fields a few years before for 10 cents an hour. If it hadn't been for the scholarship, I could not have gone to college.

The scholarship took care of the tuition, but that's all. I needed a place to stay, and of course, food. A Nampa lady who knew my family and ran a boarding house, said, "Joe, if you come down to my boarding house, I can't give you a room, but I'll let you sleep in the hall, and you can eat with us and I won't charge you anything." I had my tuition, room and board, so I was pretty well set. I worked Saturdays at whatever odd jobs I could get. That is how I went to college for my freshman year, 1942-1943.

Scott: Nampa must have been quite close to the college, for you to board there and go to school.

Penzien: Yes, Caldwell, where the college is located, is about ten miles northwest of Nampa. I hitchhiked back and forth each school day. The local motorists were very generous at giving the college students free rides.

Moving to the University of Washington

Penzien: I finished that first year of college, but then my scholarship had run out. "What do I do now?" Well, the same brother-in-law who encouraged me to attend the College of Idaho had decided to go to Seattle and work for Boeing in the aircraft industry. Of course, with the war industry activity, he could easily get a job. He went to Seattle, started to work for Boeing, and then also started operating a gas station that he rented. When he got off work at Boeing at 5:00 pm, he would open the gas station and run it until about 11:00 pm at night. He said, "If you come out to Seattle, you can live with me in the apartment, and help run the gas station." So I went out, managed to get enough from it to pay my tuition at the University of Washington, and I continued college there. I worked my way through college at the University of Washington.

When I had been at the College of Idaho I knew I would be drafted, and decided to go into the Navy V-12 program. Do you remember that? If they passed the physical and written exam, college boys could go into the V-12 program, get a college education, and a commission once they graduated. I passed the written exam, but when I went down for the physical, I failed.

Serious Health Problem

Penzien: I knew something was not right, because my heart was beating too fast. They said, "You have very high blood pressure." I got concerned and went to the doctors in Nampa, Idaho to try to check out my condition and find out what was wrong, but they didn't know. When I went to Seattle, I went to several doctors there, and each time I got a different diagnosis as to what was wrong. I knew only one could be right, if any of them was right. I figured, "Maybe they are all wrong." None of them had given me any hope, and my blood pressure was way up there. I could walk up a single flight of stairs, and then I would have to stop.

Scott: So it was not only very high blood pressure readings, but also your condition was affecting your physical performance and how you felt.

Penzien: Oh, yes. I did not have to hold my pulse to count my heart rate. I could just sit and hear it.

Scott: Did that condition come on pretty fast?

Penzien: Yes, it did come on pretty fast. I also developed an irregular heartbeat. Sitting and trying to do my schoolwork, I could feel my heart beating, and every once in a while it would skip a beat, and then I would feel a kind of surge after the skipped beat. It scared me. One of my brothers, working in Seattle in shipbuilding, I think it was for Kaiser, asked me to come over on a Saturday. He said, "I'll make an appointment with a doctor who lives next to us—they say he is a good doctor, but he is almost deaf. Why don't you come over?"

So I went over. Being nervous about seeing the doctor, my heart was pounding away. The old doctor puts the cuff on my arm, pumps it up, lets it out, pumps it high, and still had to go higher. He had to pump it so hard it was just squeezing my arm! He looked at me, let out sort of a sigh, and asked if I had had my condition checked. I told him about the different doctors and what they had said.

He said, "You've got a goiter." My thyroid gland was out of whack. Sure enough, that was it. It was amazing. If I had not been diagnosed correctly, I would have died within a few years. In South Dakota a lot of people had that condition, due to the lack of iodine in their diet. There was no iodized salt in those days. And they ate almost no fish, which is a good source of iodine. We hardly ever had fish in South Dakota! Only a few bullhead catfish on rare occasions that we caught out in the creek.

The doctor started treating me with iodine. He told me, "Every day when you get home from school, don't do anything until you lie down and put an ice pack over your heart to calm it down." After a number of treatments, he told me that my condition was too far gone, and that I would have to have an operation.

Scott: Did you get your blood pressure down and your heart condition stabilized?

Penzien: As I recall, my metabolism rate had been plus 25. Zero is where it should be, and it can actually be plus or minus. Some years later I asked a doctor friend what the plus 25 signified. He said, "Your body was burning up energy so fast you could not live long." I was eating a lot. I would eat and eat, but was losing weight. My body was burning it up.

Shortly after the surgery, when they took the metabolism test again, it had been totally corrected. The cause of the problem had been corrected. But the effects went on for about fifteen years. It took about fifteen years to get wholly rid of that irregular heartbeat.

Scott: What about the blood pressure?

Penzien: It was down, although I still tend to have high blood pressure. But it got down to where I could at least continue to live. The operation must have been in late 1943. My parents in the mining camp got worried and came to Seattle to live, and I lived with them in the apartment. My dad worked in a machine shop and my mother worked in a hospital. That is how I managed to continue college. My brother-in-law who had the gas station was drafted and went into the army. So then I worked at night parking cars in a parking garage. I did that until I graduated.

Bachelor's in Engineering

Penzien: Going through undergraduate school was a struggle for me, but I graduated from the University of Washington in 1945, and went to work for the Corps of Engineers.

Scott: So you got a bachelor's degree in engineering?

Penzien: Yes, a Bachelor of Science degree in civil engineering. I liked mathematics very much in high school. I would have liked to have majored in mathematics in college, but in 1942 it was very difficult for mathematicians to get a job. Someone told me that engineers used a lot of mathematics, so I decided to go into engineering. The University of Washington had a very good undergraduate engineering program. I took a broad range of required civil engineering courses—structures, hydraulics, surveying, transportation, sanitary engineering. Structures and hydraulics were my favorite courses. The professors that impressed me most were Al Miller, C. C. More, and S. Sergev in structures, and C. W. Harris, H. Moritz, and R. B. Van Horn in hydraulics.

Army Corps of Engineers

Penzien: The civil engineering department found a job for me with the Corps of Engineers, in Bonneville, Oregon. I went there in June of 1945, right after I got out of school.

Scott: Was the assignment with the Corps related to their work on dams?

Penzien: Yes. I worked in the hydraulic models laboratory. Earlier they had made models of Bonneville Dam and other dams on the Columbia River. When I got there, my job, along with another young engineer, was to establish vertical and horizontal control points so they could start laying out templates and build a model. We called it the Umatilla Dam, although later I think they named it McNary Dam.

Chapter 4

Beginning a Career

I had never thought of teaching before.

Penzien: I worked for the Corps of Engineers until the spring of 1946, when I got a call from Professor Wilcox, the chairman of the general engineering department at the University of Washington. He said, "I'm calling to see if you would be interested in coming to the university to teach." I had never thought of teaching before. That came as a real surprise. I had been a good student, and of course they were looking at some former graduates to come back and teach.

It was 1946, and all these veterans from World War II were coming back and going to college under the G.I. Bill of Rights. In the military, a lot of them had gotten experience with radar and so on, so engineering was very popular.

Scott: Yes, I remember those years well, and all the students under the G.I. Bill.

Penzien: And they were loaded with students. I said, "When would you want me to start?" He said, "Classes began about a week ago. We need you now." This was a Friday afternoon, and I said, "This is very difficult for me to think through and decide what to do—can you give me till Monday? I'll call you and let you know." I gave it a lot of thought over the weekend. I had never thought about teaching, and the idea of getting up before a group of students scared me. I was an old farm boy from South Dakota and I wasn't used to being around a lot of people, let alone getting up and speaking to them. Somehow I got up enough nerve over the weekend to call him on Monday and say, "All right, I'll come."

First Year as a Professor

Penzien: They assigned me three courses, one of which had two sections. You had to grade all your own students' problems. There were no readers. I worked very hard, but I loved it. I was down in that office working every night.

Scott: It took a lot of work to keep up. That was quite a heavy load.

Penzien: Oh, it was a terrible load.

Scott: But you really enjoyed it, nevertheless?

Penzien: Yes, I did. But the first time the professor took me into the class to introduce me to my class, which had already been going on, oh I was scared. They were older than I was. The average age of the students was about 25, while I was 21 at the time.

Scott: And being veterans, they were a rather mature 25.

Penzien: Oh, they were a mature 25. They were not like the 25-year-olds around campus now! They had been hardened, gone through a lot of hard knocks and things. But they were very good to me. They accepted me as their teacher, and were really very good to me. They knew I was single, and on holidays someone among them would invite me to their home.

I think I was very successful as their teacher. They were on the quarter system, so I finished out the spring quarter, and then taught the next full academic year, 1946-47. I was teaching freshmen courses like statics, surveying, drafting, and a course called engineering problems. It was pretty elementary stuff. Although in fact I ended my career here at Berkeley teaching statics. So after 35 years of teaching, I had made the full circle.

University of Washington's MIT Connection

Penzien: The University of Washington faculty in civil engineering had a good connection with Massachusetts Institute of Technology. In fact, Professor Charles Head Norris at MIT was a 1931 Washington graduate. After graduating from the University of Washington, he went on to graduate school at MIT, where he received his M.S. and Sc.D. degrees. Then he continued on as a faculty member in the civil engineering department. I believe it was through his connection that for years the very best University of Washington civil engineer students would get a scholarship to MIT. I applied in 1947, and they gave me a full scholarship to do graduate work.

It never occurred to me to do graduate work until I started to teach. Many of the teachers then just had bachelor's degrees, but you could see it coming that if you wanted to progress you would have to get a master's degree. At the time, I still owed money from my undergraduate college days. In fact, it took until some years after I got my doctorate to get my mother paid off for money she had loaned me.

Chapter 5

Doctorate at MIT

Dynamics of structures is really my specialty, but as a student I never had a course in that subject. Such courses didn't exist in the civil engineering department at that time.

Penzien: With the scholarship, I went back to Cambridge, Massachusetts in the summer of 1947 and started graduate school at MIT. The first fall semester, Professor Robert Joseph Hansen, who also got his B.S. from the University of Washington and his M.S. and Sc.D. degrees from MIT, had a project from the Corps of Engineers on blast effects on structures. That is when they were all concerned about the atomic bomb and its effects. He had been appointed Assistant Professor in the fall of 1947, and in January 1948 I became his very first research assistant. I worked in the lab on a machine that would impose impulse loads on reinforced concrete beams.

So I did my dissertation in experimental work. But Professor Hansen said, "You skip the master's." I believe I was the first in structural engineering at MIT to skip the master's degree, and I think I was the first in the whole department. I got a doctorate but no master's.

Scott: The reasoning was that you did not need it?

Penzien: He wanted me to save time. He needed me, and did not want me to stop and take the time to write up a master's thesis. He said, "You can carry that work on, and write it up and add on to it in your doctorate." So that is what I did. I worked on blast effects on structures. Nate Newmark of the University of Illinois and Robert Hansen collaborated for a while, working on Corps of Engineers blast effects projects.

We research assistants had to be taught on the job. Nate Newmark came over and gave some lectures. That was when Newmark started his numerical analysis procedure of solving differential equations of motion. So I learned some of the basics from one of the old MIT professors and from Nate Newmark. I really did not learn my specialty in the classroom, but outside.

I finished all the work for the doctorate in about two and one-half years. I started in the summer of 1947, and by the end of the fall of 1949, I had essentially finished my entire doctoral program, although I had to wait until June 1950 for graduation to get the degree. In the spring of 1950 I worked for Stone and Webster in Boston as a part-time engineer, and also worked part-time in the lab helping Hansen.

But think of that—two and one-half years—whereas now our doctoral students typically hang around here four or five years. Some six. And some…are hard to get rid of. But at that time there weren't many graduate courses. They were limited in number, and I took essentially all the courses in structural engineering. But my advisor did not want me to get out and go into mathematics and other things. He sheltered me too much. So even though dynamics of structures is really my specialty, as a student I never had a course in that subject. Such courses didn't exist in the civil engineering department at that time.

Scott: Later on in studying dynamics, did you find that you needed more mathematics as well as other background?

Penzien: Oh, I did. And I went back to MIT for my first sabbatical in 1959 and took a lot of math courses and other things. But for that, I did not go to structural engineering courses, but instead got what I needed from the mathematics and aeronautics and mechanical engineering departments.

Marrying

Reitherman: When you were a doctoral student at MIT, you were still a single fellow. How did you meet your wife-to-be?

Penzien: I met my first wife, Jeanne Ellen Hunson, at a dance in Cambridge. Then we lost track of each other—I was busy with my studies. Somehow a year or so later I bumped into her again at the same place, and then we started going together. We ended up getting married in April of 1950, just a couple of months before I graduated, and then we moved down to Albuquerque—I can tell you about the Sandia Laboratory in a moment.

Our first child, my son Robert Joseph, was born in 1952, after we had moved to Fort Worth, Texas. Then, after we came to Berkeley in 1953, we had three daughters: Karen Estelle, Donna Marie, and Charlene May. And of course, my children's generation has now extended to the generation of my grandchildren. I now also have six granddaughters, and I just became a great-grandfather of twin girls [November 2003].

Reitherman: Do your kids live near your home in Lafayette, California?

Penzien: One daughter in the family moved to Roseville, near Sacramento, but the rest are all within a 30-minute drive of my home. To complete the story of my family, I should tell you that my wife Jeanne died of cancer in 1985. Then later I married my current wife, Mi-Jung Park.

Reitherman: Did you meet her in Korea or over here?

Penzien: In Korea. In 1986, Al Ang invited me to a seminar in Korea on critical structures. Al was at U.C. Irvine at the time. It was organized with Japan, Korea, and the United States. I was hesitant to accept because I wasn't sure if I wanted to write another paper. Because Al is a good friend, I said okay, I'll go. Then at one of the social functions that was held, I met Mi-Jung Park. We got married in June 1988 in Seoul and have gone back many times to visit her relatives. I guess you could say it was a bit of fate to somewhat hesitatingly agree to write yet another paper and have to go off to another engineering conference, and then meet my future wife there.

Chapter 6

Working on Blast Effects

If I had gone from the graduate program straight into teaching, I would have wondered, "Well, how are engineers really working? What is it like on the job?"

Work at Sandia Laboratory

Penzien: After I left MIT in June of 1950, I went to Albuquerque, New Mexico and worked for Sandia Laboratory. Sandia Laboratory was under the University of California, like Los Alamos National Laboratory still is. I went to work there on a blast effects program. I was working on measuring blast pressures on structures. We built some structures in one of the canyons outside Albuquerque, instrumented them to measure forces, and set off TNT charges.

While I was at Albuquerque, I went out to the Pacific to participate in "Operation Greenhouse." Remember when they built the structures at Eniwetok and had atomic tests there? I participated in that test program.

Scott: How much time did you spend out there?

Penzien: I was out in the Pacific only about a month. I had worked on blast effects in Albuquerque, so I went out mainly to see the tests that were conducted in April and May of 1951.

Convair: Blast Effects on Bombers

Penzien: I had been at Albuquerque about a year and a half when someone told me that Convair in Fort Worth, Texas was looking for someone to work on blast effects. They knew about the blast effects program in Sandia Laboratory and came to interview me. They wanted someone to work on a special, rather highly secretive project, to see whether the B-36 airplane could deliver the H-bomb. Anyway, I went to Fort Worth, and found it very interesting.

As I recall, at that time we had the H-bomb, but this was not known to the public. Its yield was very high compared to the A-bomb. They had delivered the A-bomb in World War II with the B-29. The H-bomb was to be delivered by the B-36, the Convair airplane that was our heavy bomber at the time. It had the propellers behind the wings, pushing: six of them, three on each side.

Scott: They wanted to know about blast effects on the plane that delivered the bomb?

Penzien: Yes. The plane drops the bomb, and as soon as the bomb is released, the plane goes into as sharp a turn as possible and starts flying roughly the opposite direction to get as far from the explosion as it can. The bomb goes on down in the general direction the plane had been flying. The bomb is released at 45,000 feet and is set to explode at 5,000 feet—at least in our study that was the altitude. In the time it took the bomb to follow its trajectory from 45,000 ft down to 5,000, the plane could have traveled quite a distance in the opposite direction. Nevertheless it would be hit by a terrible thermal shock, followed later by the blast. Our study showed that the plane should survive and get away, but all the exterior panels would be kind of crushed in.

Scott: So the explosion would affect the plane very substantially.

Penzien: The plane would come back, but it would be pretty beat up. Anyway, I finished that project, and they put me on another project on stress analysis of the delta wing. Delta wing aircraft were quite new then. I remember setting up about 35 simultaneous equations. If I could solve them, I thought that would give a pretty good analysis of the wing. I turned it over to the computer center. Today, to invert a 35 by 35 matrix is nothing. One of my engineers can do that easily, no problem. But not then. They struggled and struggled with it, and still hadn't solved it when I left in the fall of 1953 to come to Berkeley.

Three Years in Industry Helped Career

Penzien: While I was in Fort Worth, I got to thinking I would like to get back into teaching. When I left MIT in 1950, they offered me an assistant professorship to stay there. I turned it down because the pay was much lower than at Sandia Laboratory. After working in industry, however, I got to thinking I would like to go back into teaching. On the other hand, I feel that the three years I spent in industry really helped me when I came to Berkeley.

Scott: Would you discuss that a little? How do you feel that the time you spent in industry was particularly helpful?

Penzien: It helped build my confidence. I felt I had some experience with engineers and the kind of work they do.

Scott: It helped you to feel that you could be something more than a textbook teacher. That can be significant.

Penzien: Yes. It was quite significant to me. If I had gone from the graduate program straight into teaching, I would have been wondering, "Well, how are engineers really working? What is it like on the job?"

Of course, over the years since coming to Berkeley in '53, I have done quite a bit of consulting. In consulting, you get more experience with how engineers are working and what their problems are.

Chapter 7

Coming to Berkeley

I told Ray [Clough] that I would like to get back into teaching and asked whether there were any opportunities at Berkeley.

Scott: Would you talk about the process by which you came to Berkeley after you decided to go back into teaching? Why was it Berkeley?

Penzien: In the spring of 1953, I wrote to Ray Clough, who was on the Berkeley faculty. Ray Clough had been a classmate of mine at MIT. He graduated from MIT in 1949 and went directly to Berkeley. I graduated MIT in 1950, then spent the three years at Sandia and Convair. I told Ray that I would like to get back into teaching and asked whether there were any opportunities at Berkeley. I got a letter back saying, “Yes, there are.” I guess Ray had strongly recommended me to Professor Howard Eberhart, who was then head of the structures group at U.C. Berkeley. As you know, Howard later conducted research on prosthetic devices and co-established the Biomechanics Institute at the University of California, San Francisco, Medical Center.

That was when they could see that they had to build a graduate program. That is what really put Berkeley on the map, when they built up graduate programs and hired a lot of young faculty. It caused problems much later, when they had to retire them all at about the same time.

I finally got a call from Howard Eberhart, saying, "Yes, we would like you to come." I did not come to Berkeley for an interview or anything like that. In those days, they did not do that. They just went on Ray Clough's recommendation. But I never received an appointment letter. As I waited in Fort Worth, Howard would say, "We haven't received an official okay. It's still going through committees, but we are sure it will be approved." Well, it got down to the time when classes were about to start, and I knew that if I were going to start teaching in the fall, I had to be there. I called Howard again, and he said, "Well it still hasn't been approved, and I can't tell you to come, but…"

Clearly he wanted me to come anyway, so I came. We were on the semester system then, but at that time it started in September. I came out not knowing for sure whether I would be appointed. I took the chance and came out.

Scott: No doubt that meant quitting your Convair job in Fort Worth.

Penzien: Yes. Oh, it was a big risk. I quit my job and came out. And you know, that appointment was never made official until November! So the slowness of our process was evident then. But back in those days, they hired all of us in my academic generation—Clough, Alex Scordelis, myself, and others—without an interview, and without the candidate giving a lecture.

Hiring Faculty was a Simpler Process

Scott: There was none of the process and ceremony that now surrounds candidate screening and appointments.

Table 1

Hire Dates of Selected U.C. Berkeley Faculty in the Civil Engineering Department

Faculty involved in earthquake engineering research, World War II to 1970

Boris Bresler	1946
T. Y. Lin	1946
Egor Popov	1946
Robert Wiegel	1946
Ray Clough	1949
Alexander Scordelis	1949
Harry Bolton Seed	1950
Karl Pister	1952
Frank Baron	1953
Joseph Penzien	1953
Jerome Raphael	1953
Jack Bouwkamp	1957
Hugh McNiven	1957
Vitelmo Bertero	1960
Jerome Sackman	1960
Bruce Bolt	1963
William Godden	1964
James Kelly	1965
Edward Wilson	1965
Graham Powell	1966
Anil Chopra	1967

Penzien: None of that. Now you have to advertise, and maybe get 100 applications for one faculty position. You have a faculty committee start screening them. Then they have to come to campus and give a lecture. And go through all these committees.

Back then, there was none of that. In retrospect, it makes you wonder. Do we need to go through all that? Clearly, they hired good people back in those days. A solid core of the U.C. Berkeley civil engineering faculty, in particular those involved in earthquake engineering, joined the faculty a few years earlier than I or within a few years after. One difference, of course, is that so few people were getting doctorate degrees in engineering then. Very few. I suppose you could say that, with maybe a few exceptions, those few were pretty top people.

Now, however, the mill is turning them out by the hundreds, and they are not all top people. The variation among those getting the doctorate is very wide, much wider than it should be. I suppose, considering all these things, this complicated procedure we use now is necessary.

Scott: The present process does take a lot of time, on the part of everyone who participates in any way, especially the applicants and the screening members. A lot of people spend a lot of time.

Penzien: A tremendous amount of time. I do believe they ought to think about ways to streamline the procedures. You have to go through many layers of screening. One screen may not be effective enough, so you go through another. Finally you go all the way up to the provost, who might even reject the leading candidate. But whoever the provost might be, I am not sure he or she will have any more wisdom than the department chair, where down the line the process started.

Golden Years at Berkeley

Penzien: I shouldn't be sounding too critical of the University. The University was good to me over the 35 years, although I like to think that I earned whatever I got. But those were golden years. Maybe it will come back, but as I see it, the golden years are gone. It is really a pity the way the University is struggling now [1993].

Scott: Say a little bit more about what you are referring to when you say "the golden years"? Roughly what time period do you mean, and what are the reasons for calling those years golden?

Penzien: Beginning in about 1950, a few years before I started teaching at Berkeley, that's when the golden years for funding academic programs and hiring new professors began. They were able to get budgets and increase numbers of faculty.

When I came in 1953, the governor was very much supportive of the University. That continued through the 1950s and well into the 1960s during Pat Brown's governorship.[1] The University budget was increased, and the importance of building graduate programs was emphasized. External research funds were, however, still very tough to get. After I came in 1953, I wrote many research proposals and sent them off to various places, but they were always being rejected. Of course, I was unknown. But it was very discouraging.

1. The governors of California, beginning with Professor Penzien's time on the faculty at U.C. Berkeley, have been Earl Warren (1943-1953), Goodwin Knight (1953-1959), Edmund (Pat) Brown (1959-1967), Ronald Reagan (1967-1975), Edmund (Jerry) Brown (1975-1983), George Deukmejian (1983-1991), Pete Wilson (1991-1999), Gray Davis (1999-2003), and Arnold Schwarzenegger (2003-present). During the tenure of Pat Brown, and with Clark Kerr (1911-2003) as President of the University of California, the California Master Plan, implemented in the Donahoe Act of 1960 (Senate Bill 33, Amendments to California State Education Code Sec. 22500-22705), coordinated three state-funded higher education components in California: the University of California, the State Colleges (later renamed State Universities), and Community Colleges. Penzien's postearthquake highway bridge research and work on panels of engineering experts after the San Fernando, Loma Prieta, and Northridge earthquakes, which are discussed later, occurred during the terms of Governors Reagan, Deukmejian, and Wilson, respectively.

Chapter 8

Finding My Way in Research

It was clear that they were building a graduate program, and a graduate program meant research.

Penzien: When I came in 1953, Howard Eberhart was head of the structures group, and I knew that they were emphasizing to young assistant professors that they had to start doing research.

Scott: They were emphasizing that going on up the ladder depended at least in part on a record of doing research?

Penzien: Yes, it was clear that they were building a graduate program, and a graduate program meant research.[2] I remember Harmer Davis, Chair of the Department of Civil Engineering,[3] had us over to the Faculty Club for a luncheon one day and cracked the whip. The message was "You young fellows had better get in that laboratory and get busy with research."

2. Egor Popov brought in the first Ph.D. student to the Structures and Mechanics program in the Civil Engineering Department (now Civil and Environmental Engineering) at U.C. Berkeley. That student was Mihran Agbabian.

I really didn't know where to turn—what should I do? What field should I work in? My background had been in blast effects. I went and talked to Howard Eberhart, who said they were interested in me because of my background in blast effects. But I did not know where I could get funding to continue that. That was pretty much classified work anyway.

For the first couple of years, I worked on a few little things. I remember testing some channel sections, trying to understand the stresses in simple channel sections. But it was the kind of thing where, looking back now, the analytical solutions were fully available. But Boris Bresler had been working on it, and kind of left that and encouraged me to continue. So I went up in the lab and made some tests.

Next I somehow made a connection with the Navy. I think it was a carry-over from the blast effects work. I did some tests in Davis Hall, in the old lab over there, measuring damping in prestressed concrete beams. The Navy had a lot of prestressed concrete beam construction, and was concerned about dynamic loads. They were concerned with blast effects. They wanted to know the damping.

Boeing's Summer Faculty Program

Penzien: Then about 1954 or '55 I went up to Seattle, and worked on the summer faculty program at Boeing, like Ray Clough had done. Ray had gone up a year ahead of me.

Scott: Describe the summer faculty program at Boeing.

Penzien: Boeing set up an in-house project that brought in faculty from all over. They would hire them for the summer, and put one faculty member in this group, and another in another group and so on.

Scott: Were these professors teaching in some way, or helping with research?

Penzien: They were just helping in Boeing's engineering programs as best they could. They assigned me to the dynamics group, where they handled the flutter problems. When the wind blows on a flag, the flag flutters because it is so flexible. At high speeds, excessive flexibility of an aircraft wing can allow it to begin to flutter. So now I was involved in aircraft dynamics.

Ray Clough had been there the summer before me, and had worked with John Turner in the dynamics group. That was when Ray started the finite element method. Ray is the father of the finite element method, and he started that at Boeing in his summer faculty work there.[4]

3. Harmer Davis (1905-1998) became head of the first transportation research center in the U.C. system in 1947, then called the Institute of Transportation and Traffic Engineering, and now named the Institute for Transportation Studies (*Harmer Davis, In Memoriam.* University of California, Berkeley. 1998.). Davis Hall on the Berkeley campus, the main civil engineering building, was named after Raymond Earl Davis, (1885-1970) another civil engineering professor, who was influential in the development of materials and structures laboratory facilities. Raymond Davis was involved in engineering research projects for the design of the San Francisco-Oakland Bay Bridge and Boulder Dam. (*Raymond Davis, In Memoriam.* University of California, Berkeley. 1974.)

4. Clough, Ray, "The Finite Element Method After Twenty-Five Years: A Personal View," *Computers and Structures*. Vol. 12, 1980, pp. 361-370.

Beginning Work on Random Vibrations

Penzien: After I had finished whatever analysis I had been working on that summer, my boss came to me and said, "Joe, could you look over these papers, try to understand them, and maybe help us with this?" It was the beginning of research on random vibrations. It was defining the air pressures caused by air turbulence, and noise from the jet engines, where it comes as a stochastic random process. You cannot approach those problems deterministically, you have to approach them as random processes.

Scott: You mean more of a statistical or probabilistic approach?

Penzien: Yes. Those papers were dealing with basic theory of stochastic processes. How you define it—through power spectral density and those kinds of things. This was my first exposure to that. Since then, I have taught courses in that subject over many years and have become somewhat of a specialist in it. But that's how I started.

Scott: It probably looked a little intimidating at first.

Penzien: Yes, it really was. All these mathematical expressions. It was strange. In fact, I am not sure I helped them much on that, because I did not really understand it. You see the treatment of random vibrations started with applied mathematicians, Norbert Wiener at MIT and people like that.

Scott: It seemed like pretty high-powered stuff, I guess?

Penzien: It was high-powered stuff. But a few years later when I took my sabbatical at MIT, I went into the aero-elasticity group. They gave me a desk, and I studied with them on aero-elasticity, the dynamics of aircraft. Then I went over to Steve Crandall in mechanical engineering at MIT. That year, he taught the very first course in random vibrations. I took his course, though not for credit. I was just sitting in. I began to figure out what I had seen at Boeing, but hadn't really understood. That was the start of my interest in random vibrations.

Scott: So the Boeing experience had some significant influences on you. Also, it sounds like a rather enlightened and progressive policy on the part of Boeing to bring in all those summer faculty. I suspect it paid off. What do you think?

Penzien: I am sure it did pay off. Just look, for example, at Ray Clough's finite element contribution that started there.

Scott: I don't understand the method, but I know that the term is widely used in earthquake engineering discussions.

Penzien: In all of our structural problems we now use finite element analysis. And it is used in other fields as well, such as fluid mechanics. The use of finite elements allows one to approximate a continuum by dividing it into discrete elements, each having its own physical properties. This method allows solutions to complex problems for which closed-form solutions are not possible. That was the start of my interest in random vibrations. It really began at Boeing, when I was given some subject material and did not understand it.

Sabbatical at MIT: Hard Work

Penzien: I guess I held onto my interest in dynamics, but sort of struggled for a while, until 1959 when I went back to MIT. I made up my mind that year during the sabbatical, and sat in on about six courses each semester. Boy, I'll tell you, I worked hard! Nearly every night I would be up working until midnight, just like a regular student.

Scott: You didn't just audit the classroom lecture and discussion, but also did the problems.

Penzien: I did most of the homework. I wanted to get everything I could out of that. I swear that I learned more in that one year than I had in my whole doctoral program. But by then I had reached the point where I knew what I was missing, knew what I wanted, and had the incentive and dedication to do it. I came back from that sabbatical a year later a different type of engineer. Among other things, I had learned a lot of mathematics.

I didn't look at that year as a year when I was going to enjoy myself. I looked at it as a year of opportunity to improve myself. Improvement takes hard work, and I worked hard. That was 1959-1960.

Work on Elasto-Plastic Models

Penzien: In the 1950s I had begun to see that to continue in dynamics, the best opportunity was in earthquake work. About 1956, I started on a study of the elasto-plastic response of structures. I remember setting up a nonlinear, hysteretic, single-degree-of-freedom system, similar to the models we use today. I put in earthquake inputs and got nonlinear responses. But I struggled a whole year programming that single problem. We had the old IBM 701 computer over in Cory Hall. You had to program in machine language then. Oh my, I had to get somebody from the computer center over there to help me, and found it very difficult. It was hard to do things like keeping track of where the decimal points went. You had to tell the machine where to store them. You do none of that today.

Scott: Would you give a layperson's brief definition of elasto-plastic response?

Penzien: Elasto-plastic is when a material is strained beyond its linear elastic limit. The material stress-strain relation then becomes nonlinear and history-dependent, making the prediction of structural response much more difficult.

That experience changed my outlook on what I should do. I worked a whole year, finally finished the work I wanted to do, and wrote a paper. This was about the same time when John Blume was making oral presentations on his very famous, and I consider valuable concept, "reserve energy." [5,6] In fact, I think it was John Blume's early work on this topic that stimulated me to do what I did. So I worked on that single-degree model, and then gave a paper in 1958 or 1959 at an American Society for Civil Engineers (ASCE) conference in

5. Blume, John A., "A Reserve Energy Technique for the Design and Rating of Structures in the Inelastic Range," *Proceedings of the Second World Conference on Earthquake Engineering*. 1960.
6. Blume, John A., "Structural Dynamics in Earthquake-Resistant Design," *Transactions*. American Society of Civil Engineers. Vol. 125, Part I, Paper no. 3054, 1960, pgs. 1088-1139.

southern California. That was back when engineers thought that if a structure went above the yield level under dynamic loading, it would go on yielding to the point of collapse. Engineers were still thinking that they could not allow yielding, because the thing would collapse. Of course, now we know differently. I remember that after I finished giving my paper, at a coffee break, John Rinne came over and looked at me, kind of puzzled. Clearly this was kind of new.

Engineers learned that the strengths they had been designing into their structures to resist a strong earthquake had to extend past the linear range and on into the nonlinear range. If you wanted to keep the old design philosophy of keeping structures elastic, you would have to make their elastic capacity very large, but compared to the large magnitude of the loading, which was beginning to be better understood at that time, it was realized that this was not the economical way to go. So, out of necessity, we found that we had to compromise and allow structures to go inelastic, and in the 1950s it was found that the structure can go inelastic and respond quite well. Prior to that, engineers thought of the static load on a structure, and if you try to load it above its yield strength level, it's just going to fail. But this isn't the case in a dynamic environment; it can oscillate back and forth and perform satisfactorily.

Multi-Degree-of-Freedom Model: Second World Conference Paper

Penzien: I continued that work, and went on from a single-degree model to multi-degree models.

Reitherman: Could you provide a simple definition of "degrees of freedom" for readers who aren't engineers?

Penzien: The number of degrees of freedom refers to the number of independent displacements, or number of independent displacement patterns, used to characterize a system's response under dynamic conditions. Strictly speaking, any distributed mass system has an infinite number of degrees of freedom. However, approximate solutions of response are usually obtained using a finite number, which could be as low as one. An example of a system with only one displacement pattern is a cart being pushed forward and backward in a straight line. That's a single degree of freedom. In addition, if you push it from the side, making it slide sideways at the same time, you have two degrees of freedom. Then if you allow it to vibrate up and down, that's three. Introduce rotations about those three axes (roll, pitch, and yaw)—that's three more, for a total of six degrees of freedom in this example. Each one of these patterns of displacement will increase the number of degrees of freedom.

I presented a paper in 1960 on elasto-plastic response[7] at the Second World Conference on Earthquake Engineering, which was organized by Kiyoshi Muto. What we call the "First" World Conference, held in Berkeley in 1956,[8] was not then thought of as starting a whole series. But it did.

7. Penzien, J. , "Elasto Plastic Response of Idealized Multi-Story Structures Subjected to Strong Motion Earthquake," *Proceedings of the Second World Conference on Earthquake Engineering*. 1960.

Scott: But in retrospect, it was treated as the First World Conference, and the conferences are numbered that way.

Penzien: Yes, you're right. Then at the Second World Conference in Tokyo, many of the papers were coming in on elasto-plastic response. Newmark and Andy Veletsos were involved in this problem at that time, also Glen Berg from Michigan, and myself, among a lot of others. It could have been called the International Conference on Elasto-Plastic Response. In the early days we were always concerned about what the hysteresis loops should look like. Today, it's just common terminology. So, as I look back I think it was easier in our day to find a new area and new things.

The Tokyo conference occurred while I was still back at MIT on sabbatical. I went from Boston to San Francisco, to Hawaii, and then on to Tokyo. One had to do some island hopping in those days because the planes could not fly farther without refueling.

I was still pretty young at the time of that conference, and talking in front of people like Nate Newmark scared me. At the conference, oh, I was scared. I rehearsed and rehearsed my paper. As the time for my session drew close, Nate Newmark came over and said, "Joe, I read your paper." I had been worrying that maybe what I did was worthless. Newmark said, "Your paper is very good." Then he said something like, "You beat us to it." At the University of Illinois, they had been planning to do the same thing, go from a single-degree to multi-degree systems.

Scott: You had beaten them to it.

Penzien: Yes. But after he told me the paper was very good, when I got up to give it, instead of being scared to death, I had my confidence.

8. *Proceedings of the World Conference on Earthquake Engineering*. Held in Berkeley, California, June 1956. Earthquake Engineering Research Institute, Oakland, California. For some years, prior to the establishment of an EERI office, these one-volume Proceedings were kept in boxes under desks in the office of Karl Steinbrugge at the Pacific Fire Rating Bureau in San Francisco, where his secretary could fill orders for the document.

Chapter 9

Earthquake Engineering Research Center

We just had to go ahead with confidence that when the next phase of construction would come along, funding would be there.

Earthquakes in 1964: Alaska and Japan

Penzien: The Alaska earthquake of 1964, and the earthquake in Niigata, Japan, which followed shortly afterward that same year, were the events that pushed us towards creating a center here at Berkeley. Without those two events, we would not have gotten started in earthquake engineering research the way we did at that time.

Scott: Those major earthquakes got worldwide attention, and especially, of course, in the field of earthquake engineering.

Penzien: They certainly did. It was after the 1964 Niigata earthquake that engineers really began to realize what the effects of liquefaction were. For example, the spectacular damage when those apartment houses in Niigata slowly turned partially over during the earthquake shaking. Frank Press was commissioned to head a study and make a report on the Alaska earthquake. As a result of his commission's report, we started getting funding for earthquake engineering research. It was also in the late '60s that the Alquist Committee started up in California.[9] In the year of the Alaska earthquake, 1964, the National Science Foundation (NSF) started funding earthquake engineering research at the level of one-half million dollars per year. Later, in 1972, as a result of the 1971 San Fernando earthquake, the level was increased to $2 million per year. It wasn't until 1977 that the Cranston Bill[10] was finally passed, setting up the National Earthquake Hazards Reduction Program (NEHRP). The NSF earthquake engineering research budget was then increased to $8 million per year. This level then steadily increased, reaching $14 million per year in 1991. Some further increases brought the level to nearly $18 million per year as of 1993. Since then, the number hasn't changed much, although because of inflation, the funding has declined in real terms.

The possibility of more funding for earthquake engineering research stimulated some of us on the faculty to start discussing it, primarily Jack Bouwkamp, Ray Clough, and myself. The earthquake was in 1964, and then I took an industrial leave from the University in academic year 1964-1965. An industrial leave gives you permission to leave for a year, but you give up your salary.

I spent time in Japan at the International Institute for Seismology and Earthquake Engineering, where I was a UNESCO lecturer and advisor. I was still at the Institute in January 1965, when I joined the Japanese delegation and traveled to Auckland, New Zealand to attend the Third World Conference on Earthquake Engineering. Ray Clough was at the conference, as well as Jack Bouwkamp and other professors from Berkeley. In January it was summer in Auckland, and during a break in the conference, Jack Bouwkamp and I sat out on the lawn discussing what we might do at Berkeley in developing a major program of earthquake engineering research. We discussed it with Ray Clough, probably sometime soon afterward. That was the start.

Berkeley Short Course Results in Book

Penzien: I continued on with my short assignment for UNESCO in Tokyo until the summer of 1965, and returned to Berkeley. Shortly after I got back, we gave the first short course at Berkeley on earthquake engineering, organized by Bob Wiegel.

Scott: That resulted in a major book.

Penzien: Yes, the big book, *Earthquake Engineering*,[11] which sold copies for many years. We brought in all the well-known experts to participate in the short course, such as Nathan Newmark, George Housner, Don Hudson, and others. It was a very successful course, and all this was providing momentum to our thinking about earthquake engineering.

9. State Senator Alfred Alquist chaired the Joint Committee on Seismic Safety. Penzien was an advisor to the Committee. *Meeting the Earthquake Challenge: Final Report to the Legislature*. State of California, 1974.

10. National Earthquake Hazards Reduction Act of 1977. Public Law 95-124.

Dynamics of Structures

Reitherman: Let me ask you about another famous text in the earthquake engineering field before we return to the train of thought about the origins of earthquake engineering research at U.C. Berkeley from your 1993 interview with Stanley Scott. The book you co-authored with Ray Clough, *Dynamics of Structures*,[12] has become a classic structural engineering textbook for university classes, and it's standard practice today for a civil engineering department to include a course on that subject in its curriculum. But perhaps you could explain some of the early background to the book and the origins of the teaching of that subject. For example, at the U.C. Berkeley-CUREE (Consortium of Universities for Research in Earthquake Engineering) Symposium in Honor of Ray Clough and Joseph Penzien,[13] Anil Chopra said in his talk that he took structural dynamics at Berkeley in 1962, with the first half being taught by Ray Clough, and then when Ray was on leave, you taught the second half.

Penzien: That goes back a long way, but I suppose Anil did start out studying dynamics as a student then. Of course, that's become his area of specialty as you know, with his textbook[14] and EERI monograph [15] on the subject. My doctoral dissertation was on blast effects on structures, which I started in the spring of 1948—it was then that I got involved in dynamics of structures where the excitations were blasts. When I finished MIT in 1950, I went to the Sandia Laboratory in New Mexico to work on blast effects. So that was an early exposure to the subject, though with regard to blast, not earthquakes.

When I came to Berkeley in 1953, Ray had already been working in dynamics. Ray got his doctoral degree from MIT in 1949, and came to Berkeley in the fall of 1949. We started sharing the teaching of dynamics courses, which were limited to deterministic dynamics. Then I went back to MIT on my sabbatical in 1959, and I got interested in random vibrations. So when I came back later that year to Berkeley, I set up a course in random vibrations, and I think that was in the fall of 1961. In 1964 I went to Japan and spent the year at the International Institute for Seismology and Earthquake Engineering, so I was teaching dynamics there and I had to write notes to give to the students. Earlier, back at Berkeley, Ray and I had written notes also. So when I came back in 1965 I remember telling Ray that we ought to write a

11. Wiegel, Robert L., editor, *Earthquake Engineering*. Prentice-Hall, Englewood Cliffs, New Jersey, 1970. Chapters in this text were authored or co-authored by John A. Blume, Bruce Bolt, M. G. Bonilla, Jack Bouwkamp, Ray Clough, Henry Degenkolb, George Housner, Donald Hudson, T. Y. Lin, Nathan Newmark, Joseph Penzien, Dixon Rea, John Rinne, H. Bolton Seed, Karl V. Steinbrugge, and Robert Wiegel.
12. McGraw-Hill, New York. 1975.
13. *Proceedings of the U.C. Berkeley-CUREE Symposium in Honor of Ray Clough and Joseph Penzien*, Berkeley, California, May 9-11, 2002. Consortium of Universities for Research in Earthquake Engineering (CUREE), Richmond, California, 2002.
14. Chopra, Anil, *Dynamics of Structures: Theory and Applications to Earthquake Engineering*. Prentice Hall, Englewood Cliffs, New Jersey, 2000.
15. Anil Chopra, *Dynamics of Structures, A Primer*. EERI, Oakland, California, 1981.

book on dynamics, and he agreed that was a good idea. So we went from there. Ray had a good contact with McGraw-Hill. I believe in earlier years he was one of their editors. McGraw-Hill immediately agreed to publish the book. I don't remember when we signed that agreement to write the book, but the book finally came out in 1975. I expect there was about 10 years in between starting the process and getting the book finished.

Reitherman: Prior to that, dynamics simply wasn't taught as a civil engineering subject?

Penzien: In civil engineering, dynamics of structures wasn't taught to my knowledge before 1949 when Ray came to Berkeley. When I was at MIT as a graduate student, they didn't have a course on dynamics of structures, we were just taught a little in that area, I believe, along with a mechanics course. That was a pretty new subject to civil engineers in the early 1950s. Ray Clough had taken dynamics from the aeronautics department, before it was introduced as a course in civil engineering. There were many other courses that started at that time when we were beginning to build up the graduate program that are considered standard parts of a civil engineering curriculum today.

Reitherman: For readers who are not engineers, can you give a short definition or explanation of dynamics? What is it that differentiates statics from dynamics?

Penzien: A static problem is where the loads do not change with time, so the structure is in static equilibrium as you carry through your analysis. There are no motions of the structure, so there are no damping forces, no equations of motion. It depends on the static force-displacement relations. When your loads are time-dependent, then the response will be time-dependent. If the response is changing in time, that means there are accelerations occurring, and that means mass times accelerations or inertial forces. There will be velocities, so usually velocities are associated with some kind of damping forces, which absorb energy. But the essence of dynamics is the fact that the situation is time-dependent, which originates in the fact that the ground motion is not the same from one fraction of a second to the next.

Reitherman: The 1943 City of Los Angeles building code's seismic lateral force equation has been cited as an early, albeit simplified, inclusion of a dynamic factor in earthquake-resistant design methods.[16]

Penzien: The first building codes treated earthquake loading as a static loading, which was just a percentage of the weight of the building applied laterally, and originally this static load was vertically distributed in a uniform way. The big step forward was to realize you had to make the seismic loads depend upon

16. Berg, Glen, "Historical Review of Earthquakes, Damage, and Building Codes," *Proceedings of the National Structural Engineering Conference: Methods of Structural Analysis*. American Society of Civil Engineers, p. 393. See also the EERI monograph by Berg, *Seismic Design Codes and Procedures*. EERI, Oakland, California. 1982. If there were 12 stories above (13 stories was the height limit in Los Angeles at the time), this worked out to be a base shear equal to 0.0364 times the mass above the ground story. The formula was designed to indirectly account for the response reduction typically experienced by longer period structures.

the natural frequency of the structure. Of course, any structure has many modes and frequencies, but in the early days you thought of the fundamental mode as the dominant mode. So about the time the response spectrum came in, Professor George Housner, along with Professor R. R. Martel at Caltech, who was developing response spectra, started to work on getting spectral values dependent upon the fundamental period of the building. Then they started to use that to get a base shear and assumed that shear at the base was produced by a loading that is distributed in the inverted triangular distribution. Everything has been changing more and more in recent years.

Reitherman: When seismic codes introduced the seismic design force applied at the top of the structure, the F_t or so called "whiplash" force, did that provide a way to account for response at the second mode or higher modes?

Penzien: Well, you could say if it was a tall building, you could think of it as a shear wave going up the building or call it the whiplash. That's higher mode effects. A long tall shear building could be represented as a shear beam with traveling waves, which is one approach, using wave formulations. The other approach is through the modal representation. So that whiplash up there represents the effects of a lot of higher modes.

Reitherman: When the dynamics textbook you and Clough published in 1975 was published in its second edition in 1993, was there much difference?

Penzien: Yes, quite a bit. In the second edition, Part 5 was almost totally rewritten with a lot of new material. Most of that was a result of my experiences setting up and teaching the graduate course called Earthquake Engineering. I don't recall the year, but it was between the first edition coming out and the second edition, obviously. Steve Mahin has taught that course, and now I think it is taught by Anil Chopra. One other big change was that the second edition has equal emphasis on time domain analysis and frequency domain analysis, all the way from the treatment of single-degree-of-freedom systems through multi-degree systems. In the first edition, frequency domain analysis is only in the section of random vibrations, not in the other parts on deterministic analysis. I felt we should give equal emphasis, although in practice now it's pretty much time domain analysis. I don't know why it's taking so long to get frequency domain analysis into practice.

Reitherman: When you started teaching your course called Earthquake Engineering, I take it that was a novel thing. Civil engineering departments in the 1970s didn't routinely have courses called earthquake engineering?

Penzien: I suppose that's true. It's relatively common nowadays, though only at the graduate level.

Reitherman: Now, going back to Stan Scott and his interview with you in 1993, I think you said that the first and last university courses that you taught were on statics. Is that true? Could you elaborate?

Penzien: I started teaching statics at the University of Washington in 1946, along with several other freshman engineering courses. It was towards the end of my career that I taught statics again at Berkeley. I had reached the point that the department chairman felt that I should teach a lower division course, which is a Berkeley policy to expose the younger students to experienced faculty. I did my best, but it was quite a change from when I taught it at Washington in 1946-47. At Washington, that was when all the veterans were returning from WWII and were all very motivated. So, when you went to teach your students, they would all be there and be very dedicated. But that changed from the early years when I came to Berkeley. Later on, students somehow weren't so motivated in that course.

Reitherman: So the statics subject matter to be learned had stayed the same but the students learning it had changed?

Penzien: The students weren't as motivated. I guess perhaps they were taught statics in their physics courses and didn't seem to think it was important to learn it as an engineering discipline. I don't really know, and I don't want to be critical of the students in the 1980s, but there was a big change—big change. If it was just my section I would have been worried, but everyone teaching the course said the same thing.

Reitherman: Even though you have taught higher-level courses, do you still find some sort of satisfying elegance to the basics of statics and the process of explaining those principles to somebody who is learning them for the first time? To me, it's a very elegant concept: If you can take an object and analyze all the vertical forces, all the forces acting along the two horizontal axes, and all the moments about those three axes, and if you can manage to get each of those sets of forces and moments to sum to zero, you can design that object to stay put, to be in equilibrium.

Penzien: Yes, I always enjoyed it. Statics can be taught in a very sophisticated way, using virtual work and other techniques, which is a very powerful way to treat the subject. In some ways I enjoyed teaching the subject, though I suppose I got more fulfillment from my teaching experiences in the higher-level courses.

Starting the Earthquake Engineering Research Center

Penzien: Jack Bouwkamp had the idea of setting up an organized research unit in structural engineering at Berkeley. I believe it was to be a structural engineering research institute. I recall Jack, Ray Clough, and myself going to the vice-chancellor for research, Alan Searcy, to discuss our concept and see what he thought. He was very polite and nice, but did not encourage us to go ahead. So no formal proposal went forward to the administration.

Scott: You pretty much dropped that idea?

Penzien: Yes, we dropped that. Jack had it in mind, however, that there would be a sub-unit on earthquake engineering within the proposed structural engineering institute. Since the umbrella-unit idea did not "fly," Jack dropped that idea, and then Ray and I took the lead to go back to the administration with a formal proposal to set up an earthquake engineering research center.

Scott: Did Searcy talk much about the reasons why he did not encourage you to proceed with the larger research unit idea?

Penzien: I suppose he gave us some reasons for his lack of enthusiasm, but I do not recall what they were. It was a long time ago. There had been a structural engineering research laboratory set up by Professor R. E. Davis—the old structures laboratory with big machines. At one time I believe it was an organized research unit. I can't be sure of all these facts right now, but I think that the program established by Davis had faded out by that time. Clearly, it did not continue as an organized research unit (ORU). There may have been some history in that which contributed to their discouraging us. Maybe they thought, "Why should we start that up again?"

In any event, Ray and I felt there was a definite need for a research unit in earthquake engineering. Getting the administration's approval to establish an ORU is very important for a U.C. campus. We wrote a formal proposal, sent it through the channels, and it was finally approved. The process of getting it through the administration and the various committees probably took a year. I believe an organized research unit has to be approved by three committees, including the Budget Committee and the Research Committee. The final approval by the U.C. Berkeley Senate Budget Committee was in December 1967. The other committees had already approved, so it then went to the chancellor for his approval.

The proposal had provided that I would become the first director. I got a letter in early January of 1968 announcing the creation of the new Earthquake Engineering Research Center and appointing me director. So we had the center approved, and had a director, but that was all. The whole thing was all on paper. The center had been approved in concept, but without a budget.

Scott: No money at all, or maybe a little seed money?

Penzien: In the very beginning there was no Regents money from the statewide U.C. budget at all.[17] I served as director for a number of years without any reduction in my U.C. Berkeley teaching load.

Scott: So your responsibilities as director were added to your pre-existing full teaching responsibilities? It "came out of your hide," so to speak?

Penzien: Yes. In setting up the center, the first thing we needed was an office. For a very short time, the office was located in one of the small offices of the Structural Research Lab at RFS (Richmond Field Station), where the big testing machine is located.[18]

17. The vice-chancellor for research at that time, Leo Sammet, commented at the 1972 opening ceremony for the shake table that EERC "received its first continuing state-funded budget in the present fiscal year," which was fiscal year July 1, 1971-June 30, 1972. Sammet, Leo, "On the Advancement of Research and Education in Earthquake Engineering," p. 39, in *Dedication of the Earthquake Simulator Laboratory*, June 24, 1972. Earthquake Engineering Research Center, University of California at Berkeley.

18. This high-bay lab is Building 484 at the Richmond Field Station, Richmond, California, and the testing machine is a "universal testing machine" with a capacity of 4 million pounds in compression and 3 million pounds in tension (about 18 million Newtons and 13 million Newtons, respectively).

Building a Shaking Table

Penzien: We then proceeded to plan for a big shaking table, or earthquake simulator, which of course would mean a separate building. Look at the very first EERC report,[19] the only one that has a yellow cover, the very same type of cover used by the Division of Structural Engineering and Structural Mechanics (SESM) in their reports. That first EERC report was to the California Office of Architecture and Construction, now the Division of the State Architect. We had made a proposal to them for their support of our planning a big shaking table, which was to be a 100 ft. by 100 ft. platform. The table that we went ahead and built is 20 ft. by 20 ft. We got about $12,000 to make that study. That doesn't look like very much today.

We tried to promote this big shaking table. I gave a presentation on it to the Structural Engineers Association of Northern California (SEAONC) and to the statewide SEAOC.[20] The practicing structural engineers were very interested in it. We proceeded with the idea, trying to get the funds from the NSF. It was right at the beginning of the time when money was becoming available from NSF.

19. Penzien, J., J. G. Bouwkamp, R. W. Clough, and Dixon Rea, *Feasibility Study: Large-Scale Earthquake Simulator Facility.* Earthquake Engineering Research Center Report No EERC-67-1, September 1967, University of California at Berkeley.

20. Penzien, J., "Feasibility Study of a Large-Scale Earthquake Simulator," *Proceedings of the 36th Annual Convention of the Structural Engineers Association of California*. 1967.

We were, however, concerned about the feasibility of powering the big shaking table with hydraulic systems, and having sufficient control to simulate real earthquake motion. The motion produced had to be pretty similar to that of a real earthquake. I took a trip with Jack Bouwkamp to various places to check with companies on the feasibility of powering and controlling such a big table. We went to MTS in Minneapolis, which had developed hydraulic power systems for the Air Force in World War II. They provided quasi-static testing equipment for aircraft structures. They were coming along very fast in providing such equipment for other purposes. We visited, I think, two other firms on that trip seeking advice on shaking table control.

We were getting what I thought were conflicting views on whether you could control that large a table. That suggested proceeding very carefully, so I initiated a servo-control study for a shaking table. NSF funded the study, and I believe it lasted about two years. MTS got very interested in the subject and began at the same time to put their own effort into developing servo-controls.[21]

21. A servo-control system for a shake table allows an input signal (a computerized record of earthquake motion) to cause the table to move, and then it regulates the table's movements to reduce discrepancies between the desired simulated ground motion and the resulting table motion. Without this feedback and control during every split second of a shake table test, the resulting motion would lack fidelity. For example, the servo-control of the table must correct for the rocking tendency (overturning moment) imparted to the platform by the response of a structural specimen.

Scott: Where was your study done?

Penzien: We did the study here, at Berkeley. I was responsible for the project, and assisting me was Dr. Dixon Rea, who later became a professor at UCLA. I also got Professor Yasundo Takahashi, a servo-control expert in mechanical engineering, to work on it. Also Professor Rogers, an electrical engineer here on campus, worked on it. They were the real specialists on servo-controls helping us on that project. While we were working, we would have meetings with MTS on the progress they were making. As a result of these investigations, we decided it was feasible. We gave MTS a go-ahead to design and build a system. This was the project that got MTS into the shaking table business. Today [1993], virtually all the major tables around the world have MTS hydraulic and control components.[22]

Scott: Were you still planning on the big table?

Penzien: No. We had these feasibility studies, but I guess we were still not sure of ourselves. So we thought that rather than go ahead with the big table, we would build a medium-sized table. It was rather risky to go ahead with such a large facility when the servo-control was not fully developed and there was uncertainty about the degree of control we could get. That was one of the main reasons why we decided on the medium-sized table. In building the smaller table, we would continue to develop the controls, and the table we got would be a good facility in its own right. We just delayed the idea of the big table in order to move ahead with the medium-sized table. Actually, the table we built is quite large compared with most of the shaking tables here in the United States.

Finding Military Surplus Hydraulic Power Units

Penzien: We even went so far as to get quite a large number of hydraulic power packs from the military. These were power units coming out of the Titan I missile silos. We needed quite a few of those to power that big shaking table. There must have been from six to ten of those units, and we went up and got them. One of the units we brought out to RFS is powering the present shaking table. A second one provides hydraulic power for all the testing in Davis Hall. Anyway, we brought down a whole bunch of those things, although I cannot remember the exact number. I believe we brought back enough power packs to power that 100 ft. by 100 ft. table.

Scott: Did you get those for free, for educational or research use, or what?

Penzien: We got them for free as military surplus, although we had to pay for transportation to get them out. The Titan I had become obsolete and was being replaced. Anyway, we got the power units here, and stored them out in the old Ford plant in Richmond that the University had acquired.

22. A decade later, during the 2000-2004 construction phase of NEES (Network for Earthquake Engineering Simulation), of the $82 million provided by NSF for this developmental work, approximately $30 million was subcontracted to MTS by the universities building NEES laboratory facilities.

Problems Along the Way

Penzien: As we built the medium-size table, not the 100 ft. by 100 ft. table, we still encountered a lot of problems along the way. The pumping equipment that came out of the Titan silos had to be modified to meet our need. That was a big job. We finally got it installed on a concrete pad out at the Richmond Field Station, near Berkeley. Then we got into the rainy season, it was raining and raining, and we had no cover for it. Metal starts to rust and all of that.

Ray Clough and I went to Roy Carlson, a civil engineering professor who had worked with R. E. Davis. Carlson was a specialist in concrete dams and had developed strain meters. I think he was the cousin of the inventor of Xerox, and acquired stock in Xerox very early. I don't know how wealthy he was, but he did all right. We all knew he had done well, so now and then we would ask, "Could you help us out?" Ray and I told Carlson, "We really are in trouble. We need a cover over that pumping station." He asked how much we needed, and we said, "About $10,000." He gave us the amount from his personal funds. The faculty lounge in Davis Hall is named after him, and his picture hangs there.

We proceeded to build the shelter over what is now the pump house, which is the small concrete block structure located between the high bay structures lab, Building 484, and the building that would eventually house the shake table, Building 420. Then we started with the pit or basement underneath where the table would be, but we still did not have a building funded. I remember writing proposals and trying to get funds from the Fleischman Foundation and others, but I was unsuccessful. Fortunately, NSF finally came along and provided funds to build the structure.

Scott: Before that, you had started digging the pit out in the open?

Penzien: Yes. Oh, we really bootlegged that project! We believed in what we were doing, of course, but we did start without being able to see the funding to the finish. Certainly no agency, NSF or whoever, could guarantee funding all the way through. We just had to go ahead with confidence that when the next phase of construction came along, funding would be there. That was the way we worked during those years, building that laboratory.

Scott: You say "During those years…" What was the time span?

Table 2.

Number of EERC reports published during the center's first dozen years

1967	1
1968	5
1969	16
1970	10
1971	8
1972	12
1973	27
1974	15
1975	41
1976	32
1977	30
1978	29

Penzien: From about 1965 until the center started in January of 1968. The first couple of years were occupied with the servo-control study.

We finally finished the building, and moved the headquarters of EERC from the Structures Lab, Building 484, to the mezzanine level of the Earthquake Simulator Lab, Building 420. I set up the EERC Library in one small room, I used another for my office,[23] and there was a small reception area in between.

Shortly after we finished building our table, Kajima Corporation built a similar table in Japan. Later, many similar but smaller tables were built in Yugoslavia, Mexico, China, and other countries.

The capability of the original table control system was to provide motions in the vertical and one horizontal direction. The table was upgraded in the late 1990s and it can provide motions in the vertical and two horizontal directions; and, it can control three components of rotational motion as well. Great advances have been made in hydraulic-powered electronically controlled shaking tables since the 1960s, so that the upgraded table can more accurately produce specified earthquake motions.

Rapid Expansion of EERC

Penzien: Well, the lab was completed. NSF had financed it and had funds available, so they started to fund the research. They not only funded experimental work on the shaking table, but also analytical and other experimental research for many faculty members. We started to grow fast. You can look at the EERC report list to see how rapidly we expanded.

The way we were growing, it was clear we needed more space for the headquarters. The old Institute of Transportation and Traffic Engineering of Harmer Davis decided to move to campus. They moved out of their office building at the Richmond Field Station, and we were able to get the university to assign us three of the four wings in the building (Buildings 451, 453, and 454). I was a little worried that I was asking for too much space and concerned that they would not want to give it up, but they did.

We moved the library from that one small second floor room in the Earthquake Simulator Lab into its present location. When we first put our little collection there, it looked lost. I was afraid the dean would come by and say, "You don't need this space." But the library grew, and if you have visited anytime recently you see that it is a big library, a wonderful collection. Soon after we started it, the library became the core of NISEE, the National Information Service for Earthquake Engineering. I'll return to that a little later.

The Golden Years

Penzien: We went through some "golden years" in the center when we had substantial funding. I recall something like $1.25 million per year funding for research at one time. We had little "19900" funds in the beginning. The Regents of the U.C. system could allocate 19900 funds to a particular program at a given

23. By coincidence, CUREE became a tenant of these same rooms at the Richmond Field Station 25 years later, and the present interviewer's (Reitherman's) office is the same one that was first occupied by Penzien.

campus, providing a steady base of support. So we were operating almost entirely on soft money, the funds brought in from various research contracts and grants. At that time, the 19900 money was less than one percent of our budget. Every year I would request more 19900 funds, but presumably because we had substantial soft money coming in, the administration seemed to think we were getting along all right. Once when asking for more funds, we were told, "Oh, you guys are rich."

We accepted that and went ahead, although I did feel the center deserved more hard money, rather than being dependent on each year's research grants. On the other hand, as long as we were successful in getting soft money and doing what we wanted to, it didn't bother me too much. I didn't complain.

Pressure to be Interdisciplinary

Scott: When were you director of the Earthquake Engineering Research Center?

Penzien: I was director twice. I was the first director from January 1, 1968 through, I believe, June 1973—five and a half years. Then I was ready to go and take a rest. I recommended Ray Clough to replace me, and the administration appointed him. Ray was director from July 1973, but did not serve a full five-year term. Probably it was more like four years, as I recall—from July 1973 to mid-year 1977. Ray then asked me if I would come back as director.

All research units are under some pressure from the administration to become more interdisciplinary. Most of the participating faculty in most research units are from one department. EERC was no exception, as most of its faculty was from the Department of Civil Engineering, later called the Department of Civil and Environmental Engineering. Yet a university interdisciplinary unit must cut across departments as far as the research is concerned. Hence, we were under this pressure to become interdisciplinary, and I think Ray was not so interested in pushing that.

So I came back as director in 1977, and served until mid-1980. During those years, I tried to develop more interdisciplinary involvement with an architecture group. I got a professor from electrical engineering involved, and a couple from mechanical engineering. Professor Henry Lagorio was the main person from the architecture school. One result of getting seismologists at Berkeley involved was the SMART-1[24] project that Bruce Bolt and I, along with Dr. Yi-Ben Tsai from National Taiwan University, worked on together for a number of years in Taiwan.

Scott: How did you get others outside the Department of Civil Engineering involved? Did you beat the bushes to find them?

Penzien: I suggested some things. As director, I never believed in trying to demand, but tried to encourage and support people. If you can get funds for something, people will come along because they want to do research where they have funds. My job was to see what I could do to obtain funds, and to encourage interdisciplinary efforts and help in any way I could. It actually worked out quite well.

24. Strong Motion Accelerograph Array in Taiwan.

Scope of Research

Scott: Talk a little about the nature, scope and direction of earthquake research. How much of it is closely related to the shaking table, and how much is not? From an outsider's view, the shaking table was a big thing at EERC.

Penzien: Yes. The shaking table facility was in the limelight. We'd perform tests, and people would come to witness them. It attracted a lot of attention, but did not account for the largest share of the total research program. Only a small percentage of all faculty doing earthquake engineering research were using the shaking table facility. More professors were involved in analytical research or experimental research other than of the shaking table variety.

The other experimental research was primarily quasi-static testing of structural components and frames under displacement-controlled cyclic conditions to obtain their nonlinear hysteretic force-displacement relations. This work was carried out in the Structures Laboratory at the Richmond Field Station and in Davis Hall on campus.

Knowing the backgrounds and interests of the faculty, and the wide interest in earthquake and geotechnical engineering and other fields, it was clear an earthquake engineering research center would have to cover very broad territory. Focusing just on the one experimental facility would be very, very narrow. Only a small percentage of EERC's publications deal with research coming out of that facility. On the other hand, the shaking table was and still is a very important facility. I believe it is still the best shaking table facility in the U.S. There are however, all these other important aspects of EERC—for example the library and the National Information Service for Earthquake Engineering (NISEE) program.

Origins of the NISEE Program

Penzien: In the early days, we had no funding for the library. I always gave all the reports I received to the library. We'd keep getting reports from all over the world. Ray Clough and I would pass everything we received on to the library. I also gave the library a lot of books and things, and so did Ray. I suppose others did too. We started building the library that way, just private contributions from our own collections.[25] The NISEE program started very early, soon after the center was established. The object was to set up an information transfer system. NSF was very interested, and helped support it. As I mentioned, it was set up shortly after EERC was established. Two universities were involved when it was set up, Caltech and Berkeley. For many years there has been a branch of NISEE at each of these universities, although the one at Berkeley was always the largest.

NISEE was originally set up with two other activities besides the library. One was publication of the *Abstract Journal of Earthquake Engineering*, which is a very thorough and complete periodical covering worldwide literature. Ruth Denton, now Ruth Wrentmore, has handled that from the beginning.[26] The other activity was a computer software dissemination pro-

25. The librarians in charge of EERC's library have been: William Berges, Anita Chui, Aileen Donovan, Joy Svihra, Katherine Frohmberg, and currently Charles James.

gram, conducted by Ken Wong until his retirement in 2001. Ed Wilson contributed greatly to the early development of this computer program activity, which is now conducted in Davis Hall.

Scott: Where are these computer programs generated?

Penzien: Everywhere. Any earthquake engineering-related computer program developed under NSF funds is supposed to end up in NISEE. And of course, other computer programs are also included, in addition to those developed under NSF. NISEE distributes a listing of computer programs, and the whole thing has been very successful.

After EERC Directorship

Scott: You and Ray Clough were EERC directors for about its first twelve years. Your second term ran for three years, from mid-year 1977 to mid-year 1980. Would you discuss the period after you left the directorship of EERC the second time, in 1980?

Penzien: My second term ended in 1980, and in academic year 1980-1981 I went on sabbatical and went over to National Taiwan University. In general, after that I continued to be busy with my own work. Consequently, I did not keep track of what was going on at EERC.

Scott: In the 1980s, there was a fairly rapid turnover of directors. You think this is not necessarily for the best?

Table 3.

Directors of the U.C. Berkeley Earthquake Engineering Research Center

1968-1973	Joe Penzien
1973-1977	Ray Clough
1977-1980	Joe Penzien
1980-1985	Hugh McNiven
1985-1987	Harry Seed
1987-1988	James Kelly
1988-1990	Vic Bertero
1991-2002	Jack Moehle
2003-	Nicholas Sitar

(Brief interim director terms not shown)

Penzien: There have been quite a few. Following me when I left the second time, Hugh McNiven became director in 1980. Then Harry Seed followed Hugh McNiven; Jim Kelly followed Seed; then came Vic Bertero, briefly Robert D. Hanson, and then Jack Moehle, the current director [2002].

By the way, it may seem correct now to use Vit for Bertero's first name, because his full first name is Vitelmo. But my generation calls him "Vic." He and I were both from MIT, so when he arrived at Berkeley in 1960 we talked a little and he asked me what his nickname should be. He figured that Vitelmo was too unfamiliar to Americans, and said Vitelmo was the Spanish name for Victor. I said, "How about Vic?" and my generation has called him that ever since.

Scott: That is six directors in the center's second twelve years, an effective average term of only two years.

Penzien: That is what I was referring to. Back in the old days, they'd appoint a director of a research unit and he stayed in that position

26. In mid-2002, NISEE announced that 100,000 abstracts had been compiled and made available over the worldwide web.

about as long as he wanted to. Harmer Davis for example, at the Institute for Transportation Studies.

Scott: He must have been director for between twenty and thirty years.

Penzien: Yes. But that was back when they did not have, or did not enforce, the policy of rotating directors or chairs. I think they had the policy all along, but never enforced it until recent years. Now [1993] they do enforce it. A director's term is five years, same as the chair of a department. After five years, if the organization is doing well because of the director's leadership, then the administration can approve a second five-year term. A third term however, should be discouraged. Also, in the interest of internal continuity within an organized research unit, partial terms of only two or three years should be avoided if possible.

Advantages and Drawbacks to Directing a Center

Scott: Being director of a research center has its advantages, but it also has its drawbacks and headaches. People are not always eager for the post.

Penzien: Being director of a center is a sacrifice, although it depends on how you look at it. If you see it in terms of promotion, it is probably a negative, especially if you become a director when you are fairly young and still moving up through the ranks. Being a director takes time. Also when you are considered for promotion, the fact that you have served as an ORU director does not help much.

Scott: No, they look principally at your publications record.

Penzien: That's right, so seen from that standpoint, it is a negative. You can also look at it positively, however, in terms of feeling you have a duty to serve in an administrative role. All these tasks have to be done by someone. Somewhere along in their careers, everyone should share some of the burden. There is another positive side to it, in that as director you gain a certain reputation outside the University. Inside, I'm not sure it helps.

I always felt that the director of EERC should provide help to the faculty in getting funds and providing services. For many years our NSF funds came in one big umbrella proposal. I coordinated the faculty in writing the big proposal. I asked all of them to send in to me what they proposed to do. Their proposal statements would be funneled to me and I would rework them. If two were proposing the same thing, I would call this to their attention and recommend reworking them so they did not overlap too much. Of course, people do the kind of research they want to work on. All I could do was advise them to get together and decide how they wanted to go, so at least their proposals did not look like duplication. I think this worked well in those years. Faculty members all found their own specialty areas, each significantly different from the others. I think our system was one of the reasons for this. The research guides you into your specialty. If overlap of research is avoided, a wide spectrum of specialty areas will be covered. Our faculty did that, and I think it was one of the strengths of the Berkeley group.

Scott: Evidently the center was very helpful to a wide range of faculty members and to the whole earthquake engineering program at Berkeley?

Penzien: No question about it. EERC has really been a very good unit. A lot of good things have come out of it.

Visiting Scholars and Ph.D. Students

Penzien: After it was established, EERC became very popular in a relatively short time. For a number of years, we had a lot of visiting scholars coming to the center. We funded some, and some brought their own funds. We also provided office space at the center. Certain faculty members, such as Ray Clough or myself, or others would invite visitors to consider coming here. Back at one time, there were a lot of visiting scholars out there at EERC.

Many graduate students wrote their doctoral dissertations on research carried out in EERC. I cannot give you an accurate total number due to the large number of faculty members involved; however, as an indicator, my own Ph.D. students working in this field included S. C. Liu, M. Dibaj, A. K. Malhotra, P. Ruiz, M. K. Kaul, B. Berge, K. Kawashima, T. Minami, B. Ataly, M. C. Chen, D. D. Liu, M. C. Lee, S. Gupta, D. V. Dao, F. Medina, C. S. Oliveira, T. J. Tzong, C. H. Chen, G. S. Liou, W. D. Liu, and H. Hao. Numerous post-doctoral researchers also worked at EERC.

Shifts in Funding

Scott: You spoke earlier of "golden years" when you got pretty good funding. How long did that last?

Penzien: I'd say ten or twelve years, judging from the number of faculty that were funded in earthquake engineering research, people in geotechnical engineering, structural engineering, and a few others. I think for a number of years many of us had all the funds we needed. That, of course, is different today [1993]. The funding from NSF for earthquake engineering research has stayed constant in dollar amounts for many years. Inflation has progressively reduced the value of the funding received.

Also in those "golden years," many of the staff people were paid from NSF funds. When EERC started, the people working on and being paid from NSF projects were young, and have since had substantial increases in salary due to both promotions and inflation-caused salary adjustments. Another factor that has increased cost is that the university overhead has increased.

Scott: All those factors depreciate the value of the constant-dollar funding.

Penzien: Yes. Furthermore, in the late 1980s, there then appeared other commitments of NSF funds, especially those supporting NCEER, the National Center for Earthquake Engineering Research headquartered at the State University of New York at Buffalo. The total dollars in NSF's earthquake engineering research budget stayed relatively constant, but big chunks were then being taken for other purposes. The result is that there was not very much money left, and universities in California have really been hurting because of the shortage in funds. That is why I referred to those earlier years as the "golden years," when many of us had all the funds we needed.

I always felt the umbrella approach to funding was the right way. We followed that umbrella approach of submitting a coordinated package of proposals for a number of years, but eventually NSF apparently decided they did not want to fund big umbrella proposals. So the approach started to break up a bit.

Scott: When did NSF shift to funding smaller research projects and individual proposals?

Penzien: I don't recall just when it happened, but it did seem to be a policy decision. It may not have been stated in writing as a firm policy, but in effect that is the way it worked. I thought the best results came out of Berkeley when we were coordinated and went in with an umbrella proposal. Then NSF created a national center, NCEER, and funded it with one big lump sum of money. That center decided internally how those funds are divided up and who does what. Now that really was an umbrella way of operating!

The Loma Prieta earthquake did lead to something of an upsurge in funding for EERC, from Caltrans, for example, which we can talk about further on the topic of highway bridges [see Chapter 13]. A lot of work for Caltrans has been done through the center, e.g., a big project to evaluate the Bay Bridge East Crossing. A large group worked on that. A lot of retrofit work has been done because of the Loma Prieta earthquake. Some local engineering firms would be in difficulty from lack of work if that earthquake had not come along.

NSF Centers

Penzien: I had mixed feelings on the subject of the new centers set up in 1998. NSF funded three: MAE (Mid-America Earthquake Center), MCEER (Multidisciplinary Center for Earthquake Engineering Research), and PEER (Pacific Earthquake Engineering Research Center). On the positive side, a center does permit a large, coordinated effort on an important problem area which otherwise would be most difficult to implement, e.g. performance-based design, which is the main focus of the PEER Center. Also, the matching funds made available by the creation of those centers—they each had to put up one dollar of state or other non-federal match for every dollar of NSF funding—helps considerably. However, on the negative side, it seems to me their creation has led to an unfair disadvantage to researchers not included in these centers, i.e. individual researchers who have good ideas worthy of financial support, even though in some cases there exists the risk of being unsuccessful. Unfortunately, very limited funds are available to support those individuals. Also, I feel centers tend to divide faculties into the more-favored groups and the less-favored groups when it comes to the allocation of research funds. For this reason, I support greater funding for individual research, made through the standard peer review process, even though it has its flaws.

Reitherman: Would you care to comment today [2002], on one of the large new earthquake engineering programs at NSF, namely

NEES, Network for Earthquake Engineering Simulation?

Penzien: It was good to hear that NSF made available $82 million to develop NEES. I am also pleased to hear that reasonably adequate funding is expected to be available to operate these new and enhanced experimental facilities at the universities, once they are up and running in 2004. No doubt these facilities will considerably improve research capability. I am, however, skeptical that all the lofty goals of NEES will be met. NSF states on its website that NEES will "shift the emphasis of earthquake engineering research from current reliance on physical testing to integrated experimentation, computation, theory, databases, and model-based simulation." It's also supposed to make a big, sudden, change in the practice of engineers and the speed with which research is incorporated into codes and design procedures. The information technology investment in NEES is supposed to make it possible to rapidly disseminate experimental or analytical data as soon as the researcher generates them. This implies that the assistant professor who is dependent on publishing research results to get tenure will see all the hard work of his or her experiment immediately picked up by other people on the sidelines who will be able to publish the experimental results first.

There are valid reasons why researchers need time to make sense of and publish their own data and interpretations. I'm skeptical that NEES will change all of that. If the experimental facilities themselves are well-designed and operated by skilled researchers, we will get some valuable earthquake engineering research. But aside from the investment in laboratory equipment, the NEES plan is being built on a large amount of untested theory about earthquake engineering research. Over the years, I think NEES will increasingly trend toward a more practical approach.

Chapter 10

Ground Motion and Soil-Structure Interaction

If you don't define the input well, you can't expect the design to come out well.

Early Analysis of Accelerograms

Reitherman: Would you explain how you got so heavily involved with studying ground motions, since you're an engineer and not a seismologist?

Penzien: I got involved because being in the field of structural dynamics and looking at the seismic response of structures, you have to prescribe some kind of input. I've always felt from way back that about half of the structural engineer's problem in developing a seismic-resistant design is to have some kind of prescribed seismic input that is realistic and really represents the future ground motions that the structure will see. If you don't define the input well, you can't expect the design to come out well. I got involved in ground motion research back in the 60s originally, when Ray Clough and I studied ground motions and we had a doctoral student, Victor Jenschke from Chile, who was working with us.[27] We took ground motion recordings we had at the time and studied their characteristics. We did that mainly through the frequency domain. At the time, all of the recordings were in analog form, so we were concerned with baseline corrections to the accelerogram. If you didn't apply a baseline correction and you integrated for velocity or integrated a second time to get displacement, you'd find unrealistic drifts would take place. So in those days we were always applying baseline corrections. Another task was correcting the time histories by recognizing the characteristics of the recording instrument, so we made that correction also.[28]

Reitherman: How many accelerograms were available then? Your U.C. Berkeley research report with Victor Jenschke was published in 1964, I assume based on work done in 1963 and earlier, and your Third World Conference on Earthquake Engineering paper on a similar topic was published the following year.[29] The 1971 San Fernando earthquake did not produce its abundance of strong motion records until several years later.

Penzien: We didn't have a lot of records in those days. We worked with the limited number we had. One of the primary records, and the one that was most often used, was the 1940 El Centro record. The 1952 Taft record from the Kern County earthquake was also popular.

Reitherman: Was the 1940 El Centro record thought of by engineers and seismologists as representing the high end of what you would see in an earthquake, until other recordings came along that documented more severe motion?

Penzien: At that time, and for many years, it was assumed that this record represented upper-bound motion. It was the most commonly used record in research not only in the United States, but also all over the world. Peak acceleration in a severe earthquake was thought of as being about a third of gravity, because that was the peak of the El Centro record. It has gone up since then. At some time we shifted and said: not a third of g, but half of g. Of course, now we're up to a level more like 1g and even above. The assumption as to an upper bound of ground motion has changed, because we've recorded motions with higher levels.

This 1940 El Centro record also had a characteristic that we didn't recognize back in the early days. As you know, we now talk a lot about near-field effects, "near-field" meaning that the recording station is within maybe 10 kilometers of the fault. This will produce velocity pulses caused by fault rupture directivity and elastic rebound of the ground. That 1940 accelerogram has velocity pulses in it, but we didn't think about them in the 60s yet. We have other earthquake records now that have velocity pulses in them. We are still lacking in near-field recordings, but gradually we're getting more and more.

27. Jenschke, Victor, Ray Clough, and Joseph Penzien, *Analysis of Earth Motion Accelerograms*. U.C. Berkeley Department of Civil Engineering, SESM 1964/01, January 1964.
28. Jenschke, Victor and Joseph Penzien, "Ground Motion Accelerogram Analysis Including Dynamical Instrumental Correction," *Bulletin of the Seismological Society of America*. Vol. 54, No. 6, December 1964.
29. Jenschke, Victor, Ray Clough, and Joseph Penzien, "Characteristics of Strong Ground Motions," *Proceedings of the Third World Conference on Earthquake Engineering*. Wellington, New Zealand, Vol. I, p. III-125 to III-142, 1964.

Principal Axes of Ground Motion

Reitherman: What was the next phase of your involvement in studying ground motions?

Penzien: In the early 1970s, there was great interest in the ground motions that were being used for nuclear power plant design and evaluation. There was some concern with regard to how the two horizontal components of motions correlate. I was a visiting professor at the University of Tokyo in the academic year of 1973-74. I spent that year studying that problem with Dr. Makoto Watabe, and we came up with the idea of principal axes of ground motion.[30]

Reitherman: Why is it that usually the earthquake's peak accelerations at a site are described in terms of the two recorded horizontal components vis-à-vis the instrument, rather than describing the largest acceleration in a particular 3D orientation that occurred at some instant during the earthquake?

Penzien: The standard way for many years has been just to look at each component separately, and to ignore how one component is phased in with the other. That's about the time we started to get interested in this. You can imagine a north-south component that has a certain time history. Now I'm going to give you a pure case that won't occur, but let's just assume your recorder showed exactly the same time history in the east-west component. Now you know the resultant of acceleration is at the 45-degree direction and the motion in that case would be only in one direction. So you take one of these time histories and multiply it by the square root of 2 and that would give you the motion at the 45-degree direction. That's perfect correlation. That means that the correlation coefficient is plus one, fully correlated. If the cross-correlation is zero, it means the motion in one component looks totally unrelated to the other. If you multiply one component by the other component and integrate the product over time and it comes out to zero, you'd have zero cross-correlation. Of course it's never zero; it does take on a value, which can be plus or minus.

When I got involved in the idea of the principal axes of motion, working with Watabe, we found that if you take XYZ components of free-field ground motion or any motion time histories, you can transform these same components to a different set of reference axes, which is like the transformation of stress in mechanics. Consider an element that has normal stresses in three orthogonal directions, and it has shear stresses on all the planes. You can transform that state of stress by rotating the directions of the axes to where there are no shear stresses, only normal stresses, then the largest of the normal stresses is the major principal stress, the smallest is the minor principal stress, and you have an intermediate stress. Ground motion transforms in the same way.

30. Penzien, Joseph and Makoto Watabe, "Characteristics of Three-Dimensional Earthquake Ground Motions," *Earthquake Engineering and Structural Dynamics*. Vol. 3, p. 365-373, 1975.

Site Response and an Unintended Use for the Shake Table

Reitherman: Early editions of design codes or site-specific design procedures did not take site conditions into account in defining design ground motions. Later on, this began to be routine. When you were beginning to study ground motions, were engineers asking if site conditions affected ground motions, or were they asking how site response affected ground motion?

Penzien: Many years ago, back to the 50s and early 60s, there was a big debate going on as to how much the local site conditions would affect the characteristics of the ground motions. One side felt that the local soil properties had a significant effect on free-field surface motions, while others said the differences in ground motion characteristics came from differences in source mechanism. This of course gradually changed, and Professor Harry Seed of Berkeley was probably one of the leaders in convincing people that local soil effects do change the characteristics of ground motion. Now it's standard for design site-specific motions to incorporate local soil effects. Source mechanisms are another factor, but it is well-established now that local soil properties can significantly modify ground motions.

Reitherman: At the Clough/Penzien Symposium,[31] Roger Borcherdt told a story in his presentation about when he was a graduate student at Berkeley, when the shake table facility had just been put into operation at the Richmond Field Station, and it was being put through its paces to get the machinery in order. He used the vibrations imparted from the actuators through the foundation into the ground to study site response. How did that research happen? Was that just fortuitous?

Penzien: Well, that type of study certainly wasn't what we had in mind in designing the shake table facility. That wasn't in our mind at all. But on the other hand, Roger could see there was a good source of putting energy into the ground. He was interested in how that motion or the energy would be transmitted away from the source. Roger is a very good engineering seismologist and he took advantage of that opportunity, as well he should have.

When we first completed the table, we had to test it out before experimenting with specimens on it. So we shook it, producing high-intensity motion. That generates large forces in the foundation. The foundation then starts to vibrate, and waves transmit out from the foundation through the soil. Dr. Dixon Rea was running these tests, shaking the table harmonically, starting at a low frequency and gradually changing the frequency to a much a higher level. Dixon was only interested in how well the table was performing and how well the commands were producing the intended motions. But on the other hand, it was an excellent opportunity for Roger to measure the ground motions. When the frequency of the vibrating foundation matched a characteristic site frequency, a lot of energy was transmitted out into the surrounding soil.

I was then the director of the Earthquake Engineering Research Center, which was just starting up, and I got word back from the administration that Professor Fred Dickinson, who was the director of the U.C. Forest Products Laboratory nearby, was very concerned. He knew we had built an earthquake simulator

31. *Proceedings of the U.C. Berkeley-CUREE Symposium in Honor of Ray Clough and Joseph Penzien*, Berkeley, California, May 9-11, 2002. Consortium of Universities for Research in Earthquake Engineering. Richmond, California.

facility, and during the testing he was feeling the vibrations in his laboratory. He got quite concerned about what it was going to lead to in the future, thinking it would get worse. So the administration asked for a review of what motions would be produced by our testing and whether we would cause a problem. There was a committee of two appointed, John Lysmer and myself. John was a geotechnical engineering professor on the faculty who was a specialist on foundations and vibrations. We came back with a report that said we felt this wasn't a problem, because we would be testing structures not harmonically but under characteristic earthquake motions, which have a whole spectrum of frequencies.

Random Vibrations Theory

Reitherman: Do you want to comment again here about random vibrations theory, which you mentioned before in connection with your sabbatical at MIT? The fact that earthquake motions are not harmonic motions relates to how predictable their characteristics will be in a given earthquake, which seems to be relevant to the random vibrations topic.

Penzien: In trying to answer this, let me go back to the mid 60s, when Professor Wiegel organized the short course in earthquake engineering I mentioned earlier [see Chapter 9]. I was on leave in Japan that year, but he wrote to me and asked if I would participate in a short course and lecture on this whole issue of whether you can characterize the seismic ground motions as a random process. I had already been teaching random vibrations for some time. Before I went back to the United States to give the lecture, I reviewed recent work that had been done. The first publication that I found, which I was aware of, was by Professor Emilio Rosenblueth. His doctoral dissertation at the University of Illinois was on that topic. He represented ground motion by a random walk process, which leads to random-type motions.

About the same time in England, they were using white noise. White noise can be used to represent sound or other vibrations where all frequencies are at the same amplitude. So I reviewed the existing work, gave the lecture, and later wrote a paper on it.[32] To improve on using white noise, you can start to filter or shape the white noise so it's not uniform over the full frequency range. The most common filter that has been used is one called the Kanai-Tajimi filter. People started to generate a whole family of ground motions, not just one, with the whole family representing a random process. We were defining seismic input as a random process. The concept is sound in that it says you're not looking at just one event, you're looking at many events, and so you get averages. You can also get probability distributions coming out of that approach. The Nuclear Regulatory Commission and designers of nuclear power plants were using a lot of simulated motions, so computer programs were developed to generate them. One popular program called "simquake" (SIMQKE) was written by Professor Erik VanMarcke when he was

32. Penzien, Joseph, "Applications of Random Vibration Theory in Earthquake Engineering," *Bulletin of the International Institute for Seismology and Earthquake Engineering*. Vol. 2, 1965.

at MIT. That approach is not used as much today as it was sometime back.

Reitherman: Is that because more earthquakes have been recorded?

Penzien: We have a lot more records. We're getting more and more records with different site conditions and different magnitudes. Today, we normally will pick recorded motions that best fit the soil conditions at the site of interest, adjust those motions to make them response spectrum compatible, and then use them as specified seismic inputs rather than use randomly generated motions. Once an event occurs and you record motions, they are then fully known, so they are not considered random. Random really means motions that are known only in a probabilistic way, such as future ground motions.

Reitherman: How did you get involved in studying the spatial variation of ground motions?

Penzien: The motions around the boundaries of a dam or under the various supports of a large bridge are not the same at the same instant, and I had some involvement in design projects for large structures such as these. So we started to study spatial variations in the ground motions. Bruce Bolt and I, along with Yi-Ben Tsai, who was the founding director of the Institute of Earth Sciences in Taiwan, made a proposal to set up an array of instruments in Lotung, Taiwan. We proposed to the National Science Foundation that they provide funds to buy the instruments, and to the Taiwan government that they pay the costs of installation, along with maintaining the instruments. This was the SMART-1 Array. These instruments were placed in concentric circles. The idea was to have them spaced so that we could look at the motions, record them during an event at all station locations, and then study the spatial variations of these motions.

Digital Recording of Earthquakes

Penzien: I think there's an interesting point to make with regard to the instrumentation. The SMART-1 Array instruments were the first digital instruments that were used to record earthquake ground motions. When we made the proposal to NSF, we specified that we would use digital instruments. Up to that time they were analog. Because of our experience at the Richmond Field Station with the shaking table, where we did a lot of work changing the time histories from analog to digital, I felt that it was time to go to the digital recording system. We needed to digitally record the earthquake itself. When the reviews came back from NSF they were all supportive of the array, but quite negative about shifting to a digital system. We had to hold out and keep pressing to use the digital system, and we finally got approval. We went ahead then, hoping we could be successful, and went out to find a supplier for the instruments.

Reitherman: These instruments did not exist yet? There were no digital accelerographs?

Penzien: Not at that time, at least not for the whole system. One of the leading manufacturers of instruments was somewhat hesitant to go in that direction, so we went to another instrument-making company, Sprengnether. They agreed to provide the system, and it worked out well. We never had much trouble with the digital instruments. So that was the beginning of digital recording of earthquakes.

Reitherman: After all that effort went into obtaining funding, finding a manufacturer for the new instruments and installing them in the concentric array, did the earthquakes cooperate and provide you with something to measure?

Penzien: Oh, it was amazing. After it was installed, it was only a matter of months, and we had an earthquake of a pretty good size. We found that we had obtained good recordings. Other earthquakes occurred after that, as well.[33] The site was selected because there are a lot of earthquakes on the eastern side of Taiwan, which is very active seismically, but we were fortunate to have so much activity soon after the SMART-1 Array was installed. The results were then used by researchers all over the world. You'll find papers on ground motions that are published by researchers in many other countries where they got the data from SMART-1. So it was an extremely successful project. I also looked at such data working with Dr. Loh and Dr. Tsai.[34] Later there was a very tight array, installed by the U.S. Electric Power Research Institute (EPRI) and the Taiwan Power Company, next to the SMART-1 Array. That array, of course, provided a lot of valuable data also.

Reitherman: Explain how the data from this dense array was useful for design purposes.

Penzien: I should mention that Norm Abrahamson has done a lot of work on spatial variations of ground motions using data from this dense array. He is one of the top specialists in characterizing spatial variations. I also had Carlos Oliveira and later Hong Hao, students at the time, working on SMART-1 data. In our paper,[35] there's a procedure for generating time histories while taking into consideration spatial variations as characterized through what we call coherency functions, i.e. functions that quantify the coherency between the two components of motion at two different locations with a certain separation distance. In some ways, it's similar to what I called cross-correlation of two components of motion at a point. Except now we're talking about how the X component at station A correlates with the X component at station B, separated by some distance. In addition, there is a wave passage effect. The energy passes over an array, and you can look at the data and see how this energy has been moving across the array, passing at a certain apparent wave velocity. Now in design today, for site-specific motions, we're generat-

33. Bolt, B., C. H. Loh, J. Penzien, and Y. B. Tsai, *Preliminary Report on the SMART 1 Strong Motion Array in Taiwan.* U.C. Berkeley Earthquake Engineering Research Center, EERC 82/13, August 1982. The SMART-1 Array became operational in September 1980. During its first year of operation, its 37 accelerometers recorded 15 earthquakes with magnitudes ranging from 3.4 to 6.9.

34. Loh, C. H., J. Penzien, and Y. B. Tsai, "Engineering Analyses of SMART-1 Array Accelerograms," *International Journal of Earthquake Engineering and Structural Dynamics*. Vol. 10, 1982.

35. Hao H., C. S. Oliveira, and J. Penzien, "Multiple-Station Ground Motion Processing and Simulation Based on SMART-1 Array Data," *Nuclear Engineering and Design III*. February, 1989, p. 293-310.

ing ground motions having spatial variations that include the wave passage effect and which show incoherencies in the motions.

Coherency of Ground Motion

Reitherman: At what scale are those effects significant? Take the Golden Gate Bridge at one extreme and a single-family house at the other.

Penzien: Here are the coherency functions. [Penzien has taken a report from his bookshelf and points to a graph.] They are functions of frequency. If you have very high frequencies, you'll find that in a relatively short distance, the motions will become significantly uncorrelated. If you have low frequencies, it will take a much bigger distance to lose the same correlation.

Reitherman: So even in a hundred meters from one end of the structure to another, if it's high frequency you get a fairly big difference in the motion?

Penzien: Yes, but typically a structure has its fundamental frequency in the range of 1/2 to 4 cycles per second. From this graph, we can see that you're not going to have too much of a loss of correlation in 100 meters for frequencies in this range. Normally, considering the length of a typical building and the fact that it doesn't tend to vibrate at a very high frequency, you don't need to consider spatial variations. It is when we consider long extended structures that we have to bring it into the design process, like the Golden Gate Bridge or the new San Francisco-Oakland Bay Bridge East Crossing that is now being designed.

We don't have a requirement in the standard code for bridges to consider this spatial variation in ground motion, but for big bridges, we do now consider this. To do so requires a rather sophisticated analysis. Not everyone can pick up a program and quickly consider these variations. But it is certainly done here in the Bay Area on the retrofit projects for the toll bridges. Nonlinear time history analyses are now often required in assessing the seismic performance of large bridges as originally designed or as planned for retrofit. But that's not done for the smaller, standard bridges.

Reitherman: Are there any other topics you would like to cover under the heading of ground motions?

Penzien: Our ability to characterize ground motions has greatly improved over the years. Today, there are more and more probabilistic methods used to characterize ground motions. As you know, now we often use uniform hazard spectra. One spectrum may represent 500-year mean return period spectral values, another 1000-year mean return period values, and so on. To generate such hazard curves, you have to go through the complete probabilistic approach in representing such motions. Of course, it is only in recent years that this approach has become common in practice. Professor Allin Cornell was one of the leading pioneers in this effort.

One thing I would like to add, now that we've talked about various things I've done over the years on the subject of ground motions—I really don't consider myself an expert on this subject. I regard myself as something of a jack of all trades, rather than the authoritative source on these topics we're discussing.

Early Studies on Soil-Structure Interaction

Reitherman: Perhaps you could give a description or definition of the phenomenon of soil-structure interaction.

Penzien: Soil-structure interaction is what we have called this in the past, but recently we have begun to call it soil-foundation-structure interaction. When we have big foundations we have interaction between the foundation and the soil, and also interaction between the foundation and the structure. If the structure is not there, the ground is moving in what we call a free-field motion. But now if you put a structure at that location, and put a rigid mat foundation on the surface supporting the structure, and if you want to make an analysis of the dynamic response of the structure, you can't take that free-field motion that was there and specify that as the motion of the base of the mat. The response of the structure feeds forces back down into the soil, which in turn changes the motion. Changing the motion changes the dynamic response. So it is this interaction back and forth that we call soil-structure interaction. It can become more complex if you have very deep piles, because there is now an interaction all the way down through the whole foundation, interacting with the free-field motion.

Soil-structure interaction and its importance go way back. I think it was the nuclear power plants that first made it necessary to consider soil-structure interaction with the big containment structures, which are very massive and heavy. The amount of soil-structure interaction will depend on the relative stiffness of the structure and the soil. If you have a very stiff and massive structure on a soft foundation, there will be a lot of soil-structure interaction. On the other hand, if you go to the other extreme, and have a light structure and it's founded on rock, the feedback forces from that light structure into the rock aren't going to change the rock motion from what it would be without the structure.

Reitherman: Does soil-structure interaction always tend to reduce the loading on the structure?

Penzien: Generally, but not necessarily. If you have a lot of soil-structure interaction, you could change the fundamental period of the structure, which depends on mass and stiffness, and a change in period can either increase or decrease response.

Reitherman: What was one of your earliest projects in this area?

Penzien: In 1962, I was involved in the Caltrans Elkhorn Slough Project, which was when we first started treating the interaction between bridge piles with the soil. We had to develop elements that would represent that interaction. I was working with Harry Seed at the time, and also Charles Scheffey, who was then on the U.C. Berkeley faculty. There was a student, Richard Parmelee, who was working with us on modeling pile-soil interaction.[36] There were a number of students in the late 70s who worked on modeling of pile foundations.

---

36. Scheffey, C.R., R.A. Parmelee, Joseph Penzien, "Earthquake Response—Bridges on Piles Extending Through Deep Sensitive Clays," *Proceedings, First Chilean Conference on Seismology and Earthquake Engineering*. July 15-19, 1963.

One of my doctoral students, Ma-Chi Chen, worked on soil-structure interaction related to bridge abutments. At that time, this field was very young, and the results that we generated were not used then. Just recently, I had a Caltrans Seismic Advisory Board meeting, and there was a presentation of modeling of this very problem in a project now being done by Caltrans. Brian Mahoney from Caltrans did his doctoral dissertation not too many years ago at U.C. Davis, where he built an abutment, including backfill, and did experiments.

Reitherman: I noticed that there is a soil-structure interaction paper that you co-authored with Anil Chopra in 1977 for a conference in Taiwan.[37]

Penzien: Yes, that was an overview paper on modeling of soil-structure interaction. It was a workshop as I remember, so it wasn't on a specific piece of research. I believe we treated it using the substructure method that Chopra had used quite a bit. He started using substructuring earlier than that, which means you model the foundation separately from the structure, then you couple the two together and satisfy all of the conditions of continuity. Later, with Sunil Gupta, we published a paper in 1981 on the concept of making a theoretical cut through the soil around and under the foundation, so you ended up with a hemispherical portion of soil, within which was embedded the foundation, and sticking above that was the structure.

Reitherman: What analytical device was introduced to separate the hemisphere from the rest of the earth?

Penzien: You have to put in boundary elements. The soil in that hemispherical body of soil is made up of three-dimensional finite elements. Then of course the foundation, if it's piles or some other type of foundation, would be modeled with finite elements. But on the hemispherical boundary, you have to get the seismic input into that soil. To do so, each finite element node around the boundary is connected to a three-dimensional impedance element. The motion that you're putting into that impedance element is the free-field soil motion that you calculate separately. That is the motion of the soil without the presence of any structure or foundation.

The impedance element reflects the interaction at the boundary, representing mass, stiffness, and damping effects. The first student working on that was Sunil Gupta. His work was done assuming the soil was a uniform half space. Half space is when the body extends to infinity below the surface. You can think of an infinite body in all directions, but when you slice it in half, you have a half space. While I was working with Gupta, Professor T. Y. Lin, and Professor C. S. Yeh came over from Taiwan and worked with us on the same concept. Then I had another doctoral student, T. J. Tzong, and he worked on the same problem of generating these boundary impedances around this hemisphere. But the difference was we were working with the layered half space.

37. Chopra, Anil K. and Joseph Penzien, "Earthquake Response of Structures Including Structure-Soil Interaction," *Proceedings of the Advanced Meeting on Earthquake Engineering and Landslides*. Taipei, Taiwan, August 29-September 2, 1977.

When I was a consultant in the 1980s for Tokyo Electric Power Services Company, we continued development of the computer program called HASSI, Hybrid Analysis of Soil Structure Interaction. We developed HASSI 1 and HASSI 2, and so on over a number of years and finally ended up with HASSI 8. Wen Tseng started in the very beginning of the programming. It shifted over in the later years to C. H. Chen, who was another one of my doctoral students.

Then later there was a paper with Francisco Medina in 1982.[38] This was kind of branching off from the work that we were doing with Gupta. Instead of the impedance elements, he used a different model, using what are called infinite elements. These are infinite in one dimension. You have a little area and there's the end of an element, but it goes on out into the half space to infinity. That infinite element will characterize radiation damping.

Reitherman: Did you have any opportunities to conduct experimental, rather than analytical investigations, on the subject of soil-structure interaction?

Penzien: There was a big, cylindrical, very rigid structure built in Taiwan near the SMART-1 Array, placed there specifically to study soil-structure interaction. It was a fairly soft site. That whole project was sponsored and funded by EPRI and the Taiwan Power Company (TPC). A lot of earthquakes came along not too long after they built that test structure, and it was well instrumented to measure very accurately its response as compared to that of the free-field motion. There was a program sponsored by EPRI and TPC inviting different groups to use the same data and make correlation studies. Wen Tseng was a very key person in that work. He was then at Bechtel. C. H. Chen was then on the faculty of National Taiwan University, and he was helping us on different problems in my office in Taipei. That was probably one of the most valuable field investigations that focused on soil-structure interaction. There was a workshop in Palo Alto sponsored by EPRI and TPC. The Nuclear Regulatory Commission was also involved in this workshop, which took all of the measured results and correlated them with analytical predictions of soil-structure interaction. Wen used different methods of modeling and analysis to predict results and then compared them with the field-test results.

38. Medina, F. and J. Penzien, "Infinite Elements for Elastodynamics," *International Journal of Earthquake Engineering and Structural Dynamics*. Vol. 10, September-October, 1982.

Influence of 1971 San Fernando Earthquake Ground Motion Records

Reitherman: Looking back on the 1971 San Fernando earthquake, how influential was it in changing the way engineers regarded ground motion, both in terms of how great the accelerations could be and the existence of near-fault effects?

Penzien: The Pacoima Dam record had a very high peak acceleration, over 1g, much higher than what we had been considering up to that time. So of course there was some debate whether that high acceleration was truly present in the free-field motion, or perhaps it could have been affected and increased because of a fracture in the rock that took place directly under the instrument. But in any case, it was clear that ground motion could be much higher than what we had previously thought. I got involved with Tetsuo Kubo, who is now a professor in Japan. He worked with me and analyzed a lot of the San Fernando records.[39] It was a follow-up to the work I did with Watabe. He was using all three components, looking at directions of the principal axes and how they change with time. He had a moving window in time where he was looking at the changes in principal directions.

Reitherman: How did the improvements in understanding of strong ground motion affect design procedures?

Penzien: Much work was done at the University of Illinois in the 1960s. Nate Newmark, Andy Veletsos, and Bijan Mohraz all did a lot of work there looking at response spectra. They contributed a great deal to how you could develop a response spectrum for design. Then somewhat later, Nate Newmark joined with John Blume, and they developed spectra that were adopted for nuclear power plants. Various forms of these spectra were incorporated into many codes. Of course in more recent years, other changes have been made in the spectra. There are more and more site-specific spectra being used.

Reitherman: By the time of the 1971 San Fernando earthquake, the tripartite plot of earthquake spectra had become common in the earthquake engineering literature. When did that graphic means of depicting ground motion become popular?

Penzien: The tri-partite plot for a seismic response spectrum started with Newmark and Veletsos nearly 50 years ago. They probably picked it up from the field of mechanical engineering, where engineers had done some similar things. But for our earthquake engineering field, it was Newmark and Veletsos that started to present response spectra on tri-partite plots. One very nice thing about it, on the same plot you see the acceleration spectrum, velocity spectrum, and the displacement spectrum. There is a frequency range over which the acceleration spectrum will plot somewhat close to a straight line on the tri-partite plot. Then it will change to where the velocity spectrum becomes rather flat, and that's over a velocity controlled range. Then you go to the very long period range and it will change over to a nearly constant displacement spectrum.

Awards

Reitherman: Newmark's name crops up several times in your interviews, and another connection with him is the fact that you received the Nathan M. Newmark Medal of the American Society of Civil Engineers. Do you remember when you first met him?

Penzien: I first knew *of* him when I was a student, perhaps even an undergraduate. Then

39. Kubo, T. and J. Penzien, "Simulation of Three-Dimensional Strong Ground Motions Along Principal Axes – San Fernando," *International Journal of Earthquake Engineering and Structural Dynamics*. Vol. 7, No. 3, May-June 1979. See also T. Kubo and J. Penzien, "Characteristics of Three-Dimensional Ground Motions Along Principal Axes, San Fernando Earthquake," *Proceedings of the Sixth World Conference on Earthquake Engineering*. New Delhi, India, 1977.

I got to know him well in 1949 when he came over to MIT and I was working on my research in blast effects on structures. I was involved in the Operation Greenhouse test structure, which was subjected to the atomic blast. Newmark was involved in that. He came over to teach us how to carry out the dynamic analysis. Newmark really started the structural analysis numerical procedure of solving the differential equations by step-by-step numerical procedures. That was new then. So he came over and went into the classroom with myself, Bill Wells, and Professor Harry Williams from Stanford and showed us the method on the blackboard. After that I organized a short course in Taiwan and I invited Newmark to come over. On a one-on-one basis he was a really great guy and someone that you could really respect. Then there was a short course in Bogota, Colombia with the lectures of Newmark, myself, and Bob Whitman from MIT. I would see Newmark at all the World Conferences, so we got to know each other well.

Reitherman: Another ASCE medal you were awarded was the Alfred M. Freudenthal medal in 1986.

Penzien: I didn't know Freudenthal personally, but I knew of his work. It's one of ASCE's big honors; I'm not sure who nominated me.

Reitherman: There's a medal of yours on your *curriculum vitae* named after someone you know well: the Housner Medal.

Penzien: I was the fourth person to receive that honor from EERI.[40]

Reitherman: Do you remember when you first met George Housner?

Penzien: I suppose I got to know him well when I started my activities in Japan. I knew him quite well starting in 1960. We were later together in Japan at the International Institute. He served as a consultant to that Institute on one of the oversight advisory groups. Later on, he went off of that group and I replaced him. I remember traveling in Japan and sitting together on the trains and talking about whatever.

Reitherman: I know from these interviews you're modest about your accomplishments, but let me mention a few more honors, awards, and medals and ask you to comment on them. The Alfred E. Alquist Medal of the California Earthquake Safety Foundation, 1996.

Penzien: That one has a connection with Alquist himself, going back to the early 1970s when I was on an Alquist committee set up by the California legislature. State Senator Alquist was really pushing earthquake engineering and earthquake safety. Karl Steinbrugge was a key person in that effort.[41] Governor Reagan had an Earthquake Engineering Advisory Committee around the same time. I have the letter from Reagan at home thanking me for serving.[42]

Reitherman: Tell me about the 1969 NATO Senior Science Fellowship.

Penzien: I was on a NATO Fellowship spending my time in Portugal.

Reitherman: Portugal? I know you've spent time in Japan, Taiwan, and Korea, but I hadn't heard of Portugal before. What did you do in Portugal?

40. The first three recipients were George W. Housner (1990), John A. Blume (1991), and Donald E. Hudson (1993).

Penzien: I spent several months in the Civil Engineering Laboratory of the Portuguese government, which is located in Lisbon.

Reitherman: Was Ferry Borges the director at that time?

Penzien: Yes. Ferry Borges came to Berkeley. He taught a course on risk analysis at that time. That was one of the reasons I was interested in going over there. I was interested in trying to understand better the extreme value theory. This was a subject that I was interested in, but it was very difficult. The people were very good over there in that subject.

Reitherman: In 1977 you were elected to the National Academy of Engineering.[43] How does one receive membership in the Academy? Do you suddenly get a phone call or letter and somebody says "Congratulations"? Or do you know you're going through some sort of process of being considered?

Penzien: No, you don't know there's a process underway. It is supposed to come as a surprise. That's the process. It's the same process used today. If someone who is a member of the academy thinks of someone they feel is qualified and should be a member, they can nominate the person. You have to have support from three or four other members and make your case. There are a very large number that are nominated, but a small number elected each year.

I'd have to say with regard to all these honors that the way it works is in two phases in a career: First, it's difficult to get the attention and respect of peers to earn the first one, but then the first one pulls others along with it and usually you're not even sure why you were picked out from among other worthy people to get an award. It's an honor to receive these awards, but you can't let them give you an elevated opinion of yourself.

---

41. Penzien was a member of the Advisory Group on Engineering Considerations and Earthquake Sciences, along with Clarence Allen, Clifford Cortright, Henry Degenkolb, Charles De Maria, Gordon Dukleth, George Gates, Richard Jahns, Carl Johnson, John Meehan, William Moore, Gordon Oakeshott, H. Bolton Seed, George Simonds, and Karl Steinbrugge. Steinbrugge was also chair of the Executive Committee of the overall effort, and Robert Olson and Stanley Scott split the overall managerial responsibilities as assistant directors. The historic report produced was *Meeting the Earthquake Challenge: Final Report to the Legislature*, State of California, by the Joint Committee on Seismic Safety. January 1974.
42. Penzien served on the Research and Investigations Committee of the Governor's Earthquake Council; *First Report of the Governor's Earthquake Council*. November 21, 1972.
43. The word "engineering" in the seismic design field generally refers to "civil engineering," including sub-disciplines in that field such as structural or geotechnical engineering. Here the word has a much broader meaning: The 12 Sections of the National Academy of Engineering are Aerospace, Bioengineering, Chemical, Civil, Computer Science, Electric Power/Energy Systems, Electronics, Industrial-Manufacturing-Operational Systems, Materials, Mechanical, Petroleum-Mining-Geological, and Special Fields-Interdisciplinary. *Engineering and the Quality of Life: Directory of Members and Foreign Associates*. Washington, D.C. National Academy of Engineering. July 2002.

Reitherman: Your curricula vitae lists "Election in 1978 to be a Fellow of the American Academy of Mechanics." Perhaps you should explain this has to do with a branch of applied physics rather than auto repair.

Penzien: As you know, we have mechanics in civil engineering. The mechanics specialists are those people dealing with plate theory, shell theory, theory of elasticity, etc. Those are all in the field of mechanics.

Reitherman: Also here on your curricula vitae is another honor that must have a story behind it, and it brings up yet another country I had no idea you had any connection with. In 1979, you were elected an Honorary Member of the Peruvian Association of Earthquake Engineering.

Penzien: After our shaking table had been installed at EERC, I went to various places to advise people on shaking table facilities, such as to help Kajima in Japan when they set up their table. It turned out that the development of the Berkeley facility had a significant beneficial effect in a number of countries.

I went down to Peru to advise them on the design of their new shaking table facility at the Catholic University in Lima. I had previous contact with Professor Julio Kuroiwa, who had lived in Peru. There are a lot of Japanese living there. I've known Professor Kuroiwa a long time. He invited me to go to the annual meeting of their Association of Earthquake Engineering, and that was when they told me that they had made me an honorary member.

Chapter 11

Tunnels, Dams, and Offshore Platforms

In medicine, we have preventive medicine, and then we have cures once someone is sick. It's better to prevent it than to let it happen and cure it.

Effects of Earthquakes on Tunnels

Reitherman: Your experience with bridges is widely known, but let's turn first to the subject of tunnels. How did you get started in that area?

Penzien: My first involvement came in 1981, when Mr. Fan, who was then Chairman of the Board of China Engineering Consultants, Inc., contacted me and asked if I would help them in assessing the seismic performance of the Kaohsiung Cross-Harbor Tunnel, which they were designing. So I undertook that work, along with Wen Tseng. We carried out a seismic performance evaluation using several different models.

My next work on tunnels was working on a network of rock tunnels in a mountain in Taiwan. There were multiple tunnels intersecting each other. These tunnels were to be used by airplanes operated by the Air Force Defense Ministry. There were big stress concentrations at the intersections of these tunnels. With finite element modeling, Wen and I carried out analyses of the seismic waves interacting with the tunnels. The next project was the Taipei Railway Tunnel, which came about when they decided to put the railway underground through Taipei. So C. H. Chen and I made analyses of that tunnel, along with the underground stations. This work was done for T. Y. Lin Taiwan.

Later, I carried out a seismic analysis of the BART bored tunnel lining in San Francisco. Currently, our company, ICEC, International Civil Engineering Consultants, is consulting on the transbay tube portion of the BART retrofit project as a subcontractor to Bechtel. Another San Francisco project on which I served as consultant to Bechtel was the MUNI Turn-Around Project, which consists of a bored tunnel with steel lining for part of it and a cut-and-cover reinforced concrete box for the remaining portion.[44]

Reitherman: For a tunnel going through rock, what is the mechanism by which the lining is damaged? It seems like the inertial forces in the concrete lining itself would be insignificant.

Penzien: Yes, these inertial forces are usually insignificant—it is the deformation in the rock that takes place because of the seismic waves. A round cross-section tunnel—that is, the empty space excavated through the rock—tends to deform into an oval shape, and the tunnel lining is then forced to undergo that same deformation. If you have intersections in tunnels like the project for airplanes in Taiwan, high stress concentrations take place at these intersection locations, which could cause a problem. If rock fractures are already present, they may allow some rock to break loose during an earthquake, causing damage to the lining.

Reitherman: A tunnel was badly damaged by an earthquake in 1952 in Kern County, California where the fault rupture extended through a railway tunnel. Has the fault rupture problem been present in any of your tunnel consulting projects?

Penzien: Well, that's another real concern. We have the BART tunnel going out through the Berkeley Hills, and there is a crossing of the Hayward fault there, where you might get about 2-1/2 meters of offset. This is another one of the many topics in the BART project.

Reitherman: Is there anything fundamentally different about tunnels compared to underground pipelines? Or is it just a difference in scale and materials?

Penzien: Yes, they're pretty much the same in theory. The ground is going to deform, and the structure embedded in it is primarily under deformation control, not force control, as in the case of aboveground structures.

Reitherman: Please explain the Alameda Tube retrofit. You served on the Technical Advisory Panel for that Parsons-Brinkerhoff project. We should preface this question with the background that this tunnel goes underneath part of San Francisco Bay to connect Alameda Island with Oakland.

Penzien: One main concern was liquefaction of the soils around and under the tube during an earthquake, which could possibly cause the tube to float upwards. Retrofitting for possible

44. Penzien co-authored with C. L. Wu "Stresses in Linings of Bored Tunnels," in *Earthquake Engineering and Structural Dynamics*. Vol. 27, 1998.

liquefaction was carried out by putting in stone columns to provide for dissipation of excess pore pressures and to produce some densification. The whole approach was to prevent liquefaction rather than let liquefaction occur. To me, that's the right way to go. Tethering a tunnel, holding it down to contend with liquefaction, gives you the situation of a balloon on a string, free to move around excessively even though its upward movement due to buoyancy is somewhat under control. In medicine, we have preventive medicine, and then we have cures once someone is sick. It's better to prevent it than to let it happen and cure it.

Dams

Reitherman: Perhaps your Berkeley colleagues Ray Clough, Anil Chopra, or Harry Seed are more identified with the seismic analysis of dams, but you had a little experience along those lines in Taiwan, which revisits an early theme in your career as an engineer fresh out of school working for the Corps of Engineers. Tell us about the Feitsui Dam.

Penzien: I was a consultant to Sinotech when that firm was designing the Feitsui Dam in the 1970s in Taipei. The dam was a large structure that took the better part of the 1980s to construct. They asked me to help them set the seismic design criteria, meaning what type of ground motion time histories should be used. They had modeled this arch dam design with the usual finite elements, but then the finite elements were carried on out into the foundation. The finite element model was the dam itself plus a large body of the ground all the way around. Then they specified the seismic motion at the boundary of this big finite element model. The problem was that when you have a body like that, you have so many closely spaced modes, meaning the frequency from one mode to the next is very close together. So I recommended to them that the finite element model be just the dam itself and that they put in boundary impedance elements all around the dam. We used published impedances for infinite rigid strips, because the arch dam is very narrow all around. At one point it's like a rigid strip interacting with the rock. I had them program those impedances into the boundary. Later on, TEPSCO (Tokyo Electric Power Services Company) in Japan did the same thing when I was a consultant advising them.

Reitherman: There was another early paper, "Earthquake Analysis of Dams," with Chopra Clough, Seed, and Dibaj.[45]

Penzien: I worked with Dibaj on one particular part of it, which was the response of the dam to wave passage. It was an extension of Anil Chopra's Ph.D. thesis.[46]

Structural Dynamics of Offshore Platforms

Reitherman: When did you become involved in the structural dynamics of offshore oil platforms?

Penzien: The design of these offshore platforms was a very active field in the 1960s. At that time, I was teaching a random vibrations course, and I was looking for somewhere I could put my

45. Chopra, Anil, M. Dibaj, Ray Clough, and Joseph Penzien, "Earthquake Analysis of Dams," *Proceedings of the Fourth World Conference on Earthquake Engineering*. Vol. III, B5, pp. 101-110, Santiago, Chile, January 13-18, 1969.

students to work. I knew that the hydrodynamic forces on these offshore platforms really should be represented as a random process.

The flow conditions were being defined through a wave height spectrum. If you take a point on the surface of the water, and the wind-induced waves are going by, the surface goes up and down. If you take a time history of that motion, and you generate what we would call power spectral density functions, that's exactly what the wave height spectrum is. Then you can convert from that over into the flow conditions in terms of the water particle motion, but not in a deterministic way. This is defined only as a random process, which means considering the full spectrum of frequencies in the water particle motions having random phase angles. I had a student, Terence Foster, who was my first doctoral student working on the response of offshore platforms to the sea flow conditions. Then the next student was A. K. Malhotra, who was the first one to give a stochastic characterization of the sea flow. When you generate the hydrodynamic forces, there are some forces on members of the platforms that are proportional to acceleration of the water as it moves past the structure, which we refer to as added mass forces. There are other forces that are based on velocity, which we call the drag forces.

Reitherman: Do the hydrodynamic forces tend to govern for the design of offshore structures, as compared to the seismic forces?

Penzien: If you consider a very tall platform or tower, if it's off in the North Sea or someplace where you don't have too much earthquake activity, the design is totally controlled by the wave action. The North Sea has tremendous waves, so that's the issue there. If you have a tall tower, even where there is seismic activity, it tends to be very flexible, and if you have long period water waves, they can be more critical. However, if you have a shorter tower that is stiff, then you can have significant seismic excitation. So the relative intensity of these two types of excitation depends on the geometry of the structure, which in turn has to do with the offshore water depth, as well as the expected waves and earthquakes for the site.

Reitherman: In the analysis of these platforms, do you have to calculate the additional damping of the vibrating structure that is caused by the way it pushes its way back and forth through the water rather than vibrating in thin air?

Penzien: There is the structural material damping, which of course we handle in the usual way. But in addition you have to account for the hydrodynamic damping, and that's a drag force, which at any time is proportional to the square of the relative velocity between the structural member and the water. Now, Malhotra and I worked on that and developed a

46. "Funded by the State of California's Department of Water Resources, the project was concerned with seismic analysis of earth dams. Ray Clough, Joe Penzien, and Harry Seed were the Principal Investigators…Ray and Joe offered me a golden opportunity to work on this project. This became the basis for my Ph.D. research. It led to one of the early applications of the finite element method to earthquake analysis of continua." Chopra, Anil, "Ray Clough and Joseph Penzien: My Mentors," *Proceedings of the U.C. Berkeley – CUREE Symposium in Honor of Ray Clough and Joseph Penzien*. Richmond, California, CUREE, 2002, p. 7.

treatment where you linearize the damping. That became quite common in the industry as a technique to treat the nonlinear drag forces in a linearized form. Later on, one of my students, M. K. Kaul, got involved in the nonlinear modeling, using a stochastic excitation and analyzing it with a method used in different disciplines that is known as the Fokker-Planck method. In terms of sea flows, these were treated as two-dimensional problems, with the wind blowing in one direction and the flow being always in the same direction, and thus no cross-flow. Then they started to characterize the sea conditions with a directional spectrum. It is the same thing as the wave height spectrum, except it characterizes the flow and surface waves with the presence of some cross-waves. So we started solving the problem of two-dimensional surface conditions that then led to three-dimensional flow conditions, again characterizing this as a stochastic process, which means all phase angles of the harmonics are random. Bent Berge from Norway was my student who worked on that problem.

Reitherman: Is the loading combination of large water waves and a strong earthquake so improbable that you never consider the wind and seismic loads acting simultaneously?

Penzien: Well, we treated them separately in our analyses. Of course, when you get a big earthquake that you would want to consider in the design of the platform, I think it is an extremely small probability that you would get the design wave action at the same time. We never tried to combine them.

Reitherman: What about the different types of platforms? How does the basic structural system of a platform affect its seismic response?

Penzien: Not all platforms have legs that bear down on the seafloor the way a building or ordinary tower sits on the ground. G. S. Liou, one of my students, and I studied the response of the tension leg platform. In a tension leg platform, the platform is semi-submerged and you tie it down to the bottom with some tubular tethers. The buoyancy exceeds the weight of the platform, which means putting tension in these tethers that go all the way down to anchor blocks. So we studied that problem. We were very much concerned with the vertical motion, because laterally the structure is so flexible that horizontal motions weren't the problem. I think G. S. Liou is probably the last student I had who worked on that. Professor Ronald Yeung also got involved, helping us with the hydrodynamic problems. We had to calculate the hydrodynamic forces on the big blocks that are sitting on the bottom of the sea.

Wen Tseng and I also got involved in gravity platforms, which are the large ones that they build in a dock and then float out and sink, as they have done in the North Sea. Another time, I was involved in an offshore platform with Bob Wiegel.

Reitherman: Was that after the short course and resulting *Earthquake Engineering* book that Wiegel edited?

Penzien: Oh yes, much after that. The book was based on the short course in 1965, and this would be later. I got involved in the gravity structures about 10 years later, in the 70s. Wen and I wrote some papers on that for the offshore conferences.[47] I started with my first student in the 60s and ended up with my last student somewhere in the late 80s, with a final paper in 1988.[48] I got involved in the 1990s as an advisor to the Navy on the Mobile Offshore Base Project. The Mobile Offshore Base went through a study phase, but it's no longer an active project. The idea was to build a mile-long floating airbase offshore, out in the ocean. It had to be big enough for even the largest planes to land. It was mobile, so it could be moved wherever it may be needed, so it was also self-propelled.

Another unusual project for which I was a consultant was in 1997. The idea was to build a highway underwater through a tunnel, but not a tunnel lying on the bottom or embedded in the seafloor. The tube was to extend through the water and be tethered, similar to a tension leg platform. I worked on that with Bill Webster.

Reitherman: I was at the memorial service in April of 2002 at Stanford for John Blume, and Joe Nicoletti was talking about some of the early offshore platform design work Blume's office did. He explained that they were designing these large structures when it was a brand new field. Did you ever get involved with Blume on one of those projects?

Penzien: I didn't, but I know that they were heavily involved in offshore work. Earlier, that same office did a lot of work on nuclear power plants as well. John Blume was a good engineer, and Joe Nicoletti, who has served on the Seismic Advisory Board with me for Caltrans, is another good engineer.

47. Penzien, Joseph and Wen Tseng "Seismic Analyses of Gravity Platforms Including Soil-Structure Interaction," *Proceedings of the Offshore Technology Conference*. Paper No. OTC 2674, pp. 645-654, May 3-6, 1976. Penzien, Joseph and Wen Tseng, "Dynamic Analysis of Fixed Offshore Platforms: Advances in Civil Engineering through Engineering Mechanics," *Proceedings, Second Annual Engineering Mechanics Division Specialty Conference*. ASCE, pp. 510-513, 1977.

48. Liou, G. S., Joseph Penzien and R. W. Yeung, "Response of Tension-Leg Platforms to Vertical Seismic Excitations," *International Journal of Earthquake and Engineering and Structural Dynamics*. Vol. 16, No. 2, February, 1988.

Chapter 12

International Research and Consulting Work

At an evening session, I proposed that we think of cooperative research between Japan and the United States…

U.S.-Japan Large-Scale Testing Program

Reitherman: Could you explain how the U.S.-Japan Cooperative Earthquake Engineering Research Program Utilizing Large-Scale Testing Facilities started?

Penzien: It started as a result of the 1968 Tokachi-Oki earthquake. That earthquake caused heavy damage to school buildings up in northern Japan. That alarmed everyone. Following that, there was a workshop on school buildings held in Hawaii in 1969 to discuss the problems and perhaps what we should do in the future. So, that's really what got us to look to Japan for cooperative research. We had a workshop or symposium in Sendai, Japan in 1970.[49] At an evening session, I proposed that we think of cooperative research between Japan and the United States, not specifically on school buildings, but in general. They then came back and were very enthusiastic about it and took the initiative, led by Professor Hajime Umemura. He proposed that we start planning a large-scale test program, which we did. As you know, they had built the large facilities for testing in Tsukuba. So we started the planning on that, I believe in 1977.

Then in 1979, there was an agreement signed by the National Science Foundation for the U.S. and the Ministry of Construction and the Science and Technology Agency in Japan to undertake this large-scale test program. It was implemented through the UJNR[50] agreement, and so that's the start of the large-scale test program. We were already doing cooperative research with the Japanese under NSF regular sponsored research. It wasn't coordinated as one big program, but here at Berkeley we had NSF funds and I invited some Japanese to come over and work with me and Bertero and others. Mete Sozen invited someone to come to Illinois and work with him. That was a very good program; we accomplished a lot in those years and a number of papers were co-authored by Americans and Japanese that covered many different topics in earthquake engineering. In previous interviews, I talked about working on ground motion studies, for example. The large-scale test program got underway in 1980. That was the start of testing a seven-story reinforced concrete building in the Tsukuba laboratory.[51]

Reitherman: How did you agree on what that first type of specimen should be?

Penzien: I think it was influenced by the school buildings that failed, so it seemed to be logical to start with reinforced concrete. That was the first phase. After that, we went to a six-story steel building. Umemura and I were the co-chairs of the technical coordinating committee. Bob Hanson (and here I am referring to the Bob Hanson who has been EERI president, not the Bob Hansen who was my MIT professor) and Watabe were the technical coordinators. After finishing the first two phases of the concrete and steel buildings, I dropped out because I had been with the program for a long time. It was time for someone else to take over. That was then followed by tests on masonry buildings, and then the next was precast prestressed buildings. Then it went into composite hybrid type buildings, and finally the last phase was on smart structures. So, that was the whole program.

Reitherman: When did it end?

49. *Proceedings of the U.S.-Japan Seminar on Earthquake Engineering with Emphasis on the Safety of School Buildings*. 21-26 September 1970. Sendai, Japan. Japan Earthquake Engineering Promotion Society, Toyko. 1970.

50. United States-Japan Cooperative Program in Natural Resources, established in 1964. The UJNR currently includes 18 panels, one of which is the Panel on Wind and Seismic Effects.

51. Space does not allow for a complete listing of the seven major conferences or planning meetings or the dozens of individuals and institutions involved. See "The 20th Anniversary Symposium on the U.S.-Japan Cooperative Earthquake Research Program Utilizing Large-Scale Test Facilities," Tokyo, October 9, 1998. The first phase of research (reinforced concrete) was co-chaired by H. Umemura on the Japanese side and Penzien on the American; the second (steel) by the same two; the third (masonry) by T. Okada and J. Noland; the fourth (precast concrete) by Y. Knoh and N. Priestley; the fifth (composite and hybrid) by H. Aoyama and S. Mahin.

Penzien: I think it just ended this past year, in 2002. The program went on for over 20 years.[52] Chi Liu of NSF deserves much of the credit for the success of this program.

Reitherman: Wasn't a lot of the damage to school buildings in the Tokachi-Oki earthquake due to the strong beam/weak column type of layout?

Penzien: Yes, exactly.

Reitherman: Did that problem get resolved in the research program?

Penzien: Well, it made it very clear that what you don't want is very deep spandrel beams and very short columns in between, because the columns will fail in shear; they're not going to fail in flexure mode. Shear failure is a brittle failure, which we don't want. Certainly you're going to avoid that kind of design. We didn't try to duplicate that kind of strong beam/weak column kind of structure in the test program, because it was quite obvious that was not a sound approach. So we designed what we thought the structure should be, based on our knowledge at that time. Although there were some shear walls in that building, shear walls are not easy to design to be ductile. So it was a combination of frames with shear walls.

Reitherman: Was the large-scale testing program based on the pseudodynamic testing method?

Penzien: Yes it was. It is where you start to test under a pseudostatic condition, but as you measure the force-displacement characteristic of the specimen, you then change the amount of deformation you apply. Somewhere along the way, that got started. Professor T. Okada from the University of Tokyo gave a paper at the Rome World Conference, which was the Fifth.[53] He presented a paper on that concept, and then a lot of the work followed that. I know that Steve Mahin got started working on it early on.[54]

Starting the CUREE-Kajima Joint Research Program

Reitherman: In 1988, a decade after the origin of the U.S.-Japan cooperative research program, the California Universities for Research

52. "NSF honored at 20th anniversary of U.S.-Japan cooperation on earthquake research," *EERI Newsletter*. Vol. 32, no. 11, November 1998.

53. Okada, T. and M. Seki, "A Simulation of Earthquake Response of Reinforced Concrete Buildings," *Proceedings of the Fifth World Conference on Earthquake Engineering*. Rome, 1973.

54. Mahin, Stephen A. and Mary E. Williams, "Computer Controlled Seismic Performance Testing," *Proceedings of the Second Specialty Conference on Dynamic Response of Structures: Experimentation, Observation, Prediction and Control.* Atlanta, Georgia, January 15-16, 1981, ASCE. In Japan in the 1970s, Koichi Takanashi called this method "online." See "Non-linear Earthquake Response Analysis of Structures by a Computer-Actuator On-line System." English summary by the Institute of Industrial Science of that paper by Takanashi et al. in Trans. Architectural Institute of Japan, Vol. 229, March 1975. "Pseudo-dynamic," then "pseudodynamic," later became common. Today, as in the case of research plans for several NSF NEES Equipment Sites, "hybrid simulation" is used. (Stephen Mahin, personal communication, 2003.)

in Earthquake Engineering (CUREE)[55] was started. Also about that time, the CUREE-Kajima Joint Research Program began. Do I have it right that you were on the original Joint Oversight Committee?

Penzien: I did serve on that committee, but I don't remember if it was the original committee. That was a different type of cooperation, which had its origins in 1988, which would be the Ninth World Conference on Earthquake Engineering held in Japan. The first part was in Tokyo and the second part in Kyoto. There was a group of us that got together at that conference and discussed how we could get support from some of the big construction companies in Japan. Kajima Corporation was the company that we focused on, so it was decided that Al Ang and I would meet with Takuji Kobori, who was a director of research in earthquake engineering for Kajima as well as a noted professor emeritus at University of Kyoto.[56] We wanted to talk with him about the possibility of Kajima funding the program. So, we made an appointment, and Al and I met with Professor Kobori in Kyoto. He was quite positive and told us later that he would consider it. That then led to funding from Kajima to CUREE—Bill Iwan was the key CUREE person. A lot of types of research have been done and I haven't followed it in recent years, but I would say that it has been a very successful program. The work was carried out by researchers in the United States and in Japan. So each research project had an American participant and a Japanese participant.

Reitherman: Were there language barriers to overcome in these joint research programs?

Penzien: No, not really. The Japanese that are working on these projects speak English well enough to communicate. In fact it makes things rather interesting. Cultural differences make the technical work interesting. That is why I have always enjoyed interacting with researchers in different countries.

Reitherman: Why was Kajima singled out? Was it the history of earthquake engineering there, such as the research and design work of Kiyoshi Muto?

Penzien: Professor Muto was the big name in Japan. After he retired from University of Tokyo he established the Muto Institute of Structural Mechanics within Kajima Corporation and was one of the firm's executives. Later Takuji Kobori, who was a professor at Kyoto

55. Re-named and re-organized in 2000 as CUREE, Consortium of Universities for Research in Earthquake Engineering, the organization now has a national membership of 28 universities. The original CUREE university members were Caltech, Stanford, University of Southern California, and the University of California campuses at Berkeley, Davis, Irvine, Los Angeles, and San Diego.

56. Kobori retired from Kyoto University and became Executive Vice President of Kajima Corporation in 1985, and is currently the Chief Executive Advisor to Kajima and President of its Kobori Research Institute. "Dedication to Dr. Takuji Kobori," Yoshiyuki Suzuki, *Earthquake Engineering In The Next Millennium: Proceedings of the Symposium in Honor of Takuji Kobori*. International Institute for Advanced Studies, Japan, and Consortium of Universities for Research in Earthquake Engineering, Richmond, California.

University, took that leadership role within Kajima. All of us in the U.S. had the closest connection with Kajima of the big architectural-engineering-construction firms in Japan, so it was natural that we would look to Kajima. As I understand, the CUREE-Kajima program is still going strong.

Reitherman: The CUREE-Kajima Joint Research Program is now in its fifth phase, each phase being two or three years long. Before one phase has ended, the planning is smoothly underway for the next phase. Bill Iwan of Caltech is still the key CUREE individual involved, chairing the Joint Oversight Committee on the CUREE side.

Penzien: Is that still funded at the same level that it used to be?

Reitherman: It used to be funded at about $500,000 a year. Then, in the last couple of phases, it was cut by about half. Actually, Bill Iwan was very worried that the whole program would get scrapped simply because of the economic misfortunes of Japan and in particular the depressed construction industry, which has been living with a recession that has lasted about a decade. However, Kajima has found the funds to keep the joint research program going.

Penzien: It's obvious that they have been quite pleased with the program or they wouldn't continue to fund it, especially continuing through the tough times.

Setting Up an Engineering Office in Taipei

Reitherman: You established a consulting engineering practice in Taiwan and operated it for several years in the 1980s. How did that segment of your career in Taiwan begin?

Penzien: Well, Chi Liu from NSF, who was a former student of mine, invited me. He was instrumental in getting people interested in earthquake engineering in Taiwan. It wasn't until they started building nuclear reactors that they realized they had to move ahead with the modern methods of analysis and treating the effects of earthquakes. So, Chi invited me to go to Taiwan in 1978 and have a meeting to discuss what we might do in the way of cooperative work. I got to meet many of the top engineers over there.

Then I spent one year at the National Taiwan University in 1980. During that year, I also got involved as a consultant advising some of the companies on seismic criteria. One was advising Sinotech Engineering Consultants, which was then designing the Feitsui Dam in Taipei, Taiwan. I helped them develop the seismic criteria for that dam and also helped them in the modeling and analysis. I later got a letter from the director of China Engineering Consultants, Inc., Mr. Fan, asking me to help them on the Kaohsiung Cross-Harbor Tunnel design. So I got involved in that. That year, Wen Tseng also came over to Taiwan and we worked on the analyses of that tunnel. It was at that workshop in 1978 that the proposal to develop the SMART-1 Array came up, and following that, Bruce Bolt, Yi-Ben Tsai, and I were involved in the early part of setting up the SMART-1 Array. So, I finally decided to set up an office. It was the Eastern International Engineers, Inc. (EIE), a California corporation. That was set up about 1982.

I then organized a rather extensive short course on earthquake engineering. So all those activities led to setting up the company with its office in Taipei. After setting up the EIE, we were in operation until 1989. A lot of the work was being done with the Taiwan Power Company on seismic hazard analyses for nuclear power plants.

Reitherman: Did you personally go through any earthquakes over there in Taiwan?

Penzien: Oh yes, I felt many over there. They occur quite often. For a number of them I was visiting and staying over at the Howard Plaza Hotel, which is a highrise. You get a pretty good jolt. Of course, I've experienced a number in Japan also.

Reitherman: Were you back here when Loma Prieta occurred?

Penzien: Yes, I had just come back in 1989. I had returned just a month or so before.

Reitherman: Would you like to talk about the Shin Kwon Insurance building in Taipei?

Penzien: That was a project that I worked on while I was with the EIE. I believe that was then the tallest building in Taipei—50 stories tall. I worked with Huey-Ming Liao on the seismic analyses of that. He was one of my partners in EIE. He had been a doctoral student of Professor Hajima Umemura at the University of Tokyo, and I arranged for him to do some work at Berkeley. He was involved with some other firms besides EIE, and he was a licensed architect as well as engineer. The Shin Kwon building was his project and I got involved helping him with the seismic analysis.

Setting Up an Engineering Office in California

Reitherman: When did you come back from Taiwan?

Penzien: I retired from U.C. Berkeley in 1988 and went over to Taiwan to spend a year. I decided I didn't want to stay there permanently, and that's when we closed the Taipei office and came back here to set up the present firm in Berkeley. Wen Tseng and I had been working together going way back on many problems. When I came back to the United States from Taiwan in 1989, we got together, along with Kiat Lilhanand, and decided to start our present firm, ICEC, International Civil Engineering Consultants, Inc., which we opened here in Berkeley in March of 1990. So the present firm has been going for a little over a decade.

Reitherman: From what I know about ICEC, it has a special niche in the engineering world—you wouldn't call it a typical consulting engineering firm.

Penzien: That's true; it's not a typical firm.

Reitherman: What's the specialty of the company?

Penzien: The backgrounds of all of us are founded in dynamic analysis. We have stayed pretty much to that side and stayed away from going into detailed designs as most firms do. The company has consulted in basically three areas.

One is developing seismic design ground motions. Of course this means ground motions in terms of response spectra. Then for large structures, we have to generate response-spectrum-compatible time histories. In the case of these structures, like long bridges, we have to generate time histories for multiple locations, and of course those motions need to take the spatial variations into account. That's one of our specialties.

The second area is setting the seismic design criteria for the structural design; then other engineers will carry on and do the design to meet the criteria that were set.

The third area is probably where most of our effort has gone, which is in the performance evaluation of critical facilities to dynamic loadings. I suppose most of the dynamic loadings have been seismic loadings, but a lot of work we've done in recent years is on dynamic loads produced by moving wheel loads. These studies are of high-speed rail aerial structures. We've done a lot of them. We've also done projects where the concern was hydrodynamic loads. Hydrodynamic loads are produced by seismic excitation such as the loadings on the big water storage tanks. There you get the seismic input when the water starts sloshing and exerts hydrodynamic forces on the tank. We've worked on hydrodynamic loads on gates in the Olmsted Dam on the Ohio River.

Reitherman: Has ICEC specialized in the analysis of large civil engineering works, rather than buildings?

Penzien: Yes, such as bridges, tunnels, and pipelines. We have not been actively working with buildings, though I did get involved in the tall building at 101 California Street in San Francisco—the one with the big lobby that slopes way up to about the fourth story on one side. That was designed by a Texas firm. The local engineers, who are very much experienced with seismic effects, didn't like that design. They wanted to know what was going to happen to that lower portion. Obviously it doesn't look very symmetrical there.

The design was based on the standard concept of allowing inelastic behavior in the structure during the seismic event, which of course reduces internal forces and the resistance required. In this case, that concept was carried all the way up throughout the height of the building. The local engineers were very concerned about how that lower portion of the building would perform. I think they were right to be concerned about it. When you get above that irregular portion, you're in a more normal kind of system. So, there was that controversy going on, and I was asked to come in. My contribution was the recommendation, which both sides accepted, that they not allow any yielding in that lower portion. I was also involved with two buildings in Taiwan, but as far as the company is concerned, we are not trying to compete with all of these engineering specialists who work on buildings.

Reitherman: When you started ICEC, there were three of you: you, Wen Tseng, and Kiat Lilhanand. Then for a while, did it remain the three of you?

Penzien: It was the three of us for a while. I can't remember when the workload got heavier, so we hired the next person. Then it grew up to ten people, eight of whom were engineers.

Reitherman: Is the way the firm operates today basically the same model as when you set it up?

Penzien: Yes, it's still pretty much the same—doing performance evaluations primarily. In some cases, these require very sophisticated modeling and analyses. I'll just mention one; I won't get into many of the details. It's accessing the ground motions that are produced by high-speed rail—high speed rail moving on aerial structures. The train is interacting with the structure, and the structure is interacting with the foundation, and the foundation with the soil, and motions are being transmitted away from the foundation. We developed a methodology that was the first to be used for this problem, which is very complex. That's the kind of work that I guess could be done by a research university, and papers could be written about such research projects, but we've done these applied research projects as a company.

Reitherman: Do you use the firm's own computers to run all your own analyses?

Penzien: Yes, we do it all in house. As you know, computers now can handle problems that we couldn't feasibly solve before. Years ago we'd have to go to a big computer center, but not anymore. When we first started, we did tie into computer centers. But today, these small computers we have in the office are so fast and have such big storage that we can handle everything.

Chapter 13

Thoughts on Bridges and the Future of Earthquake Engineering

I'm sure the next 50 years will bring wonderful advances, but I'm glad I had a chance to live my career in what you might call the pioneering era.

Reitherman: While some people may not know about your work with tunnels, dams, and offshore platforms, most people in the earthquake engineering field know of your work with regard to bridges. How did that start?

Penzien: My first involvement started right after the San Fernando earthquake of 1971. I hadn't worked on bridges prior to that time, but I got a call right after the event occurred from Charles Scheffey, who was a former colleague at Berkeley. He asked, "Joe, would you and Ray Clough go down to San Fernando and survey the damaged bridges? The Federal Highway Administration needs a report on that." That was the first earthquake where bridges failed because of vibrations produced by the earthquake input. Prior to that, you'll find many bridge failures during earthquakes, but they were always caused by ground failures, such as liquefaction, slides, or spreading. So looking at bridges that had collapsed because of ground motion-induced vibrations was new to us. We went down, wrote a report, and sent it in. Following that, the Federal Highway Administration asked us to write a literature survey of damages to bridges. I brought Dr. Toshio Iwasaki from Japan, and the two of us worked on that, and Ray Clough was also involved. We surveyed the damages to bridges all over the world as best we could. The damaged bridges in Japan were a large part of that report.[57] It turned out to be a nice report with a big demand for copies.

Several of the big overcrossings collapsed in the San Fernando earthquake, including the large Interstate 5-Highway 14 overcrossing. Scheffey arranged for funding for a research project to do model studies of that overcrossing, so we built a model and put it on the shaking table. Professor William Godden headed that experimental research.

At about the same time, Wen Tseng started to develop a computer program to make dynamic seismic performance evaluations of bridges. Once developed, one could use the results of the model tests to make correlation studies. Wen was developing that program shortly after 1971, and I think he worked on it a little over two years. That computer program is called NEABS, Nonlinear Earthquake Analysis of Bridge Systems. It is still used today; for example, it is the main program used by Imbsen & Associates in their bridge consulting work.

After the program was developed, we used it to correlate the experimental results. I had a number of people that helped on that correlation work. Godden headed up the experimental work, and I headed up the correlation studies. David Williams was the first doctoral student to work on the correlation studies, followed by Kazuhiko Kawashima, who came over from Japan, where he is now a professor at the Tokyo Institute of Technology. I believe Ma-Chi Chen also worked with us. Working on that project led to the Federal Highway Administration wanting to support additional bridge research. So they provided the funds, which later were channeled through Caltrans. That funding went on until about a year before I retired from the University in 1988.

There were many aspects of bridge behavior that were studied by students during those years. I had Roy Imbsen, who is the head of the Imbsen & Associates bridge engineering firm, also David Liu, Rick Nutt, M. C. Chen, and Donald Liou. So I had a number of doctoral students working on bridge research. Imbsen got his doctorate at U.C. Davis, although I was on his research committee and guided a lot of his work here at Berkeley. Rick Nutt was then an employee of Imbsen & Associates, so he wasn't a doctoral student, but the other three were.

Reitherman: What explains the fact that highway structures had not collapsed or become severely damaged during earlier earth-

57. Penzien, Joseph, T. Iwasaki, and R. W. Clough, *Literature Survey: Seismic Effects on Highway Bridges*. Report No. U.C.-EERC 72/11, Earthquake Engineering Research Center, University of California at Berkeley, November 1972.

quakes in the United States? Did their simple configurations with short spans give them inherent resistance to earthquake shaking, even though they were designed to seismic standards that now seem very low, if not primitive?

Penzien: Previously, most of California's bridges that experienced strong earthquakes were small structures, single spans with an abutment at each end transferring loads directly into the deck, which is very strong. Of course you can have some damage to the abutments, but that tends to be less significant than column damage to multi-span bridges. The simplicity of the single-span bridge leads to an inherently stronger structure than the seismic requirements of the older code would imply.

Reitherman: In building design, engineers and architects are guided toward more redundancy by penalties in the code for nonredundant structural systems. Yet many highway bridges today have single-column configurations for the supports. Why do so many new highway bridges in California have nonredundant configurations?

Penzien: It's just the way the architects like to design them. I think you'll agree that the aerial structures with single-column piers are more attractive than if you put up piers with multiple columns. But it does lead to essentially no redundancy. If you have no redundancy, you just have to be more careful about the design and how much nonlinear behavior you can allow. If you have a highly redundant structure, it tends to distribute the loads as yielding takes place here and there. So it's an advantage to have redundancy.

1989 Loma Prieta Earthquake: Retrofitting Existing Bridges

Reitherman: Did the 1989 Loma Prieta earthquake start another wave of research?

Penzien: Yes, it did in a way, on a different type of activity. After the 1989 Loma Prieta earthquake, the governor appointed a Board of Inquiry. George Housner was the chair, and I was a member of that Board and vice-chair. We had many hearings and wrote a report entitled *Competing Against Time*.[58] That led to Governor Deukmejian issuing an Executive Order very soon thereafter charging Caltrans to oversee that all existing public transportation structures were studied and brought up to a proper level of seismic performance. In 1990, Caltrans appointed a Seismic Advisory Board, with many of the members of the Governor's Board of Inquiry continuing on that new Board, including myself. George Housner was the first chairman. After about five years, George dropped off of the Board and I became the chairman. I've chaired it ever since.[59] The Board's activity has been advising Caltrans on policies and procedures related to seismic safety. Most of the work deals with bridges.

Then in 1994, we had the Northridge earthquake. The Caltrans Seismic Advisory Board prepared a report called *The Continuing Challenge*.[60] During my time on the Board, which

58. Housner, George W. et al., *Report of the Governor's Board of Inquiry on the Loma Prieta Earthquake*, George W. Housner, Chair; Charles C. Thiel Jr., editor, Office of Planning and Research, Sacramento, California 1990.

59. Penzien retired from the Caltrans Seismic Advisory Board in November 2003.

has now [2003] been about 13 years, we've advised Caltrans on numerous seismic safety issues, followed the progress of the retrofit work, and pushed very hard to get the research budget increased. We have also been involved in reviewing the design work on the new San Francisco-Oakland Bay Bridge East Crossing, which was first to be a retrofitted bridge, but which will now be a completely new structure.

Bridges in New York City

Reitherman: You've been involved as a consultant with the current Golden Gate Bridge seismic retrofit project and for the new Tacoma Narrows Bridge, both of which are in high seismic regions. But how did you come to be a seismic expert working for New York on some of their bridges, when that city's location is only moderately seismic. New York City has had the nation's largest collection of major bridges for over a century, without any significant earthquakes. It would seem that it might be hard to get people in New York to suddenly spend money on the potential seismic problems of bridges.

Penzien: New York is one place that has taken the danger of earthquakes seriously and has started a retrofit program. All of their main bridges have been reviewed for retrofit needs. The intensity level of ground motion used is lower than in California, and it is very difficult to decide which intensity level to use. If you try to go through a probabilistic hazard analysis in New York, it's very difficult because the seismic sources are as not well defined as they are here in California. If you look at response spectra generated for different sites in the New York City area by different consultants, you'll find large variations in those spectra, which indicates to me the very large uncertainty in generating response spectra for New York City.

Reitherman: You have a graphic illustration of that in your EERI Distinguished Lecture paper, showing the large discrepancies among different experts' uniform hazard curves.[61]

Penzien: As a result of the big variations in those curves, the specialists are still at it even now, late 2002, trying to agree on a spectrum for use in the metropolitan area of New York.

High-Speed Rail Bridges

Reitherman: You've been involved with the seismic design of several high-speed rail projects. Tell me something about that consulting work.

Penzien: As a consultant, I've been involved in the design of the Taiwan High Speed Rail project, setting criteria for aerial structures, along with George Housner and the late Professor Keizaburo Kubo from Japan. And then George, Bruce Bolt, and I served on the consulting board that set the seismic design criteria for all the BART extensions in the San Francisco Bay Area, first the East Bay extensions, one going out to the community of Pittsburgh

60. Housner, George W. et al., *The Continuing Challenge*. George W. Housner, Chair; Charles C. Thiel Jr., editor; California Department of Transportation, Sacramento, California, 1994.

61. Penzien, Joseph, "Earthquake Engineering for Transportation Structures – Past, Present, and Future," EERI 2000 Distinguished Lecture. *Spectra*, Vol. 17, no. 1, February 2000.

and the other one extending to Pleasanton. Then later, the three of us served again as consultants, setting the criteria for the San Francisco Airport extension of the BART line. I've also been involved in the Department of Rapid Transit Systems in Taipei as a consultant.

Reitherman: What is the difference in seismic design criteria for the rail structures as compared to highway bridges?

Penzien: Well, with rail aerial structures, you have to be much more careful with the amount of transverse deflection and how much misalignment you tolerate. On a highway, if an automobile is traveling across a bridge during an earthquake, a small misalignment doesn't matter that much. That's one significant difference in terms of operational safety.

History of Seismic Criteria for Bridges

Reitherman: I have a question regarding your EERI "Distinguished Lecture" for 2000.[62] You trace the history of seismic design criteria for bridges over the past 50 years, explaining that early bridge design criteria merely copied from building codes. What are some examples of the establishment of bridge seismic design criteria in their own right?

Penzien: As a result of the 1971 San Fernando earthquake, Caltrans developed bridge-specific seismic design criteria, which were published in 1973. Then in 1981, the ATC-6 project was conducted, and we developed guidelines for bridge criteria,[63] which were adopted by AASHTO (American Association of State Highway and Transportation Officials). Then in 1996, the results of another ATC project I was a part of were published as ATC-32.[64] There has been a program of seismic research on highway bridges since 1992 at MCEER (Multidisciplinary Center for Earthquake Engineering Research, formerly NCEER, National Center for Earthquake Engineering Research) in Buffalo, New York. I've been a member of the Highway Seismic Research Council there since they started their bridge research for the Federal Highway Administration. That was quite some time back, but it is still going on, and that work includes development of seismic design criteria.

Reitherman: Was the evolution of seismic design in bridges roughly parallel with that of buildings?

Penzien: The development of seismic design criteria started earlier for buildings. There were no seismic criteria for bridges for quite a long time. In 1961, when the first seismic loading criteria were introduced in the AASHO[65] code, they were exactly the same as in the code for buildings. The seismic equivalent lateral load was 2 percent, 4 percent, or 6 percent of

62. Ibid.

63. *Seismic Design Guidelines for Highway Bridges*. Applied Technology Council, Redwood City, California 1981. Funded by the Federal Highway Administration.

64. *Improved Seismic Design Criteria for California Bridges: Provisional Recommendations*. Applied Technology Council, Redwood City, California, 1996.

65. The American Association of State Highway Officials added "Transportation" to its name in 1973 and became AASHTO.

the dead weight of the structure, depending on the foundation conditions. When using this seismic loading in combination with other loads you could increase the allowable stresses by 33-1/3 percent. The resulting design loads were essentially nothing.

A big change came following the 1971 San Fernando earthquake. Caltrans then immediately got real busy and came up with new code criteria, which specified big increases in the seismic loads. They advanced from just pure static loads to loads that had to be a function of the fundamental period of the structure. That was the first time dynamic characteristics of the bridge were considered in deciding on the lateral loads. Then came the transition from equivalent static loads to response spectra. Generally, this advancement of analysis techniques has led to an increase in design loads, and the analytical procedures have shifted from static analysis to dynamic analysis. In recent years, the trend toward more realistic and accurate analysis methods has gone even further, since our designs specifically anticipate that these bridges will go into the inelastic range. Now we are requiring nonlinear time history analyses for important bridges. That is a little hard for some practicing engineers even today to implement. This is where we are at the present time.

That pretty well covers my activities in bridges. As you see, I started out investigating the damage after an earthquake 32 years ago, and I've been working on the topic of bridges ever since: first in the research area, then as a professional engineer.

The Next 50 Years?

Reitherman: Let me read you a sentence from your EERI Distinguished Lecture paper and then ask you a question. First, your sentence: "Revolutionary changes have taken place over the past 50 years in earthquake engineering as applied to transportation engineering."[66] Now the question: Do you foresee revolutionary changes in earthquake engineering over the next 50 years?

Penzien: Well, I can't predict the future, I can only hazard a guess. I would say no, the changes during the next 50 years will be incremental, not revolutionary, which is not to boast about what people of my generation accomplished. You have to realize where we were starting from—there was so little known, so much to discover. My colleagues and I and our doctoral students could pick up a challenging new earthquake engineering problem that hadn't been solved or even accurately framed as a problem, survey what was known, conduct research along new lines of thought, and come up with something fundamental. We would publish papers that were sometimes the first time when even the terminology was used, let alone the concepts. Then, we could turn our attention to some other fascinating problem and try to come to a basic understanding of its principles and work out some practical consequences of use to the practicing engineers. I'm sure the next 50 years will bring wonderful advances, but I'm glad I had a chance to live my career in what you might call the pioneering era.

66. Ibid.

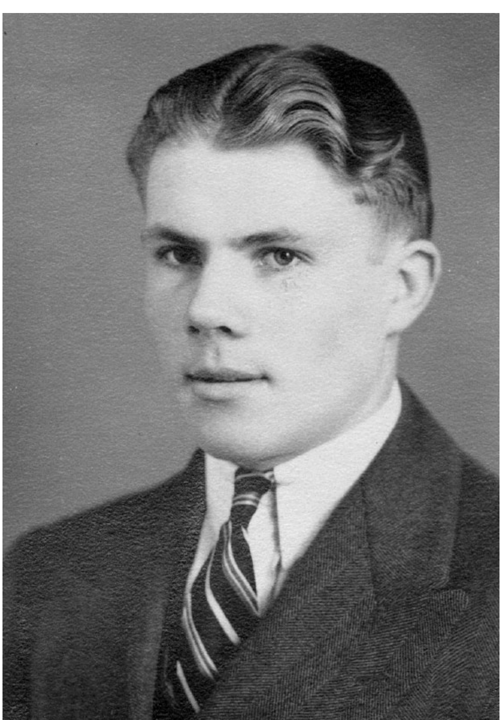
Photographs

Joseph Penzien.

The picture from my early days (1930s) growing up on the farm shows me in the center holding the owl's head. The owl was shot by an older brother or my father because the owl was attacking our chickens.

My brother Bill (sitting on the radiator) and I (pumping up a flat tire) spent much time keeping the old Model T Ford running. We patched the inner tubes in the tires so many times we finally gave up and stuffed the tires with straw. The picture was taken about 1935 or 1936.

I am on the left, and behind me is my father, John Chris, and standing next to him are my two brothers, Bill and John. Next to me in the front row are my two sisters, Mamie and Gladys, and my mother, Ella May (1941).

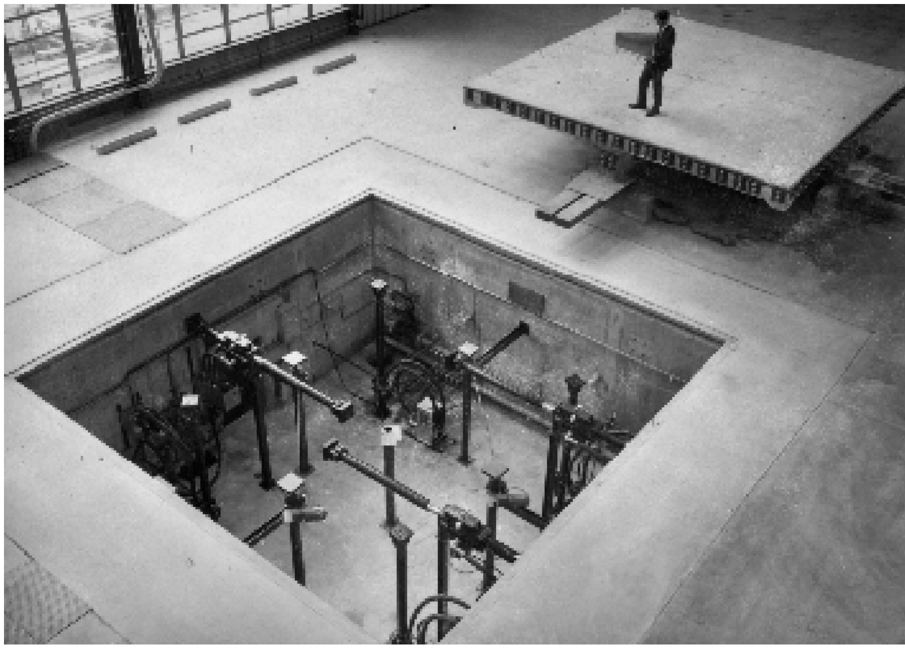


Graduation picture from Nampa High School, Nampa, Idaho (1942).

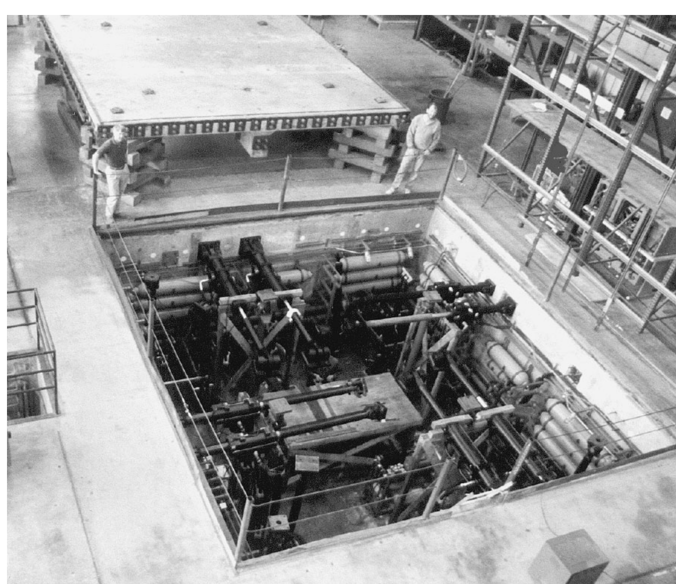
My first job after graduating from the University of Washington was with the Corps of Engineers at Bonneville, Oregon where the Bonneville Dam on the Columbia River is located. I worked in the hydraulics laboratory on model testing of the Umatilla, Oregon Dam (now called McNary Dam). I am standing on a walkway leading to the lab (fall 1945).

Joseph Penzien (right) and Senator Alan Cranston at the Richmond Field Station shortly after the shaking table facility was completed in 1972. (Photo: U.C. Berkeley)

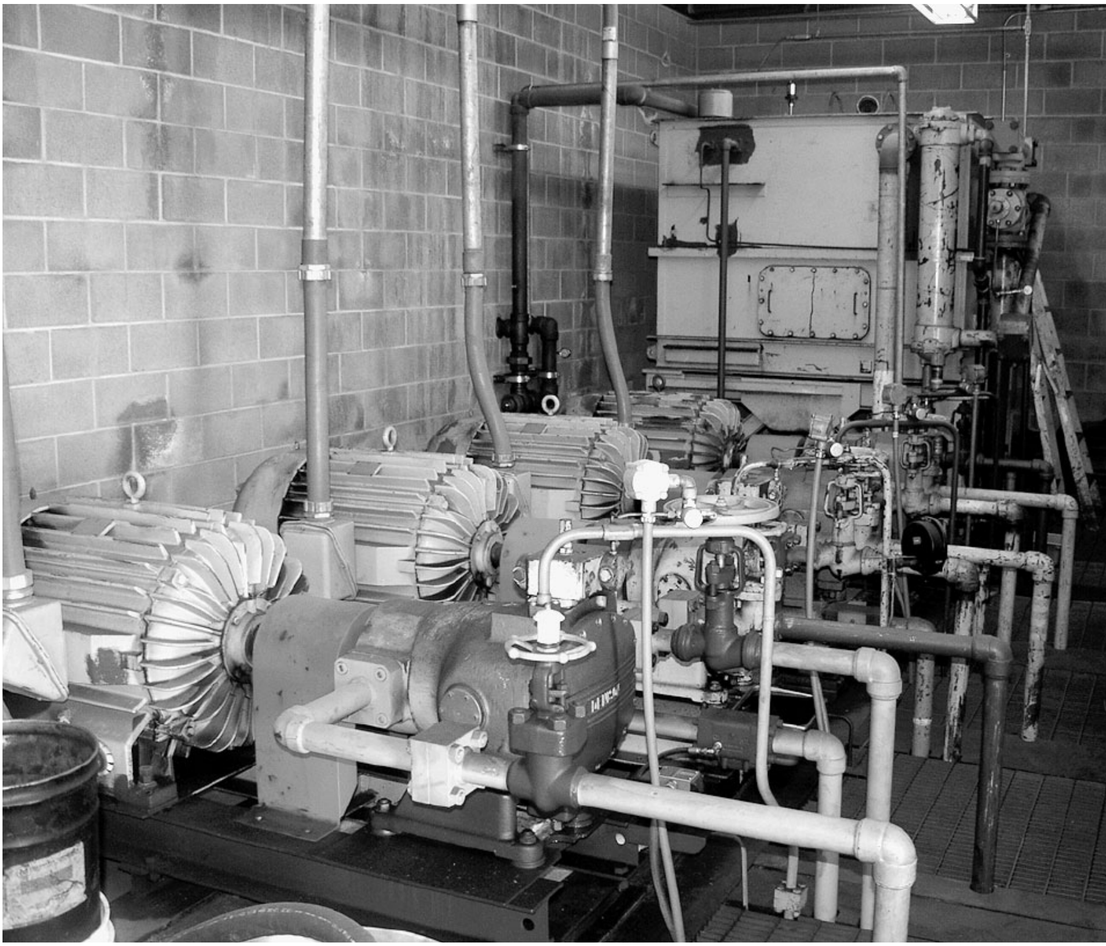
The University of California, Berkeley shaking table built close to the campus at the Richmond Field Station was the first of its kind in the world. Officials from the Kajima Corporation in Japan visited our new Earthquake Engineering Research Center to be briefed on the design of our system. Then they went back to Japan and built this shaking table nearly equivalent to the one we had at the Richmond Field Station. Dr. Kiyoshi Muto (far left) and Dr. Y. Hisada of Kajima Corporation requested that Dixon Rea (third from left) and I (fourth from left) come to Japan to check out their system and help put it into operation. Dixon was a key person in the design of our system. (Photo: U.C. Berkeley)



The U.C. Berkeley shake table nearing completion in 1969 at the Richmond Field Station. Dixon Rea is shown on the platform, which measures 20 feet by 20 feet in plan. (Photo: U.C. Berkeley Earthquake Engineering Research Center)



New actuators and other equipment at the EERC shake table in the pit under the original platform during the upgrade to triaxial capability in 1996. On the left is Don Clyde, Senior Development Engineer; on the right is Wesley Neighbor, Associate Development Engineer. (Photo: CUREE)



Four original hydraulic pumps obtained in the 1960s from surplus missile bases and used to the present day [2003] for the shake table and other hydraulically-powered structures-testing equipment at the Richmond Field Station. (Photo: Robert Reitherman)

A dinner party at Frieder Seible's mountain home outside San Diego attended by Caltrans Seismic Advisory Board members. Left to right: F. Seible (with only hand showing), Joe Nicoletti, Joseph Penzien, Bruce Bolt, I. M. Idriss, and Jim Roberts (2002).

A group photo of American and Japanese research collaborators in Tsukuba, Japan in the 1970s. Penzien is in the center, directly in front of the building column. (Photo: Building Research Institute)

I recently met my grade school teacher, Esther Naramore Sain, at her home in Escondido, California on October 16, 2003. This was the first time I had seen her in 70 years—since I was a student of hers in first, second, and third grades (1930-33).

My first wife, Jeanne Hunson Penzien, in a photo from 1950.

I was very fortunate to meet Mi-Jung Park at a social function in Seoul, Korea where I was attending an engineering conference. She was working at Korea's museum of modern art at the time. We married June 16, 1988 very shortly after I retired from U.C. Berkeley. This picture was taken in Tokyo shortly after we were married. She has since graduated from the California College of Arts and Crafts with a Bachelor of Fine Arts degree. She now keeps busy as a painter of oils and watercolors.

Former Ph.D. students of Ray Clough and Joe Penzien at the U.C. Berkeley-CUREE Symposium in Honor of Ray Clough and Joseph Penzien, May 9-11, 2002. Clough (left) and Penzien (right) are in front, center. (Photo: CUREE)

A

B

C

D

E

F

G

H

I

J

K

L

M

N

O

P

R

S

T

U

V

W

Y

ISBN 1-932884-01-7
90000>
9 781932 884012