
CONNECTIONS

The EERI Oral History Series

**George W.
Housner**

Stanley Scott
Interviewer

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Earthquake Engineering Research Institute

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

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Published by the Earthquake Engineering Research Institute

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E-Mail: eeri@eeri.org
Web site: <http://www.eeri.org>

EERI Publication No. OHS-4
ISBN 0-943198-58-5 (pbk.)

Library of Congress Cataloging-in-Publication Data

Housner, G.W. (George William), 1910-

George W. Housner/Stanley Scott, Interviewer.

p. cm. — (Connections: the EERI oral history series : 4)

Includes index.

ISBN 0-943198-58-5 (alk. Paper)

1. Housner, G. W. (George William), 1910- —Interviews.
2. Civil engineers—California—Interviews.
3. Earthquake engineering—California—History.
4. Earthquake resistant design—History. I. Scott, Stanley, 1921- . II. Earthquake Engineering Research Institute. III. Series

TA140.H68A3 1997

624.1'762'092—dc21

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97-17122
CIP

Printed in the United States of America

1 2 3 4 5 6 7 8

02 01 00 99 98 97

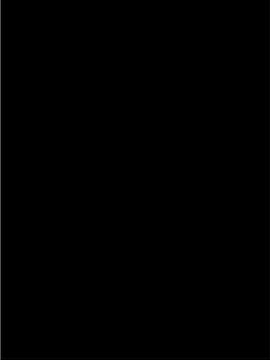


Acknowledgments

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.

Quite a number of readers looked over all or parts of the oral history, and provided valuable advice and suggestions. Those who commented include Clarence Allen, Bruce Bolt, Ray Clough, Jim Gates, John Hall, Don Hudson, I.M. Idriss, Bill Iwan, Roy Johnston, Frank McClure, Joe Penzien, Vernon Persson, Clarkson Pinkham, Roland Sharpe, and Tony Shakal, among others.

EERI also gratefully acknowledges partial funding of this project by the Federal Emergency Management Agency (FEMA).



The EERI Oral History Series

This is the fourth 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 pioneered in earthquake engineering and seismic design. The field of earthquake engineering has undergone significant, even revolutionary, changes since individuals first began thinking about how to design structures that would survive earthquakes.

The engineers who led in making these changes and shaped seismic design theory and practice have fascinating stories. *Connections: The EERI Oral History Series* is a vehicle for transmitting their impressions and experiences, their reflections on the events and individuals that influenced their thinking, their ideas and theories, and their recollections of the ways in which they went about solving problems that advanced the practice of earthquake engineering. These reminiscences are themselves a vital contribution to our understanding of the development of seismic design and earthquake hazard reduction. The Earthquake Engineering Research Institute is proud to have part of that story be told in *Connections*.

The oral history interviews on which *Connections* is based were initiated and are being carried out by Stanley Scott, formerly a research political scientist at the Institute of Governmental Studies at the University of California at Berkeley, who has himself for many years been active in and written on seismic safety policy and earthquake engineering. A member of the Earthquake Engineering Research Institute since 1973, Scott was a commissioner on the California State Seismic Safety Commission for 18 years, from 1975 to 1993. In 1990, Scott received the Alfred E. Alquist Award from the Earthquake Safety Foundation.

Recognizing the historical importance of the work that earthquake engineers and others have been doing, Scott began recording interviews in 1984 with Henry Degenkolb. The wealth of information obtained from these interviews led him to consider initiating an oral history project on earthquake engineering and seismic safety policy, and in due course, the Regional Oral History Office of the Bancroft Library approved such an oral history project on a continuing, but unfunded, basis. First undertaken while Scott was employed by the Institute of Governmental Studies, University of California at Berkeley, the effort was continued following his retirement in 1989. For a time, modest funding for some expenses was provided by the National Science Foundation.

Scott's initial effort with Degenkolb was extended to a number of other earthquake engineers who have been particularly active and close observers of seismic safety policy and practice.

Key members of the Earthquake Engineering Research Institute became interested in the project when asked to read and advise on the oral history transcripts. This led to EERI's decision to publish *Connections*.

The Earthquake Engineering Research Institute was established in 1949 as a membership organization to encourage research, investigate the effects of destructive earthquakes and the causes of building failures, and bring research scientists and practicing engineers together to solve challenging engineering problems through exchange of information, research results, and theories. In many ways, the development of seismic design is part of the history of EERI.

EERI Oral History Series

Henry J. Degenkolb	1994
John A. Blume	1994
Michael V. Pregnoff and John E. Rinne	1996
George W. Housner	1997

Interviews completed or nearing completion include:

William W. Moore
Robert E. Wallace
Clarence R. Allen
LeRoy Crandall
Ralph McLean
George A. (Art) Sedgwick

Interviews with several others are in progress.

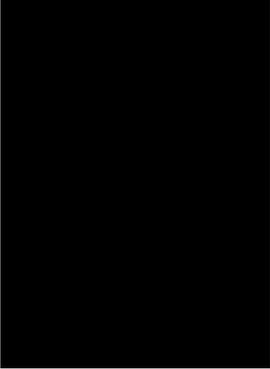
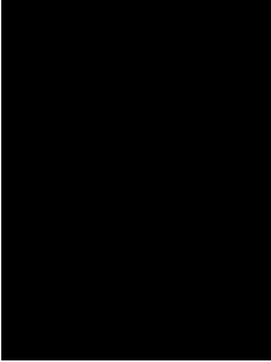


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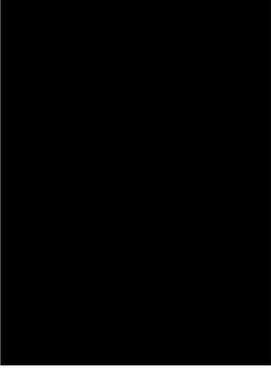
Foreword

The many interviews with George W. Housner that are the source of this oral history were all recorded at the Athenaeum, the monumental faculty club on the campus of the California Institute of Technology, during an eight-year period from December 1987 to March 1995. We met approximately once or twice a year during my visits to the Los Angeles area. At first, Professor Housner probably did not know quite what to make of me and the oral history project, but he always seemed willing to sit with me for an hour or so when I happened to be in the area. In time, as the interview files began to grow, and when his own retirement and activity shifts permitted, he began to have more time for the project and for work on the interview drafts.

Revisions, additions, and editing were done during 1995-1997. When George Housner tackled that part of the job, he did it with great care and thoroughness, as well as a skilled proofreader's eye. Some reorganization was done, although not a great deal, save for some shifting and weaving material together to consolidate overlapping discussions and reduce duplication. Many additions were also made during the final three years, after the interviews per se were completed, for example, the chapter on Housner's writings and publications.

The end result of the process, now being published by EERI, provides a unique record of a distinguished scholar, elder statesman, and activist in earthquake engineering research and seismic safety. This oral history account follows him from his early days in Saginaw, Michigan, through his schooling in Michigan and Caltech, a five-year stint in engineering practice, the World War II years, and joining the Caltech faculty in 1945. His long and productive career is unequalled in the field. His recollections of projects, problems, and how it occurred to him to solve them make this oral history a rich and fascinating read.

Stanley Scott
Interviewer
Research Associate
Institute of Governmental Studies
University of California, Berkeley
March 1997



A Personal Introduction

I first met George Housner when I enrolled as a graduate student at Caltech in the fall of 1959—the same year that George published his landmark paper “Behavior of Structures During Earthquakes” in the American Society of Civil Engineers *Journal of Engineering Mechanics*. I still have my well-thumbed reprint of this study, although the paper, George, and I have all aged somewhat since that time. Because of my interest in structural engineering, George was assigned as my academic advisor and guided me in the selection of my courses for the Master’s degree. He also was the professor in one of my classes, a year-long course devoted to problems in elasticity, plates and shells and other topics in structural mechanics. Another one of my courses was dynamics and vibrations taught by George’s friend and colleague, Don Hudson. Soil mechanics was taught by a new Caltech professor, Ronald Scott. It was a good place to be at that time—the early stages of a very important period for earthquake engineering and for Caltech’s role in this growing field.

As a classroom instructor 38 years ago, George was careful and well-prepared. His style was calm and thoughtful, rather than entertaining, and his writing on the board was not a model of penmanship. As a result, some of the students seemed not to like the class very much. This puzzled me, because what I saw was a depth of understanding that was truly impressive. George showed a command of the interplay between the mathematics and the mechanics of a problem that was beyond anything I had seen before. He knew what each term, and each constant in each term, meant to the character of the solution and to the underlying physics of the problem. This knowledge intuitively guided him through complexities of analyses and often seemed to lead him to the development of simplified, approximate solutions. It was as if the terms of the equations were actors in a well-known play, which he directed with a light hand, knowing not only how the plot comes out, but how each actor develops his role along the way.

Later, I was very pleased to be allowed to stay on at Caltech and study for the Ph.D. under George’s supervision. George and Don were already widely known for their research in earthquake engineering and my classmates included many students drawn to Caltech because of interest in that subject. W. K. Tso, Bill Iwan, Bob Hanson, Gerald Brady, Howard Merchant, Michael O’Kelly, Norby Neilsen, Allan Porush, Raj Malhotra, and Willard Keightley were among the cohort of students studying various topics in earthquake engineering and structural dynamics during this period. George’s style as a Ph.D. advisor was to help the students find their interests and to guide them only gently in their selection of a

thesis problem. This is in contrast to many professors who seem either to tell the students what to do or to give the students a choice among a limited set of suitable topics. For a student to choose and develop a good thesis problem is a maturing and difficult task, leading to what George has characterized as “some floundering by the student.” However, once one works through this stage and is well into the research, the wisdom of George’s approach becomes evident. The realization sinks in that for success in research, the selection of a problem is often both harder and more important than solving it.

When I returned to Caltech in the mid-sixties as a young faculty member, I got to know George as a colleague, rather than as student. The first step in this process was to start addressing him as “George,” rather than “Professor Housner.” This took some getting used to, and was not so easy the first few months. As a colleague, George has a great talent for encouraging and mentoring young faculty. He was then and is now very influential in his field, and in engineering and science in general, and he has often taken the opportunity to involve younger colleagues in important projects where he had a major role. Thus, I had opportunities early in my career to work on such projects as the National Academy of Sciences study of the 1964 Alaska Earthquake and the important National Research Council position study, *Earthquake Engineering Research*, published in 1972. He also took me along to lunch at Caltech’s famous Faculty Club, the Athenaeum, because “it is important to meet your colleagues in other parts of the university.” George was also quite helpful elsewhere within the university where his leadership influenced the tenure process for young faculty in his general area.

George is no doubt best known to EERI members for his contributions to earthquake engineering. Don Hudson selected some of his papers in this field for a volume in the American Society of Civil Engineers Civil Engineering Classics series entitled “Selected Earthquake Engineering Papers of George W. Housner,” a book which should be required reading for any young earthquake engineer in practice or research. This volume includes many of the remarkable contributions George has made over his long and productive career. Because of its emphasis on earthquake engineering, however, it did not include one of my favorites, which is an analysis of the vibrations of a pipeline filled with moving fluid, a problem that caught his interest when he consulted on the construction of the Trans-Arabian pipeline. I studied this paper while using the problem as an example in a class I was

teaching; what makes it memorable to me is how George somehow managed to include all the second-order effects that were important, and none of those that were not. Like some of the other problems he and Don Hudson came across in their research and consulting, this one contributed to the homework problems in their textbooks *Applied Mechanics-Dynamics* and *Applied Mechanics-Statics*.

George retired from active faculty status almost 17 years ago, but retirement made little change in his working habits. Part of this is a consequence of the university's far-sighted policy towards emeritus faculty, but most is due to George's good health and continuing interest and activity in earthquake engineering. Even at 86, he comes to his office every day that he is not traveling. He is chair of the U. S. Panel on Structural Control and is now involved in the preparation of a report on the Past, Present and Future of Structural Control. He participated in organizing the International Association for Structural Control and served as its first president. He also is spearheading a project to translate from the Chinese a multi-volume report on the great Tangshan earthquake. In addition, he continues his widespread reading and his habit of supplying his colleagues with copies of articles, news items or humorous cartoons—items which he believes will contribute to our education or lighten our day.

His style of office housekeeping, which many have marveled at when they have visited him in the Thomas building, continues in the same "keep-and-stack" mode that he always used. His many activities and widespread interests mean that he gets a lot of things sent to him. Most of these things end up in his office, and very little gets discarded. Once when the appearance of his office seemed especially crowded, I asked him why he didn't just throw some of the stuff away. His reply was very insightful as well as humorous: "It's important to throw things away, but not too soon."

The walls of his office are lined with bookshelves. They appear like retaining walls built to contain the papers, slides, building plans, photographs, correspondence, and gadgets that lie within. These bookshelves hold a large and outstanding collection, particularly of historical books in seismology and earthquake engineering. This collection, and what is stored in the large back closet, is all the more impressive when one remembers that a large part of his collection was used to start the Earthquake Engineering Research Library at Caltech, a library that now serves the local professional community.

George's career spans almost all of earthquake engineering as we know it. His was one of the first Ph.D. theses written on earthquake engineering in the United States and he has been intimately involved ever since in almost all of the important developments in the field, including the establishment of EERI and the International Association for Earthquake Engineering, the establishment of the World and U.S. National Conferences on Earthquake Engineering, most of the major national studies and government commissions, and many of the important engineering projects of the last half-century, including BART, the California Water Project and the earthquake resistant design of tall buildings, dams, nuclear power plants and offshore drilling structures.

This EERI oral history preserves some of George's insights and remembrances as he looks back on his remarkable career.

Paul C. Jennings
April 22, 1997

CONNECTIONS

The EERI Oral History Series

George W. Housner

Early Years to World War II

“When ...I faced a special problem, I would proceed to work out the solution, ending up either with an equation, or with a diagram from which the solution could be read.”

Housner: At the very outset, I hope it will be understood that this oral history project will not attempt to write a comprehensive history of earthquake engineering, covering all the significant people and events. Such a history would require much more effort than these interviews, which are intended for a different purpose.

Scott: Yes, I believe readers will understand that the oral histories based on interviews like these are more in the nature of personal memoirs than comprehensive histories of a subject.

Family Background

Scott: Let's begin by your giving a little on your early background.

Housner: My full name is George William Housner. I was born on December 9, 1910, in Saginaw, Michigan. Both sets of my grandparents were residents of Saginaw, having emigrated from Europe as young people and settled there. My mother's

family came from Switzerland, and my father's family from Germany.

My paternal grandfather was also named George William Housner, and my paternal grandmother's maiden name was Mary Popp. My Housner grandfather died on December 9, 1908, exactly two years before I was born on the same date in 1910. My father, Charles Housner, died when I was a year old. After that, my mother, Sophie Schust Housner, returned with her two children to live with my maternal grandparents. Consequently, I did not have a close relationship with my father's extended family, but did with the children and grandchildren of my maternal grandparents.

Henry and Sophie Heilemann Schust had left the city of St. Gallen in Switzerland in their early twenties as a newly married couple and came to Saginaw. Henry Schust and his four sons built up the Schust Baking Company, which sold cookies and crackers to grocery stores throughout the state of Michigan. The company was sold to the Sunshine Biscuit Company in the 1930s, which was also more or less the time when my cousins and I made our exodus to various other parts of the United States. None of my cousins now lives in Saginaw.

In all, I had twelve first cousins who lived respectable lives, and several attained a certain eminence. Ralph Schust became vice-president of the Sunshine Biscuit Company, and Edward Heinemann became chief engineer of the Northrup Aircraft Company. Florence Schust Knoll became a well-known furniture designer and founder of the Knoll Furniture Company. My second cousin, Howard Kehrl, was an

engineer and became vice-president of General Motors.

Scott: Some years after you left Saginaw and were established in California, I believe your closest family members, your mother and sister, came out here?

Housner: Yes, in the late 1930s my mother and my sister Esther joined me in California. My sister was an invalid as a result of having contracted polio in her youth.

Growing Up in Saginaw

Scott: Can you give some background on your early years, and on what might have motivated you to turn toward engineering?

Housner: I think my interest first developed from my reading during my high school years. I attended Saginaw High School, and graduated in 1928. No one in my family was an engineer, and in fact, I was the first one in the extended family to attend college. While many of my cousins—younger than I—also attended college, I was the first. So there was no previous tradition of intellectual achievement in the family. I suppose just from reading I decided I wanted to be an engineer.

Scott: Had you been particularly interested in math, for example?

Housner: It's hard to say, because I don't remember. In looking back I can't remember that I learned anything in high school. I must have learned something, but I do not now remember what. I do recall, however, being a great reader of books.

Scott: You were a great reader of books. Say something about what kinds of things you read.

Housner: When I grew up in Saginaw, then a town of about 40,000 people, there was not much intellectual stimulation there. Actually a lot of eminent people came out of Saginaw—it was a good place to come from. For an ambitious young man, however, there was nothing to do in a town of that size, where I lived until 1928 when I finished high school and went to college in Ann Arbor.

So during my boyhood I did a lot of reading. The city had a public library—one of the Carnegie libraries—and I was a great patron of that. Steel magnate and philanthropist Andrew Carnegie gave money to put up library buildings in any city that would agree to maintain the buildings as a library.

The people in Saginaw said their public library building was modeled on the family chateau of Alexis de Tocqueville, although I do not know whether that was actually true. De Tocqueville is of course famous for his book, *Democracy in America*, and when he was in America prior to writing that book, he also wrote a small book called, *A Fortnight in the Wilderness*. He and a friend decided they wanted to go westward to the "frontier," and this book is an account of their journey, which they made in 1835.

They journeyed westward to Detroit and asked, "Where is the wilderness?" They were told to go north, and with a guide they traipsed 100 miles northward through the woods, ending up at the Saginaw River. They were told, "This is the wilderness—the frontier." At the time, they found only a couple of trappers' cabins and a few Indian wigwams. But that is one of Saginaw's chief claims to fame—that Tocqueville visited the site in 1835, when only a

handful of people lived there, and a good while before it became the City of Saginaw.

Incidentally, while no one seems to know where the name Saginaw came from, I note that Thomas Jefferson signed a peace treaty between the United States and the Sanguinam Indians, who lived in that part of Michigan. So I think "Saginaw" is just a variant of the name "Sanguinam." Since the Sanguinam Indians did not write, it seems likely that the name would end up in a form easier for English speakers to pronounce.

Scott: Having a Carnegie public library in Saginaw was a godsend for you. Those libraries did a lot of good in this country. What kinds of things did you read?

Housner: I was an omnivorous reader—I read everything. I was a great reader, and certainly the Carnegie library did have a big influence, no question about it. My reading spanned from Beowulf to Jules Verne.

Scott: That kind of reading in your youth can contribute a great deal to your cultural and informational background.

Housner: Yes, and without the library, I could not have done that. Saginaw was originally built up as a lumbering community. I think the town began building up in the 1870s, a little after the Civil War. The state had been shaved smooth by glaciers during the ice age, and then forested over after the retreat of the ice. So the entire state was covered with forests when de Tocqueville was there, but they were all cut down in the last half of the 19th century. The lumber obtained from what had been the wilderness was used to build the homes of the Midwest. Although the original forest is thus

long gone, in some places they now have forests of second-growth trees.

University of Michigan

Housner: Starting in 1928 I attended the University of Michigan in Ann Arbor, and graduated there with a B.S. degree in civil engineering.

In retrospect, it seems like the Dark Ages. The courses offered were old-fashioned, and looking back now I can see why. For one thing my professors at the time would have gotten their degrees around 1900. Railway engineering was one of the required courses I had to take.

There had been a big expansion of railroads from about 1870 to 1900, and railroad engineering was then very important. By the time I was in college, however, they had hardly built a railroad in thirty years. Nevertheless a course in railway engineering was still being required.

Scott: Aside from feeling that the curriculum was somewhat old-fashioned, do you think you got a reasonably good educational grounding there?

Housner: No, although I do not suppose it was really any different than in other colleges at the time. University life and behavior were, of course, quite different in those days from what they are now. When I was at Ann Arbor there were 6,000 students—now it is about 40,000. One sign of how times have changed was the fact that when I was there, students were forbidden to drive automobiles in the city of Ann Arbor, period. While you were a student you could not drive an automobile. And if you were caught doing it, you were in real trouble.

Stephen Timoshenko: Made a Big Impression

Housner: The one professor who did make a big impression on me was Professor Stephen Timoshenko, who was then at Michigan, and I took a couple of courses from him. One was on the theory of elasticity, and the other the theory of plates and shells. It was clear that Timoshenko was of a different caliber from the others.

Scott: Timoshenko was a major figure in engineering in the U.S., and also in Europe, I believe.¹

Housner: Yes, he was a major figure. In the 1920s, after the Russian Revolution, he came over to the United States and got a job at the Westinghouse Research Laboratory for a few years, and then went to the University of Michigan. He stayed at Ann Arbor until after World War II, and then he moved to Stanford University and was there for quite a number of years. Egor Popov, now at UC Berkeley, was one of his Ph.D. students at Stanford. When he retired from Stanford, he went back to Switzerland and lived with his daughter there.

Timoshenko had a very dim view of America. The word was that one winter day in Ann Arbor a student came to his office and knocked on the door. "Come in." The student came in wearing a stocking cap, which he did not take off when he entered. That episode seemed to have convinced Timoshenko he was in with barbarians, and he apparently never got over that feeling.

1. Timoshenko, Stephen P., *As I Remember: The Autobiography of Stephen P. Timoshenko*. Translated from Russian by Robert Addis, 1968.

Scott: What were Timoshenko's classes like?

Housner: They were very interesting, and looked at the subject more rigorously than the others at Ann Arbor did. Later on, when I was at Caltech, I had a couple of courses from Theodore von Karman, and found the difference between the two really striking. Timoshenko was what you would call a "blackboard artist." He came to class and talked, and all the time put it on the board. It all went neatly and perfectly, until just at the end of the hour he would get to the end of the board. In contrast, what von Karman would put on the board was rather disorganized.

I realized later that Timoshenko's approach was one in which he concealed the difficulties—everything he presented was smooth. Whereas von Karman emphasized the difficulties. Intellectually, I was much more influenced by von Karman, who taught us more how to think about a subject on our own.

Student Years at Caltech

Housner: I graduated from the University of Michigan in 1933. It was the middle of the Depression, and there were no jobs for engineering graduates. Not one member of my graduating class in civil engineering had a job lined up at the time of graduation. I came out to California to attend the California Institute of Technology, and got the M.S. degree here in 1934. Among my regular classmates that year were Bill Moore and Trent Dames.

Influence of R.R. Martel

Housner: While at Caltech, through Professor R. R. Martel, under whom I later got my Ph.D., I got interested in earthquakes. His full name was Romeo Raul Martel, and he was of French-Canadian descent. He had gotten especially interested in earthquakes through the big Tokyo earthquake of 1923, and then the Santa Barbara earthquake of 1925. In addition, of course, there was the Long Beach earthquake of 1933.

When I came to Caltech as a student in 1934, there was still a lot of excitement in engineering circles about the destructive 1933 earthquake in Long Beach, and Professor Martel was very much interested in all that. He was giving evening courses for practicing engineers, and had the students present part of the demonstrations. That's when I got to know Martel, and also when I first got involved in the earthquake end of engineering.

As Martel's friend John R. Freeman observed, at that time there was nothing in any of the engineering books that talked about earthquakes. (I will have more to say about Freeman later.) Engineers were used to thinking only of gravity loads that push straight down, and of constant wind loads. Now, however, they needed to think about structures getting pushed sideways by earthquake forces. How should they design for such forces? These were seasoned engineers who had been practicing for 20 years or so. It took a lot of talking by Martel to explain how they could analyze and design for earthquakes.

Scott: Apparently in the late 1920s or early 1930s, Martel redesigned one or more Caltech

buildings while in the blueprint stage. In an oral history interview, Ralph McLean told me that when he was a student here, he and another student worked as draftsmen under Martel's direction in redesigning building drawings prepared by a New York architect or engineer. Martel considered the original designs seismically inadequate, and set about correcting this by getting them redrawn to put in some earthquake resistance.

Housner: Yes, Martel was appointed to the Caltech faculty in 1918, and he began influencing the design of Caltech buildings—those constructed after he became active are much more earthquake resistant than those that came before. Previously, the buildings Caltech put up lacked earthquake resistance, which I presume is also true of early buildings on the campuses of other universities in California.

This building we are in now, the Athenaeum, was one of those Martel redesigned. He was instrumental in improving the buildings on the campus. When I first came here, there were some very hazardous old buildings on campus, but we have since gotten rid of all those. It is clear that much more thought about earthquake design had been given the buildings erected here after Professor Martel came to Caltech.

The school was small enough to permit him to have a significant influence on what was done. Since Martel's time, the kind of thing Martel did has been continued. Any new Caltech building is reviewed by a small faculty committee—right now John Hall, Paul Jennings and I are the review committee. We are brought in by the department of buildings and grounds to meet with the engineer working on any new

building design. Our job is to make sure things are done right regarding earthquake safety. That sort of thing can be done at a small school like this, but probably could not be done at a big university.

Martel was also a consultant on what I believe was the first commercial building in California to have been *officially* given earthquake consideration in the design. This was before there were any requirements in the code. The Southern California Edison Building, the company's central office building on Fifth and Hill in Los Angeles, was built in 1928. It was a ten-story welded steel frame building, and Martel was consultant on the seismic design.

To the best of my knowledge that was the first building in California to have the benefit of a seismic consultant—where the owners said, officially, "We are going to design it seismically." Martel's consultation on this building was probably done around 1925. Purposeful seismic design had, of course, been done previously in Italy and Japan. Also, I know that in these early years, some California engineers did give earthquake forces some consideration, although this was not requested by the owners. In any event, the building is still there but is no longer occupied by Southern California Edison.

Scott: Unofficially, apparently a few engineers in California—probably a very few—were trying to design their structures with some degree of seismic resistance. For example, Mike Pregnoff spoke of the work of R.S. Chew in San Francisco, who apparently had his own methods of putting some resistance in buildings he worked on. Apparently, he sort of did

this on his own, although not required by the code or requested by the owners. In fact, the owners may not even have known about it.

Housner: Yes, of course, some engineers in California did seismic designs before 1928, but I believe not officially at the owner's request. Also we know that official seismic designs were done in Italy and Japan long before 1928.

Martel was a major influence in many ways. Partly, of course, he was influential through his students and propagated the faith with them. He also had some very clever ideas that proved very useful to those who consulted him. I remember for example when the Southern California Gas Company put up a new building fifteen or twenty years ago. They ran into some trouble, and a man came over and talked to me about it. He said that the president of the company told him, "Always go to Caltech if you have a structural problem," saying that it all started when they first brought gas in from Texas.

In the old days the gas company cooked coal to produce gas, which was stored in big tanks. There used to be several of those big tanks in Los Angeles. When they were bringing the gas in from Texas by pipeline, they wanted to put up new tanks that they thought would be needed. They came to Professor Martel and asked what he thought their new tanks should be like. He said, "Well, you already have a big tank, one that is a foot in diameter and 1,000 miles long." They said, "That's right, the pipeline has a lot of storage capacity." So they did away with using the tanks.

In another case, the City of Glendale built its own electric power plant. In the 1920s and

1930s, when some of the communities got angry with the Edison Company, several of them, such as Pasadena, Los Angeles and Glendale, broke free and set up their own systems. The Glendale people talked to Professor Martel about building their new plant, which was to have a special building for the electric generator—a big piece of equipment. They wanted his advice on how to design the building. He asked them, "Does it hurt the generator if it is rained on, or if the wind blows directly on it?" "No, that wouldn't hurt it." Martel said, "Well, then, leave the building off." That is what they did, and occasionally I drive by and still see the generator there with its earthquake-resistant non-building.

Scott: So again, Martel apparently thought of a simple, straightforward solution that the people coming to him had not thought about.

Housner: Professor Martel also had a role in the origin of the structural engineers association in our area. It has since developed into a very potent, very effective organization, now active statewide under the umbrella of the Structural Engineers Association of California (SEAOC), and with several regional organizations, including the Structural Engineers Association of Southern California (SEAOSC). SEAOSC was the first, its forerunner being the organization that Martel helped get going at the end of the 1920s.

Martel told me that in the 1920s several of the practicing engineers would come and talk to him about difficult engineering problems. One of them in particular was Oliver Bowen, an early figure in structural engineering here. Martel recommended to Bowen that the local

engineers get together and discuss these matters. So Bowen arranged for a dozen of the more prominent engineers to meet regularly for lunch—about once a week—and talk about their problems. They called themselves "The Dirty Dozen."

After that group had met for some time, other engineers wanted to get involved, and this led to the organization of SEAOSC in 1929. A year or so later the Structural Engineers Association of Northern California (SEAONC) was formed, and then other regional associations. Then SEAOC was formed, the statewide organization. So Professor Martel was influential in prompting the local Los Angeles area engineers to start that organizing process.

Influences on Martel: Hardy Cross

Scott: Martel really did have major influence, through his students, through his contacts with the practicing engineers, and also through the advice in response to inquiries such as those from the Edison Company and the City of Glendale. He apparently was unusually active, and started very early—you mentioned his influence on the design of Caltech buildings after he joined the faculty there in 1918. Do you have any ideas as to what in his background—or what influences on him—may help account for his seeming to be ahead of his time?

Housner: I believe that Martel was greatly influenced by one of his young instructors when he was an undergraduate student at Brown University. The instructor was Hardy Cross, and he later became famous at the University of Illinois when he developed a method of moment distribution for calculating the

bending moments in a steel or concrete frame building. Cross made a deep impression on Martel. From Martel's student days, he and Cross remained life-long friends. As evidence of his esteem for Cross, Martel named his son Hardy Cross Martel. The son became professor of electrical engineering at Caltech.

Martel told me one amusing story about Cross in the classroom. It was on a spring day in 1914 when Cross was teaching a class. While he was talking, Cross usually pulled out his \$1.00 Ingersoll watch now and then to see what time it was. On this occasion, he pulled out the watch and looked at it—then he held it to his ear and listened to it. After listening a moment, without missing a word of his lecture, he tossed the watch out the window and kept on talking. This made an indelible impression on the students.

Another person who made a big impression on Martel was an engineering professor, Kyoji Suyehiro. Following the 1923 Tokyo earthquake, Suyehiro was made the first Director of the Earthquake Research Institute at Tokyo University. I have the volume of his collected papers, which give the impression that he was a very able man. Martel met Suyehiro when he went to Japan after the 1923 earthquake, and each developed a high regard for the other, as is shown by their correspondence, which I inherited after Martel died.

Martel was a good teacher, although not in a formal sense. His classroom attitude was very informal. He had the knack of implanting in a student's mind an idea that might not mature until later, when a light would suddenly go on.

Theodore von Karman

Scott: Could you say more about recollections from your student years at Caltech?

Housner: When discussing Timoshenko earlier, I referred to Theodore von Karman, contrasting him with Timoshenko, who was at Ann Arbor when I was there. Von Karman did more to help students understand the process of dealing with problems, and taught them to think on their own.

When seeking a solution to a problem, von Karman was very good at looking into the essential physics of it. In our business there are two approaches. One is to study a problem, write a differential equation, and then work away at the mathematics of solving it. The other approach is to look at the physics of what is going on and try to decide what really is the crux of the problem. That is, you do a mental abstraction to get away from all of the irrelevancies. Von Karman was very good at that, and could come up with a very good answer that was not obscured by lengthy mathematics. In the foreword to his book on applied mathematics he said that we use mathematics as a tool to help us understand the physics of a problem. That was what von Karman did, and not everybody does that.²

When I started to work on my Ph.D. thesis on the dynamics of buildings, Professor Martel asked von Karman about the differential equation for a vibrating beam. Von Karman gave him a write-up on the back of an envelope,

2. Karman, Theodore von, and Maurice A. Biot, *Mathematical Methods in Engineering: An Introduction to the Mathematical Treatment of Engineering Problems*. McGraw-Hill, 1940.

which he passed on to me. I looked at what von Karman had done, and later when I met von Karman on campus he asked, "Did you get that note?" "Yes." "Did you understand everything?" I said, "Yes, everything except the equation for eigen frequencies—I could not understand where that came from." "Oh," he said, "That was just a hot flash." It was not derived from what he had done. The true equation was much longer and more complicated, but he had thought of a simple way to get a good approximation. It was just a "hot flash" that came to him!

So von Karman was not one for great mathematics, but rather for seeing the basics of problems. Caltech Professor Richard Feynman, a physicist, was also noted for such abilities. A physicist friend once told me about being at a physics conference where a very interesting paper involving very complicated and lengthy mathematics was presented. That night Feynman thought the matter through, and then the next day asked for conference time to make a brief presentation. He was able to get through to the answer in only ten minutes or so. According to my physicist informant, Feynman then got a standing ovation. Von Karman was very good at that sort of thing. He could think his way through to the essence of a problem.³

Ed Simmons and Work on the Torsion Pendulum

Housner: My student years were times of hard work. In working on my thesis we developed equipment for calculating the spectrum by means of a torsion pendulum. I got assistance on this from Edward Simmons, who had gotten his M.S. degree a couple of years earlier.

He was one of the kind of people whom you find hanging around most universities. They are undisciplined types who, after getting their degrees just hang around because they like the environment. They do odd jobs for the professors, and often are the source of bright ideas. Ed Simmons would not do anything for you unless he liked you. If he did not like you, he would have nothing to do with you.

He was interested in what we were doing, and helped put together the equipment for calculating the spectra by means of a torsion pendulum. I mention all this because he then became the inventor of the bonded electric wire strain gauge used in stress analysis circles. That method of measuring strain is now universally used. So that contact with Ed Simmons was one of the interesting aspects of my student days.

He is still around here, by the way. A curious fellow. When he was working with me, we thought he was living in one of the labs, and had some corner where he bedded down. About the only clothes he had were dirty yellow corduroy trousers and a green cardigan sweater. After the war, around 1949, he was given an award by I believe the Franklin Institute of Philadelphia for inventing the strain

gauge. I saw a photograph taken when he was back East and receiving the award from the organization's president—there he was in his dirty yellow corduroy trousers and green cardigan sweater.

In more recent years he has taken to wearing something of a Renaissance costume with tights. I would be taking visitors around campus and they would see him and ask about his attire. I would say, "Oh, he is probably a professor of humanities." I did not say anything about his being an engineer.

Not having seen him around recently, I inquired about him of Cal Gongwer, a friend and Caltech alumnus. Cal has an engineering company that makes special equipment, and retains Ed Simmons as a consultant for his good ideas. He had told Ed, "You can come down to consult, but you have to wear pants." Ed complied for a time, but later I guess they let him come without the pants, wearing tights. Cal offered to build Ed a little apartment at the plant and let him stay there. But Ed said, "No, Pasadena is a cultural center, and I want to live there." Cal told me that Ed is now living in his auto in the west end of town, where he has a warehouse full of stuff he has accumulated.

Ed is now apparently very active at the Huntington Library, having gotten interested in Renaissance English literature. He used to come to our seminars here at Caltech. I am told that a student asked him, "Why do you dress like that?" Ed said, "Well, I am rich and I can dress any way I want to." I don't think that really answered the question, and I think he dresses that way to attract attention.

3. Professor Theodore von Karman was an eminent European academic from Budapest, who was educated in Germany. In 1928 he was brought to Caltech, where he headed the Guggenheim Aeronautical Laboratory, and later played a prominent role in U.S. and international aeronautical and astronautical research. He was the founder of the Caltech Jet Propulsion Laboratory. Von Karman's biography appears in the book *The Wind and Beyond*, published by Little, Brown and Co., 1967. Von Karman also appears on a 29-cent United States postage stamp, which was issued in the early 1980s.

Lack of Communication Among Students

Housner: I also recall from my early days that there was not enough communication among my classmates. We students really did not profit from communication among ourselves as much as we should have. I think that is probably still the case. At that age, and as students, you are competitors, and this inhibits you from intellectual intercourse related to your course work. You view your colleagues as competitors for grades, and are inclined to go it alone. I do not mean to suggest that we did not communicate at all among ourselves, but rather that greater communication would have been beneficial.

There were some exceptions—Don Hudson and I for example were close—but in retrospect I think I could have profited if I had talked to the other students more. I think this lack of communication still characterizes students here. Maybe it is a phase you go through as a student. The professors do not have that inhibition, and communicate openly with their colleagues and their students.

Scott: The lack of communication among students seems unfortunate, because students can learn a lot from each other in informal discussions—bull sessions. From what I have observed, students in the social sciences do not seem to have such a keen sense of competition, at least not to the point of limiting communication with each other, or inhibiting their arguing and debating. I also think their communication plays a valuable role, helping them better understand problems, topics, and concepts, and sharing advice on dealing with professors.

Housner: The Caltech students do communicate in bull sessions, but not so much about serious studies. My feeling is definitely that the students in the technical end of things do not communicate so well as students in the social sciences. I am not saying we did not talk to each other, but there was not a lot of intellectual communication.

Professor Zwicky—Influential, and a Character

Scott: You mentioned a lot of hard work in your student days. I guess you also found it pretty rewarding—you were learning a lot that you considered of real value?

Housner: Yes. In my graduate studies, one other professor who made a big impression on me was Professor Fritz Zwicky. He was an astrophysicist, and came from Switzerland. He gave a course in theoretical mechanics that I took. He was also a well-known character, one of the types who is not a formal lecturer-presenter. But, like I did from von Karman, from Zwicky I got the idea of how to look at problems. He exerted a big influence on my development.

Scott: When you referred to Zwicky as a "character," did you mean he was a bit eccentric?

Housner: He was very gruff and outspoken, and had a rather abrasive personality. The students were all afraid of him. There were a lot of stories about him. One story was that in the aeronautics department they were studying the roughness of surfaces and the effect on air flow. They had developed some equipment for measuring minute irregularities on the surface of

aluminum plates. Von Karman took Zwicky over to show him what they were doing. Zwicky found it very interesting, and asked, "Well, what is the unit of roughness?" Von Karman said, "That unit is the zwicky, but it is too big, so we use millizwickys."

Scott: The full "zwicky" was just too rough!

Housner: Here is another Zwicky story. He married late, and then had a baby—a daughter. He commented: "Well, it's better that way, because it would be very tough to be the son of a genius."

An Epstein Story

Housner: There were many more characters here on campus then than there are now. I think in more recent years we are all sort of brain-washed into fitting into the system. Here is another story dating back to that earlier time, involving Frank Marble, a professor here about my age, who is now retired. When he was a student he was studying for his Ph.D. under von Karman. When some problem came up, von Karman said, "Why don't you go talk to Professor Epstein about that?"

Epstein, educated in Europe, was a professor of physics and also a well-known character. It was generally known that he did not like to have anybody come and ask him a question without forewarning him. You had to give him some advance notice. Also he did not want any students to ask questions in class—that upset him and made him angry. So in all innocence, Frank went over to Epstein's office, which was across the hall from the little physics library. Frank knocked, and Epstein said, "Come in." Frank went in and posed the question. Epstein got up

and walked out of his office, with Frank behind him, walked into the physics library, around the table there, back out the library door and back into his office, closing his office door in Frank's face. Frank got the idea—Epstein wanted him out of there.

My Years as a Structural Designer

Housner: After graduating from Caltech in 1934, I worked for some years as a structural designer in the Los Angeles area. In 1934 there were many unreinforced brick school buildings in southern California, and they all had to be retrofitted according to the requirements of the Division of Architecture. So for several years my job was to inspect the buildings and do the necessary engineering for retrofitting. Then I worked on many other projects, including new school buildings, commercial buildings, office buildings, industrial buildings, and an expansion of the Rose Bowl. I also worked on the design of the Santa Fe Dam in the San Fernando Valley, some theater buildings and some college auditorium buildings.

Looking back now I realize that I had a misplaced confidence in the 10-percent-g method of design used at that time. I found structural engineering to be very interesting, and it showed me the difference between the practice of engineering and the theory of engineering. Also, even now when I am driving along, I sometimes recognize a building and say to myself, "Oh, I designed that."

Reflecting on my years of practice set off a train of thought that I might note here. When as a practicing engineer I faced a special problem, I would proceed to work out the solution, ending up either with an equation, or with a diagram

from which the solution could be read. I would preserve this in my design notebook, so that if I encountered the same problem again later, I could determine the answer without further thought.

You could say that this was an example of the principle of minimum intellectual effort, which I think all of us utilize, and which is necessary for the advancement of science and engineering. On the other hand, if the principle is followed blindly, it can lead to unhappy results. The experience of the Baldwin Locomotive Company is a good illustration of this point. According to my friend, Reuben Binder, the company apparently carried the principle to extreme lengths.

Binder told me that in 1925, when he graduated from engineering school, he got a job with the Baldwin company, which was the premier manufacturer of steam locomotives, and back in the 1880s had a work force of 4,000, and manufactured about 800 locomotives per year, of about 300 different models. Binder described the procedure followed in filling an order when he was employed there.

The sales department would send an order for a new locomotive to the engineering department, and the chief would mark on his checklist all the drawings that were needed to manufacture it. The list was given to Reuben, and it was his duty to go to a room with hundreds of flat drawers, each containing a different drawing. Checklist in hand, he would collect all the necessary drawings, which were reproduced and then sent to the manufacturing unit. Using the drawings, the manufacturing unit would put the new locomotive together.

As I say, this procedure carried the principle of minimum intellectual effort to its extreme limit, and no real thinking was involved in the process. This was, in fact, the major defect in their approach. The lack of real thought about what was being done meant it was very difficult to get new ideas into the picture. In time the Baldwin Locomotive Company and the steam locomotive both went the way of the dinosaurs. I guess the moral of this is: Too much of a "good" thing is not good.

After five years of practice, in 1939 I went back to Caltech to work for the Ph.D. degree. I had always intended to return to school and become a professor. But in 1934 California and the nation were in the depths of the Depression, and there were many unemployed engineers. I considered this to be a challenge: Could I get a job and hold it? I could, and I did. My years of working as an engineer provided me a valuable nonacademic education that I have found helpful. Also I liked the challenge of having to design a structure, and I liked seeing my design actually built.

Later, even after I was on the Caltech faculty, I occasionally consulted on a building's seismic design. An interesting example of one such earthquake-related job was at the Huntington Art Gallery in San Marino following the 1971 San Fernando earthquake. The Huntington complex, which includes the Library, the Art Gallery and the Gardens, is a cultural resource of international renown, located on the former estate of Henry Huntington, about two miles from Caltech. You might say it was developed as a Disneyland for intellectuals.

After the earthquake, the curator, Robert Wark, asked me to come to the art gallery and

give him advice. I found that a portion of the Art Gallery was in Huntington's original home, which had been built around 1910. The building had concrete floors and roof supported on concrete columns, but with double hollow-tile walls making up the exterior and interior walls. Such construction was not uncommon for the time, but is a very hazardous type of construction in earthquake country. In the San Fernando earthquake, the tile walls had been cracked by the ground shaking, which produced about 15-percent-g peak acceleration at the site.

I told Bob Wark that the building was very hazardous and should be strengthened, particularly as it was open to the public. He responded with a bit of humor: "Oh, you can always make more people, but you can never reproduce this

art." The upshot was that the building was retrofitted with reinforcing bars and gunite on the walls. When Wark found that it would take \$350,000 to finance the retrofitting, they held a big dinner for the Huntington Associates, and immediately collected the necessary funds. In its post-retrofit condition, the building looks exactly like it did before.

Return for Ph.D. at Caltech

Housner: In the fall of 1939 I came back here to school at Caltech to work on my Ph.D. degree. I got the degree in 1941, shortly before we entered the war, and then went to work for the Corps of Engineers here in Los Angeles. I worked there for a year, not on seismic things, but on things that had to do with blasts.

World War II

"[We studied] missile penetration into concrete, and the effects of bomb detonation on structures, and so on."

Operations Analysis Section

Housner: Shortly after that the U.S. got involved in World War II. In 1942 I signed up with a Division of the National Research Council. It was stationed at Princeton University, and I signed up to become a member of what they called an Operations Analysis Section for the Air Force.

The Division was under the leadership of John Burchardt, who was a professor of architecture at MIT. His unit had to do with studying missile penetration into concrete, and the effects of bomb detonation on structures, and so on. This was being done for the war. I went back to Princeton to familiarize myself with what they were doing. Then I was assigned to a section of the Air Force overseas.

In March of 1943 I departed to become a member of the Operations Analysis Section of the Ninth Bomber Command, stationed in Libya, outside of Benghazi. Because the German army was active in northern Africa, the standard way of getting from the U.S. to the eastern Mediterranean region by air followed a tortuous route that crossed the Atlantic from Brazil. I was alone when I left Washington, D.C., but picked up colleagues en route. At that time, of course, there were no civilian air lines as we know them today. I flew by military plane from

Washington by way of Miami, British Guiana, and Belem, Brazil. From Belem I flew to Accra, then to Kano, Maiduguri, El Fasher, Khartoum, Cairo, and Benghazi. I have done a lot of traveling in my career, about 95 percent of it related to business, and much of it very interesting, but this journey to Libya during World War II was the most unusual trip I ever took.

In Libya we lived in tents outside of the city of Benghazi, which had been completely evacuated and emptied of inhabitants. Our unit was trying to study the operations involved in bombing. One of the things I did was in connection with the planning for a low-level bombing raid on oil fields at Ploesti in Rumania. The oil fields (refinery units, storage tanks, and so on) were protected by barrage balloons, which flew at an elevation of about 2,000 feet. Actually, the barrage balloons were flown like kites. They took advantage of the breeze to fly them up higher. They were tethered by high-strength wire about a tenth of an inch diameter.

Their intention was that if an attack plane came through in low-level flight, it would hit the wire and spin out of control. So the question was, if we made the low-level flight, what would be the effect on our planes of flying into these wires? I was given the task of figuring out what that effect was likely to be. I could show what would happen when the plane hit the wire—it would pull the wire with it, while stress waves ran up and down. I could figure out how long it would be in contact before breaking, and what the forces would be. You could then show that a force of that magnitude for such a short time would not pull the plane out of control. The wire would break first—in fact you could show that at a certain speed the

wire would break instantly. When our planes did fly and returned we checked them, and found that five showed the marks of having hit a wire. The pilots did not even know they had hit a wire at all—they just went right through. I devised a neat equation for the stress developed in the cable when impacted by velocity v .

$$s = E \left(\frac{1}{2} \left(\frac{v}{c} \right)^2 \right)^{2/3}$$

Where: s = stress of the cable
 E = modulus of elasticity
 v = velocity of airplane
 c = velocity of stress propagation in cable

Then we moved from Benghazi to Tunis in preparation for going to Italy, after the southern part of Italy had been secured by the U.S. and British armies. I think it was in November of 1943 that we moved into Bari, Italy. We were attached to the newly formed 15th Air Force, and moved into the headquarters building of the Italian Air Force. At that time I became chief of the section, and was there for the rest of the war.

It was an interesting experience, but it had a psychological effect on me. When I first got into this business, I felt quite uneasy about participating in dropping bombs on people. I took comfort from the fact that we were aiming at military targets and not cities, but when we first moved in to Bari, the German air force made a raid on us, dropping bombs. I can remember that it was quite alarming. At first the anti-aircraft guns went into action. It was at night, with all the colored tracer bullets, and looked

like the world's biggest July 4th celebration. Occasionally, it would stop, and then you could hear the a bomb whistling—it appeared to be coming right at you.

Then in the middle of the raid a German bomb hit a ship in the harbor loaded with our bombs, setting off a tremendous explosion. Bari was a small town and the harbor was close by. A year later there was a similar explosion in which something happened—without enemy action—that touched off a shipload of bombs, and up it went. What I'm getting at is that after being a recipient of bombing attacks, your attitude changes. You do become hardened.

After the Army had progressed beyond Rome, then I wanted to look at some of the bridges we had attacked to see what shape they were in. I got up to one place where a bridge was supposed to be—but there was no evidence of a bridge. It had been bombed so heavily that everything had gone underwater. I went to where the bridge should be, and nothing was there. A woman was sitting there, perhaps on the remains of the bridge abutment. I went up to her and asked if there wasn't a bridge there. This is an example of how you get hardened. I wasn't thinking about the effect on the civilians, but after saying, "Yes," the woman began crying. The bombs had killed her husband and children. That was a real upsetting incident for me. I had just been thinking about the bridge, and had ruled those other considerations out of my conscious mind.

As I say, however, the war was an interesting experience. If you tried to sum it up in one sentence, you might say, "Like a Boy Scout camp with guns and bombs." That's the best way to describe it. The Air Force was just made up of

young guys. When we were in Italy there were 100,000 personnel in the Fifteenth Air Force—10,000 were flyers: pilots, co-pilots, bombardiers, gunners, and navigators. The other 90,000 were to provide support, to keep the flyers going. They were mostly in their 20s, sort of like our graduate students. The atmosphere was really like a Boy Scout camp.

I still recall how surprised I was when one of the officers at the headquarters said to me, "I'm going to fly to Cairo to get some ice cream, do you want me to bring you some?" I wondered—fly some 500 miles from Benghazi to Cairo for ice cream? But then he explained that the real reason for the trip was to comply with regulations requiring him to fly at least 1,000 miles each month. Getting the ice cream was an incidental.

I'm glad I don't have to do that again, although it was certainly a big change for me, and a very striking experience. I was overseas for two-and-a-half years in an environment quite different from anything I'd ever experienced before, or since. It was nothing like being a professor or an engineer. It was very interesting, however, partly because in one sense, we—in our group—knew more about the operations of the Air Force than anyone else. We'd check on the bombing, the gunnery, everything, to see if improvements could be made.

Improving the Accuracy of Gunnery

Scott: Can you give some more examples of your unit's work.

Housner: To give you an example of what we did, when our unit of eight men got to Libya, we began finding out how the gunners in our

bombers were shooting their machine guns against attacking fighters. We discovered that the gunners had been trained incorrectly. They were trained to aim as if they were shooting from on the ground. If you are on the ground and try to shoot a bird in flight, you aim ahead of the bird. But when you're in a plane, your high velocity means that you have to aim *behind* the target. When we saw that, we prepared a booklet of instructions that told the gunners how they should aim. This book was then reproduced in the States and became part of the Air Force educational system. We titled it *Get That Fighter*. I think that was an important accomplishment.

Runway Dust Suppression

Housner: Another example is from Libya, where the country has been denuded of topsoil. What was exposed on the surface was just a fine clay. When the airplanes took off, their propellers would churn up a big dust storm. They took off one after the other, in groups of about 30 planes. So their engines were breathing in this terrible dust and it was wearing them out. We were asked to take a look at the problem. We found an old lake nearby that had been used for making salt from seawater that had been brought into the lake. Well, at the bottom of what had been the lake was a soupy, salty material, which was hygroscopic (tended to absorb water). We tested it by putting it on a few places to see if it kept the dust down, and it did. So we recommended that they get a tanker truck to apply this on the runways. They did, and it worked.

So that's another example. It's also an example of the difficulty posed by human nature. When

we proposed using a tanker truck to do that, the officer in charge of the engineer company attached to the Command objected. He said, "Oh no, you can't do that." He argued, "If you put it in the tanker it would spoil the tanker, and you won't be able to use it for anything else." He had all sorts of reasons. But the commanding general overruled him and they went ahead and did it. Sometime after the war was over, I was amused when I ran across the report the engineer company had written. It said that they had the idea and they did it all, and it was successful.

Projecting Air Attack Losses

Scott: Did the work make use of a substantial amount of your engineering background?

Housner: No, it was mainly a matter of thinking and judgment. We did all sorts of different things. Another example was when they put on the famous low-level air attack on the Ploesti oil fields in Rumania. The general asked me to estimate the losses that might be expected. We knew where the anti-aircraft guns were, and he wanted to know the probability of getting hit. We made this study and came out with a number that projected quite a severe loss of about 30 percent. The raid was put on, and the losses were of that percentage. The general himself rode along on the raid as a copilot. He was the only one among them who knew about the high loss estimates—and he did not tell anyone else. But fortunately he was one who came back.

There were a lot of interesting problems of that sort. They were not something you would be able to prepare for at school.

Scott: Was the oil-field raid considered successful, despite the high casualties?

Housner: I don't think so. We went into it knowing that it would not be a success. It was dictated by Washington. It was a demonstration and a public relations matter—its purpose to show we had hit the enemy. But you could tell the oil fields were not a big operation, they were just a bunch of little operations. There wasn't a lot of oil there, either.

Figuring Direction of Least Exposure for Air Raids

Housner: Another later example also involved air raids, when we were bombing places in Yugoslavia, Austria, and France. When you lay on a raid, the significant targets are always well-protected by anti-aircraft guns. What you want to do is come in from the direction that gives you the least exposure, taking into account the number of guns and the distance, because the farther you are from the guns, the less accurate they are. The problem is also complicated by the fact that you should take the wind velocity into account, because it affects the time of exposure. And you don't know wind velocity until the day before the raid. They were having trouble in planning, and everyone felt it wasn't being done right.

So we looked at the problem and saw that you could make a map locating the gun positions. Then you could make a set of plastic overlays, which permit you to calculate the probability of being hit when you fly across from any direction at a prescribed elevation. You would make a map of the probability. Then you could adjust the thing by putting another overlay on that

one, showing the wind velocity, and orienting it in the right direction. If you do that, you can immediately read off the probability of being hit. It could be done right at the planning session the day before the raid.

We thought that was a good accomplishment. I was especially pleased, too, when we got back to Washington and I came across the report by the corresponding group for the Navy. They did the same thing for the Navy, and they gave our report as a reference, but they made a big to-do about their own work.

Improving the Effectiveness of Bridge Bombing

Housner: When we first went to Italy, one of our continuing problems was to stop the north-south movement of traffic, both rail and road. There were hundreds of bridges on the east and west sides of the Appenine Mountains, crossing rivers and canyons. When our attack on bridges first started, in their bombing runs the planes would fly *parallel* to the bridge they were trying to hit, and attempt to drop all their bombs on it. This would have been the best approach if the accuracy of the bombing had been near-perfect. In fact, however, bombing accuracy was far from perfect, as the planes flew at an altitude of five miles.

When we studied the problem, we saw that much better results could be obtained if the planes flew *perpendicular* to the bridge they were attacking. They would then try to lay the relatively widely spaced bombs down in a string that intersected the bridge, hoping there would be one hit. The central element of the problem was this: Given the level of accuracy, it was not

desirable to optimize the number of bombs that hit on or fairly near bridges, but rather to optimize the number of bridges that were hit destructively by at least one bomb. Convinced that our analysis was correct, the Air Force adopted this approach.

The bridges were mostly masonry arch bridges, and when a bomb struck such a bridge it would destroy one of the arches. Whereupon the German Army Engineer Corps would repair the bridge, so traffic could be resumed. When one of the German Engineer Corps soldiers was captured by our Army and questioned, the interrogation report reached my desk. From the list of equipment he said their Engineer Corps had available, I saw that the longest I-beam was 15 meters (about 50 feet) in length.

Thus, when the span of an arch that was destroyed was less than 15 meters, it would be relatively easy for the Germans to put some I-beams across the gap. On the other hand—if the span exceeded 15 meters—to repair the bridge, an intermediate pier would have to be built to enable the available I-beams to span the gap, demanding much more effort. So our process of selecting bridge targets was modified to take advantage of this information and bomb bridges with spans exceeding 15 meters.

To sum up, there were a lot of things we felt we had contributed, either to more effective bombing, or to reducing losses. In the beginning when things looked tough, people listened to us when we recommended things. As time progressed, however, and you could see that we were winning the war, we had a more difficult time getting the Air Force to listen. But I guess that's an aspect of human nature. After the war,

much interest developed in operations analysis kinds of studies as applied to large industrial companies. Many companies now have an operations research group, and there is an operations research society with technical journals.⁴

War's End and Appointment to Caltech

Housner: At the end of the war I received a medal for my contributions, but I'd had enough of war. I was glad to get back home after being overseas two and a half years. I returned in May, 1945. I was all set to go to the West Pacific for the Pacific war, but that came to an end before I departed.

Actually, I was never officially in the service. It was an odd thing. It was decided that Operations Analysis should be out of the line of command, which I think was probably good. Otherwise they wouldn't have listened to us at all. But we wore a uniform, and had what is called a "simulated rank." It showed where you'd be in the general pecking order—your equivalent rank. But I was glad to get out of it after two and a half years.

In 1945, shortly after I got out of uniform, I received an appointment at Caltech. When I first came back to Caltech and joined the teaching staff, I was in a sense Martel's assistant, because I was interested in the same things he was—especially earthquake effects. I have been

4. See also Chapter 20, *Discussion of Selected Publications*, for reference to a brief history of that war-time experience, written in 1945 for the Air Force files, and recently reprinted in a limited edition.

here at Caltech ever since, and have found my career very satisfying.

History of Response Spectra

“After the graphic approach, our next step was to develop an analog, using the vibrations of a torsion pendulum excited by twisting the support.”

Scott: At this point it seems appropriate to ask you to review the development of earthquake response spectra, inasmuch as you are given credit for this basic contribution to earthquake engineering. As I understand it, an important part of that story begins in the early 1930s, about the time that you arrived at Caltech as a student.

Martel's Students: Biot, White, and Housner

Housner: I'll start by discussing the pre-war doctoral earthquake engineering work at Caltech that led to the work on response spectra. This was the earthquake engineering research done at Caltech by three students of Martel's—Maurice Biot, Merit White, and myself, in that order.

Maurice Biot was an extremely able man, and was the first to get his Ph.D. with Martel for work on earthquakes. In his thesis he analyzed the calculation of the response spectrum. Biot actually came from Belgium, but got his Ph.D. here at Caltech in 1932. Biot worked in the U.S. until he retired, and then went back to Belgium, where he died in 1987.

Merit White was the second of Martel's graduate students to get a Ph.D. for work on earthquakes. Merit became a professor of engineering at the University of Massachusetts. Then I was the third, doing my Ph.D. work in 1939 to 1941. By the time I had finished my doctorate, our Ph.D. efforts had worked out the theory of vibration enough so you could see how the response of buildings to earthquake motions would reflect the natural period of the building, how the height determined that, and how the spectrum indicated the intensity of motion.

Prewar Work on Response Spectra

The 1920s: Suyehiro's Pendulum

Housner: In looking back at the history of response spectra, I can identify a number of things that led up to it. Of course, I don't know how much people knew of these things, but I would say that the beginning was an instrument that Kyoji Suyehiro made in Japan. Suyehiro was the first director of the Earthquake Research Institute at Tokyo University. His instrument comprised a half dozen pendula with increasing periods of vibration, his idea being to see how the different pendula would respond to earthquake motion. The maximum amplitudes of the six pendula noted in an earthquake would give six points on the displacement spectrum curve.

Although Suyehiro's instrument recorded some weak tremors, it did not record any strong shaking because Tokyo has not experienced such shaking since 1923. In the 1930s, the U.S. Coast and Geodetic Survey's Seismological Field Survey in San Francisco made such an instrument, which was copied from Suyehiro's,

but they never got any records either. Still, Suyehiro's instrument did exist and may have affected the thinking of those who were trying to work on the earthquake problem, although I do not know whether it actually did.⁵

Early 1930s: Benioff Article and Biot Thesis

Housner: The next thing I identify is a 1932 article by Hugo Benioff, published in the *Bulletin of the Seismological Society of America*.

Benioff's article conceived of an instrument similar to Suyehiro's. The projected instrument would record the displacements of pendula of different periods, thus crudely defining a displacement spectrum curve, and Benioff proposed that the area under the curve would indicate the destructiveness of the earthquake.

Then came the work of Maurice A. Biot, whom I have already mentioned as Martel's first earthquake engineering graduate at Caltech. Biot had written his 1932 thesis on the dynamics of earthquake engineering, and in it he proposed what we would now call a "response spectrum."⁶

Our Work Immediately Before World War II

Housner: I mentioned that in 1939-1940, after several years of engineering practice, I showed up here as a graduate student, having come back to get my Ph.D. Martel was my

5. Suyehiro, "Engineering Seismology: Notes on American Lectures," *Proceedings of the American Society of Civil Engineers*, Vol. 58, no. 4, pp. 1-110, May 1932. While Suyehiro's 1932 article did indicate that useful records were obtained, showing a persistence of motion at 0.3 seconds, Housner observed that the instrument did not have control of damping, so any results could not be applied directly to buildings.

faculty advisor. We decided to calculate the response of structures to recorded earthquake ground motions, first using graphical procedures. At that time we did not have the computer or the computing capability we have now.

It was a theoretical analysis, not experimental, and to do it we had to compute the response of the structure to the recorded ground motion, which could not be expressed in mathematical form. Now we do that with a computer, digitize the record, and go through it that way. At that time, however, we did not have those capabilities. Anyway, we calculated the response, and got different values depending on the natural period. I recall my intent then was to make such calculations and ascertain the effect of the different kinds of ground, of soil, on the response. But when I did that, there was no discernible effect in the records we had. It was a false start.

About that time Biot, who had gone off to Columbia University, also calculated the response to ground motion, using a small torsion pendulum.⁷ When he did that, he did it

for a number of periods, like Suyehiro's pendulum, and drew what we would now call the response spectrum of the ground motion. His calculations were made without taking into account the actual damping of the pendulum.

Then Martel and I started making our calculations. This work was made possible by funding that Caltech got from the Los Angeles County building department. We made the computations, calculating the spectra for zero damping. This was an interesting challenge, which required computing the response of a single-mass system to the ground motion of a number of earthquakes.

Approaches to Response Analysis

Housner: The first time we tried to do this analysis with a hand-operated calculator, it proved to be an enormous task, quite beyond carrying out. You are carrying out the integral of the product of the ground acceleration multiplied by a trigonometric function. If you could express the earthquake motion in a simple algebraic equation, you could carry out the integration very quickly. But of course earthquake motion is extremely complicated, so you cannot write an equation to represent it, and it has to be done numerically.

In those days, when I was a student, the only calculator we had was a Marchand, on which you added and subtracted by punching the keys and then turning the hand crank. To do that numerically you have to divide the accelerogram into small increments, read off the ordinates, and then multiply for the corresponding times by the value of the trigonometric function. For a small earthquake record, this could involve hundreds of numbers to multiply, and

6. In his 1932 Caltech Ph.D. thesis, "Transient Oscillations in Elastic Systems," Biot suggested a method for the possible evaluation of random impulses on vibrating systems—e.g., earthquakes on buildings. These ideas were further developed in Biot, "Theory of Elastic Systems Vibrating Under Transient Impulse With an Application to Earthquake-Proof Buildings," *Proceedings of the National Academy of Sciences*, Vol. 10, no. 2, February 1933.
7. Biot, Maurice, "A Mechanical Analyzer for the Prediction of Earthquake Stresses," *Bulletin of the Seismological Society of America*, Vol. 31, no. 2, April 1941. In Biot's words, this was an attempt to draw "a curve representing some kind of harmonic analysis of an earthquake, where the acceleration intensity is plotted as a function of frequency."

then adding up the sum. So it was impossibly time-consuming to do.

The next approach was to do it graphically. We could draw up to a large scale the accelerogram of the earthquake and perform multiplications and integrations. When we did that, each point of the spectrum took a day to calculate. After the graphic approach, our next step was to develop an analog, using the vibrations of a torsion pendulum excited by twisting the support. The equations for that motion are the same as the equations of a single-mass oscillator whose base is shaken by the ground acceleration. So you could do it with a torsion pendulum and then convert the results. Accordingly, we made a torsion pendulum, using a wood-turning lathe to input the ground motion.

Scott: Could you say a word or two about how your pendulum differed from the torsion pendulum Biot used and described in his articles?

Housner: I never saw Biot's torsion pendulum, but it was small and not suited to analyzing numerous accelerograms. Our pendulum was larger and was designed to be suitable for processing records and computing spectra. The lathe came with a long screw which actuates the blade that carves the wood. We removed that and substituted a table, so that the lathe would move the table along at a slow, uniform speed of about four inches per minute.

The accelerogram was mounted on the table, and as the table moved we manually followed the accelerogram with a point on the end of an arm that actuated the top of the pendulum support, and the twist of the pendulum support was proportional to the amplitude of the accel-

eration. Twisting the top of the pendulum support transferred the lateral acceleration into a torsional displacement.

Scott: That was an ingenious setup, but I imagine the manual operation took a lot of care and attention. Also I note from the drawing that you used a mirror to reflect a light beam onto a graduated scale. I presume that the torsional displacement was recorded by taking visual readings of the motion of the light on the scale. That would also have taken some close attention.

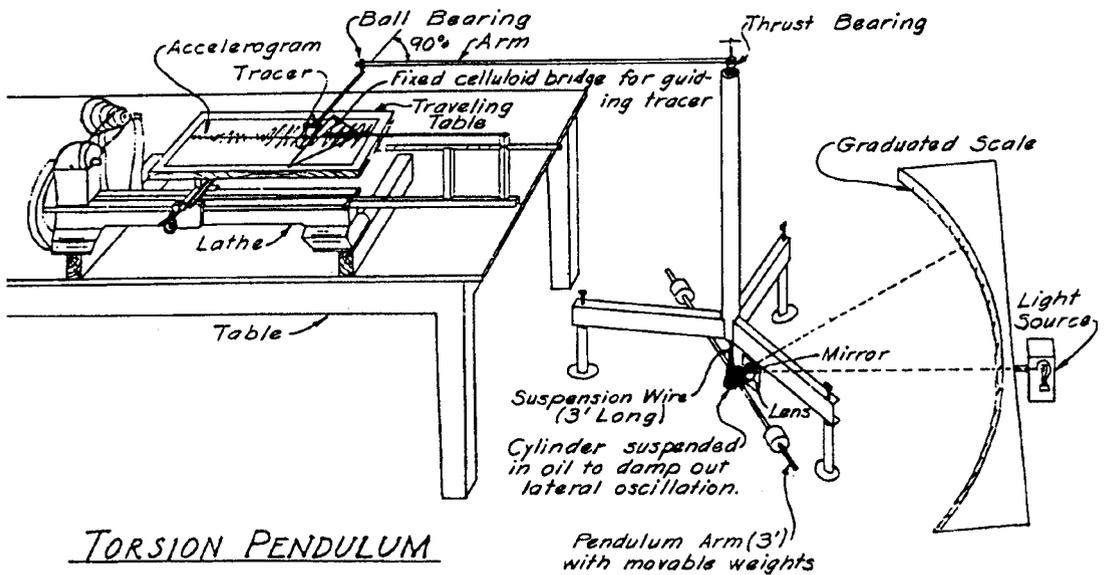
Housner: Yes, the process did require careful attention, but we thought it was a big advance, because it was about 30 times quicker than doing it graphically. Thus, we could compute a point on the spectrum in 15 minutes, allowing time for the operator to rest, instead of taking up a whole day. I will provide a copy of the drawing of the torsion-pendulum setup that was included in my Ph.D. thesis.

Scott: How did you compute each successive point on the spectrum?

Housner: We moved the weights along the pendulum arm to change the period of vibration, and then repeated the process.

At that point, World War II broke out, putting an end to earthquake research considerations for a while. So when my Ph.D. thesis was complete, that was the end of the torsion pendulum work. The pendulum and the table were still here at the end of the war, but were never used again.

Scott: How did that pre-war work relate to the spectrum work you started shortly after the war?



This is a drawing from Housner's Ph.D. thesis showing the setup of the torsion pendulum used in 1940 to calculate the response spectrum of El Centro (1940), Helena (1935), and Long Beach (1933). Around 1950 Housner and his colleagues began computing spectra with an electric analog computer, which was about 60 times faster than the torsion pendulum analog. Now the digital computer is about 60 times faster than the electric analog.

Housner: The spectrum work I started after the war was a completely new effort, and not related to the torsion pendulum.

Postwar: Work on the Dynamic Response of Buildings

Housner: When we came back after the war and started looking at the problem again, we got some funding from the Office of Naval Research. That office had been set up during the war—and it still continues today. It was really the forerunner of the National Science Foundation. We made more extensive calculations, taking into account the damping and periods, and calculated the response spectra for all the earthquakes for which we had

strong motion records, using an electric analog computer.

The first effort used an electric analog computer that Professor Gilbert McCann had developed in the electrical engineering department [at Caltech]. In the 1950s and 1960s, our principal efforts focused on analyzing earthquake records, calculating the spectra and building response. Before the digital computer came on the scene, we used a special electric analog computer that we developed to do the calculations. It was more efficient than McCann's computer, and took about 15 seconds to determine one point. These post-war spectra were the first to include various damping values.

Another facet of the research was on the dynamic response of buildings to earthquake motions. This was partly theoretical and mathematical, studying the mode shapes and vibratory forces involved when structures are excited. It was partly experimental, measuring building vibrations made with shaking machines, and also using the few records obtained in buildings in actual earthquakes.

Response Spectrum and Design Spectrum

Housner: From our early postwar results I could perceive a similarity in the general shapes of the response spectrum for each of the earthquakes we had records for. That made me think that you should not design a building based on the calculated response spectrum, because that is for a particular earthquake which has already happened and it is not going to happen again. Instead, we should look at the average shape of the response spectra, and base our design on the smooth average curve, which we call a design spectrum. I emphasize that it is a *design spectrum*.

We must distinguish between the design spectrum and the response spectrum. The response spectrum is calculated from a particular earthquake record—a recorded accelerogram. It describes the property of a particular earthquake record. The calculated response spectrum tells you something about the ground motion in a form that is significant for engineering. From it, you can see what the building vibrations will be like for different periods.

A design spectrum is a way of telling the engineer how strong to make his building. It does

not describe a particular earthquake record, but instead is a curve that engineers use to design their buildings, and achieve a uniform factor of safety in the different parts. The design spectrum is also commonly used for special projects, such as a nuclear power plant, a big dam, taller buildings, and the BART system.

Scott: Would you discuss these terms a little more, and perhaps give some background. Apparently, there is a considerable amount of uncertainty about precise meanings of the terms and proper use of the concepts.

Housner: Yes, spectrum curves have been a source of confusion, which started right at the beginning, when Biot published his ASCE paper on earthquake engineering.⁵ Biot said the calculated spectrum could be used to design buildings to resist earthquakes by simply reading off the appropriate value of the response spectrum for that period of vibration. But that was incorrect, because the spectrum of future ground motion will not duplicate the spectrum of the past ground motion.

This is how I see it. The calculated spectrum curves of a recorded ground acceleration characterize the ground motion in a way that is very significant to engineers. I do not know who first called it a "response spectrum," but unfortunately the term leads people to think that the spectrum characterizes the building's motion, rather than the ground's motion. Nevertheless "response spectrum" has become standard terminology.

5. Biot, M.A., "Analytical and Experimental Methods in Engineering Seismology," *Transactions*, American Society of Civil Engineers, Vol. 108, 1943.

The "design spectrum" is different from the "response spectrum"—it is a way of specifying the design values to be used by an engineer. Many people, however, incorrectly call the "design spectrum" a "response spectrum." Sometimes the "design spectrum" is called "design response spectrum," but that is also incorrect. Actually, the term "design response spectrum" should be reserved for the spectrum calculated from the simulated ground acceleration that has in turn been calculated from the "design spectrum." I realize this all sounds very complicated, but I believe the earthquake engineers among the readers will understand what I have been saying here.

Tripartite Log Paper

Scott: While on this subject, would you comment on the special logarithmic graph paper now used to plot spectra? When I first saw it years ago, it looked pretty complicated, but it is now widely used in earthquake engineering to present a lot of data compactly on one graph.

Housner: Yes, I would like to mention the so-called "tripartite" log graph paper. It can best be explained by reference to some actual examples of spectrum curves plotted on this type of graph paper. The period (or, if desired, the frequency) is plotted along the horizontal axis. The peak velocity of the oscillator relative to the base is plotted along the vertical axis. Both are log scales. In one 45-degree direction there is another log scale that reads acceleration, and in the other 45-degree direction there is a log scale that reads displacement. Thus, for any specified period, a point on the spectrum

curve describes the peak velocity, acceleration and displacement of a sinusoidal vibration.

The paper was invented by Edward Fisher, who worked at the Westinghouse Research Laboratory in Pittsburgh, and specialized in vibration problems. Although I believe Ed Fisher never got the recognition he deserved, the paper he invented has since proved very useful in many fields. Thus, in earthquake engineering this paper became popular for drawing spectrum curves.

I first met Ed in the 1960s, when I was a consultant on the seismic design of the Southern California Edison nuclear power plant at San Onofre which was a Westinghouse-type plant. Afterwards I used to see Ed at various meetings, but have not encountered him in the last few years.

I first saw Fisher's tripartite graph paper in 1958, when Charles Creed, a Caltech professor of mechanical engineering, gave me a sheet, which he called "Fisher paper." The nature of the log-log-log paper is such that when the information is properly plotted you can read the peak velocity, peak acceleration and peak displacement of a sinusoidal vibration of any specified period.

When used in earthquake engineering, it is customary first to compute the displacement produced by the ground motion. When this is done for a sequence of periods, it defines the spectrum curve for a specified damping. The resulting curve gives a neat description of the impact of the ground acceleration on vibrating structures. Next, the design spectrum for an engineering project is usually drawn on this

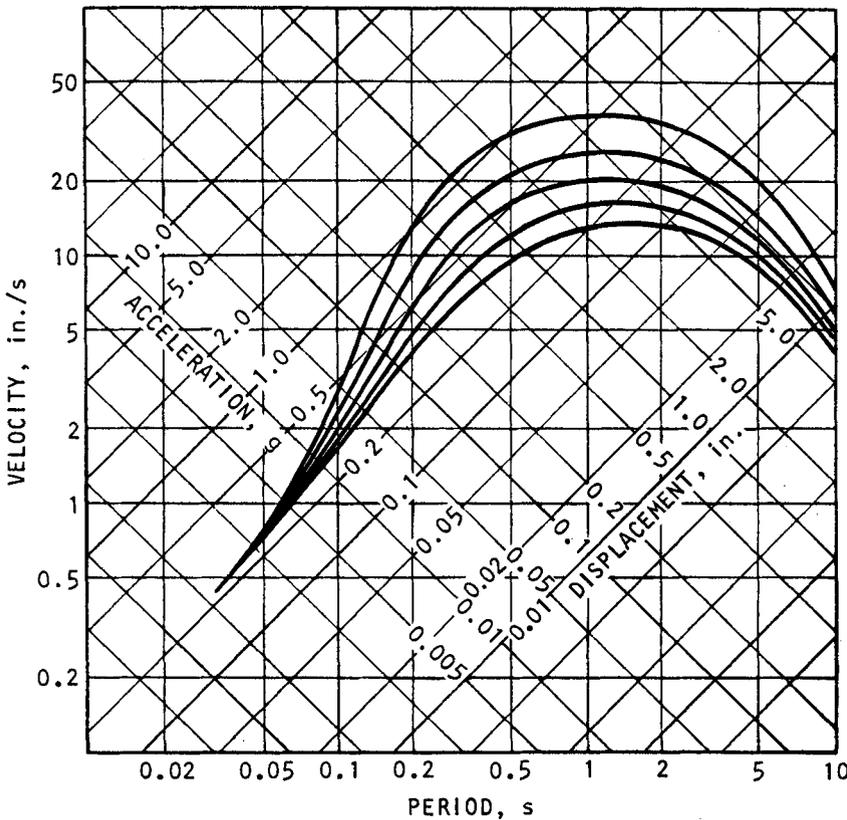
paper as a smooth curve, or as a segmented series of straight lines.

Furthermore, from the design spectrum it is possible to compute an artificial ground acceleration whose calculated spectrum is checked against the design spectrum to verify that it does indeed represent agreement with the design spectrum. This calculated curve could be called the design response spectrum. The artificial ground acceleration is used to calcu-

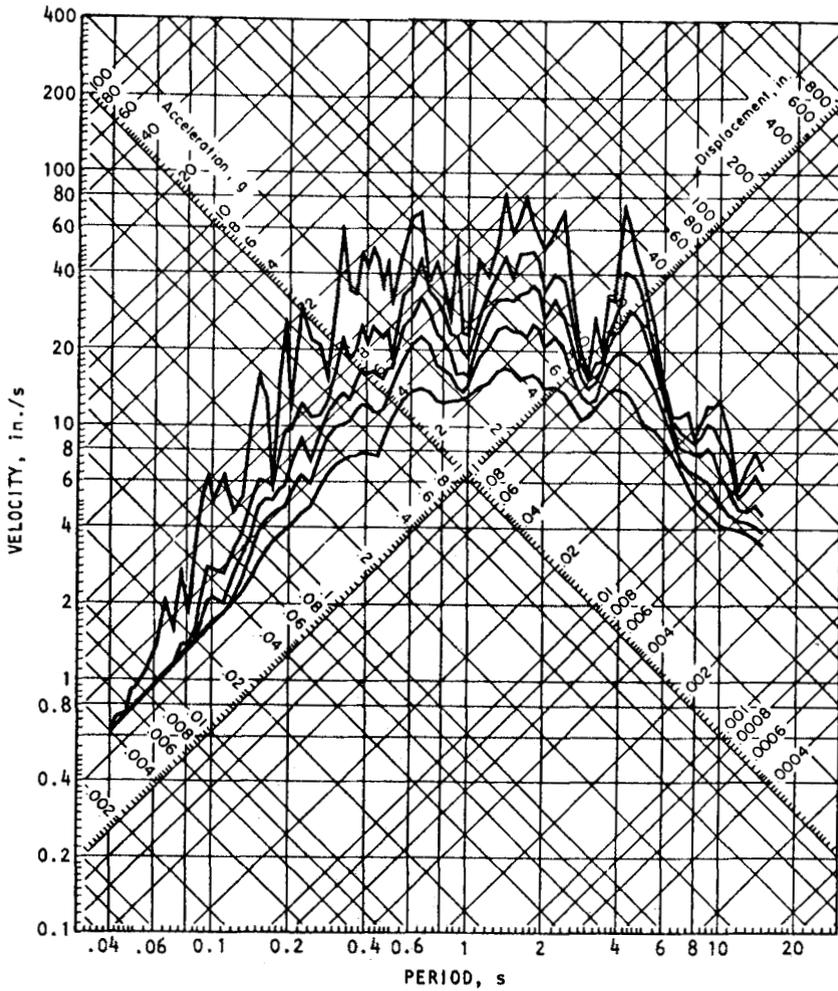
late the response of more complicated structures—that is, multi-mode structures.

Scott: It would probably be helpful to include here some examples of figures drawn on tripartite graph paper.

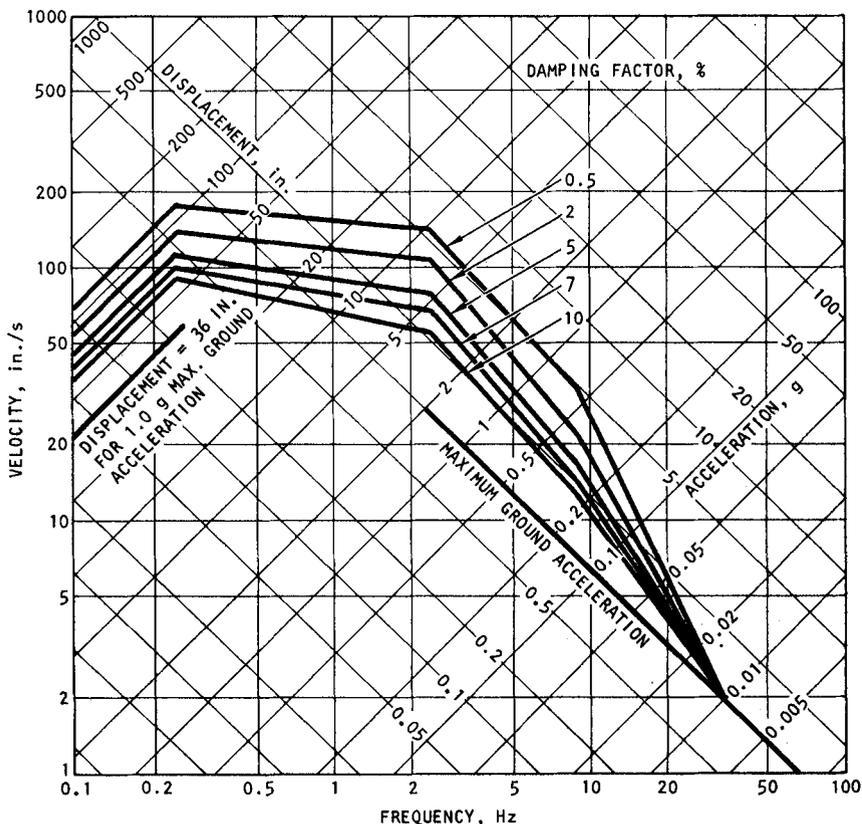
Housner: We can include three from the Housner-Jennings monograph, in Figure 23, p. 59; Figure 24, p. 60; and Figure 30, p. 69.



Example of smooth design spectrum based on Holiday Inn response spectrum, San Fernando earthquake of February 9, 1971 (0, .02, .05, .10, .20 of critical damping). Arriving at the right shape and amplitude for the smooth design spectrum requires good engineering judgment. [Reprinted from Housner-Jennings monograph Earthquake Design Criteria for Structures, EERI, 1982].



Response spectrum of north-south ground acceleration recorded at Holiday Inn, approximately five miles from the causative fault in the San Fernando earthquake of February 9, 1971 (0, .02, .05, .10, .20 of critical damping). [Reprinted from Housner-Jennings monograph Earthquake Design Criteria for Structures, EERI, 1982].



Design spectrum curves used for the design of nuclear power plants. Plotting design spectrum curves with straight line segments was first done by N.M. Newmark in the late 1960s. The design spectrum reproduced here (horizontal component) is from NRC Regulatory Guide 1.60 and was developed by Newmark, Blume, and Kapur in the early 1970s. [Reprinted from Housner-Jennings monograph Earthquake Design Criteria for Structures, EERI, 1982].

Design Based on Dynamic Properties: Adoption in the Los Angeles Code

Housner: The seismic code generally used before WWII was based on a seismic code developed in Italy after the 1908 Messina earthquake. Buildings in Messina were limited to two stories, and their design approach essentially reduced a problem of dynamics by using a simplified approach that assumed static lateral

forces, proportional to the weight, and applied to the side of the building. This method was adopted in southern California following the 1933 Long Beach earthquake. While this approach was not very realistic, in California there was very little interest in improving the situation until the mid- to late 1940s.

Then around 1943, a new design formula was developed for the Los Angeles code and adopted in 1944. The formula specified how

the forces were to attenuate with the period of the building and vary over the height. This was based in part on research that was done at Caltech, and was a first step beyond the constant 10 percent g lateral force requirement.⁵

With the calculated spectra and the theoretical analysis, you could see how design ought to be done, what the distribution of forces ought to be, and how the force ought to depend on the height of the building. What was put into the Los Angeles building code in 1944 was based on that work.

This work was funded by the Los Angeles County Building Department. The head of the department was Col. William Fox, whom I did not know personally, as I was just a student at the time. Apparently, he felt something more ought to be learned about earthquakes, and he provided funding for it. So that went into the Los Angeles city code in 1944, and I suppose also into the county code. As I say, that was the first time anywhere in the world that the code was based on the dynamic properties of structures, but it specified seismic forces that were too low, and it did not recognize that inelastic deformations would occur.

At that time San Francisco had no seismic provisions in its building code. The first San Francisco seismic code—the "Harry Vensano" code—was adopted in 1948. Then in response to controversy caused by the Vensano code, a "Separate 66" effort by a northern California joint committee was developed for the San Francisco building code. I will talk about this

later when discussing the development of building codes.

Slow Use of the Design Spectra

Housner: Up until I guess the late 1950s, however, you could not get people concerned about the design spectrum. Practicing engineers were not too interested.

Scott: Did the practicing engineers at first not realize the significance of the design spectrum?

Housner: They did not. It was different from the code, and I think they just did not want to modify the code significantly. Probably, they did not realize that they were already using a crude design spectrum. In effect a crude design spectrum had already been written into the code, which specified the forces as a function of the period of vibration of the building. You would have to say that really is a design spectrum. But it was not called a design spectrum. Actually, its shape is similar to a 15 percent damping spectrum curve, which implies large energy loss.

Scott: This crude design spectrum had some of the characteristics of the more sophisticated version?

Housner: Yes, but did not take damping into account explicitly, and it greatly underestimated the true values of the earthquake forces. Because of the practicing engineers' reluctance to employ the design spectrum, I think it was essentially the nuclear power business that got the spectrum into widespread use. Starting in the 1950s, the federal regulatory agency required that the design spectrum be used in

5. "g" refers to the acceleration of an object at the surface of the earth caused by the pull of gravity.

nuclear power plant design. It was also used in other special projects.

Scott: That requirement was applied much more broadly than just in the more highly seismic areas. It was required in some fashion in the East and other parts of the country, I believe.

Housner: Yes, it was, and again, that was because of the nuclear power industry. So the nuclear power industry really forced engineers all over the country to be aware of the spectrum and seismic design.

Terminological Ambiguities: Peak Acceleration and Magnitude

Scott: Bruce Bolt asked that you comment on the steady increase in maximum peak accelerations recorded in earthquakes. What has been the effect on engineering evaluations?

Peak Ground Acceleration

Housner: I have observed that a good idea can sometimes be a source of confusion and misunderstanding, and perhaps be misused in practice. One example is "peak ground acceleration" and another is "earthquake magnitude." Steady increases in the recorded maximum peak ground acceleration have been noted over the years. For example, for many years the strongest shaking recorded was that in the 1940 El Centro earthquake, which registered a peak acceleration of about 0.33 g. Of course, there were very few instruments then, and we were only getting a few samples of motion. But later as more and more instruments were put out, we began recording higher accelerations.

Scott: I presume the steady increase in the recorded maximum peak acceleration is due to our getting more information from more and better instruments and from more sites close to the faults. So in terms of simple statistics, you might expect such increases as the initially quite limited sample of earthquake records gets larger.

Housner: Yes, but the steady increase in the recorded maximum acceleration puzzled the older engineers considerably. At first they were talking in terms of a lateral force of 10 percent g. Then along came the El Centro earthquake with 33 percent g. They were confusing the true acceleration with the forces that they were using in the code. I think that in the beginning these engineers were much surprised when we measured the earthquake motions in buildings and recorded accelerations of 1 g or more at the roof.

Of course, in those days the code just said, in effect, "Put so much strength in the building." But it did not say anything about the building's resistance to damage. I notice the younger people coming along understand and are not confused—by younger I mean people now in their forties. Essentially, it takes a whole generation to change attitudes. I guess it is another example of, "You can't teach an old dog new tricks." I now have experimental verification of that.

Important: The Area Under the Curve

Housner: Anyway, the steady rise in recorded peak accelerations was a source of great confusion, because peak acceleration had come to be used as a convenient measure of the severity of shaking. Actually, the significant thing is not the height of the peak in itself, but

the *area under the curve*, which represents the pulse. The accelerogram is made up of these pulses. It is the area underneath the curve, the area representing the pulse, that is significant.

We find that the higher peak accelerations tend to be associated with narrower pulses. For example, the pulse recorded at Pacoima Dam in the 1971 quake had a peak acceleration of 1.25 g. But it was a narrow pulse, and the area under the curve was about the same as the area under the 0.33 g pulse on the El Centro record. High accelerations have been recorded for relatively small earthquakes, but, again, the area under the pulse is always quite small because the pulse lasts only a very short time. The effect of earthquake shaking on a building depends on the frequency characteristics of the ground motion and of the building, acting together, and on the area under the pulses, and the duration. If the shaking continues longer, it can do more damage.

Scott: Longer duration also means more pulses.

Housner: Yes. But because a single peak pulse is of short duration, the area under the curve is relatively small even when the peak is high—it is a narrow spike. The peak represents a quick shove that the building really does not have time to "feel" and respond to significantly. Emphasizing the importance of the area under the curve helps clarify this.

So in a sense, from the design point of view, the peak acceleration is not really the significant thing. It would be significant if all accelerograms were the same, and you were just scaling up and down. Then if you doubled the peak, you would double the force. But that is not

the way it works. The damaging capability is indicated by the area under the pulse, not just the height of the peak, and by the duration of the shaking. Although some of us have been preaching this, I think it is still not well understood.

Scott: Some engineers who express concern over the higher recorded peaks say, "We really cannot design for those high readings, either practically or economically."

Housner: That is due to a misunderstanding. They are confusing peak ground acceleration with percent g force prescribed by the code.

Scott: So they should not even be trying to design a building to resist these very high accelerations, nor should they apologize for not designing for such peaks?

Housner: No. The design should be for the resulting motion of the building—and this can be done, especially by permitting ductile deformations. We should also note, however, that when ground motions were recorded just above the causative fault in the Northridge earthquake, there was a big velocity pulse in the motion. The possibility of such a pulse must be considered in future seismic design. Some very large pulses have been recorded close to the fault, and this is of concern to engineers.

Earthquake Magnitude

Housner: In 1935, Caltech seismologist Charles Richter published a paper in which he defined the magnitude of an earthquake on the basis of the amplitude of the ground motion as recorded 100 kilometers from the earthquake. He did this as a means to the classification of earthquakes according to their size.

The earthquake measurement scale thus invented by Richter proved extremely valuable to engineers.

Without such a magnitude scale, engineers had no way of describing an earthquake, other than to say something like, "It was big" or "It was very big...." and so forth. An accepted magnitude scale is valuable to engineers because it gives them a consistent measure of earthquake size. Unfortunately, however, almost from the beginning of the Richter scale's use, other magnitude scales were also proposed and used, such as MS, mb, MW, Mm, and MJ (this last is the scale used in Japan). I believe there are also still other magnitude scales. With this proliferation of scales, we really do not have a consistent measure that is universally accepted.

Scott: That was brought to our attention many times when I was on the Seismic Safety Commission, and someone would be reviewing data about a new earthquake, using some of those different scales. Every so often Bruce Bolt, our seismologist commissioner, would explain more or less in lay terms how one scale is especially useful for one specified purpose, while another is good for another purpose, and so forth. Whereupon some of our non-seismologist Commissioners would usually complain: "That may be fine for the specialists, but in general discussions of earthquakes we ought to stick with the measure that most people have learned to relate to. In the U.S. that is the Richter scale." But Clarence Allen tells me that the scale now used to report "Richter magnitudes" is not really the *original* Richter scale.

Housner: Clarence is right in saying that the original definition of the magnitude scale has been modified to make it applicable to distant large earthquakes. With regard to the complaints of the non-seismologist Commissioners, I can understand why they felt as they did about the multiplicity of scales. Moreover, there are still other problems with the calculated magnitude data we typically see after each new earthquake. If we look at a published table of earthquake magnitudes, I do not think that any engineer—and that includes me—can say how those numbers were derived. After each significant new earthquake, almost every major seismological laboratory announces a magnitude number, and these usually differ from one lab to another. The differences are not large, but they are confusing. An example is the 1995 Kobe earthquake, which in the U.S. was said to be magnitude 6.9, whereas in Japan its magnitude was 7.2.

The rest of us do not understand the reasons for the differences—whether they are due to differences in the ground shaking, or in the location of the laboratories, or in the methods of calculating magnitudes. After each earthquake a single "official" Richter magnitude is later announced by the USGS seismological group in Golden, Colorado, but I do not know precisely how they arrive at their number. I hope they are consistent, but am not at all sure that is the case. Clarence Allen says, however, that the seismologists are now getting their act together, and that in the future there will be less confusion about magnitudes. The engineers will be grateful for this.

Early Leaders in Earthquake Engineering

“They were ... trying to promote earthquake engineering and a better understanding of the need for earthquake research and seismic awareness.”

Early Leaders: Martel, Freeman, Jacobsen and Dewell

Housner: I mentioned earlier that I got interested in earthquakes through Professor R. R. Martel at Caltech. The Long Beach earthquake of 1933 occurred before I came here, and Martel was very actively involved in earthquake concerns. Before the Long Beach earthquake, there had been little earthquake interest among the structural engineers, although a few people wanted to try to do something. Among the notable exceptions were Martel at Caltech, and his remarkable friend, John R. Freeman. Another exceptional engineer was Henry Dewell, a practicing engineer in San Francisco. Those three were the people in California who seemed to be most active in the 1920s and early 1930s.

I base this judgment mostly on correspondence exchanged between Martel, Dewell, and Freeman at the time when Freeman arranged for Professor Kyoji Suyehiro, director of Tokyo University's Earthquake Research Institute, to come from Japan to the U.S. in 1932 to give a series of earthquake lec-

tures. In addition to the three just mentioned, another important early-day leader whom I would like to discuss is Lydik Jacobsen at Stanford, as well as his student, John Blume.

Freeman's Remarkable Impact

Housner: I talked earlier about Martel and his role as something of a pioneer in earthquake studies and earthquake engineering. Now I would like to discuss Martel's rewarding friendship with John R. Freeman, an eminent civil engineer in the eastern U.S., with whom Martel had a close working relationship in the 1920s and early 1930s. They were important early-day leaders in trying to promote earthquake engineering and a better understanding of the need for earthquake research and seismic awareness. Freeman played a particularly active and effective role.

I don't know how the friendship between Martel and Freeman started, although I suppose it began at the time when they were planning the Colorado River aqueduct. That planning effort probably started in 1923 or 1924. Freeman and Martel were both consultants on that project. I think that's how they first got together, and they hit it off very well.

Freeman was really a very remarkable man whom I single out here both because of his relationship with Martel, and because of his major role in this story of the development of seismic design and earthquake engineering. He graduated from MIT in civil engineering, got a job with a consulting hydraulic engineer, and worked about 10 years in the consulting business, and also doing research as part of his job. Then he switched careers and went with the Manufacturer's Mutual Insurance

Company. Probably a business depression had come along and work for consulting engineers had dwindled.

A Late-Blooming Interest in Earthquakes

Housner: Freeman was a very energetic and bright man, and in a few years he was president of the Manufacturer's Mutual Insurance Company. He did research, looked at the company's operation—mostly fire insurance—and saw that virtually nothing was known about the hydraulics of fire hoses and fire nozzles. So he launched a research project on the subject. Consequently, in mechanical engineering he became widely known for this pioneering research on hoses and nozzles.

While he was president of the insurance company its business expanded 40-fold. Only about half of Freeman's time was occupied by the insurance business, however, and in the other half he was an engineering consultant. He consulted on the Panama Canal, the New York City water system, San Francisco's Hetch-Hetchy Water Project, the Owens Valley Water Project for Los Angeles, the Colorado River Water Project for the Metropolitan Water District in Southern California, and other such projects.

Freeman was obviously a very unusual person in many ways. For example, as far as I know, he was the only man who served as president of both the American Society of Civil Engineers and the American Society of Mechanical Engineers. Another very unusual accomplishment was his late-blooming but extremely vigorous and productive activities on earthquakes. Freeman was already 70 years old when he got interested in earthquakes. That was after the

1925 Santa Barbara earthquake, and also another earthquake that same year near Quebec, Canada. His company headquarters were in Providence, Rhode Island, and apparently they felt the shaking from the Quebec earthquake. Anyway, at the age of 70 Freeman started looking into earthquake engineering. I know all this through his correspondence with Martel, whose files I inherited on his retirement. In those files I found some very interesting letters from Freeman and saved them.

Freeman, a very energetic man, would send off letters whenever the whim took him. Presumably, he dictated the letters, and his correspondence ran to about four letters for every one from Martel. At one time Martel, then in his mid-forties, said he had gotten a long letter from Freeman—then 73 years old—saying that he was in Japan, and recounting all the things he had been doing there. Martel replied, "I got your letter, and it seemed to be so full of youthful enthusiasm it made me feel quite old."

Freeman's Significant Role

Housner: It was very interesting to see from the correspondence what Freeman was doing. For example, he said he had looked at engineering books, and found that not a single book told how to design against earthquakes. "That's a really sad commentary, you've got to do something," he said to Martel. Then Freeman and Martel both went to some engineering conference in Japan, and saw the remains of the Tokyo earthquake of 1923. While there they met Professor Kyoji Suyehiro, the first director of the Earthquake Research Institute at Tokyo University. Freeman was very favorably impressed with Suyehiro, and wanted him

to come to the United States to give lectures on earthquake engineering. Moreover Freeman brought this about by having the American Society of Civil Engineers invite Suyehiro, although Freeman himself put up all the money needed. Freeman also put up all the money needed for the society to publish the Suyehiro lectures afterwards.

Freeman saw that it did not make sense to talk about earthquake-resistant design without first knowing more about the forces involved in earthquakes. This kind of information was unavailable, because no instruments existed that could capture good records of destructive shaking. He wrote a paper on instrument needs that appeared in the *Bulletin of the Seismological Society of America* in 1930. He decided something ought to be done about that, and set about getting it done. Freeman was not easily discouraged when he set his mind to something. In the letters he wrote to Martel, he explained, "Well, I talked to so-and-so," the head of the seismological end of the Coast and Geodetic Survey, "but he was not a good listener."

Then he went and talked to somebody else, and his letter said, "I went and talked to the Secretary of Commerce." It turned out that he was a graduate civil engineer, "and a good listener." So Freeman talked to him and convinced him that earthquake instrumentation ought to be pushed. In fact, one letter also said he was at some function where President Herbert Hoover showed up. He took advantage of that opportunity to tell Hoover how important earthquake research was.

Scott: Herbert Hoover also had an engineering background, being among other things a distinguished mining engineer, and with roots at Stanford University.

Housner: Yes, in the early 1900s Hoover was a mining consultant living in the city of Tangshan, China, and when I visited there I was shown the site of the building in which he lived. The building itself had been destroyed by the disastrous 1976 earthquake. Anyway the Coast and Geodetic Survey put money in the budget for the Seismological Field Survey to be established here on the West coast, with offices in San Francisco. For many years Franklin Ulrich was the head of that program. This was the first time instruments were put out to record destructive shaking. They made their initial installation in 1932, and then in March, 1933 came the Long Beach earthquake and they recorded the motion in Long Beach, Los Angeles, and Pasadena. It was an eye-opener. That was the first time anyone knew what strong earthquake motion really looked like. Unfortunately, Freeman died shortly before the Long Beach earthquake, so he did not live to see the results.

In his 1932 book Freeman describes the properties that an accelerograph should have: the natural frequency, the paper speed, the amplification, etc. This instrument turned out to look just like the Wood-Anderson seismograph. So I presume that he spoke to Caltech people, probably Harry Wood, about the accelerograph, and that the design was in fact based on the Wood-Anderson instrument.

Freeman's book came out just before the Long Beach earthquake. Publishing that book is indicative of how he worked. He said, "There

ought to be a book, because people don't understand about earthquakes and insurance, so I'll write it." Incidentally, have you noticed the odd-looking typography, using a lot of boldface type. Apparently, he was told he should not do that, but he said he was paying for the book and he wanted it that way, so it was done that way. Things he wanted emphasized were set in boldface. It is the typographic equivalent of pounding the table, which I suspect he may have done with some of the people who were not good listeners.¹⁰

Freeman played a very significant role in earthquake engineering—all done after he was 70 years old. I think he died at the age of 77. But when I got hold of his obituary, prepared by the insurance company, mentioning all the things he'd done, I was surprised to find that it never mentioned earthquakes at all. This despite the fact that he had built up an excellent earthquake library at the company.

But Freeman's accomplishments in earthquake engineering show what an energetic individual can do. Without him we'd have been a long time in getting strong motion instruments. You can see that it took somebody who could talk to the President of the United States and the Secretary of Commerce. In general, people are not

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10. Freeman, John R., *Earthquake Damage and Earthquake Insurance, Studies of A Rational Basis for Earthquake Insurance; Also Studies of Engineering Data for Earthquake-Resisting Construction*, McGraw-Hill, 1932. This 900-page compendium, unique for its time, compiled observations, earthquake data and interpretations, and ideas on earthquake-resistant design. The author presented a comprehensive, forward-looking earthquake research program, especially urging deployment of many strong motion instruments in areas of expected future earthquakes to record the kinds of shocks structures receive.

aware that he was responsible for getting the strong motion program going. I never met Freeman myself. I did not get interested in earthquakes until I came out here as a student, and by then he had already passed away.

Lydik Jacobsen at Stanford

Housner: Lydik Jacobsen was another early-day leader whom I would like to discuss.

Lydik's family lived in Palo Alto, so he naturally enrolled as a student at Stanford. He once said that if his family had lived in Berkeley, he would have enrolled in "that other place." Jacobsen later became a professor of mechanical engineering at Stanford University. He must have joined the staff there sometime in the 1920s, as he once mentioned that he was at Stanford at the time of the 1925 Santa Barbara earthquake. Jacobsen was a very well-known person, and influential in his time.

While his basic interests were not particularly in structures, but rather in mechanical engineering and applied mechanics, Jacobsen was quite active in certain aspects of earthquake engineering research, and was very helpful in talking with the structural engineers in the Bay Area. Jacobsen was also involved in the promotion of strong motion studies and the formation of EERI, both of which I will discuss a little later.

Jacobsen had John Blume as his student at Stanford, and Bruce Bolt asked for my evaluation of John Blume's role in the structural engineering and earthquake design discipline, and also about his contribution to EERI. This seems a good place to respond to Bruce, as I can do it in connection with this discussion of John Blume's mentor, Lydik Jacobsen.

Shaking Table and Highrise Model

Housner: In the late 1930s and early 1940s, Professor Martel had a grant from the Los Angeles County Building Department, and funded some of the work Professor Jacobsen was doing. Jacobsen made a small shaking table that would shake back and forth, like an earthquake. He also had a student named Nicholas Hoff working with him on earthquake research. Hoff later became a well-known aeronautical engineer, and I think he was a professor at Brooklyn Poly, and then at Stanford.

In the 1930s Jacobsen also made a rather elaborate dynamic model of a 15-story building, which was subjected to decaying sinusoidal base motion. The model represented the floors of the building with masses, and had springs between the floors. That got a lot of attention from engineers, because they could see the model building vibrate. It was written up in a paper published in the *Bulletin of the Seismological Society of America*, vol 28, no. 4, October 1938, and written by Lydik Jacobsen and Robert Ayre.¹¹ Ayre was a Ph.D. student of Jacobsen's in the 1930s, who later became a professor of civil engineering at the University of Colorado. Because he was co-author, I always presumed that Bob Ayre was mainly responsible for building the model.

Scott: The Jacobsen-Ayre article notes that the prototype for the model approximated the Alexander Building, and that the masses, rigidities and dimensions were calculated by John

11. Jacobsen, Lydik S. and Robert S. Ayre, "Experimentally Determined Dynamic Shears in a Sixteen-Story Model," *Bulletin of the Seismological Society of America*, Vol. 28, no. 4, October 1938.

Blume and Harry Hesselmeier in their Stanford masters thesis. Also, John Blume's oral history briefly discusses the model and his thesis work with Harry Hesselmeier, including a couple of photos of the model, and mentions working on it in 1934.¹²

Housner: After the Jacobsen-Ayre article appeared, Merit White, a Ph.D. student of Martel at Caltech, was lead author of a follow-up article in the *Bulletin of the Seismological Society of America*.¹³ The article showed that a theoretical analysis could be made and used to verify the accuracy of the modeling and experimentation. The experiment was found to have been done very accurately, except for some inaccurate displacements near the top of the building. In this case the model could be described in convenient mathematical terms that could be solved analytically. Now, of course, researchers do not resort to physical models, for the computer enables the response to be calculated for any conceivable structure.

I heard an interesting story about that model from Reuben Binder, although I cannot guarantee its truth. When you make a model like that, say of a 10-story or 20-story building, it is most convenient to make the natural period of vibration of the model the same as the actual building. When this is done, then the displacement of the floors on the model are the same as

the displacement of the floors of the real building. That is what was done in this case. Some of the northern California engineers said that when the model was shown to prospective builders and they saw it vibrating, in their minds they pictured the actual building with exaggerated motion, and it scared them out of going ahead.

I can see how that could happen, because something similar happened when the engineers were making the seismic analysis of the twin Arco Tower buildings in Los Angeles. They had come to us, and we gave them the kind of ground motions to use in their calculations. They calculated the response and then made a movie of it. The computer calculates the motion of the building and at successive instants of time the motion is photographed. When this is played back on a movie projector, you see the building vibrating.

The real building, which is 52 stories high—520 feet—might move 5 feet in double amplitude at its roof (approximately 1/100 of the height). When shown on the screen, of course, the building's image is only 3 feet tall, and a displacement of 1/100th of 3 feet (about 0.36 of an inch) is barely perceptible to the viewer. So they made a new movie in which they multiplied the displacements by 50. This exaggerated displacement would correspond to a double amplitude of 250 feet at the top of the real building, and as shown in the new movie gave a very striking picture of the deformations of the building during an earthquake. But it was so frightening to laymen that they never showed it to any prospective client.

12. Scott, Stanley, Interviewer, *John A. Blume, Connections: The EERI Oral History Series*, Earthquake Engineering Research Institute, 1994.
13. White, Merit, and Ralph Byrne, "Model Studies of the Vibrations of Structures During Earthquakes, Based on Ground Accelerations Instead of on Ground Displacements," *Bulletin of the Seismological Society of America*. Vol. 29, no. 2, April, 1939.

Damping Theory, and Water Tank Research

Housner: Jacobsen was the inventor of "equivalent linear damping." In real structures the damping comes from a variety of sources of energy dissipation, and is not a simple matter. Jacobsen showed that an equivalent linear damping could be determined that would dissipate the same amount of energy per cycle as the real damping, and this would permit analysis of vibrations to be made in a simple manner. Jacobsen also analyzed fluid pressure forces in a ground-based water tank—that is, the impulsive pressures produced by ground shaking. He also did other earthquake-related projects. You could say that he worked at the boundary between mechanics and civil engineering.

Jacobsen in Retirement

Housner: Professor Jacobsen retired from Stanford when he was 65, which I think was in the 1960s. Around that time, he and Mihran (Mike) Agbabian formed a consulting firm. After he retired, Jacobsen worked with the firm for a while and then decided he had enough, and sold out. The firm then became Agbabian and Associates, and in recent years it was sold to OYO Corporation, the same company that bought Kinematics a few years ago.

After he retired, Jacobsen told me about his hobby of collecting antique pistols. Over the years he had assembled quite a number of them. He said that when he retired, he had gotten in touch with Sotheby's Auction House. They said yes, they would like to auction them off, and took them to England. He got enough money out of it to buy a large power boat. Not a yacht, but a boat. He said he made a large

amount of money out of it. That's a case where the hobby paid off.

John Blume: Led Engineers in Earthquake Studies

Scott: Jacobsen had a big influence on his student John Blume, who from his early days in practice played quite an active role in seismic design efforts, and in promoting other practitioners' awareness of earthquake engineering. Would you comment on your observations of Blume's work?

Housner: I believe it was Blume's working on Jacobsen's research project that led to their close relationship. Along with Jacobsen, Blume also had a role in the formation of the Earthquake Engineering Research Institute (EERI), which was an outgrowth of an advisory committee set up to help with the strong motion program of the U.S. Coast and Geodetic Survey's Seismological Field Service. When EERI was organized in 1949, Blume was one of the founding members, and later served as EERI president in 1978-1980.

Since the 1933 Long Beach earthquake, practicing engineers in California have been much interested in the earthquake problem, and in the design of buildings to resist earthquakes. I believe that John Blume was the first practicing engineer to take a deeper interest in earthquake engineering that went beyond the code requirements. He played an intermediary role between researchers and practitioners, and I think he played an important role in educating other engineers. He was good at getting up and explaining things. In the early days, engineers did not know about earthquake sources or

earthquake vibrations, and he was good about giving them background information.

His influence was felt even more through a lot of engineers who came through his office and worked for him. Young men would go there and work for a number of years, and he would get them into earthquake engineering. I am thinking of such people as John Wiggins, Joe Nicoletti, and Peter Yanev. Quite a few earthquake engineers actually got into the game through working in Blume's office.

Blume also played an important role in consulting projects in which he made recommendations on the ground motion that should be used in designs, and also did seismic designs himself.

Scott: Are you thinking of nuclear plant designs?

Housner: Both nuclear power plants and highrise buildings. For example, he did earthquake engineering consultation on PG&E's Diablo Canyon nuclear power plant. I think his was also the first practicing engineer's office that developed a capability in calculating the response of buildings to earthquake motions, making use of a computer.

Blume also apparently decided that his education—which had been through the master's degree—was not enough, so he went back to Stanford and got a Ph.D. degree, rounding out his education in dynamics, use of the computer, calculating the response, and so on.

Scott: Yes. He did that when he was in his mid-50s, and managed his very active practice at the same time.

Housner: You have to give him credit for that. Not many people would do that. He also

gave some kind of endowment to Stanford to help found the John Blume earthquake research center.

Scott: What about Blume's other activities as a practicing engineer, or as a writer of papers. For example, he has been a contributor of papers to the world conferences.

Housner: Yes. I could not say what all the subjects were, but he authored a number of papers, and they all had a distinctive John Blume flavor.¹⁴

Other Leaders in Earthquake Engineering

Housner: While talking about leaders, I also want to mention the names of several other earthquake engineering types who played a role in developing the discipline. Ronald Scott of Caltech, Robert Whitman of MIT, and Harry Seed of UC Berkeley were the early workers in soil mechanics and earthquakes, a subject whose importance has continued to grow. Nathan Newmark, William Hall and Mete Sosen were at the University of Illinois, where they and their students played an active role. Ray Clough and Joseph Penzien were early workers in the field at UC Berkeley, and Ray has the distinction of being an originator of the finite element method of analysis. They also set up the Earthquake Engineering Research Center at UC Berkeley. Joe Penzien was the prime mover in building the large shaking table there. Glen Berg at the University of Michigan and

14. Many of Blume's writings are discussed and a selected bibliography included in *John A. Blume, Connections: The EERI Oral History Series*, 1994.

Donald Hudson at Caltech were also early participants in the program.

Among the somewhat younger types, I might mention Haresh Shah at Stanford, Robert Hanson at the University of Michigan, Andy Veletsos at Rice University, Sami Masri at the University of Southern California, and Doug Foutch at the University of Illinois. There are many others. When it comes to the still younger ages, the number of names gets too long to list, and anyone seeking additional names can be referred to the National Research Council reports on earthquake engineering research.

In foreign countries there were Giuseppe Grandori, Italy; Nicholas Ambrasays, UK; Emilio Rosenblueth and Luis Esteva, Mexico; Rodrigo Flores, Chile; Julio Kuroiwa, Peru;

Juan Carmona, Argentina; Jose Grases, Venezuela; Kiyoshi Muto, Kiyoshi Kanai, and Shunzo Okamoto, Japan; Hui-Xian Liu, China; Jai Krishna, India; and Thomas Paulay, New Zealand. Twenty-five years ago, I knew everybody involved in earthquake engineering research, but that is no longer the case. There are now many younger engineers in the earthquake business—too many to name.

Nor can I mention all the earthquake engineering students who passed through Caltech—there were many. One of the fringe benefits of my working in the field of earthquake engineering has been the contacts with other concerned individuals, and with my students. I now look back on my career in earthquake engineering with great satisfaction.

Caltech Earthquake Engineering Group

“My contact with fresh young minds was an important intellectual stimulus. It is a major fringe benefit of being a professor.”

Scott: Would you discuss the beginning of the Caltech seismology laboratory, the growth of earthquake engineering in the Caltech program, and the development of what Bruce Bolt and others call the Caltech earthquake engineering group.

Early Days to World War II

Housner: In about 1925, the Pasadena Seismological Laboratory, previously part of the Carnegie Institute, was merged into Caltech, where Beno Gutenberg, Hugo Benioff and Charles Richter became professors of seismology. Beno Gutenberg came to Caltech from Germany, where his family had a business that he was running at the same time that he was an active seismologist. When R.A. Millikan invited Gutenberg to come to Caltech, he accepted the invitation and came. Hugo Benioff told me that as a high school student in California he had gotten a job as helper to Professor Albert A. Michaelson, who was working on his experiment for measuring the speed of light. Charles Richter was a southern California boy, and got a Ph.D. degree in physics from

Caltech, as did Benioff. Richter had hoped to become an astronomer when he got his degree, but at the time no such jobs were available, so when he was offered a job at the seismology lab, he took it, and became a seismologist.

Martel's graduate students in the 1930s included, among others, Maurice Biot, Merit White, George Housner, Trent Dames, William Moore, Ralph McLean, James Jennison, and LeVan Griffis. There were of course many other graduate students at that time, but I mention these names because of their involvement in earthquake matters while at Caltech. Jennison and Griffis constructed a small earthquake shaking table to test the operation of gas shutoff valves. In the early 1930s Martel and his students actively studied the Long Beach earthquake, contributed to reports on it, and provided extension courses in earthquake design for local engineers.

The Postwar Period

Housner: When World War II came along, it of course disrupted the earthquake studies at Caltech, and also disrupted the normal functioning of the entire school. The regular teaching program was canceled, and instead courses were taught to students who were in the military. For example, Ray Clough was a student in this program, and got his masters degree in meteorology at Caltech in 1943. At that time he was not involved in earthquake studies, although later he had a remarkable career in the field.

Then when WWII was over, Caltech developed a new program of earthquake studies, and in addition to Martel built up a considerable staff over the years. These have included names

like Donald Hudson, George Housner, Paul Jennings, Ronald Scott, Wilfred Iwan, Thomas Caughey, James Beck, John Hall and Frederic Raichlen. While these Caltech men had different backgrounds and teaching responsibilities, they were united by a common interest in earthquake engineering and seismic safety.

Hudson and Housner

Housner: First I will discuss my postwar collaboration with Don Hudson. In 1946, after we got back to normal civilian life again, Don and I were assistant professors on the Caltech staff. I had done my Ph.D. thesis on the earthquake problem, and was still very much interested in the subject. Then sometime in 1946 Professor Martel had a visitor, a man called Beauregard Perkins—a southern name—who was with the Office of Naval Research. When Martel told Perkins about the earthquake problem and the seismic studies we were doing, Perkins was interested and offered to fund our program. For several years he funded our studies on the earthquake problem. In particular, money from the Office of Naval Research enabled us to compute a large number of response spectra for different earthquakes. When we explained to him what we were doing, he said, "I will fund that, send in a proposal." Nowadays it is not so easy.

At that time Don Hudson got interested and involved. His field was mechanical engineering, and I came out of civil engineering, so his background complemented mine. We had a mutual interest in applied mechanics. My interests were largely in the performance of buildings themselves. Hudson, on the other hand, was much interested in vibration theory and

response, and also in the instruments needed to get earthquake information. In short, our interests did fit together very well.

We also worked with others of the Caltech staff, for example on the vibration generators. When I was president of EERI in the very early days and we were still trying to get research going, I proposed to the State Division of Architecture—Jack Meehan—that we make some machines to vibrate buildings strongly, so we could reliably measure the natural periods, the damping, and the mode shapes. In the 1950s they provided the funds for this, and sometime in the 1950s—I've forgotten exactly when—we did the work at Caltech. Making these machines was sort of a revolutionary step, as they were immensely superior to anything that went before, and this changed our whole picture of real building vibrations. These machines are now standard and are used all over the world.

The machines' superiority was basically attributable to work by Caltech professors Thomas Caughey and Dino Morelli on their design and construction. Caughey did the electrical controls, and Morelli was responsible for the rotating mechanism that exerted the forces. By knowing our Caltech people and their interests, we were able to bring them in on problems where their expertise was particularly helpful. I could never have done this kind of thing alone. With regard to Hudson, I should mention that he later was president of the International Association for Earthquake Engineering, and the Seismological Society of America. (I also served as president of IAEE and SSA.)

Jennings and Iwan

Housner: Paul Jennings and Bill Iwan became graduate students about 1960. Both of them had commitments to the Air Force. At that time, in between the Korean War and the Vietnam War, we still had the military draft. You could get a deferment while you got through your education, but you had to commit yourself to two years of military service.

It was interesting. Professor Archie Higdon—I guess his name was Archibald—came around to Caltech. He was a professor at the Air Force Academy and the author of a textbook in applied mechanics. Higdon said, "I need people to teach at the Air Force Academy, so I go through the lists of all the young men who are committed for two years of service to see how smart they are. Then I choose the ones we want to come to teach." I guess the ones chosen don't have any choice. If they are picked, they have to go there. He said, "I have Paul Jennings and Bill Iwan on my list—I'm going to get those two to go back and teach." And sure enough he got them. They both went back and taught at the Air Force Academy in Colorado. It was sort of a novel idea. Instead of just taking anybody, he was looking and choosing, and he had some very good people go through there.

Scott: It also probably meant they were able to make the highest and best use of their talents, while also doing their military service.

Housner: Yes, it was a very good use of their time.

Scott: After their time at the Air Force Academy, I presume Bill Iwan and Paul Jennings then become part of the Caltech group?

Housner: Yes. I was advisor to Paul Jennings, who was in civil engineering and did his thesis on earthquake problems. Whereas Bill Iwan was in mechanical engineering, and he and Tom Caughey had Hudson as their Ph.D. advisor. That's how those two got involved in earthquake studies.

When the great Alaska earthquake occurred in the spring of 1964, Paul Jennings was still at the Air Force Academy. When I became chairman of the Engineering Committee of the National Academy of Sciences project to prepare a report on the Alaska earthquake, Don Hudson recommended that, because I would need some expert help in getting the report together, I should ask the Air Force to assign Paul to Caltech to help. I made the request, Paul got the assignment, and indeed was an indispensable assistant.

Paul Jennings was also EERI's president at a crucial time—when EERI was preparing for the 8th World Conference in San Francisco and the journal *Spectra* was being started, as well as some other important activities. Bill Iwan was a long-time member and chairman of the California Seismic Safety Commission and has played an important role in the advancement of earthquake safety. He is also a member and chairman of the National Research Council's Board on Natural Disasters, and was the first president of CUREe (California Universities for Research in Earthquake Engineering). Through CUREe he arranged for research funding from several Japanese companies to support earthquake studies by faculty of member universities.

Such extracurricular activities require a special commitment. I consider it appropriate to men-

tion these kinds of organizational posts, which do require a substantial contribution of time, but can also have a significant influence on the profession. The Caltech earthquake group has been quite active in that way.

Relationships with Foreign Research Centers

Housner: One of the fringe benefits of the earthquake engineering program at Caltech has been the cordial relationships we have with foreign centers of earthquake studies. Many of these centers were begun by people who had spent time at Caltech, and I believe the first was Jai Krishna, who spent his 1958 sabbatical at Caltech and became interested in the earthquake problem. The following year Don Hudson and I spent some time at the University of Roorkee in North India, and while there we helped organize the first Indian conference on earthquake engineering. We also helped the engineering department start its program in earthquake engineering. This has been an active program that plays an important role in earthquake safety in India.

After that, Juan Carmona was here from the University of San Juan, in northwest Argentina, a highly seismic region. It was 1960, the year of the great Chilean earthquake. We had asked Carmona to set up a Wood-Anderson seismograph in the basement of the Athenaeum here at Caltech, just for the practice of doing it. While working at that, he came to us saying he was unable to do complete the job successfully because he could not get the seismograph's needle to stay at rest. The reason for this was that the seismic waves from the Chilean earth-

quake were then reaching Pasadena, and at that great distance the dispersion of the waves produced faint shaking that lasted several hours.

When Carmona went back to the University of San Juan, he established an active earthquake engineering group. Later, Julio Kuroiwa from the University of Lima in Peru studied with us, and when he returned to Lima he organized a very effective earthquake engineering group at the university.

In 1963 an earthquake caused severe damage in the city of Skopje, Yugoslavia, which of course is now in the new country of Macedonia. After the earthquake, a cooperative NSF project was set up between Caltech, Kinematics, and the University of Skopje. The United States provided strong motion accelerographs for installation in Yugoslavia, and a team came from there to learn more about earthquakes, and about how to record the strong motions. A very active earthquake engineering department was set up at the University of Skopje, and Professor Jakim Petrovski was director for many years. One of the members of the Yugoslav team was Angel-Mark Sereci, who later came to this country permanently and is now president of Kinematics.

Around that same time, Sheldon Cherry of the University of British Columbia spent a year with us, and has since developed an active program of earthquake engineering at the university there. He was elected president of IAEE at the Eleventh World Conference on Earthquake Engineering in Acapulco, Mexico in June 1996. Others who got their degrees at Caltech or spent their sabbaticals there include Kiyoshi Kanai of the University of Tokyo, Robert Hanson, who went to the University of

Michigan, Norby Nielsen, who went to the University of Illinois and then to the University of Hawaii, Douglas Foutch, who went to the University of Illinois, James Yao, who is now at Texas A&M, and of course many others of a younger generation.

Three Generations at Caltech

Scott: There appear to have been some three generations of participants in Caltech's long history of work in earthquake engineering?

Housner: Yes. The most senior types would have been R.R. Martel and Frederick Converse. I have already noted how Maurice Biot did his thesis under Martel, although Biot's real interest was more in aeronautics. The first generation after those would be Don Hudson and me, and Ronald Scott. The younger types would be Paul Jennings and Bill Iwan, and then John Hall and Jim Beck. Then of course over the years there were always students coming through that would not join the faculty but would go off somewhere else.

Scott: When they graduate, do they typically go into academic work or research, or into private industry or practice? My impression is that in the 1930s and 1940s a lot of Caltech engineering graduates went into private practice. But I presume that many graduates now go into teaching or research.

Housner: Many are teaching, and they are all over the world, but many others are not in academic life. People tend to think that Caltech puts out mostly professors, Nobel Prize winners, and researchers, but in fact many of our graduates go into other activities. For example,

Chester Carlson invented the Xerox process, and Edward Simmons invented the bonded wire strain gauge. The "R" and "W" in TRW stand for Simon Ramo and Dean Wooldridge, respectively, both Caltech alums. The most prominent Caltech alums in civil engineering are Trent Dames and William Moore, founders of Dames & Moore, the geotechnical consulting firm. Other civil engineering graduates went on to have productive careers outside of academia, one of many examples being LeVal Lund, who is active in earthquake engineering and who worked for the Los Angeles Department of Water and Power. I could list still more graduates, but these illustrate the point being made.

Influence of Clarence Allen

Housner: Caltech geologist Clarence Allen is not an engineer, but I should mention him because of his contacts with and influence on the earthquake engineers, through EERI and elsewhere. I will be referring to him a number of times when discussing various committees he has headed or in which he has participated, playing an essentially interdisciplinary role.

A good example of his contacts with engineers has been his work with the Caltech organization "Earthquake Research Affiliates," whose members are representatives of agencies that have significant earthquake concerns, e.g. Southern California Edison, Santa Fe Railway, Caltrans, and so forth. This was started after the 1952 Tehachapi earthquake.

Their annual dues go to support research by seismologists and earthquake engineers. In alternate years we have a conference or a field trip. On field trips to the sites of earthquakes,

Clarence has given very lucid talks on geology, faults, and earthquakes, from which we engineers profited greatly.

Scott: In a recent oral history interview Clarence talked a little about some of his field trip activities, which are continuing in his retirement. In 1995, for example, I believe he led a field trip to Patagonia for the Caltech Alumni Association. Those sound like fascinating interdisciplinary affairs.

Teaching and Student Contacts

Scott: Before ending this discussion of Caltech's program, would you say a little about your own courses and student contacts?

Housner: My main responsibility as a professor has been teaching both undergraduate and graduate courses. I have always liked teaching, and have tried to be a good teacher. For example, when I was first teaching I noticed that, while the instructors did in fact know their students, the students were unaware of that. So when I got the class list from the registrar's office I made it a practice to memorize the names of all the students—usually about 20 of them. Then in the first week of class, I would enter and recite off the names, fixing each student with a steady glance, so each would know that I associated his name and his face.

Recently when I was eating lunch in the Caltech Faculty Club one of the members of the fund-raising department came over and said, "I have one of our alumni here who says he was in your class forty years ago. Would you come over and chat with him?" I did so. The alumnus said. "What made a big impression on me was your knowing everybody's name in the

class." Another student, after being away for twenty-five years, said to me, "I got a very positive impetus when I was at Caltech, and this raised the trajectory of my career." Once when I was in Japan, Professor Heki Shibata said to me, "When I was a student in the 1950s I studied your mechanics book—that is how I learned English," and his English was passable.

Another time I received a letter from a man, presumably an engineer, who told me that the equation his professor had given him for the natural period of oscillation of water in a cylindrical tank differed from the equation in one of my papers. He said, "So I took my garbage can and filled it to various depths with water and measured the periods of oscillation. As the diagram I have enclosed shows, your equation proved the most accurate." I think that may have been the first time that hydraulic research was carried out using a garbage can. So you see that, although teaching does not generate many exciting events, it does have its satisfactions.

Scott: I should say so! Would you say something about the courses you have taught and the textbooks you wrote?

Housner: Over the years I have given a variety of courses, including statics, dynamics, strength of materials, theory of elasticity, structural design, and earthquake engineering. In addition, I have co-authored three textbooks:

Applied Mechanics—Statics (1949), with Don Hudson; *Applied Mechanics—Dynamics* (1950) also with Don Hudson; and *The Analysis of Stress and Deformation* (1966), with Thad Vreeland. I have also been involved in writing papers, some by myself alone, and others co-authored.¹⁵

I have especially enjoyed my intellectual contacts with graduate students working for their Ph.Ds. Clifford Truesdale, a Caltech alum and professor of theoretical mechanics at Johns Hopkins University, once said, "You should always try to get graduate students from whom you can learn something." I certainly did learn from my students, both graduate and undergraduate. My contacts with fresh young minds was an important intellectual stimulus. It is a major fringe benefit of being a professor.

I would like to close by observing that freshmen entering Caltech in recent years have been much better prepared than freshmen entering fifty years ago. This observation seems to run counter to the criticisms made of the quality of elementary and secondary education in this country. But I think education must be quite satisfactory for students who are bright and have reasonable advantages when they are growing up. Perhaps what this means is that the able students like those entering Caltech can make up the deficiencies in the education that is generally available.

15. See also Chapter 20, Discussion of Selected Publications.

Earthquake History and Reporting

“In most cultures, earthquakes were thought to be acts of god, and hence worthy of a written account.”

Scott: I know that you have been very much interested in the history of earthquakes. Would you describe how your interest developed, and outline briefly some of the eras and kinds of earthquakes included?

Housner: As a young man, I had no particular interest in history, probably because I simply did not then have enough knowledge to appreciate history. By the time I was 30 years old, however, I began developing a real interest in historical items relating to earthquakes, and began looking for historical accounts.

Ancient and Biblical Reference

Housner: While earthquake engineering and seismology are both very recent disciplines, and so lack much history of their own, there are many old records of and references to destructive earthquakes going back some 3,000 years. In most cultures, earthquakes were thought to be acts of god, and hence worthy of a written account. In India, for example, Shiva was

the earth shaker, and in New Zealand it was Ruaumoko, the God of Earthquakes.

Scott: And there are of course references to earthquakes in the Judaeo-Christian Bible. What about historical records going back 3,000 years?

Housner: There are Chinese accounts dating back that far, although my interests have been mainly in Europe and the Middle East. For example, the Greek author Philo, who lived in the first century A.D., said that the earth could not have existed as it is for all eternity, as the action of water in rain, torrents and rivers would through the ages have worn down and smoothed the entire earth. He was certainly right, and that would have happened, except for the action of what we call plate tectonics.

Philo's account caused me to think of a corollary that relates to earthquakes. That is, the earth cannot have been in static equilibrium for eternity, because the state of stress in the crust would then have been pure hydrostatic compression. The fact that earthquakes occur tells us that shear stresses develop by motions in the earth's interior.

The ancient Greek writers often comment on earthquakes. Pliny The Elder mentions earthquakes frequently in his book *Natural History*, and in fact says that the Greek philosopher Pherecydes of Syros in the 6th Century B.C. actually predicted an earthquake by observing changes in water elevations in a well. Actually I think he used such observations to predict that an earthquake happened somewhere.

Scott: Changes in well-water elevations have been observed as a consequence of some recent earthquakes, and I believe some modern seis-

mologists have suggested such observations as a possible means of prediction as well, but that does not seem to have worked out. Is that correct?

Housner: Yes, that is true. You mentioned biblical references, and I would like to refer to a very interesting account found in the Bible in Psalm 114. I was aware that it contained an earthquake reference, but it took me some time to realize how surprisingly detailed a description it was. The relevant passages are as follows:

The sea saw it and fled. Jordan was driven back.

The mountains skipped like rams, and the little hills like lambs.

Tremble, Oh earth, in the presence of the Lord.

Who turned the ground into standing water.

And the firm ground into fountains of water.

We can infer from this that an earthquake occurred under the eastern end of the Mediterranean Sea, and that it generated a small tsunami whose initial evidence was a withdrawal of water along the coast. This means that the faulting was such that there was a drop in the elevation of the sea floor, so the shock was probably magnitude 6.5 or greater. The ground shaking had high frequency components, so the epicenter was probably not a great distance away, and the earthquake caused soil liquefaction with some flooding.

Scott: I had not heard that interpretation before, but the way you put it sounds pretty

convincing. You mentioned liquefaction, which can be accompanied by sand boils and upwelling spouts of water. These might be the "fountains of water" referred to.

Housner: Yes, that's true, and a remarkable example of such soil liquefaction occurred during the 1964 Niigata earthquake in Japan. Except for the Psalm just quoted, the earliest poetic description of an earthquake that I have found is by the Japanese poet Kokan Shiren, which seems to describe shaking caused by a large earthquake some distance away. He felt the shaking in Kyoto, so I think the earthquake must have been similar to the 1995 Kobe earthquake, as felt in Kyoto.

EARTHQUAKE

Still things moving,
 firm becomes unfirm;
 Ground like ocean waves,
 house like a boat.
 A fearful time,
 but exhilarating as well;
 No wind, yet the wind-bells
 are chiming.

Kokan Shiren, 1278-1346
 Kyoto, Japan

Early Observations by Scientists

Housner: Robert Hooke was a contemporary of Isaac Newton, and is well-known in engineering circles because of Hooke's Law. In his book *Lectures and Discourses of Earthquakes and Subterraneous Eruptions*,¹⁶ I came across this

very interesting comment: "It seems not improbable but that the greatest part of the inequality of the earth's surface may have proceeded from the subversion and tumbling thereof by some preceding earthquakes." This is, I believe, the first intimation of plate tectonics. I don't know whether geoscientists are aware of Hooke's hypothesis about earthquakes, but it is indeed a very interesting thought.

Scott: Hooke was a major scientific figure in his day, and like many of the others seemed to be endlessly curious about the world he observed.¹⁷

Housner: In many years of watching, I have noticed some curious differences in the way engineers and geoscientists approach new and unusual ideas. Geologists and seismologists will make rash-sounding observations, such as that earthquakes will be predicted in ten years, or that a great earthquake will occur in the next fifty years, or that an earthquake will hit Parkfield in 1988, plus or minus a few years. Engineers, however, seem to be quite cautious in what they say—they appear almost to abhor making rash statements.

Thus, in an 1848 paper Robert Mallet, an English engineer, says this of a geologist: "The second of Michell's hypotheses is one of those examples of geology run wild, by which if only

16. Hooke, Robert, *Lectures and Discourses of Earthquakes and Subterraneous Eruptions*. First published 1705. Arno Press edition, New York, 1978.

17. Robert Hooke was one of the founders of British science. Among other things he constructed a telescope and improved astronomical instruments, formulated the theory of planetary movement as a mechanical problem, described microscopic observations of cells, and developed Hooke's Law, i.e. that within the elastic limit, the stress on a body is in direct proportion to strain.

a sufficiently monstrous postulate be granted, anything may be accounted for." This comment appears in Mallet's paper, "On the Dynamics of Earthquakes," in the *Transactions of the Royal Irish Academy*, vol. 21, 1848, pp. 51-105. Michell advanced hypotheses regarding the generation of a tsunami wave whose first evidence is a receding of the water along the beach. In the second hypothesis referred to in Mallet's comment, Michell supposes that the ocean floor may be suddenly elevated by pent-up steam beneath it, then let down again on the steam's escape. Thus, Michell speculates that what is observed as the water's retreat is due to the collapse of a cavity.

Scott: The pent-up steam hypothesis does sound a little far-fetched. Of course, back in those days developing satisfactory explanations for many earth processes called for a lot of speculation. Understandably some of it might be a bit wild. Engineer Mallet was calling geologist Michell to task in no uncertain terms. I guess you are suggesting that geologists may still have a touch of that adventurous spirit.

Housner: Mallet was quite an important figure. He investigated and reported on earthquakes in Italy, invented the terms seismology, epicenter, and isoseismal map, and is seen by both seismologists and earthquake engineers as the founder of their disciplines.

A peculiar criticism is seen in the comment by Charles Davison about Clarence Dutton. Dutton was a member of the U.S. Geological Survey who among many other things prepared reports on the New Madrid earthquakes of 1811-1812, and the 1886 Charleston earthquake. In mentioning Dutton, Davison makes the following curious comment about him:

"Not many seismologists have made so few original contributions to the science." I wonder whether he would make the same statement if he were writing today?

Davison was at Cambridge University in the early years of this century, and wrote a number of books on the history of seismology, one being *The Founders of Seismology*.¹⁸ He places the beginning of seismology around 1750, "...when those who studied earthquakes drew their illustrations from contemporary records and no longer from the writings of Aristotle, Seneca, or Pliny."

Davison tells about the formation of the Seismological Society of America, which was created on November 20, 1906 as a consequence of the great San Francisco earthquake earlier that year. The first issue of the *Bulletin of the Seismological Society of America* was published in March, 1911. Davison comments, "It is interesting to notice the gradual expansion of this journal and the increasing importance of its articles."

Davison also mentions Thomas Young, who is well-known in civil engineering circles through Young's modulus of elasticity. Young himself was very much interested in earthquakes, and compiled a great list of them. He also published a two-volume work called *Lectures on Natural Philosophy*, in which earthquakes are discussed.¹⁹

18. Davison, Charles, *The Founders of Seismology*. Cambridge University Press, 1927.

19. Young, Thomas, *Lectures on Natural Philosophy*. In two volumes, 1807.

Reports of U.S. and California Earthquakes

Earlier Records Are Sparse

Scott: There are historical records of earthquakes in New England, some fairly good accounts of the well-known great earthquakes in the New Madrid area, 1811-1812, and Spanish Mission era evidence of California earthquakes. But I believe that such information is mostly pretty scarce.

Housner: Yes, the recorded history of the United States is much shorter than that of Europe or of China, and our historical records of earthquakes only go back a few hundred years. There have been large historical earthquakes in California, however, notably the 1906 San Francisco earthquake, the Owens Valley event of 1872, and the Fort Tejon earthquake of 1857. These three were probably of Richter magnitude 8 or greater. It is also clear that there have been many prehistoric large earthquakes on the San Andreas fault, which of course is a continuing threat to both the San Francisco and Los Angeles regions. There are many other faults in the Los Angeles region that pose threats to the metropolitan area. The Hayward fault, which passes through Berkeley and the East Bay area, is also a threat to the San Francisco region. So the earthquake danger is constantly with us. Meanwhile, in the last 150 years the population of California has gone from about 20,000 to about 30 million, and this growth has greatly increased the state's vulnerability to earthquake hazards.

San Francisco, 1906

Housner: The first formal earthquake report ever made in California was the so-called Lawson report²⁰ on the San Francisco earthquake of April 18, 1906. At that time, there really was no such field as earthquake engineering, and the Lawson report did not address engineering problems. Some engineering papers on the earthquake were written, however, and published in the *Transactions* of the American Society of Civil Engineers.

Scott: As you point out, the Lawson report did not really talk about engineering as such, but focused on observations of earthquake damage, on seismology, and earthquake theory. Back in the early 1900s we had no experience at all with the kinds of earthquake engineering reports that we are now accustomed to seeing EERI put out after every major earthquake. Moreover I guess the engineering profession of the day was not really prepared to do a meaningful report on the 1906 event.

Even if it did not cover engineering, however, I believe many considered the Lawson report one of the really classic efforts to do a thorough investigation and report on a major earthquake,

20. *The California Earthquake of April 18, 1906*, Report of the State Earthquake Investigation Commission, Carnegie Institution of Washington, D.C., two volumes, plus atlas, 1908-1910. Editor's Note: Volume I, by Andrew C. Lawson, presented an extensive record of observations of earthquake effects and damage. Volume II, by Harry Fielding Reid, presented and discussed instrumental records and data, described the theory and operation of the seismograph, and gave Reid's elastic rebound theory of earthquakes, based largely on findings from the 1906 earthquake. Contributions by Reid and G.K. Gilbert of the U.S. Geological Survey, had a profound influence on earthquake science.

and to publish the results in a well-compiled consistent way.

Housner: An interesting sidelight is how we got the Lawson report reprinted. It had been printed in a very limited edition just before the first World War, and when Don Hudson and I came along no copies were available. We wrote to the publisher about reprinting, but were not successful until we also got some eminent geologists to write, and then we purchased copies of the second edition.

Santa Barbara, 1925, and Long Beach, 1933

Housner: You are right about the engineers not being ready to do the comprehensive in-depth type of earthquake engineering report in 1906. In fact, no real earthquake engineering reports as we know them were published on the Santa Barbara earthquake of 1925, or even on the 1933 Long Beach earthquake, or the 1940 El Centro earthquake. There was a good concise *policy* report on the 1933 event, however, when the Joint Technical Committee on Earthquake Protection was organized to consider ways to minimize loss of life and property damage in future earthquakes.²¹

President Robert A. Millikan of Caltech chaired the committee, and Martel was vice-chairman. Fourteen other members represented a variety of participating organizations and societies of structural engineers, architects, geologists, contractors, etc., and supported by the Los Angeles Chamber of Commerce. The committee's report reviewed the earthquake

effects, and made some farsighted recommendations on earthquake-resistant design, retrofitting of existing structures, fire prevention, and disaster planning.

In a curious coincidence, 38 years later, following the 1971 San Fernando earthquake, the county organized a similar Los Angeles County Earthquake Commission; President Harold Brown of Caltech chaired it and R.R. Martel's son, Hardy Martel, was vice-chairman.

El Centro, 1940

Housner: At the time of the 1940 El Centro earthquake, I was a graduate student at Caltech and drove down to inspect the damage. There did not then seem to be available manpower or funding to prepare and publish a report, so no earthquake engineering report was made on the 1940 event. Nevertheless, the 1940 earthquake did figure quite prominently in later seismic design, due to the ground motion record by an accelerograph operated by the Seismological Field Survey. For many years, this was the strongest ground motion that had ever been recorded, and the El Centro earthquake became quite well-known for it.

A few years ago, a Japanese visitor came by Caltech, saying he was on his way to visit El Centro, just to see where the famous accelerogram had been recorded. In 1940, of course, El Centro was a decrepit little town, quite different from what it is now. Much of the big change since has been due to the continuing influence of the All-American Canal. It brought in Colorado River water and made the Imperial Valley an extremely fertile and productive agricultural region.

21. *Earthquake Hazard and Earthquake Protection*. Joint Technical Committee on Earthquake Protection, Los Angeles, 1933.

In fact, orange groves near El Centro helped make that earthquake a real eye-opener for me. The fault's surface expression went right through an orange grove, offsetting the regularly spaced trees by about 10 feet. Those offset rows of trees were a memorable sight. It was a very significant amount of offset, about half the size of the 1906 earthquake's offset. The size of the El Centro offset made it very clear to me that engineers must not only consider earthquake shaking, but also the faults that generate the earthquakes.

***First True Engineering Report:
Tehachapi, 1952***

Housner: I believe the first true earthquake engineering report was made for the 1952 Tehachapi (Bakersfield) earthquake. Karl Steinbrugge and Donald Moran prepared the report, which was published by BSSA.²² Karl and Don were both engineers with the Pacific Fire Rating Bureau, and their report was originally prepared for that organization. Karl's predecessor at the Bureau had been Harold Engle, a very active proponent of improved seismic design who was known as a 10 percent g advocate.

***Alaska 1964—My Education in
Earthquake Reporting***

Housner: The great Alaska earthquake of 1964 was an education for me in several ways. At the time I was a consultant to the Pacific

Gas and Electric Co. on nuclear power plant design. I read some garbled newspaper accounts of the earthquake that made it appear to have been unusually strong in Anchorage and to have had remarkable effects. This tended to give the public some erroneous impressions of earthquake hazards, and I considered it advisable for us to visit Anchorage to get the true story.

Hugo Benioff and I, along with Ferd Mautz of PG&E, and Elmer Marliave, a consulting geologist, visited the Anchorage area. From the evidence of ground shaking in Anchorage, we concluded that it had been less severe than first indicated. That agreed with the fact that Anchorage was a full 75 miles from the fault. We also found that the underground river the newspapers had reported as running through Anchorage, and as having collapsed in the earthquake, was actually the back of a large landslide. All the California papers had given quite misleading accounts, but the *New York Times* did give a reliable story.

This taught me not to depend on newspaper stories, and also demonstrated how such unreliable sources can seriously mislead the public, as well as legislators and other policymakers. This convinced me that we should help educate reporters and news gatherers on the realities of earthquakes and their hazards, to ensure more reasonable and more accurate future reporting.

An example of this is a phone call I received from a reporter on the staff of the magazine *Nature* (published in London), shortly after the 1995 Kobe earthquake. Television reporting had shown only collapsed buildings and burned-out areas, and this had convinced the reporter that most of Kobe had been destroyed.

22. Steinbrugge, Karl V., and Donald F. Moran, "An Engineering Study of the Southern California Earthquake of July 21, 1952, and Its After-shocks," *Bulletin of the Seismological Society of America*, Vol. 44, no. 2B, April, 1954.

She found it hard to believe me when I told her that this was not the case. I pointed out that a careful look at the background of the disaster-scene pictures showed that most of the city was still standing. When the report came out in *Nature*, it was accurate and not misleading. So we are making progress.

In these circumstances, I have always tried to be very careful and explain clearly the basic earthquake situation to newspaper and magazine reporters, radio commentators, and TV news people. Since 1964 I must have responded to hundreds of such inquiries with information and interviews. I believe that the time spent on this by myself and others has helped improve earthquake news stories and articles significantly. On the other hand there is a comparatively rapid turnover in reporting personnel. Thus, each successive inquiry seems to come from a reporter new to the business, who has only some vague ideas about earthquakes and their effects. In short, although we have made significant progress overall, it is very important to persevere in the effort.

Scott: That is certainly true!

Earthquake Engineering Report on the Alaska Earthquake

Housner: The Alaska earthquake itself was truly enormous, having a magnitude of 8.4 and a slip length of fault that ran about 450 miles. That mighty earthquake shock aroused the interest of everyone who was concerned with earthquakes—earthquake engineers, seismologists, geologists, hydrologists, biologists, oceanographers, and social and policy scientists. Each of those groups wanted to prepare a

report on the earthquake, and did. The National Academy of Sciences formed a Committee on the Alaska Earthquake, and each of the groups got busy preparing a report.

Paul Jennings and I were responsible for the engineering report on the earthquake, which was 1,190 pages long and included 32 separate papers. Many well-known earthquake engineering names appeared as authors. Also, many important papers were prepared by the Army Corps of Engineers office in Anchorage—Warren George was the chief engineer. Fortunately, I was able to get Jennings to be vice chairman of the panel on engineering. He was doing his two-year stint as an instructor at the Air Force Academy. We contacted the Air Force in Washington, and Paul was assigned to Caltech.

Our report was completed in 1967, but was delayed in the publishing process. Engineering was put at the end of the queue. The complete NAS report came out in nine volumes, which was by far the largest earthquake report yet made, either before or after the Alaska earthquake.²³

It was a mistake to have organized the effort to appear as a single report, with each part having the same format, typography, cover, etc. The enormous job of publishing all this material extended over six years. So the engineering report, although ready in 1967, was not actually published until three years later, in 1970. Instead of trying to cover all aspects of such an earthquake in a single coordinated effort and

23. *The Great Alaska Earthquake of 1964—Engineering*, National Research Council, Committee on the Alaska Earthquake. National Academy of Sciences, 1973.

report, it would have been much better for each disciplinary group to go its own way and prepare its own report, without trying to coordinate with all the other groups and their reports. This would have been a much better approach, even if there might have been a bit more overlap and duplication.

Scott: The separate-discipline approach seems to be the one used now. In recent earthquakes, those preparing the earthquake engineering reports have not made any special effort to coordinate with the other disciplines, have they?

Housner: I think that is correct. After having participated in many investigations and in the preparation of many reports, I have reached the following conclusions as to the best way of handling the earthquake engineering part of such efforts:

1. As soon as an earthquake occurs, the country of origin should—through its local IAEE society—place on the World Wide Web a brief description of the earthquake's most salient features. That way within a few days of an event, everyone will be informed of the earthquake's magnitude and location, important ground motion recordings, and preliminary estimates of damage.
2. The event should then be documented in a general report describing the earthquake, its effects, etc. For a major earthquake, the report preparation and publication will probably take several years of work.
3. Independent of the general report, specific reports should be prepared by and

for engineers. These technical reports should focus on specific problems of interest to engineers, such as recorded ground shaking, the performance of transportation structures, of steel frame buildings, of soils, and so forth, and should include technical analyses. This reporting should focus on those features that have engineering significance.

I should point out that the three-fold scheme I have outlined does not mention such things as EERI's Reconnaissance Report on the Kobe earthquake, or the Quick Reports of the CDMG Office of Strong Motion Studies. Those reports are, of course, very valuable, and I believe that in the future the Office of Strong Motion Studies will put the Quick Report on the World Wide Web.

Funding for Earthquake Engineering

Housner: It became clear that the earthquake engineering activity was not funded in a way that compared to geology and seismology, which were funded by the U.S. Geological Survey. So in 1964 I made a point of discussing the matter with people at the National Science Foundation (NSF). I remember speaking with John Ide, who was the head of the engineering section of NSF at that time, and I talked to Mike Gaus. But at that time I did not get any significant response regarding the need for more funding of research in earthquake engineering.

The work on the Alaska earthquake was spread out over quite a number of years, however, and the National Science Foundation was approached for funding of various aspects connected with the overall Alaska earthquake

project. They gave some money, but not specifically for engineering.

While working on the Alaska project I talked to the NSF people a number of times. Mike Gaus did promote an earthquake engineering effort. We—Caltech—got some funding allocated for earthquake engineering research. It was a modest amount, but that was the first time something had ever been set aside for earthquake engineering research at NSF. That was a few years after the Alaska earthquake. Mike succeeded in building up a modest program in earthquake engineering.

*Policy Reports After Alaska, 1964—
Proposing a State Commission*

Housner: In 1967 a small report came out, entitled *Earthquake and Geologic Hazards in California*. Hugo Fisher, Director of the Resources Agency, had appointed the committees that prepared the report, but I presume that the impetus had come from the Division of Mines and Geology and the Department of Water Resources. The two agencies were very much concerned with geologic hazards, and with the problems of earthquakes.

A total of 18 members were involved, including many well-known earthquake activists of the time. The committee was asked to prepare a report on earthquake hazard in California and to recommend hazard-mitigation programs. To do its work, the committee promptly split into two committees, with Clarence Allen the chair of one, and myself of the other. We made recommendations on specific programs, but also urged establishment of a State Geologic Hazards Advisory Board to be appointed by the Governor and to serve without pay. While no

such board was created at the time, this recommendation may have provided a seed that later became the Seismic Safety Commission.

The 1967 report recommended strengthened research and information-collecting programs in the geological sciences, engineering, and prediction, along with programs in education and guidance. As I noted before, in addition to specific state-agency actions preparing for future earthquakes, the report also recommended creation of a 12-15 member Earthquake and Geologic Hazards Board to:

keep...informed concerning earthquake and geologic hazards and what the state is doing about them, to advise, approve and coordinate research programs for state agencies, to recommend programs to the legislature for implementation and financing, to assist in obtaining funds for research on geologic hazards, and to make contacts for research with State agencies, universities, and private organizations.

The committee report was submitted to Hugo Fisher in typescript. Feeling that the report should have a wider distribution, the committee itself funded the publication—at nominal cost—and distributed copies.²⁴ So the report at least described what it thought was needed, and put those ideas in the heads of members of the California earthquake community.

24. *Earthquake and Geological Hazards in California*, Geologic Hazards Advisory Committee. Caltech, Pasadena, CA, April 26, 1967.

Inadequacies of Our Earlier Efforts

Housner: Beginning with the 1940 El Centro earthquake, I inspected and reported on many seismic events. In the process my colleagues and I photographed thousands of buildings that had been damaged or had collapsed. Many were old, weak buildings, and some were new, weak buildings. In retrospect, however, I conclude that we learned very little useful information, except that weak buildings will fail.

The reports were mainly engineering horror stories showing photographs of badly damaged or collapsed buildings. There was little to be learned from this, as we already knew that poorly designed and poorly constructed buildings were likely to suffer severe damage in earthquakes. In more recent reports, authors attempt to identify the engineering problems disclosed, and to comment on ways to avoid such problems in the future. I do not want to give the impression that we failed to learn from those earlier earthquake observations, but in the early days we focused too much on damage per se in ways that were not informative.

Scott: After observations of successive earthquakes, engineers and others have a better idea both of the kinds of evidence to look for and how to interpret what they see.

Housner: In some cases we actually did learn valuable lessons on the performance of newer structures, but in other notable instances we really failed to learn. An example of the latter is the case of the Macuto-Sheraton Hotel that was severely damaged by the 1967 Caracas earthquake. It was a large new concrete structure, and suffered much more severe damage

than would have been expected from such ground shaking. But we never really learned precisely why, because that would have required a large effort and a big study project, for which there was no funding. It is not clear whether you could say that we learned from the cracked steel joints in the Northridge earthquake—although we did learn that we were not doing things correctly.

Reports on San Fernando, 1971

Housner: After the 1971 San Fernando earthquake, Los Angeles County set up an investigating committee. In addition to the chairman and vice-chairman, four of us from Caltech were members: two earth scientists (Charles Richter and Clarence Allen), and two engineers (Don Hudson and myself). I thought the Commission's report²⁵ was a very perceptive document that clearly defined the region's earthquake hazard, pointed out the lessons that should be learned from the quake, and emphasized the retrofitting that ought to be done to strengthen vulnerable facilities like: 1.) hazardous old buildings, 2.) unsafe dams, 3.) highway structures, and 4.) facilities vitally needed in emergencies. It also recommended building code revision to remove shortcomings exposed by the earthquake.

Another remarkably fast job of reporting on the San Fernando earthquake was a forced-draft effort put together at the urging of Joseph Berg, a seismologist at the division of earth sciences of the National Research Council.

25. *Report of the Los Angeles County Earthquake Commission—San Fernando Earthquake of February 9, 1971, Los Angeles County Board of Supervisors, November, 1971.*

Shortly after the earthquake, Berg came to California, and made Clarence Allen, Donald Hudson and me sit down with him and write about the event. He also spoke with others such as Bruce Bolt, Karl Kisslinger and Karl Steinbrugge.

Then he went back to Washington and got a small "Quick Report" printed and distributed in March, within six weeks of the event described. That set a record for speed, and the

report, although only 24 pages long, showed the value of being able to issue reliable information soon after an earthquake occurs. It is no longer possible to move so rapidly at NRC. Many more rules and requirements are now in place, so it takes months just to get a report approved. Now, however, the World Wide Web offers an opportunity to get information distributed quickly.

Records of Earthquake Motion

“We developed...the seismoscope [which] was essentially a universal pendulum having a period of 3/4 second and 10 percent damping, which recorded on a smoked watch glass plate.”

Housner: Obtaining reliable information about earthquakes has long been a major problem. Earth scientists obviously need good data for their work, but engineers also have a vital need to know as much as possible about the kinds of strong shaking that may test the facilities they design.

Strong Motion Records and the Seismological Field Survey

Scott: Would you give your perspective on the history of efforts to get the kinds of earthquake motion records that are crucial to effective earthquake engineering? Start by going back to John R. Freeman, whom you mentioned before as a remarkably influential early figure who very actively promoted the idea of earthquake engineering.

Housner: The Seismological Field Survey was established through the persistence of Freeman, who decided that the country needed some strong motion instruments and kept

talking to various people in government in Washington, D.C., trying to find a receptive ear. This excerpt from a letter Freeman wrote to the Director of the U.S. Coast and Geodetic Survey on March 17, 1930 illustrates both the thoughtfulness and persistence of his approach:

The *acceleration* of earthquake action needs to be measured and studied by some kind of instrument not yet perfected; although all assume acceleration as the very starting point for all computations of earthquake stress in a building frame, I cannot learn that this *has ever been directly measured with any reasonable approach to accuracy*. I inquired about this in Japan and was told it had not yet been done, but was shown a 3-ton instrument in course of construction which it was hoped might give data on the rate of acceleration. It seems to me there is no need of a ponderous instrument for this purpose, and that some of your experts, working in conjunction with the experts and mechanics of the Bureau of Standards, can readily design an instrument for this purpose.

Freeman spoke to the head of the Coast and Geodetic Survey but said, "He was not a good listener." Finally he spoke with the Secretary of Commerce, who agreed that such a program should be established and so directed the U.S. Coast and Geodetic Survey, which was located in his department. The first accelerograph—an instrument capable of recording strong earthquake motion—was designed by the National

Bureau of Standards in the Department of Commerce, and custom-made under its supervision. It was actually a modification of the Wood-Anderson seismograph, a sensitive instrument used in seismological studies to record relatively faint vibrations coming from distant earthquakes.

In 1932 the department set up the Seismological Field Survey here in California within the Coast and Geodetic Survey, and with headquarters in San Francisco. The first strong motion accelerographs were put out in late 1932, including several in the Los Angeles area, and so were installed in time to get records of the March 10, 1933 Long Beach earthquake. This was a milestone, as it was the first time any such records had been made anywhere in the world.

The first chief of the Seismological Field Survey was Edward C. Robison, who was in charge of the instrument installation for a time. But administrative changes were soon made, and a while after the 1933 earthquake Franklin Ulrich was put in charge of the strong motion program as chief of the Seismological Field Survey.

Scott: Ralph McLean and John Rinne mentioned Ed Robison in their oral history interviews. McLean initially worked with Robison on instrument installations in southern California prior to Robison's abrupt departure from the program, which occurred almost on the eve of the Long Beach earthquake. McLean surmises that the sudden firing was probably due to an incompatibility between Robison and Capt. Thomas Maher, who was his immediate superior. Anyway a Coast and

Geodetic Survey man named Alfred K. Ludy came over from Phoenix to cover the operation until Franklin Ulrich, who was stationed in Sitka, could be freed up to take over the strong motion program.

Housner: Meanwhile there was a problem with the record from the Long Beach earthquake. Immediately after the earthquake, the unit in Washington, of which the Seismological Field Survey was part, gave information to the *Engineering News Record* that the peak acceleration measured had been so much. But then in the next issue there was a retraction: the peak acceleration had only been half as much.

Something had gone wrong. Professor Martel told me that he thought the man in charge of the instruments had decided on his own to make them twice as sensitive as originally planned. Then when the records were sent back to Washington, they went on the basis of the original plan, reading off an acceleration that was really twice as big as it should have been. Martel believed that this man, Ed Robison, lost his job over that.

Scott: Yes, I guess quite a few thought something like that probably happened. Ralph McLean believes the error was due to a basic design flaw in the original instrument, and that the firing predated the mixup over the Long Beach record. Also apparently the instrument was redesigned in Washington quite soon after the 1933 earthquake.

Pursuing the Strong Motion Program

Housner: Ulrich took the job very seriously. I remember that he came by twice each year to

talk to Professor Martel, and Martel would have me sit in with them. Ulrich would explain his plans, what he was going to do with his instruments. Of course he only had a small number. He would say, "Well, I'm planning to take this instrument from here and put it there, what do you think of that?" They would discuss optimum locations for the small number of instruments that he had available. The program did prove very astute in locating the accelerographs, having an instrument in the area of strong shaking and thus making records of the 1933 Long Beach earthquake, the 1940 El Centro earthquakes, the 1949 earthquake in Olympia, Washington, and the 1952 Tehachapi earthquake.

In 1934 Ulrich initiated a program of recording vibrations of structures, both transient vibrations and forced vibrations. Shaking machines of the "run-down" type were developed to vibrate buildings and measure natural periods of vibration. I believe the Seismological Field Survey funded construction of two such machines. The first was built at Stanford University by Lydik Jacobsen and John Blume, and later a larger machine was constructed, but I am not sure where. The shaking machines were used for measurements of natural periods of some structures in the San Francisco and Los Angeles areas, but most building periods were determined by measuring ambient vibrations. These records left a good deal to be desired; shaking machines with positive speed controls were not developed until probably the late 1950s.

Then at the end of World War II, when I came back from overseas, I remember that when I was in Washington for a month or two, Ulrich

looked me up and asked me if I would like to join the Seismological Field Survey, but I preferred to come back to Caltech. Ulrich then got Bill Cloud to be his assistant, and then after Ulrich's death, Cloud became chief of the Seismological Field Survey.

In his quiet way Bill Cloud was a valuable citizen and contributed a lot to earthquake engineering. I believe he never really got the full recognition that he deserved. For example, when we persuaded an organization to purchase an accelerograph for installation, Bill Cloud agreed to install and maintain the instrument and process its records. Thus, he willingly accepted additional work, without funding, something that is not usual for a government agency.

Cloud's agreeing to do the installation, maintenance and record processing also made possible the passage of the Los Angeles city ordinance requiring accelerograph installation in every new building over ten stories. Under the ordinance the instruments were purchased by the building owners, but Cloud and the Seismological Field Survey agreed to maintain them and process the records at no cost.

A bit of history that ought to be documented is the origin of the well-known diagram that plots the peak acceleration versus distance from the fault. The first time I saw it was when Bill Cloud showed me a such a plot laid out on log/log paper with peak accelerations plotted along the ordinate and distance from the fault plotted along the abscissa. That was back in the 1960s, when there were few accelerograms and the attenuation of acceleration with distance was not well known.

Bill had plotted about seven or eight points, and had drawn a smooth attenuation curve that approximately fitted the points. This struck me as a clever way to present the data, and after having seen that I prepared similar plots when I had more data points. Following the 1971 San Fernando earthquake it became standard for such plots to be made. I suppose we ought to call these "Cloud Plots" in honor of Bill. And in fact the attenuation curve often passes through a veritable cloud of points.

The Effort to Get More Strong Motion Records

Housner: Following World War II it became clear that a basic problem of earthquake engineering was the scarcity of recordings of strong ground shaking. The few accelerographs that the U.S. Coast and Geodetic Survey had installed were spread out over the entire western United States and provided only a few strong ground recordings. They did not give a good picture of the spatial distribution of the ground shaking in an earthquake.

Ulrich was very much aware of this, but with the Coast and Geodetic Survey's headquarters located in Washington, D.C., he felt frustrated because from his office way out west in San Francisco he could not get much action on his requests for funding and other support that he considered necessary. The upshot was that relatively little instrumentation was done, compared with the need.

Around 1946 or 1947 Ulrich got permission to form an advisory committee to help him in his operation, and especially to help convince the head office in Washington that more support

for strong motion instrumentation was needed. Martel and I were members, along with Lydik Jacobsen of Stanford, John Blume, and several others. The advisory committee's failure to have any effect on Washington and its resulting frustration led directly to the establishment of EERI.

My Unsuccessful Attempt

Housner: Also sometime in the 1950s when I was president of EERI, I wasted quite a bit of time trying to have my Congressman get something started. When I was trying to get support for EERI's work, I talked with Perry Byerly,²⁶ who was active in earthquake matters. He said that when they wanted to get the Department of Commerce to set up a seismological program, they went to a California Senator. I do not remember what his name was, but he arranged for the U.S. Coast and Geodetic Survey's seismological unit to be established, and it continued until USGS took it over. Anyway the Senator pulled the appropriate strings, and I remember Byerly saying that they set up an \$80,000-a-year budget item. That was what started the seismology program.

Scott: That would have been done in the 1920s?

Housner: Yes, and Byerly said that after the program got started, nobody ever questioned continuing the support. Byerly said to me, "You ought to try to get a California Senator or Representative to do the same thing for an augmented strong motion program." Prompted by Byerly's suggestion, as EERI president I wrote

up a proposal in the 1950s, stating what needed to be done and why. I asked for an increase in the budget of the Seismological Field Survey. I wrote up a description of the earthquake problem to make it clear why the funding was needed, and laid out a budget that would permit additional instruments to be put out by the Seismological Field Survey. I arranged to meet with the Under Secretary of Commerce to explain the nature of the problem, and what we in EERI were recommending, which was increasing the number of accelerographs and manpower, along with a corresponding budget increase.

I then contacted Congressman Lipscombe, the member of the House of Representatives from this Congressional district where Caltech is located. When I explained the matter to the Congressman, he was favorable and said he would carry the ball. Also, he was on the appropriate committee. I was rather naive in those days, and thought, "Well, if this Congressman is in favor of it and will carry the ball, it will happen."

Scott: I take it you made no contact with the Washington bureaucracy yourself?

Housner: I did go back once, and I spoke with the Assistant Secretary and explained my views, but without effect, apparently. In retrospect, I could see that I really had not done it right. Thus, when the item came up in the committee hearings on the Coast and Geodetic Survey budget, despite my presenting the proposal, the Coast and Geodetic Survey representatives said, "Oh, there is no need for that—we have given it consideration, and we do not see any need for strong motion instruments." So the item was turned down.

26. Byerly was a U.C. Berkeley seismologist and long-time head of its seismological laboratory.

Scott: Typically the bureaucracy comes to such budget hearings well prepared to defend their own agency budget proposal, and if anything new and different is interjected at that point, they are likely to see it as a threat to their own program.

Housner: Yes. So it was a mistake to have gone about it the way I did.

Scott: You were learning the hard way.

Housner: I agree—it was a lesson learned the hard way. Partly the lack of support was probably due to the fact that they thought of the earthquake problem as 3,000 miles away from Washington. It seemed remote, and they did not see it as that important. In any event, strong motion instrumentation was not really pushed in a major way until after the Alaska earthquake of 1964, when the City of Los Angeles instituted a program for requiring instruments in highrise buildings.

Scott: Say more about what you would do differently now.

Housner: You should give attention to briefing and convincing the people in the government agency concerned. In retrospect, what I should have done was talk to the people at the Coast and Geodetic Survey, write letters to the director, have others write letters, and build a case, until they said, "Well, all right, we think it's a good thing." Then I would go to the Congressman, and if he said, "Fine," then you probably could get action. I was too naive at the time, however, and did not go about it the right way. Looking back on the matter, I now see how it was extremely difficult to get things moving. But I also see how, once things do get started, they tend to roll along.

Commercial Availability of Accelerographs

Housner: About that time the importance of having accelerographs available commercially dawned on Don Hudson and me. Installation of strong motion instruments on a large-scale basis would become feasible only after they were commercially available. I mentioned how the accelerographs were being custom made, using the Department of Commerce designs. The Bureau of Standards got the instruments built, but after they built them they had only the drawings left. So if you wanted to buy an instrument, you would write to the Bureau of Standards, borrow their drawings, go to an instrument maker, and have one custom made. In 1950 an instrument made that way cost \$4,000. Taking inflation into account, this would be equivalent to about \$30,000 or \$40,000 now.

We could see the problem was that the instruments were not being made for sale commercially. To be commercially available accelerographs needed to be ready for purchase "off the shelf" and for a more reasonable price than the individually crafted instruments. If the instruments were commercially available and not too costly, we should be able to convince electric power companies, owners of dams and so forth to purchase and install them. But it was a long time before such instruments became commercially available.

Seismoscopes: A Stopgap Alternative

Housner: Meanwhile in the early days when there were not enough accelerographs to record motion, we developed an instrument we

called the *seismoscope*. It was essentially a universal pendulum having a period of 3/4 second and 10 percent damping, which recorded on a smoked watch glass plate. It was covered by a soup kettle that we got from the people who provide utensils and equipment for kitchens. We arranged for a little shop in Pasadena to make the whole thing and sell it for about \$100. Many of those were installed in California, as well as in other parts of the world. So instead of paying \$4,000 to get an accelerometer, people could spend \$100 and get the seismoscope. It didn't give all the information you wanted, but it would give you an idea of how strong the shaking was.

Scott: It would give a pretty good idea of the intensity of the strong motion?

Housner: Yes. It recorded the response of a pendulum having a 3/4 second period and 10 percent damping. It measured the intensity of shaking, and also gave an idea of how a structure like that would vibrate. It gave a point on the spectrum curve, plus a picture of the motion on the smoked watch glass.

Scott: The seismoscopes were in fairly wide use?

Housner: Yes, in many countries. Of course, that was in the 1950s and 1960s, when \$4,000 for a strong motion instrument was a lot of money.

Scott: Are those instruments still being used?

Housner: I think some are still in the field. In fact, in a recent earthquake near San Juan, Argentina, Professor Juan Carmona got a good seismoscope record. In other notable uses of

the instrument, I should also mention the fact that Ron Scott, by means of an ingenious analysis of a 1971 seismoscope record from Lower San Fernando Dam, was able to derive some interesting information about the dam motion.²⁷

But I do not think anybody is buying new seismoscopes anymore, because in real terms the accelerograph is now much cheaper than before. Incidentally, when we first made the seismoscopes and they began to be available for use, the local newspaper heard about them and wrote it up. Then we got a telephone call from a woman who said she was interested in a seismoscope. "What do you want it for?" "I want to put it in a building to protect it from earthquakes." That calls to mind UC Berkeley seismologist Perry Byerly, who very much wanted to obtain some California records and was failing to do so. His wry comment was that installing a seismic instrument seemed to keep earthquakes away.

Designing New Instruments

Housner: To explore possibilities for developing new instruments, I made use of some contacts with United Electrodynamics, Inc.—Bob Swain and Harry Halverson worked there, doing instrument work. They were the ones who put sensors under San Francisco Bay to get information about shaking for the BART tube. I was a seismic consultant on the BART project, so I had a certain leverage and tried to talk them into developing a commercially avail-

27. Scott, R.F., "Calculations of Horizontal Accelerations From Seismoscope Records," *Bulletin of the Seismological Society of America*. Vol. 63, no. 5, October 1973.

able instrument. I kept after them, and finally they decided to make commercial accelerographs. Of course it takes a considerable investment of money to design and build a prototype instrument, and there is a certain risk not enough will be sold to justify the initial outlay. Later, United was absorbed by Teledyne, Inc.

I remember that Bob Swain was in charge of the operation. I explained the kind of instrument needed and indicated the price range I thought would make it attractive enough to generate a market. He came back later saying they thought they could undertake design and production of an instrument, if they could sell 100 of them. I said, "I think if you make one and it costs less than \$4,000, you could probably sell 100 or so." He was interested, and they did make a Teledyne instrument called AR-240.

We had given Bob Swain and Teledyne the necessary characteristics that the instrument should have, what amplitude of motion it should be capable of recording, how fast the paper speed should be, etc. Then when they were building it Don Hudson tested the early model and advised on how to improve it. I believe the instrument became commercially available in the 1960s, they didn't lose any money, and did sell their 100 instruments and more. So you could now tell somebody to put an instrument in their building, and they could go buy one. Also, it made it possible for the City of Los Angeles to pass an ordinance requiring any new building of over 10 stories to have three instruments installed.

Later, they designed another accelerograph only half as big and half as expensive, called the RFT-250. Then Teledyne purchased Geotech,

a company based in Texas, which before that had been a principal competitor of the seismic instrument group at Teledyne. So Teledyne said, "We're going to merge you in with Geotech." But Swain, Halverson, and the others decided they would not go to Texas. Instead, they set up Kinometrics, their own firm here in Pasadena, and designed their own instrument, which they called the SMA-1. They sold these accelerograph instruments all over the world. In the mid-1980s they made their 3,000th instrument and gave it to Caltech—with a little gold dedication plate on it.

Scott: Obviously it has been very successful commercially. Would you say something about the overseas use of the instruments?

Housner: Yes, other countries, like Italy, India, Yugoslavia, and Turkey, have put in arrays of instruments, and Kinometrics even sells some in Japan. That part of their business has been successful, although the company has had its ups and downs. They are really more manufacturers of seismological instruments, and the strong motion accelerograph is only one of their items.

Harry Halverson was a major player in getting Kinometrics instruments installed in foreign countries, and was himself primarily responsible for the installation of many instruments. Approximately 10,000 Kinometrics accelerographs had been sold and installed in almost every seismic country in the world by 1995. Having affordable instruments is extremely important for advancing the program of getting records of strong motion. Without such information, you do not really know what is going on. I believe the data collected by these

instruments has given a most important impetus to the field of earthquake engineering, and Kinematics has played a valuable role in the development of the field.

Scott: Do you have any other observations about the distribution and use of strong motion instruments?

Housner: Yes, I could mention several rather strange occurrences. One involved a complaint from a Central American country. They said that the accelerograph would explode when ground shaking started. Halverson found that hard to believe but of course investigated. The instrument in question, the SMA-1, was contained in a strong metal box for protection, and was powered by a storage battery and a trickle charger. The instrument was fastened to a base, and remained at rest until triggered into action when the first seismic wave arrived. Halverson found that the battery being employed generated a small amount of hydrogen gas, and when the trigger was actuated a spark ignited the hydrogen-oxygen mixture, causing a small explosion.

In another case, we were asked to explain an unusual accelerogram recorded during a foreign earthquake. The only explanation we could come up with was that the instrument had not been fastened down to a base, but was free to slide around during the earthquake. That proved to be the answer.

Nick Ambraseys of Imperial College in London told me about a third unusual occurrence. He performed an experiment on coastal sand by detonating a buried dynamite charge and recording the motions on an accelerograph located some distance away. When he set off

the dynamite, however, it also caused an old buried bomb from World War II to explode, blowing up the accelerograph!

How Buildings Are Instrumented

Scott: Having described the development of affordable instruments, would you discuss how buildings are instrumented? How are the buildings chosen and where are the instruments placed?

Housner: We put instruments in highrise buildings so as to understand what has happened. This is important to know if the shaking is strong enough to cause damage. Some people say, "You don't need instruments in every building, just put them in a couple." But buildings are all different, so I think we need instruments in a large number of buildings. If you have only one building instrumented, or only a few buildings, you do not know how representative that is of all the other buildings.

Scott: Also, the ground motion might be significantly different, in different parts of a region shaken by a large earthquake.

Housner: Right, so of course we need the ground motion well covered. But even if you knew what the ground motion was, we would still need the records to tell us what the deformations of the building itself were, particularly if the building is overstressed and damaged. We need the records so we can understand what happened, and what can be done. If the records show that an earthquake's motion was not strong enough to cause damage, it would be helpful to the owners.

Instrumenting Structural Members

Scott: The records we are now getting show the ground motion itself, and the response of the structure, or various parts of the structure.

Housner: Yes, but your comment brings up another point. Something else that is really needed is a record of the stresses and strains in individual members. My feeling is that if we know the stress and strain in the individual structural members, and if we have information on how the ground shook and how the building deformed, we will be able to analyze the situation and understand what happened. I believe that recorded stresses and strains will not necessarily agree with the calculated stresses and strains, because we do not know all the properties of the structure.

Scott: Would it be an almost impossibly difficult or costly job to instrument so as to get data on individual members?

Housner: No, but it would be a much bigger problem. Actually, we did record it in one column, in the 52-story Security Pacific Bank Building in Los Angeles. When that was designed and constructed, we got the bank to agree to let us put a strain gauge on one of the columns. This is a very simple gauge, one that requires no electricity. It's called a scratch gauge. During the Whittier earthquake, we did record the maximum strain in that column during the earthquake. That's the first time that was done. But to do it right requires elaborate instrumentation in a building, more elaborate than we do now.

Scott: That would require sensors in quite a few locations in any one single building?

Housner: Yes, you ought to identify a number of locations that you consider the most significant for telling you what you want to know. Now, when they calculate the stresses that the earthquake should produce, they do it on the basis of computing the vibrations of the building. From the vibrations, they compute the stress in the member, but to do it they have to make a number of simplifying assumptions. So my own feeling is that the actual stress in the member could be significantly different from what they calculate by the simplified procedure.

An Analogy to Testing Aircraft Prototypes

Housner: I would like to find out how big the difference might be—whether it really is significant, or whether we can forget it. You see, what we would like is to do what they do for an aircraft. After they design and build the first model of a new aircraft, they put on strain gauges, maybe several hundred strain gauges all over on the significant parts, and then fly the plane and measure the strains. That is what we would like for a building—to put in lots of strain gauges, but economically that is not possible.

Scott: On the other hand, would it be possible to instrument a number of components of a building, the way they did that one particular column in the bank building? Or is some of that already being done, in addition to what was done on that one member in the bank building?

Housner: No, nothing is being done.

Scott: Would it be feasible, and in general a worthwhile thing to do?

Housner: I think it would be feasible. It would be a matter of what is the most efficient use of the funds. But it could be done, sure. I hesitate a little, however, partly because we do not know how long we have to wait for the earthquake. If you could say, "The earthquake is coming in three years," I'd say, "Yes, do it right away." But if it's not going to come for 30 years, then I don't know whether that would have been really a wise use of the money. That is one of our problems—we do not know when and where the next earthquake is coming. We don't know when to get ready.

I do, however, think such instrumentation ought to be done on a couple of buildings. It probably ought to be done in collaboration with a university, because somebody would continually have to look at what it was doing and to check that it was all right and so on. It would be more complicated than what the state program now does, or what the USGS program does.

Two Instrumentation Efforts Near Parkfield

The DWR Instrumentation

Housner: In the early 1960s the earthquake consulting board was advising the Department of Water Resources (DWR) on the design of the California aqueduct system that basically extends from Oroville Dam down to San Bernardino, and then will go to San Diego. The location of the aqueduct and its dams and pumping plants close to the San Andreas fault aroused concern. The aqueduct actually crosses the fault in several places.

A large earthquake had never been recorded at a site close to a major fault, so to provide information for its recommendations, we on the consulting board strongly urged DWR to install some accelerographs in an array across the San Andreas fault. The department allocated the necessary funds and contacted Bill Cloud, of the Seismological Field Survey, asking if they would install an array of accelerographs.

I knew that a segment of the San Andreas fault lay between the area of fault slippage in the 1857 Fort Tejon earthquake, and the area that slipped in the 1906 San Francisco earthquake. So I recommended that the array be put in the area where to the best of our knowledge no major slippage had occurred in historic time. After considering the matter, Bill Cloud indicated that the only practical place to put such an array was alongside the country road that passes more or less perpendicularly across the fault and goes through Cholame. For convenient maintenance, the accelerographs needed to be close to the road. It was called the Parkfield Array, as Parkfield was considerably larger than Cholame, which was only a gas station and restaurant. The general location is in central California east of Paso Robles.

Scott: Would you describe the array that was put in place?

Housner: Twelve accelerographs were located along a line perpendicular to the fault. The objective was to see how intense the ground shaking would be close to the fault, and how the intensity would decrease with distance from the fault. One accelerograph was located at Cholame, some 200 feet south of the fault,

10 others were situated south of the fault at intervals of several miles, and one was north of the fault. The array would measure the intensity of shaking out to a distance of about 25 miles.

Scott: I presume the Parkfield siting was based in part on surmising that a large earthquake of 7+ or higher was more likely there than on the southern or northern segments of the fault, where the large earthquakes you mention had occurred.

Housner: Yes, that is what I thought, but I was wrong.

Scott: The bigger earthquake has not happened yet, but you did get some early action from that array, didn't you?

Housner: Yes, the 5.3 magnitude Parkfield earthquake of June 27, 1966 occurred almost exactly one year after the instruments were installed. The earthquake was generated by a slip on the fault that started some distance north of Parkfield and extended a short distance south of Cholame. We had hoped, of course, to record a large event of the magnitude of the 1906 earthquake. Even so, we were very surprised by the record we did get from a relatively small earthquake.

Scott: What was surprising about the 1966 record?

Housner: The instrument sited adjacent to the fault recorded a displacement pulse with an amplitude of 9 inches, the ground moving at a right angle to the fault. That is, a point on the ground moved 9 inches away from the fault and then came back again, all within two seconds. Unfortunately, however, the instrument did not

record the motion parallel to the fault, due to a malfunction.

Scott: Getting a really intriguing record so promptly after installation was probably considered a great success?

Housner: Yes, it was, but I was wrong in expecting a large earthquake soon. Nevertheless the Parkfield records stimulated the interest of seismologists in strong motion recordings.

A Second Array: The Earthquake That Did Not Happen

Housner: Ironically, however, afterward the Parkfield fault segment was also the source of a really embarrassing lack of success with another and much more elaborate instrumentation effort. (I should say, however, that Caltech was not involved in any of the experiments that were to fail.) Following the 1966 earthquake, seismologists studied this segment and recognized that it contains a significant zigzag of the fault. It was believed that the jog, or zigzag, prevented a larger earthquake slip on the northern segment from progressing through it southward, and similarly prevented a large earthquake on the southern segment from progressing through it northward. The strains that were built up apparently were being relieved by a series of smaller earthquakes like the 1966 event.

It was then ascertained that five earthquakes of about magnitude 5.3 had occurred very nearly at 22-year intervals, leading to the announcement of a very high probability—with a plus and minus margin of error—that another Parkfield earthquake would occur 22 years after

1966, or about 1988. A few years before the predicted earthquake, an elaborate program was launched to make the most of the opportunity that seemed to lie immediately ahead. Special seismic instruments were installed in the vicinity of the fault, and several engineering experiments were set up to record the effect of the shaking on soil liquefaction, on a small structure, etc.

Even now, however, more than 29 years after the 1966 event, the expected earthquake has not occurred. I guess the lesson from that is basically a reminder that you have to be very cautious about extrapolating earthquake phenomena, especially if the underlying mechanisms are not well understood. I recall what UC Berkeley seismologist Perry Byerly told me when I was a young man: "You should never say anything definite about earthquakes." He then gave several examples of eminent seismologists having made explicit statements that were later disproved.

We now realize that the Parkfield portion of the fault has not proved to be the most appropriate location for the strong motion array. Also, since then, other arrays have been put out at better locations on the San Andreas fault: one at El Centro, one east of Los Angeles, and one south of San Francisco. The El Centro array recorded the shaking of the Imperial Valley earthquake in 1979, which occurred on that segment of the fault, but it was not the big earthquake that we expected.

Los Angeles Requires Building Instrumentation

Housner: I would now like to talk about the Los Angeles program to put accelerographs in major new buildings in the city. We could see from early-on that it was extremely important to get good strong motion records, and for structural engineering purposes to get them at more than one location in individual instrumented buildings. There was a big problem in proving what buildings did during an earthquake. We needed to get instruments into individual buildings to record the base motion of the building, as well as how the building itself moves and how the floors vibrate. The number of instruments the Seismological Field Survey installed in the early days was way too small.

I should make it clear that the earlier strong motion instrumentation was done just in hope of getting an earthquake record, not to explain the dynamics of buildings. Comparatively few instruments were installed, usually only one in a building. Their records told less about how the building itself performed than the sophisticated multi-sensor installations put in more recently, which can tell something about differential forces and movements within a building.

Influence of the Alaska Earthquake on Building Instrumentation

Housner: Although we had talked up the need for strong motion instrumentation quite often, only shortly after the Alaska earthquake in 1964 was the need clear to the people in the Los Angeles building department, especially to the department head, John Monning, an alumnus of Caltech. What really did the trick was the 1964 Alaska earthquake. There were only

four multi-story buildings in Anchorage—two 14-story buildings, one 10-story and one 8-story. All four of them were damaged so severely that they were not functional after the earthquake.

John Monning probably was the one who saw the significance of the highrise damage observed in Anchorage. I infer that Monning saw what happened in Anchorage, and thought, "Well, if the buildings in Los Angeles that are over five stories high were nonfunctional at the end of the earthquake, it would be a catastrophe." Monning also saw the importance of recording the movements of buildings, to get an idea of kinds of shaking in which various kinds of buildings might or might not be damaged.

Anyway, the department decided that something had to be done. Monning proposed an instrumentation requirement to the city council. Several times he asked me to go over and talk to them and explain why it was so important to do that kind of instrumentation. Finally, they did agree, so Monning got the building instrumentation provision put in the Los Angeles code. Consequently, when the 1971 San Fernando earthquake came, we got a lot of records showing how buildings vibrate.

Code Change

Scott: Would you discuss Monning's proposed code change?

Housner: I think it was around 1965 when he proposed an instrumentation program that would require all new buildings 10 stories or higher to be outfitted with three accelerographs, one in the basement, one on the roof,

and one at mid-height. This was to be done at the cost of the building owner. The owners of planned new buildings did not object, and agreed to do it. Nor did the people who were planning new buildings raise any objections, but the structural engineers did object. In fact, the structural engineers seemed to be the only people who were opposed.

Scott: It is interesting that structural engineers would be a principal source of opposition to the instrumentation program. Was the opposition expressed by individual engineers, or was it through the southern California association? Or was it a little of both?

Housner: A little of both, I think. Their opposition was expressed by word of mouth, and they opposed the proposal at one hearing that I attended, where one of the engineers spoke against it, saying he represented the Structural Engineers Association of Southern California (SEAOSC). My recollection is that the engineer spoke against it at a hearing before the building committee, a supervisory committee of the city building department.

I was tipped off by one of the city engineers to attend the meeting, where I spoke in favor of instrumenting buildings. So far as I know, nothing was ever published on this debate. As best I could make out, some engineers had an uneasy feeling that by putting in the instruments we would be spying on the engineers' handiwork. Their opposition was not strong, however, and the measure to install instruments was approved by the city council. The structural engineers now no longer oppose such instrumentation.

John Monning was a very able man who had the complete confidence of the city councilmen. Also, having been a general in the U.S. Army during World War II, Monning knew how to get things done. So the council voted to go ahead with the program. Monning asked Don Hudson and me to prepare a statement prescribing the properties of an acceptable instrument. The code change was adopted on February 14, 1965, and instruments began to be installed in the City of Los Angeles.

Don Hudson and I had previously been asked to prepare a statement for the code following the 1952 Tehachapi earthquake. It was a Richter magnitude 7.7 event generated on the White Wolf fault near the small town of Tehachapi. Although the ground shaking in Los Angeles was only moderately strong, it caused some disturbing damage to hanging light fixtures in a new office building. In a large room containing the desks of office workers, several lines of fixtures were hung on 3-foot rods, and during the earthquake the fixtures vibrated enough to break the rods, allowing everything to fall down on top of the desks. Fortunately, the earthquake occurred at a time when the room was unoccupied. In light of this observed damage, we prepared a statement on the seismic requirements for secure hanging light fixtures, and drew up a program of experimental testing that would be required to demonstrate that a light fixture would be able to survive. This was done at the request of the building department.

Monning's Try for UBC Instrumentation Requirement

Housner: John Monning also tried to get an instrumentation requirement in the Uniform Building Code (UBC), which is published by the International Conference of Building Officials (ICBO). ICBO is made up of representatives from each city that is a member of the organization. Every year they have a meeting that considers proposals to add something to the code, or to modify the code. If the conference approves a proposal, then it goes to technical people to be written up.

Monning asked me to make a presentation at this ICBO meeting, which I believe was in 1968, in Denver. I spoke there, but it was clear that there was opposition. I could see that it would take several years to get the idea accepted. You speak—but the people there are from various building departments, and they're not going to approve the thing right away. They will go back and talk to their city and to other colleagues.

It took about five years to get an instrumentation provision in the UBC, and even then it was put in the appendix rather than the code proper. There is a difference, because the appendix is special, and thus it has to be adopted specifically. Nevertheless, inclusion in the appendix was a significant step, and one would hope that most of the cities using the Uniform Building Code would require instrumentation. Unfortunately, however, Tony Shakal says that not many do.

Scott: So in time ICBO did adopt the requirement, but "demoted" it by putting it in the appendix.

Housner: Yes. Despite that, a few cities in California adopted the instrumentation requirement. San Francisco did not, however, although they would have been well-advised to do so. It is still a good idea, and one that San Francisco—and other cities—ought to consider.

Records from San Fernando, 1971

Housner: We got many records when the San Fernando earthquake came, made possible by the city's instrumentation program. By 1971, something like 60 buildings were instrumented. We got about 200 records—that is, 200 accelerograph records of ground or building motions. We got more earthquake records in that 1971 earthquake than had been obtained in the entire world before that. Moreover, all the instruments operated properly, which prompted the U.S. Coast and Geodetic Survey to give Richard Maley a citation for his excellent performance in maintaining the instruments.

The information obtained sort of laid to rest some of the engineers' questions about what buildings do during earthquakes. The 1971 records demonstrated that the seismic motion and forces in buildings are actually much larger than those prescribed in the building code. We at the universities knew this from making analyses, but practicing engineers were reluctant to believe. This finding led to changes in the code. For one thing, in May 1975 Los Angeles added a requirement for dynamic analysis when designing buildings over 16 stories.

Scott: A good deal of what was learned from the San Fernando earthquake came from those

strong motion records made possible by the earlier Los Angeles instrumentation requirement. That certainly emphasizes the importance of the instrumentation program.

Housner: Yes, having such strong motion records is of extreme importance, because it shows the engineers how the buildings vibrated, what the real earthquake forces were. As a result of having those records, in California tall buildings are now generally done on the basis of dynamic analysis, not static analysis. That is a very appreciable advance. Of course there are now many instruments in the Los Angeles area, and elsewhere in the state. Some of the instruments are put out by the USGS, some by the CDMG's Office of Strong Motion Studies (established in response to the 1971 earthquake), and some by individual organizations such as Caltech, the University of Southern California, Southern California Edison, the regional utility company, the Los Angeles County Flood Control District, and a few cities.

Scott: I presume that the utility and flood control district instrument their own structures?

Housner: Yes, that was done for their own purposes. These records have been very good. The record at Pacoima Dam in the 1971 earthquake was made on an instrument put out by the Flood Control District.

Scott: You are referring to the Pacoima record showing a remarkably strong peak acceleration?

Housner: The way that location came about was a little ironic. We had pushed the flood control people to put some instruments on

their dams, so when an earthquake came, they would know what happened. They said, "Okay, provided you go with us and tell us where to put them." So Don Hudson and I went with them. We started early in the morning, and went from one dam to the other. Then at the end of the day they said there was only one other dam they had in mind to instrument, and it was Pacoima Dam, a concrete arch dam. "But we can't make it today." So if we went out it would have to be another day, and we had already sunk a whole day in the trips to the dams. So we just asked them to describe what it was like up there at the dam.

They said it was in a rather steep canyon with rock. I pictured a canyon, a dam, and flat rock adjacent to it. They were going to put it about 50 feet away from the abutment of the dam. Anyway they located the instrument there, where we said it should be. But when we later visited the dam and saw what it was actually like, it was quite different from what I had visualized—it was a steep canyon, like this. [Gestures with hands.] The dam sits here and the instrument was sitting up here on this ridge. If I had seen the site beforehand, I probably would have told them it was not a good place, as it was the ridge of the mountain, and advised that they put it at another dam. I did not see it, however, but on the basis of their description said, "Yes, put it there." So that is where they recorded the famous Pacoima Dam record, which surprised everyone.

Another interesting record came from the Santa Felicia Dam, about 20 miles north of Los Angeles. They were interested in putting some instruments on this earth embankment dam, and came to us to ask if we would tell them how

to do it. So Don Hudson and I went out and showed them where we thought the instruments ought to be. We also put the shaking machine on the dam and recorded its properties. Then during the 1971 earthquake they got some very good records about how the dam vibrated. This again was a landmark record, as was the Parkfield record. So all of those strong motion records came not from the established program, but from special organizations that wanted to get information for their own purposes. They got some very good information. Also, in the Landers earthquake, the best record was obtained by an accelerometer that the Southern California Edison Co. had installed for constructing a future electric station.

A Sidelight on Record Availability

Housner: After the San Fernando earthquake, Leonard Murphy of the U.S. Coast and Geodetic Survey Washington office directed Bill Cloud to collect all the film records of earthquake motions and send them to Washington. Presumably, the U.S. Coast and Geodetic Survey intended to publish a report on the earthquake. Murphy did this despite the fact that most of the records came from instruments that did not belong to the Seismological Field Survey. In fact, however, Cloud did not send the records to Washington, but after collecting and processing them he sent Caltech a complete set, and also made copies available to others on request.

Caltech also had an on-request policy, making copies of almost 200 accelerograms and hiring a secretary to respond to requests. Caltech gave out over 4,000 copies of the prints of accelero-

grams. Then to our surprise an article titled "Who is Hiding the Accelerograms?" appeared in *Engineering News Record* alleging that someone, presumably at Caltech, had the accelerograms and was not letting anyone see them. We spoke with the *ENR* reporter, who said she was only reporting what she had been told, but we never did learn who had said that or why.

Scott: So things do not always necessarily run smoothly in earthquake engineering?

Housner: That is right, sometimes they don't. But let me also report on Caltech's special effort to analyze strong motion records and present the results in the "Caltech Strong Motion Reports," which were made widely available.

Caltech Strong Motion Reports—Records of Many Earthquakes

Housner: Publication of the *Caltech Strong Motion Reports* was a milestone in the development of earthquake engineering.²⁸ Funded by NSF in 1967, Caltech undertook preparation and publication of a large number of reports that analyzed acceleration records from 57 different earthquakes, but most of the records came from the San Fernando earthquake. For each earthquake the reports showed the acceleration curve, the calculated ground velocity curve, and the calculated ground displacement curve for each ground acceleration record.

This was the first time that such a comprehensive view was available. In addition, the reports

presented the calculated spectrum curves for each of the earthquakes. Copies of the reports were sent to all interested parties, and I am sure they changed many people's thinking about earthquake motions.

Scott: Analyses were done for many earthquakes, and some of those, such as the San Fernando earthquake, provided numerous recorded ground accelerations. I take it all of these were analyzed and published in the Caltech reports?

Housner: Yes, they were—every recorded strong ground motion in the western United States was analyzed. The project was directed by Donald Hudson, and took seven years to complete. It was an enormous effort, particularly because the digital computer was still in a relatively primitive state. The first step in the analysis was to make a greatly enlarged photograph of the accelerogram, and then at intervals of about 0.1 second, the ordinates of the accelerogram were measured and recorded. For each accelerogram, the data points had to be put on punch cards, which were then processed by the mainframe IBM computer at Caltech.

The computer printed out the results as a series of numbers, and these then had to be plotted by hand to show the accelerations, velocities, and displacements of the ground, and to show the spectrum curves. Methods of analysis had to be developed for processing the data. The project supported many graduate students, who worked on it. Because of the enormous amount of effort involved, we felt that the digital data points for each accelerogram should be included in the reports, together with all the

28. Caltech Earthquake Engineering Research Laboratory, *Strong Motion Earthquake Accelerograms: Digitized and Plotted Data, 1969-1975*. Pasadena, CA.

computed data points. When put on a bookshelf together, the Caltech reports take about four feet of space.

Scott: That was a huge job. With all the advances in the past quarter-century, analyses like that can now be done with much less effort, can't they?

Housner: A great deal less. Computers have now been so much improved that the same analysis could be done now with one-hundredth the effort. The California Office of Strong Motion Studies now has a very efficient method of digitizing, analyzing, and drawing the results. The accelerogram is digitized by machine, not by hand, and the digitized data put directly on magnetic media, which in turn provides input to the computer that performs the various analyses. By way of contrast, the 1994 Northridge earthquake was recorded by several hundred CDMG accelerographs, each having three components of motion, and the Office of Strong Motion Studies completed the entire analysis in about a year. In short, the old Caltech reports are now sort of like a fossil dinosaur in the early history of earthquake engineering.

Highlights of Strong Motion Programs

Scott: Would you now sum up the highlights of the strong motion work in California? You have indicated that it started in the 1930s with the Seismological Field Survey.

Housner: Yes, in the 1930s the first "Montana" accelerographs were constructed and installed by the Seismological Field Survey. Then the first commercial accelerographs

became available in the early 1960s, and were installed for special purposes by Caltech, Los Angeles County Flood Control District, Southern California Edison Company, and other agencies.

The next significant step was the 1965 ordinance by the City of Los Angeles requiring new buildings over 10-stories high to have three accelerographs installed in them, and about 500 buildings were instrumented in due course. Then the U.S. Geological Survey began its program of installing strong motion accelerographs, some in California and some in the eastern part of the United States.

Establishment of the California State Strong Motion Program as a unit in the California Division of Mines and Geology came next. It was set up after the San Fernando earthquake, and funded by a very small addition to building permit fees in the state. Since facilities like bridges and dams did not involve a building fee, it was necessary for their agencies to provide funds for instrumenting such structures.

Scott: Yes, I recall something of that program's origin. The value of the strong motion records obtained from the San Fernando earthquake convinced groups advising the Joint Committee on Seismic Safety—the "Alquist Committee"—to push for a statewide program of strong motion instrumentation for selected new buildings. It was established about 1973 as a continuing program. Later on, I believe, the Los Angeles city program was incorporated into the state program, along with at least some of the other city programs.

Housner: The state program is overseen by the Seismic Safety Commission, which

appointed an oversight committee and a number of subcommittees to advise on the different facets of installation: ground motion, building motions, bridge responses, dam responses, etc. The SSC subcommittees also advise on which structures to instrument. One remaining problem is the fact that accelerograms recorded by instruments that are not part of the CDMG or USGS programs are not automatically put in the general data bank.

I should point out that the availability of the Kinometrics accelerograph made this instrument program possible. The instruments required by the City of Los Angeles are still in place, but we don't know if all are still operational. Selected Northridge earthquake records were located and processed by Bob Nigbor of Agabian Associates, who had been hired for that job by the Office of Strong Motion Studies. I think that gives a good summary picture of the California program.

Disseminating Strong Motion Records

Scott: I know you have long been interested in the rapid dissemination of strong motion records to potential users. Would you say something about this activity?

Housner: In earlier years we often had a long wait—six months to a year or even longer—before seeing accelerograms that were recorded in a damaging earthquake. When I was on the advisory committee to the state's Office of Strong Motion Studies, I became more conscious of fact that concern is at its height and many decisions have to be made in the few weeks after a significant earthquake.

For example, building owners in the area shaken will probably ask engineers for advice. But without an accelerogram of the earthquake motion, the engineer is hampered in giving such advice.

I recommended that Tony Shakal and the state strong motion group try to get out such information on California earthquakes very quickly. They have since been issuing what they call Quick Reports. Immediately after an earthquake, the most significant records are picked up and processed. Then accelerograms are printed and faxed to selected people, who in turn can fax copies to other selected individuals. Thus, within two or three days of an earthquake, California engineers would know what the ground motions were. The Office of Strong Motion Studies has been extremely efficient in getting the Quick Reports out, and deserves a lot of credit for its accomplishment. So this service is proving extremely valuable, and Tony Shakal is now thinking of putting their data base on the World Wide Web. If that is done, anyone with the proper equipment could call in from anywhere in the world and get the information.

Scott: Were you one of the early ones on the list to receive information and then to disseminate it again?

Housner: Yes. At first I sent copies only to selected people in southern California. But I then realized that earthquake engineers in other countries would also be very much interested in such information, and started sending copies to Japan, China, New Zealand, Peru, Venezuela, etc. I believe the Office of Strong Motion Studies has now greatly expanded the

list of those to whom to fax the Quick Reports. Although the Northridge earthquake generated hundreds of accelerograms, that office handled the job very well, successively disseminating six Quick Reports, the first on January 19, 1994, two days after the earthquake.

Scott: Are other countries likely to develop such programs of their own, building on California's example?

Housner: Yes, and already some of the Japanese agencies are issuing what they call Prompt Reports. Two years ago I proposed to IAEE [International Association for Earthquake Engineering] that all association members participate in a program of issuing such quick reports, and the idea was approved at a meeting of the IAEE board of directors.

Scott: About 40 countries are members of IAEE—do you expect most or all of them to participate?

Housner: Yes, I do, and now with the World Wide Web an even better system can be developed. A few days after the Kobe earthquake, the Japan Railways put a brief description of it on the Web, along with a list of peak accelerations they had recorded on 20 accelerographs. IAEE's past president Thomas Paulay, Sheldon Cherry, the current president, and Tsuneo Katayama, the general secretary, have agreed that a standard procedure should be developed for using the World Wide Web to put out information about destructive earthquakes in any country that suffers such an event.

Wilfred (Bill) Iwan of Caltech chairs IAEE's International Strong Motion Array Council, which is working to prepare such a standard procedure listing essential items to be put on

the Web, including, of course, accelerograms and specifying how the information should be placed on the Web so that everyone would know how to find it. This should be a very significant step in treating earthquakes as the worldwide problem they really are. Our past tendency, in contrast, has treated California earthquakes as a California problem, Japanese earthquakes as a problem for Japan, New Zealand earthquakes as a New Zealand problem, and so forth. In reality, every country's earthquakes are important opportunities for us all to learn more. Unfortunately, for a time the Northridge and Kobe earthquakes disrupted the effort to put this kind of information on the WWW, but the project is again proceeding.

Some Continuing Problems Acknowledged

Housner: In addition to noting our successes with strong motion records, I also want to emphasize that there are often unforeseen problems, and there have been some unfortunate experiences with records along the way. San Francisco BART's trans-Bay tube was instrumented, but then maintenance was stopped before the Loma Prieta earthquake, and no records were obtained. Something similar happened with an offshore oil drilling platform near Santa Barbara. Paul Jennings and I were consultants and recommended that Chevron put some seismic instruments on one of the early ones they built. They did, but then an earthquake came and there were no records. It turned out that every time a ship came from shore and tied up at the platform, the banging of the ship set off the recorders. So they got records of a lot of bangs from the ships, but the

recorders had exhausted the supply of film before the earthquake came. There were some other experiences with instrumentation—such as in the Venezuela earthquake of 1967. There was an accelerograph in Caracas, but when the earthquake came, the battery was dead.

A recent example involved the cracked-joints affair in the 1994 Northridge earthquake. Surprisingly, none of the buildings found to have cracked steel joints had been instrumented under the state's strong motion program. It would have been extremely helpful to have those records, which would give a clear idea of the forces at work when the joints cracked. But the program's advisory committee of engineers had said, "No, don't instrument any steel buildings because that is a solved problem."

Scott: Before the Northridge earthquake it was widely believed that modern steel buildings designed according to good engineering practice and using current state-of-the-art know-how should be reasonably earthquake resistant. But I did not know that the state strong motion program had been steered away from instrumenting those buildings.

Housner: I think this illustrates how the thinking of practicing engineers differs from the thinking of academics. When designing a structure, an engineer must make many decisions, which depend on his judgment, and on the information available to him at the time. In order to do this, an engineer must feel confident that he is making the right decision. On the other hand, practicing engineers often criticize academics for not making decisions, but instead wanting to check and verify.

Scott: I see your point. The practicing engineers did not really see steel construction as a problem. So they understandably wanted to focus the instrumentation more on other kinds of structures where they thought problems were more likely to occur.

Housner: Yes, whereas academic researchers would want evidence to prove that welded steel was not a problem.

Coast and Geodetic Survey Transferred to USGS

Housner: After the NEHRP program was set up in 1977 there was a major reshuffling of activities. The earthquake program of the Coast and Geodetic Survey was dismantled, and the U.S. Geological Survey was designated as the federal government's seismological agency. So the work of the Seismological Field Survey was transferred to USGS. Bill Cloud retired at this time, and passed away prematurely a few years later. USGS hired Fritz Matthiesen, who had been on the staff at UCLA, to head the strong motion program, which was based at the USGS western headquarters in Menlo Park. Then Matthiesen passed away prematurely, and Gerry Brady took over the program until he left it in 1995. The USGS program concentrates on recording ground motions. The California state program of CDMG concentrates on satisfying the strong motion data needs of engineers.

Development of Seismic Codes

“While the static approach was, of course, better than nothing, it was not very realistic—especially in dealing with taller buildings.”

After the 1925 Earthquake: The Chamber of Commerce Effort

Housner: I remember Martel telling me that after the Santa Barbara earthquake in 1925, some enthusiasm was generated for setting up a state building code. I am not clear on who sponsored it. They actually did put a code together, and Martel worked on the earthquake part of it, but he said they could never get anybody to adopt it.

Scott: Yes, that would be the code finally published in 1939 by the California State Chamber of Commerce. It is rarely referred to because as you note nobody ever adopted it, so it never seemed to lead anywhere. The work leading up to it was apparently quite good for its time, and was used very effectively, especially in implementing the Field Act, passed in 1933.²⁹

Long Beach Earthquake, 1933

Housner: In any event, there was no seismic code in California until after the Long Beach earthquake of March 10,

1933. It took that earthquake to get the cities interested in seismic codes. Its magnitude was 6.2, and it was on the Newport-Inglewood fault, which goes right through the City of Long Beach. The slip ran through Long Beach and up through Compton. The earthquake did a lot of damage to old brick buildings, and least 200 people were killed. Many school buildings were severely damaged and some of them collapsed. Fortunately, classes were not in session at the time—5:45 p.m., Friday afternoon.

There are photographs of some of the streets covered by piles of brick. It was a significant earthquake, and provided the motivation for the state saying afterward that you could not build unreinforced brick buildings anymore. Also, following the Long Beach earthquake the

29. Work on the Chamber of Commerce project was initiated a year or two after the 1925 earthquake. The effort was prompted partly by concern that earthquake fears would cause construction in California to drop precipitously, unless investors got some assurance that future structures would be earthquake-resistant. The work was done by committees numbering over one hundred members, representing state and local associations of architects, civil engineers and contractors. At first the effort apparently went well, but at some point slowed down. Perhaps northern California engineer Henry Dewell's incapacitating stroke in 1932 was a contributing factor, as he was playing one of the key roles. Nevertheless, by 1933 the material was sufficiently complete to be used in implementing the Field Act, passed after the Long Beach earthquake. Los Angeles and many other cities adopted the 10 percent method of design after the 1933 event. Meanwhile, work continued on the Chamber of Commerce code, although apparently only intermittently, and the code was not published until 1939. Even then certain issues remained unresolved, so that there were two versions for lateral forces. (*California State Chamber of Commerce, Building Code for California*, ed. Edwin Bergstrom, 1939.)

state legislature passed the Field Act, which required all public school buildings to be designed to resist earthquakes.

Scott: I believe unreinforced brick masonry was outlawed principally through the Riley Act, which was passed in response to the Long Beach earthquake. Among other things, it imposed a 2 percent g lateral force requirement.

Housner: I do not recall just how that was done. In 1934, when I came here to Caltech as a student there was still a lot of excitement in engineering circles about the Long Beach earthquake, and Professor Martel was very much interested. He gave night courses to engineers on how to design for earthquakes, and as students we were also involved in those. As John Freeman had said, there was nothing in any of the engineering books that talked about earthquakes. Engineers were accustomed to thinking mostly of constant gravity loads that push straight down. To deal with earthquake resistance, engineers had to think about a structure getting pushed sideways by seismic forces. What should they do? These were engineers who had been out practicing for 20 years or so. It took a lot of talking by Martel to explain how best to make the calculations for bending moments in beams and columns, shear stresses in walls, etc.

Anyway after the Long Beach earthquake Professor Martel and his students undertook research on the damage in Long Beach. They made surveys of different damaged buildings, located on different types of ground. They collected information on how many buildings in Long Beach were badly damaged, how many

were moderately damaged, whether they were brick or wood, what sort of ground they were on, and information like that. As far as I know, that was the first time that sort of thing had been done in the U.S. after an earthquake. Martel got all his students together, and they set off and did the job. They did not wait for funding, they just did the survey. Students who participated in the work at that time were Bill Moore, Trent Dames, and Ralph McLean. I've forgotten now who else participated.

Scott: That work was published, was it not?

Housner: Yes. In 1936 a compilation came out including much of the work done following the Long Beach earthquake, and contributions from R.R. Martel, along with many other now-familiar names—Hugo Benioff, Rueben Binder, John Blume, Perry Byerly, Dean Carder, Clarence Derrick, Beno Gutenberg, Nicholas Hunter Heck, Lydik Jacobsen, Ralph McLean, William Moore, Frank Neumann, Charles Richter, Franklin Ulrich, and Charles Wailes. This was a very good report for its time, and had an appreciable amount of material that engineers found interesting.³⁰

Several years ago Karl Steinbrugge wrote saying that he had in his possession the data on the insurance claims that came out of that earthquake. He asked if we still had the card index files that Martel accumulated containing his data on the buildings and the damage. But after 50 years we did not have that data, and of course the published report did not include the raw data.

Los Angeles Post-1933 Code: Static Requirement

Housner: After the 1933 earthquake, the City of Los Angeles immediately amended its building code to require that all buildings be designed for 8 percent g base shear coefficient, except for school buildings, which were to be designed for 10 percent (a state requirement). Before the static approach was adopted, nothing at all had been done. This approach was copied from the Italians, who had developed it after the 1908 Messina earthquake.

The Italian government appointed a panel of practicing engineers and professors, and they basically specified static lateral forces, proportional to the weight, applied to the side of the building. The best place to find more information on what the Italians did is in John R. Freeman's 1932 book, *Earthquake Damage and Earthquake Insurance*.³¹ Essentially this approach reduced a problem of dynamics to a simplified approach that assumed static forces applied to the side of the building. I believe that M. Panetti, a professor at Milan University, was responsible for this approach, as he was the youngest member of the panel and was professor of applied mechanics.

The original static requirement was simply for a horizontal force of 8 or 10 percent of the weight of each element of the building. It is the same, in effect, as tilting the building to the point where there is a 10 percent gravity pull to the side—that is where the percent g came in. To clarify this to students and to the engineers,

30. U.S. Coast and Geodetic Survey, *Earthquake Investigations in the Western United States, 1934-1935*. Special Publication no. 201, 1936.

31. Freeman, John R., *Earthquake Damage and Earthquake Insurance*. McGraw-Hill, New York, 1932.

it was pointed out that they were essentially having to deal with a gravity force of 10 percent acting horizontally, but this was never stated in the code. In short, the seismic code provisions that generally continued to be used until the mid-1940s were based on the static provisions developed in Italy after the 1908 Messina earthquake. The Japanese adopted the 10 percent *g* approach following the 1923 earthquake in Tokyo, Japan, and later it was increased to 20 percent *g*, but I do not know what stresses and strains were permitted.

Scott: Do you think our own adoption of the 10 percent *g* approach after the 1933 earthquake was based in part on the Japanese code? The Japanese experience was a major influence on the U.S., and people like Martel and Freeman who visited Japan considered the earthquake work of the Japanese very important.

Housner: That is true, and the U.S. engineers could have been influenced, but both the Japanese code and our code were based on the Italian code. In any event, the 10 percent *g* lateral force design was quite a good approach for low buildings of one to three stories, which are so stiff that vibration does not come in to play. Later, however, after we learned more about the dynamics of structures, we could see that it was not a good system for general use. While the static approach was, of course, much better than nothing, it was not very realistic—especially in dealing with taller buildings. The approach also becomes impractical as buildings go higher. Trying to design a 50-story building for a static force of 10 percent *g* would be difficult to impossible.

Because of dynamics, the percentage requirement on anything over about two or three stories should be based on the properties of the structure—the natural period of vibration and damping. Up until World War II, however, there was very little interest in earthquake engineering to improve the situation. In 1943, however, a formula was developed for the Los Angeles code, specifying how the force was to attenuate with the height of the building. This was put in on the basis of the research that was done at Caltech. It was the first step beyond the constant percent *g* lateral force requirement in the Los Angeles code.

Scott: Incidentally, the 1939 Chamber of Commerce code mentioned earlier as never adopted did include a departure from the flat 10 percent *g* lateral force requirement. Michael Pregnoff first mentioned this to me in his oral history interviews.³²

Housner: Yes, that was an unusual modification of the percent *g* approach. It assumed that

32. In his EERI oral history, San Francisco structural engineer Michael V. Pregnoff described the 1939 code's lateral force idea: "...The 1939 code proposed a peculiar way to design buildings for lateral forces due to earthquakes. What they did is this. Say you are designing a tall building. At the top two levels—the roof and the next level down—you use a lateral force of 8 percent of dead load (DL) plus live load (LL). At the next levels—the third and fourth levels down—use 6 percent of DL plus LL; at the fifth and sixth levels, use 4 percent of DL plus LL. At all levels below the sixth one down—counting from and including the roof—use 2 percent of DL plus LL. The lateral force resistance at each level would be equal to a percent of the dead load plus live load adjacent to those levels. [Pregnoff adds]... their way to resist earthquake forces was not bad." *Pregnoff/Rinne, Connections: The EERI Oral History Series*, 1994, p. 48, also see Appendix.

a one- or two-story building would be designed for 8 percent and taller buildings for a smaller percent.

Seismic Code Development in the 1940s and 1950s

Los Angeles Code Change, 1943

Housner: As you know, building regulation within cities is done through the city code. Los Angeles had its own code, as did San Francisco and some of the other larger cities, while the smaller cities all adopted the Uniform Building Code (UBC). It was the Los Angeles code that sort of led the way, it being the biggest city in California.

In 1943, during the war years, Los Angeles adopted a code change that indirectly took into account the effect of the natural period of vibration. The change also varied the force requirement with the height of a building. This was based on research that had been done at Caltech, and was worked out only for buildings that went up to 150 feet in height—which essentially meant 13 stories, which was then the uniform height limit for buildings in the City of Los Angeles. No buildings could be built over 150 feet, and the new formulation had been designed to apply to buildings constructed up to but not exceeding that height.

Scott: Do you know some of the background of that?

Housner: That was done during the war when I was away. We had computed spectra using the torsion pendulum I mentioned earlier. It showed how the base shear force ought

to depend on the period of vibration, or how tall the building was.

John Blume gave you a copy of a letter by Professor Martel that provides more background on the Los Angeles code revision. In a letter dated June 27, 1946, Martel wrote to George S. Hill of the San Francisco Department of Public Works, regarding the way the Los Angeles building code requirements were drafted and put into effect. The San Francisco department evidently had previously written Martel and by implication had attributed aspects of the Los Angeles code to him.

Scott: Yes, in the letter Martel seems to be disavowing any direct responsibility, saying in effect, "I didn't really do it."

Housner: Martel said it was done by a committee, which he believed was Ernie Maag, Steve Barnes, Henry Bolin, Murray Erick, and Clarence Derrick, who were practicing engineers in the Los Angeles area. But the work of the committee would have been based on the various studies we had made here at Caltech. Martel's letter to Hill says:

I hold no brief for the specific values used in the "formula." Since these values in the form of the formula are arbitrary, San Francisco might want to express its individuality, if so, I would suggest the following: F equals 15 over the square root of N . [N is the number of stories.]

At the war's end I came back and was reviewing what had been done. Martel was of course a key person involved, and I asked him, "Why did you specify the shear force, instead of the forces

on the building?" He said, "Well, we thought that if we specified the forces on the building, the engineers would think it was a statics problem they were dealing with. Doing it with shear forces would make them realize that it was not just a simple static problem." So this represented a way of making it clear that the method was not based on a simple approach.

At that time, Los Angeles was the leader in seismic code development. Los Angeles city was big enough and their building department had enough expertise to handle such things as taking the lead on code development. Whereas most other places were like Pasadena, whose building department had no expertise in earthquakes. The usual pattern was that Los Angeles city adopted a code improvement, and a few years later it would get into the Uniform Building Code, the accepted code in California and the West, adopted and published by the International Conference of Building Officials. In that way, it would be applied by the smaller cities.

The local practicing engineers also played a key role. Thus, the Structural Engineers Association of Southern California had a committee looking at earthquake design, which would write up recommendations that would then go to the Los Angeles city building department. If the department bought it, they would try to get it adopted by the city council. If it was adopted, sometime later it would be considered and probably adopted by ICBO.

It is difficult to make major code changes, because they affect so many different people and agencies. For example, a proposal to change the code will affect interests all down the line—owners of buildings, suppliers of materials, contractors, everything. So it is very

difficult to make substantial changes in the seismic code, except right after an earthquake.

It is also true that the total amount of money available for constructing new buildings is essentially fixed, so if there is a significant increase in cost, fewer buildings will be built.

San Francisco's First Seismic Code, 1948

Housner: Unlike Los Angeles, San Francisco did not have any earthquake provisions in its building code until after World War II. It was unfortunate that San Francisco was so tardy about getting earthquake requirements in their code. They should have done something in 1933, but for some 15 years more they continued putting up buildings without specific earthquake requirements. They did not have a seismic code until the late 1940s, when they put in a lateral force requirement, due I believe especially to pressures exerted by Harold Engle, who held the position with the Pacific Fire Rating Bureau that Karl Steinbrugge later held.

Scott: There was a lot of controversy about the code change, which left the San Francisco engineers divided. So the engineers in the Bay Area formed a group, and with the guidance of John Blume, John Rinne and others, developed a procedure that was published as an ASCE paper, and was often referred to as "*Separate 66*." (A "*Separate*" was what we would now call a reprint of an article.) A modified form of *Separate 66* was later adopted in the code.

Housner: Yes, San Francisco's first seismic code was adopted in 1948—the so-called "Harry Vensano" code—and was pretty controversial.³³ The story is that San Francisco's

building official, Harry Vensano, who as a young engineer had first-hand experience in 1906 with severe earthquake damage of his own office, very much wanted San Francisco to have a strong seismic code. He wrote it himself, and it was apparently rather unique.

Separate 66: The 1951 Northern California Report

Scott: So basically "*Separate 66*," the Joint Committee report of 1951, grew out of the controversy in San Francisco sparked by the Vensano code.³⁴ A joint committee of the San Francisco section of ASCE and the Structural Engineers Association of Northern California was set up to try to get the northern California structural engineers closer to a consensus on seismic codes. This paper led to changes in the codes.

33. Both Henry Degenkolb and John Blume commented on Vensano and his code in their EERI oral histories. Degenkolb observed, "...Vensano wanted higher earthquake coefficients than had been common before this in northern California, and higher than were prescribed for buildings by the Los Angeles code. In this, he was seconded by Harold Engle and Lydik Jacobsen. The vast majority of northern California engineers thought that the coefficients were too high and argued for lower values." John Blume comments that the Vensano lateral force values ranged from 8.0 to 3.7 percent, depending on height. Apparently Vensano also changed some of the unit values for steel so they were different from the national standard values set by AISC (American Institute of Steel Construction), making his code even more controversial. The continuing unrest among the San Francisco area engineers prompted them to try to reach general agreement through a joint committee representing the American Society of Civil Engineers, San Francisco Section, and the Structural Engineers of Northern California. *Connections: The EERI Oral History Series: Degenkolb, 1994; Blume, 1994.*

Housner: Yes, the difference of opinion centered on the lateral base shear force requirement. The 1943 Los Angeles building code was an example for the development of *Separate 66*. I mention the following two differences between the Los Angeles code and *Separate 66*: 1.) the L.A. code had a height limit of 150 feet, whereas *Separate 66* had no height limit; 2.) in *Separate 66*, the distribution of lateral forces over the height of a building was "triangular," whereas the Los Angeles code's distribution increased more towards the top of a building. *Separate 66* reduced the design forces by about one half. Both the Los Angeles and *Separate 66* requirements had the same deficiencies. During an earthquake a building vibrates back and forth, but both codes treated the problem as one of statics, which was misleading. Also, the forces prescribed by the Los Angeles code and by *Separate 66* were very much smaller than the actual forces produced by a strong earthquake, a fact that neither the Los Angeles code nor *Separate 66* recognized. Neither code took into account the role of inelastic deformation in withstanding seismic motions. Engineers now

34. The *Separate 66* report was the result of a major consensus-building effort among structural engineers in northern California to try to resolve growing concerns about the cost and rigidity of existing seismic requirements. Its drafters described the method as determining the total lateral force or the base shear transmitted into the structure from the ground, and the distribution of that shear as equivalent forces applied to the structure. Anderson, Arthur W., John A. Blume, Henry J. Degenkolb, Harold B. Hammill, Edward M. Knapik, Henry L. Marchand, Henry C. Powers, John E. Rinne, George A. Sedgwick, and Harold O. Sjoberg, "Lateral Forces of Earthquake and Wind," *Proceedings, American Society of Civil Engineers*, v. 77, *Separate No. 66*, April 1951.

know that this is a critical element in earthquake resistance.

Static vs. Dynamic

Scott: One of the main objectives of the *Separate 66* effort was to agree on realistic lateral force requirements, taking building height into account. Those who wrote the report, including John Rinne, the chairman, considered it to be, in effect, a dynamic approach. But the Caltech group critique that you and Martel wrote at the time *Separate 66* was first published suggested that it did not really represent a dynamic approach. For his part, however, Rinne emphasized that their work was grounded partly on Maurice Biot's early efforts to use dynamic concepts in studying earthquake motion.

Housner: This view seems to be based on a misunderstanding of the problem. *Separate 66* did not prescribe "realistic forces" but prescribed forces that were much smaller than those produced by strong ground shaking. The San Fernando earthquake demonstrated that the seismic forces in buildings during the earthquake greatly exceeded the code-prescribed forces, and this could no longer be overlooked by engineers and code officials.

Rinne notes that *Separate 66* was based on Maurice Biot's response spectrum calculated for the 1935 Helena, Montana earthquake. Also the Los Angeles code at that time had been based on spectra calculated at Caltech in 1940 for some larger earthquakes. This does not, however, make either of these approaches dynamic. Both *Separate 66* and the Los Angeles code specified static horizontal forces, and this is a statics approach.

Scott: Was this the principal basis for your critique of *Separate 66*?

Housner: The statics approach is one criticism. Another criticism was that the prescribed seismic forces did not take into account the real forces that an earthquake would generate. This discrepancy between code forces and real earthquake forces should be taken into account by considering the need to accommodate ductile deformations. Because of this, many pre-1971 buildings are deficient in seismic resistance, a fact that is now recognized by the engineering community. The triangular distribution of forces in *Separate 66* was deficient in the upper parts of a building, and this was later modified by specifying a constant lateral force acting at the roof of a building.

Scott: From what you have said, I take it that some of the Caltech group's criticisms of *Separate 66* would have applied about equally to the 1943 Los Angeles code provision, done a few years before *Separate 66*?

Housner: Yes. Both had seismic forces that were too small, and did not consider the role of inelastic deformations. The 1971 San Fernando earthquake records clearly demonstrated this to the Los Angeles and the San Francisco engineers.

Scott: You clearly consider a static approach to earthquake design somewhat misleading. Would you elaborate on that a bit more?

Housner: The discipline of mechanics is in two parts. One part, called "statics," does not involve any motion. The other part, called "dynamics," does involve motion and the resultant inertia forces. For example, it is a statics problem to compute the stresses in a

beam acted upon by a constant force and at rest. In contrast, it is a dynamics problem to compute the stresses in a beam that is vibrating under the action of a varying force. That is a much more complex problem than a simple statics problem.

For example, during the 1989 Loma Prieta earthquake, an instrumented 47-story building in San Francisco experienced ground shaking in its basement that had 10 percent *g* peak acceleration, and on the roof the peak recorded acceleration was 48 percent *g*. The forces in the building were dominated by the *third* mode of vibration. There is no way to relate the triangular distribution to the forces that were developed during the earthquake.³⁵

"Triangular" Distribution

Scott: You have mentioned the triangular distribution of forces a couple of times. When I was interviewing John Rinne, he suggested asking whether you think the vertical distribution of lateral forces in low buildings, up to say five stories, should be based on something other than the triangular distribution of forces developed in *Separate 66* and now specified in codes? He also suggested that I ask if you consider

35. Triangular distribution refers to the distribution of horizontal design forces over the height of a building. The forces were greatest at the top of the building, and were reduced uniformly to zero at the base of the building, thus having the shape of an inverted triangle. The 1939 Chamber of Commerce Code had used a number of steps that roughly approximated this downward scaling relationship. *Separate 66* developed the idea of the triangular distribution, basing it in part on analyses of the Alexander Building in San Francisco.

sinusoidal distribution to be justified by analyses that you know of?

Housner: It is difficult to give an oral description that compares the seismic forces, so I prepared a sheet with three diagrams showing distributions under the never-adopted 1939 State Chamber of Commerce Code (Diagram A), the 1943 Los Angeles building code (Diagram B), and *Separate 66* (Diagram C). All three diagrams are for a 13-story building, and the base shear coefficient is shown on each. All three, and especially *Separate 66*, are for forces much less than the true earthquake forces.

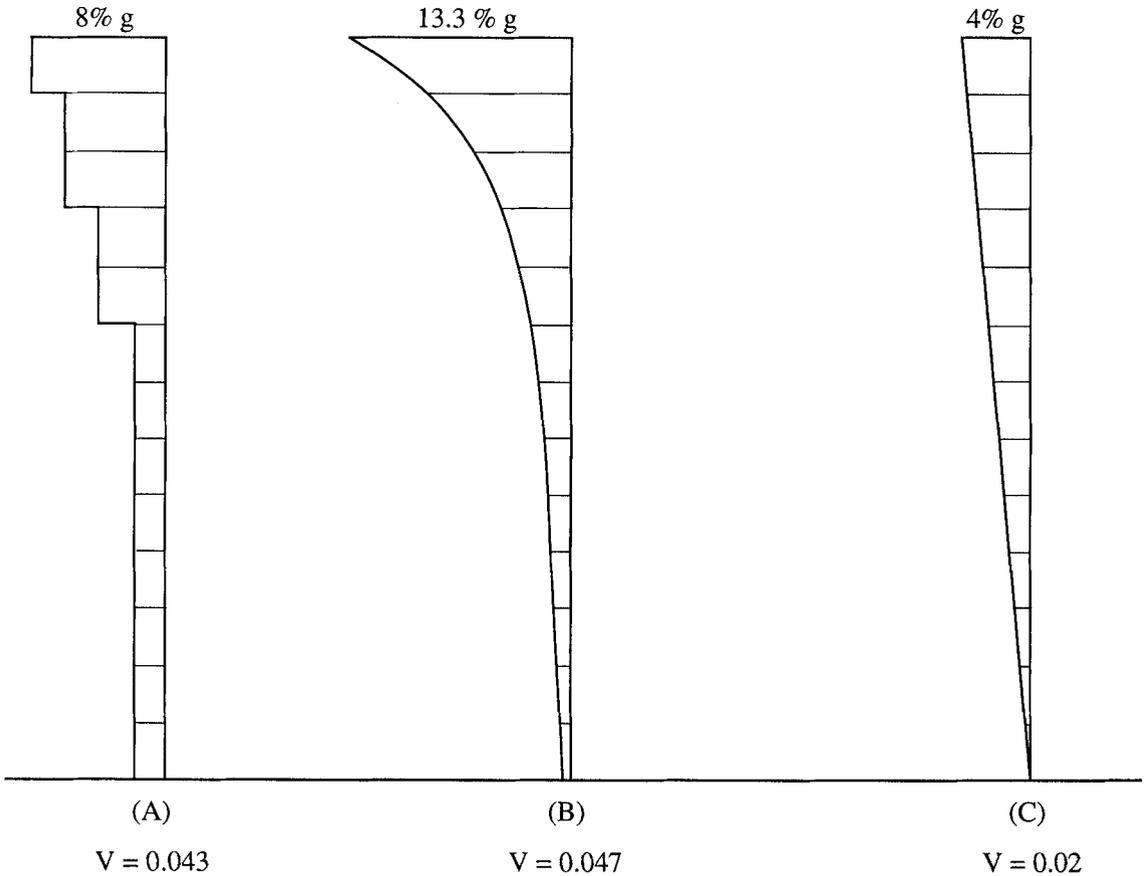
Regarding Rinne's question on the triangular distribution, I do not think it is good for low, stiff buildings—a sinusoidal distribution would be better, and so would a uniform distribution.

Conclusions on Separate 66

Housner: My feeling is that *Separate 66* and the ensuing code were defective and misleading in that they led engineers to think of the earthquake problem as a simple, low-force statics problem, instead of a high-force dynamics problem. Also this led to the design of nonductile buildings, which are now recognized as posing an earthquake safety problem.

The present code is a step in the right direction in prescribing larger forces and focusing on ductile behavior. I should also point out that it is now common to make a true dynamic analysis of highrise buildings and to take inelastic deformation into account.

Scott: The authors of *Separate 66* were sensitive to the criticisms. Rinne discussed *Separate 66* in some detail in his oral history. He acknowledged the gap between calculated



Comparisons of seismic code forces, pre-1960.

motions based on observed shaking, and the actual forces used in seismic codes. He also indicated that they relied heavily on actual observations of building performance. Furthermore he emphasized the importance in design of engineering judgment.

Housner: As to the observed performance of buildings, the San Francisco area did not experience strong ground shaking from 1906 until 1989. Today, I believe there is a much better meeting of minds, and that contemporary prac-

ticing engineers now have a pretty good understanding of the performance of buildings during earthquakes, and often design major structures on the basis of a dynamic analysis.

Scott: I think Rinne also saw the earlier controversy as past history, and in fact observed in his oral history interview: "We have progressed so rapidly and so far in various aspects of earthquake analysis, that comments on the critique by Professor Martel and his associates no longer seem to be in order at this stage."

Housner: Yes, the engineers have made a lot of progress over the years. I would like, however, to emphasize that by prescribing the design forces and allowable stresses, the code requirements were really specifying the elastic strength of the structure, and this does not represent the actual earthquake forces and strains.

Los Angeles: Dynamic Analysis of Taller Buildings

Los Angeles Height Limit Removal, 1957

Scott: You have mentioned the 13-story height limit in the City of Los Angeles a time or two. Would you say a little more about it here? Taller buildings began to go up after the limit was removed, and subsequently dynamic analysis came into use for such structures.

Housner: Yes, I noted before how the code change made in the mid-1940s was developed only for buildings up to 150 feet in height—essentially 13 stories or less—which was then the city's uniform height limit for buildings. Until that ceiling was removed, no buildings could be built over 150 feet, except the 30-story city hall.

Scott: I believe the purpose of the Los Angeles height limit was to prevent the development of downtown "canyons"—to avoid "Manhattanizing" Los Angeles. Thus, it was a matter of urban planning, rather than safety.

Housner: That is right, the 13-story height limit was established in the early years of this century and had nothing to do with earthquakes. It was a zoning matter, adopted for urban design considerations. Later there were pressures to put up taller buildings, and in 1957

they changed the zoning to allow that. Instead of the old fixed height limit, they put limits on the amount of floor area that could be built on a lot, by establishing several "height districts." That is why the taller buildings you now see in Los Angeles are surrounded by open space.³⁶

Code Requirement, 1973

Scott: As I understand it, a dynamic analysis is now done for all the taller buildings, tailor-made for each building.

Housner: Yes. As I mentioned earlier, Los Angeles had passed an ordinance requiring that tall buildings be instrumented. So a lot of records of strong ground motions and building responses became available after the 1971 San Fernando earthquake. Access to that San Fernando earthquake motion data enabled us to get records of the motion at the base of buildings, and also in the upper parts. That made it possible to take the motion at the base of the building, calculate the response, and see if the calculated results agreed with what was measured.

For example, we did this calculation for the Engineering Building at the Jet Propulsion Laboratory. Fortunately, we had installed accelerographs just for this purpose. Through such calculations you could pretty much reproduce the motion that was actually measured at the top of the building. This confirmed the

36. Height District No. 4, the one allowing the tallest buildings, provided that the total floor area in all the main buildings on a lot should not exceed 13 times the lot's buildable area. The other height districts had similar provisions with smaller multipliers.

method of design we had advised on earlier for the Union Bank Building.

In 1973, Los Angeles responded to the earthquake by modifying the city code to say that if a building is over 16 stories high (160 feet) it must be designed on the basis of a dynamic analysis. That meant the big buildings of 40 or 50 stories were done by computing the response of the building, whereas buildings of say 20 stories were probably done on the basis of the design spectrum approach, and buildings less than 16 stories in height were designed using forces prescribed by the code. To do dynamic analysis the engineers have to determine what magnitude earthquakes might occur in the region, taking into account the distance from the site, and what shaking might be at the site. Then they calculate the response of the building and design on that basis.

Scott: They cannot just use code forces.

Housner: That's right. In those circumstances the code does not simply say, "Use these forces or these stresses." So the engineer has to make a dynamic analysis, using either a design spectrum or a dynamic calculation. He does that, and then he goes to the building code people and talks to them. "Does it look all right? If we do it this way, would you be happy?" The Los Angeles Department of Building and Safety has the computing capability to check the dynamic analysis.

This change in Los Angeles came about because the people in the Department of Building and Safety saw that designers needed to know more and do more than follow the code. I think they also felt that by making the engineers think this through, they would have a

better understanding of what the buildings would do. There's no doubt that we are now getting better buildings because of that. That requirement is for highrise structures. I should note that even before the requirement was enacted, a small number of highrise buildings were designed with dynamic analysis between 1957 and 1973.

The earthquake records obtained in 1971 also confirmed that the seismic forces in buildings could actually be much larger than the forces prescribed in the pre-earthquake seismic code.³⁷ It thus became clear that the disparity between the code forces and the actual earthquake forces can be reconciled only by recognizing that during strong ground shaking a building will exceed the yield point stress and will undergo plastic deformation. The code was later modified to take that into account.

Scott: How was that done?

Housner: Larger seismic forces were specified that were more compatible with actual earthquake forces. The code also prescribes reduction factors permitted in the design forces. The reduction factor indicates the amount of plastic deformation that could take place. The reduction factor indicates to the designer how much ductile deformation he must take into account in his design. In my view this was a very significant code change. (Of course, small code changes take place almost every year.)

37. Housner and Jennings, *Earthquake Design Criteria of Structures*, EERI, Oakland, CA, 1982.

Advising on the Union Bank Building

Scott: You mentioned advising on one major building that used a dynamic approach, could you say a word more about that?

Housner: Yes. In the early 1960s, when the Connecticut General Insurance Company wanted to build the Union Bank Building in Los Angeles, Paul Jennings and I advised on the seismic design. Our procedure was first to identify the faults in the region that could produce strong shaking at the site. One would be the San Andreas fault, which might have a magnitude 8 earthquake on it, some 35 miles from the building site. Another was the Santa Monica fault, 12 miles from the site, and it could have a magnitude 7 earthquake. We gave the engineers ground accelerations corresponding to earthquakes on those faults. But we could not give any probability of those earthquakes happening. Ed Teal was the chief engineer for that project.

Scott: You estimated probable ground acceleration from each of those earthquakes at those sources?

Housner: Yes, we gave appropriate synthetic accelerograms. So they took those and computed the dynamic responses. The actual design, with ductility factors, was done from the computed shear forces and bending moments. They also used the 1940 El Centro acceleration just to see what it would do, since it was such a famous record. Actually, they computed four earthquake motions corresponding to three different magnitudes at different distances, plus El Centro. A.C. Martin, Architects and Engineers, designed the building. This was the first time such a seismic

design approach was done in Los Angeles, but now it is common practice.

Los Angeles City Code for URM Buildings

Housner: It is very difficult to deal with the hazards created when seismic codes were weaker or nonexistent. A good example is the program for old, weak unreinforced masonry (URM) buildings, which the City of Los Angeles set up in response to the 1971 San Fernando earthquake.

Immediately after the earthquake, as I noted earlier, Los Angeles County appointed an earthquake investigation commission to look at what happened and make recommendations. Clarence Allen, Don Hudson, Charles Richter and I were some of the members. It was clear that had the San Fernando earthquake been 20 miles farther south—under the center of Los Angeles—it would have been a great disaster. We estimated that at that time there were maybe 10,000 hazardous old URM buildings in the county. The commission's 45-page report had a list of recommendations, and the Number One recommendation was—get rid of those old URM buildings, because they are so hazardous.³⁸

I made several presentations before the Los Angeles city building department committee—the department is overseen by a special committee—and also presentations to the city council, saying, "It's very hazardous, and something needs to be done." Well, the city council

38. *Report of the Los Angeles County Earthquake Commission—San Fernando Earthquake of February 9, 1971*, Los Angeles County Board of Supervisors. November, 1971.

did not want to say, "Yes, do something," because that would affect 8,000 building owners, who would then have to spend money. On the other hand, the council definitely did not want to say, "Don't do anything." It was sort of a hot potato for them, and they could not come to a decision.

The county earthquake commission report had said, "Do something with the old buildings," but also acknowledged that it would take time. "We realize this can't be achieved instantly, but if you make a vigorous effort you can solve the problem in 10 years." It was not until 10 years later, however, that the Los Angeles city council first passed an ordinance initiating a URM building retrofit program.

Scott: The city ordinance was enacted approximately 10 years after the county earthquake commission's report had said the whole job could be solved in ten years?

Housner: Yes. And then they did not put any time limit on getting the job done. What the building department did was identify all of the URM buildings, and then assign them priorities according to risk. The larger and more important buildings, and those with more people in them, received the top priority and their owners were notified first. The department sent letters and compliance orders to the affected owners. The time limit for a building was not set until the notification and compliance order were sent.

A compliance order let an owner know that he had an unreinforced masonry building, and gave him options as to what could be done. They'd say, "You must get an engineer to look at the earthquake resistance of your building

and report on whether it's all right." The engineer had to look at the building and figure out what had to be done to bring it up to the established earthquake standards. The report to the building department was to include plans on how the retrofit work was going to be accomplished. For a few years the program went on that way, and they got maybe 200 buildings fixed. A few of them were torn down. But there wasn't any big fuss about the program, and at that rate it would take a long time to complete.

Scott: Yes. As I understand it from talking to Earl Schwartz, the Department of Building and Safety engineer responsible for setting up the program, it took them a good deal of time at the very beginning just to get the effort funded, staffed, and up and running. At the outset I think they intended it to be about a 15-year program, but after the relatively slow start-up Earl said they could see that it was likely to take about 20 years. After all, the earlier parapet program had taken 20 years, and it only covered parapets. Also, as Earl observed, the program "was handled as a low-key kind of thing in order not to get everybody excited and maybe start a thrust to do away with it."

Housner: Yes. Then came the 1985 Mexico City earthquake, and the city council wanted the pace accelerated. So the remainder of the owner notifications and compliance orders were sent out within six months to a year.

So it was not until 15 years after the San Fernando earthquake that the city in effect adopted something like the 10-year deadline that the earthquake commission report had originally recommended. So there was a vigorous program, and there wasn't any serious

complaint. Of the 8,000 old buildings in Los Angeles city in 1976, about 7,500 were strengthened or demolished by 1994.

Cost is of course the big problem in getting the old buildings taken care of. Los Angeles city did not say, "Bring the building up to new construction standards," because that would be too expensive. Instead they adopted less restrictive requirements aimed at eliminating the greatest hazard—the outright collapse of the old buildings. The intent is that after being strengthened, properly retrofitted URM buildings could ride through a strong shake without collapse. This was confirmed in the Northridge earthquake.

ATC Report, 1978

Housner: In discussing codes, I also want to mention the 1978 report issued under the auspices of the Applied Technology Council (ATC).³⁹ Its designation ATC-3 indicates that it was the third project undertaken. The 500-page report was written by 110 volunteer workers divided into 22 committees. Essentially it was a model seismic code for use in all parts of the country. The document stated that ten new concepts were employed, the first being: "The incorporation of more realistic seismic ground motion intensities." The report was expected to and did influence existing seismic codes. Much of the current Uniform Building Code was derived from ATC-3.

The ATC-3 project was a remarkable effort, and was really the outcome of efforts by

Charles Thiel, then head of the Earthquake Engineering Program at the National Science Foundation (NSF), which funded the project. When Thiel received his Ph.D. degree from Purdue, I believe he then went directly to the National Science Foundation as a member of the group in charge of earthquake engineering research under Michael Gaus. When NSF set up the program called Research Applied to National Needs (RANN), Charles Thiel became the head of the earthquake engineering program, which was moved to RANN. He was not directly working on the preparation of ATC-3, but was involved in a policy-making role.

Scott: Yes, Chuck Thiel has been a remarkably imaginative person who has sparked a lot of ideas over the years. In the case of ATC's original creation, I believe he operated out of Washington, while Bay Area structural engineer Roland Sharpe and maybe a couple of others locally were prime movers behind the formation of ATC-3.

Housner: In the early 1980s, Thiel left the federal government to become a consultant in the Bay Area, and was a contributor to *Competing Against Time*,⁴⁰ the 1990 report of the Governor's Board of Inquiry on the Loma Prieta earthquake. He was also responsible for the report's format and title. Thiel played a similar role in work on the report of the Caltrans Seismic Advisory Board on the Northridge earthquake, *The Continuing Challenge*,⁴¹ issued in

39. *Tentative Provisions for the Development of Seismic Regulations for Buildings*, ATC 3-06, Applied Technology Council, 1978.

40. *Competing Against Time: Report to Governor George Deukmejian from the Governor's Board of Inquiry on the 1989 Loma Prieta Earthquake*. Governor's Office of Planning and Research, State of California, 1990.

1994. I specifically mention Thiel because he is a good example of the many people who have become very much interested in earthquake engineering, and who have contributed much time and effort to its advancement. The full 110 people involved in preparing the ATC report provide other examples.

The Outlook: A Summing Up

Scott: While in many ways we now seem to be rather sophisticated in earthquake engineering, we are nevertheless still quite vulnerable in a lot of ways. Do you think the researchers, engineers and public have a pretty realistic awareness of the hazards we live under?

Housner: I think researchers, engineers and building officials are quite knowledgeable, but I doubt that the policymakers and the public really understand the nature or magnitude of the continuing problem. In 1995 the City of Los Angeles recognized that 80,000 structures designed before 1971 represent a great hazard, and the city council is wondering what to do about the problem. Only about 1,000 of these are large nonductile structures.

I should also, however, point out that in California many special kinds of projects have long been designed on the basis of dynamic analyses, and realistic estimates of ground motions and seismic forces. Such projects, which were being done well before 1971, include highrise buildings, dams and reservoirs, State Water Project facilities, the San Francisco area's BART system, offshore oil platforms, and quite a few

others. California has been the world leader in using such methods for seismic analysis and design of special projects.

Scott: Do you have any concluding observations on the current status of seismic codes used in California? How satisfactory do you think they are now?

Housner: From the very beginning of earthquake engineering, starting with the flat 10 percent g method of design, the seismic codes underestimated the seismic stresses that strong ground motion can produce in buildings. Before the 1971 earthquake, no California buildings designed under seismic codes were subjected to strong shaking. Then strong ground shaking was recorded in the San Fernando earthquake, and the damage to code-designed buildings made it very clear that the code provisions were inadequate, forcing engineers to change their thinking.

In any event, ductile deformations such as in the columns of the Olive View Hospital, severely damaged in 1971, made some of the code's weaknesses unmistakably obvious and some significant changes were made shortly afterward. The present code is certainly a great improvement over the early code versions, but there are still problems, and revisions continue to be made after damaging earthquakes. Our knowledge of earthquake mechanisms, the nature of ground shaking, and the seismic vibrations of structures has progressed far beyond the present code, however, and the discrepancy between knowledge and the code must be bridged.

In short, code improvements must continue to be made. The damage and collapses sustained

41. *The Continuing Challenge: Report on the 1994 Northridge Earthquake*. Caltrans Seismic Advisory Board, State of California, 1994.

during the Northridge earthquake put the finger on weaknesses in the present code. The earthquake demonstrated that near the causative fault the ground shaking can be very severe, and the code should reflect this by identifying locations where this kind of thing could happen, and revising the code requirements to take this into account.

I think the code, and the engineers, must develop better methods of design to accommodate the ductile deformations that can be produced by strong ground shaking. Such ductile deformations must be taken into account when designing lowrise as well as highrise buildings. The objective should be to limit damage to an acceptable degree. I believe that the damage sustained during the Northridge earthquake was not really acceptable, and that the code should be revised because of this. There are also other matters that must be considered, such as welded joints of steel-frame buildings, performance of parking garages, pre-cast concrete buildings, and so forth.

Scott: Yes. I believe it is now generally recognized that existing nonductile buildings are vulnerable—so what should we do with that knowledge? Do you see ways in which society might realistically try to reduce that threat?

Housner: That can be done by 1.) strengthening the buildings, 2.) demolishing them, or 3.) restricting occupancy. It took Los Angeles about 25 years to solve its life-safety problem with old unreinforced masonry buildings. I am counting from the 1971 San Fernando earthquake, which gave the city a powerful wake-up call about the dangers of URM buildings. I think the City of Los Angeles—and other cit-

ies—must approach the problem of nonductile buildings the same way they approached the URM hazard.

If they decided to do something about the nonductile building problem, I believe they could solve it in less than 25 years. I think the cities and the state government should begin to mitigate the hazard, step-by-step. While it is not feasible to solve the older-building problem with one big project, we should not wait for future destructive earthquakes before *beginning* an effective retrofit program. In addition, some cities, including Pasadena and San Francisco, have not implemented a URM code, or have been slow doing so.⁴²

Scott: While some might consider 25 years a long time, it is probably the best we can hope for, considering the magnitude of the problem and the effort required. And meeting even the 25-year goal will take sustained effort to educate the public and the owners, and to be sure that the engineers, architects and contractors do their share.

Housner: I think the present code does need to be improved in the light of the effects of the Northridge and Kobe earthquakes. I think better results are obtained if a design is based on a dynamic time-history analysis of the vibrations of the building, or an analysis using the design spectrum to determine the maximum response of each of the first three or four modes, together with reasonable ductility factors. Lowrise buildings, however, must be handled

42. San Francisco began a URM retrofit program in 1993. *Status of the Unreinforced Masonry Building Law*. SSC-95-05, Seismic Safety Commission, State of California, 1995.

differently. The present code needs to be adjusted for lowrise buildings in order to avoid extensive damage.

Earthquake Engineering and Seismic Design

"... I have learned that in other ways, engineering design is not always as rational as we are taught in school that it should be."

Sources of Some Problems

Scott: In discussing *Separate 66*, John Rinne posed this question: Based on experience in earthquakes in, say the last 50 years, is there not a basic fallacy in building concepts that rely upon transfer of lateral shear from outside walls to an inner service core in order to create an "open" first story?

Open First Story

Housner: In raising the problem of open first stories, Rinne was thinking of buildings like the County Services Building in El Centro, Imperial County, which was of that type and suffered severe damage in the 1979 earthquake. It was essentially a concrete box, elevated one-story up in the air, standing on columns. It was similar to the Olive View Hospital that was severely damaged in 1971.

It was a 5-story building, I believe, in which they were relying on the exterior walls to restrain the floors and the roof. But

when the shear force came down the walls, which were interrupted at the second-floor level, the columns had to take it on down to the foundation. From an engineering point of view that introduces higher stresses that would normally not be present.

Scott: That interruption or discontinuity in the path introduced higher stresses than if the design had just gone straight on down with the same basic wall all the way to the foundation?

Housner: Right. I'm sure if that kind of design had been used for the El Centro building—with the walls coming straight on down—it would have come through the 1979 earthquake without any problem. But having the building elevated on those legs introduced problems. Pretty clearly they originally had planned to have it just on the legs, on the columns. Then they found that the columns would have to be too big, because they would have to resist north-south and east-west shaking at the same time.

What they did then was put in some special stub walls in the center of the building between the ground floor and the second floor in the north-south direction—John Rinne would call these the core. It was a mistake, because as a consequence they introduced high forces in places where they had not been designed for. When the requirements in the building code were established, the drafters obviously had it in mind that the sort of thing done in the Imperial County building would not be done. Instead, they intended for the walls to go down to the floor. So although in one sense the design used for that building satisfied the specified requirements of the building code, the

design nevertheless did not conform to the intent of the code-drafters. If the designers had thought about it as a dynamics problem and used the 1940 El Centro accelerograms, the calculated stresses would have made it clear that there was going to be trouble.

When Designs Contravene Philosophy

Scott: So the design of the building that failed more or less satisfied the letter of the building code, but contravened its basic philosophy?

Housner: Yes, contravened the spirit of the code. The code tells you what forces to use, what are allowable stresses and so on, but that is really intended for ordinary buildings, not for unusual types of structure. Most design engineers are quite aware that the code has limits. When drafting codes, they are thinking of *typical* buildings, and not atypical buildings.

Scott: The code is an adequate guide only if used by an engineer who understands the thinking and philosophy that underlies the document. So first a designer needs to recognize what constitutes an atypical building, and understand that for such buildings it is essential to go beyond the code specifics and use other means of analysis?

Housner: Right.

Scott: And in this case—the Imperial County building—that was not done?

Housner: We know that the design of the Imperial County building did pass the building code, but failed nevertheless. Obviously the engineer did not say, "In 1940 there was a strong shake here—shouldn't we design for

that kind of motion?" That motion would have been much greater than the building code called for.

Scott: But the historical record of earthquakes should have told them that in El Centro they would have to expect motion at least up to the level of the 1940 quake?

Housner: Yes, they should have expected at least a 1940-level quake, and known that they should design for it. But the urge to minimize the cost came into play.

Taking Unnecessary Risks

Scott: In 1989 I interviewed Robert Hench, an architect who seemed pretty knowledgeable about the El Centro building.⁴³ He said he felt that the architect had made certain basic but unnecessary design choices that predisposed the building for problems. One choice was building a five-story building at a location where a single-story or two-story structure could have done the job quite well. Another was to elevate the structure on columns. A third choice was to put some very heavy fixtures right at the top of the building—fixtures that could just as well have gone in the basement or almost anywhere else, where they would have caused much less trouble. Hench believes those critical decisions greatly increased the stresses the Imperial County building had to withstand when the earthquake struck.

Housner: Perhaps the architect wanted a "highrise" building in El Centro. Especially

critical was the decision to raise the building up on columns. If the architect had just said, "I'll raise it up," and told the engineer, "You can make the columns as big as you want to," then they would have been all right. I asked Chris Arnold afterward, "Why did the architect do that—raise the building up on columns?" He said, "In architecture we go by fads, and at the time this building was designed, the fad was to raise buildings up, opening the ground level up and sort of inviting the people to come in." For that you do not want great big columns, but want a lot of open space.

Once the decision is made to raise a building up and have it open, however, you have the same problem they had with the Olive View Hospital that failed in 1971. The first step in designing a hospital is to go to the hospital experts after you have the basic plan of the hospital drawn, and they decide how to route things, where the inpatients go and the outpatients, and all the traffic. Again, they said the ground floor should be as open as possible. "Make the columns as small as you can." Which they did, using spirally reinforced concrete columns.

The Olive View building was similar to the Imperial County building. Olive View was a box sitting on columns that were as slender as they could be made. Those columns underwent very large ductile deformations during the 1971 earthquake, and afterward the building was demolished. It was replaced by a steel shear wall building that was designed by dynamic analysis, and that survived the Northridge earthquake.

43. Oral history interview, February 27, 1989, Robert Hench, Blurock Partnership, Newport Beach, CA.

Atypical Buildings and Owners' Influence

Housner: When confronted with the older Olive View kind of design, the engineer should have said, "Wait a minute, this is not a typical building, so we ought to rethink the design." But by that time the design process may have proceeded so far that it is very difficult for the engineer to say to the architect, "Let's stop and rethink this." The architect is likely to say, "All it really has to do is satisfy the code. That's the law." But satisfying the code is simply not good enough for an unusual building, because the code was based on the idea of a typical building.

Scott: This very crucial point in design philosophy is not as widely understood as it ought to be.

Housner: That's right, but it is very difficult, because the code seems to say that if you design for these forces and these allowable stresses, then you're all right.

Scott: You say things may be different if the engineer has made direct contact with the owner ahead of time. Do you mean contact should be made very early in the design process?

Housner: Yes, if he has established a good contact, has the confidence of the owner, and the owner is interested. We can see a big difference when the owner is interested, such as when the Security Pacific Bank put up a building that is a monument for the bank, or when an insurance company or a major oil company puts up a building. It is a monument, and they do not want the thing damaged or to have it reflect badly on them. They are there-

fore interested in the design process from the outset.

Some Irrationalities and Peculiarities

Housner: If somebody is putting up a building for speculation with the idea of selling it soon, then it is to his personal economic advantage to make the building as inexpensive as possible. Then he says, "We just want to satisfy the code, period, and if you don't want to do the job, I'll get another engineer." In those circumstances the design process is not always done in a fully rational way.

In fact, in my consulting I have learned that in other ways, engineering design is not always as rational as we are taught in school that it should be. A good example is a case involving John Minasian, a local engineer and Caltech alumnus renowned for his expertise and experience in designing unusual structures, such as television towers. He once came to my office saying that at the last minute he had been engaged to do the engineering for the Space Needle, a 600-foot-high structure that was to be built in Seattle in connection with the 1962 World Fair.

An elevated circular restaurant was to be perched on top of steel columns that converged as they went up. Minasian said that because time was so short he had already placed an order for the largest size steel members available. The question he now faced was the effect of earthquake forces on the elevated structure, since Seattle is in a seismic region and has experienced earthquakes.

I told Minasian it would first be necessary to calculate the natural period of vibrations, and

then check with the spectra of some strong motion shaking. When this was done, the pre-ordered steel members were found to be adequate, and the Space Needle was built. The restaurant opened for business at the time of the Fair, and is still in operation today. I have since wondered how often it is that a structure's major components are ordered before its design has been completed, or even begun. The preferred procedure, of course, is to design first, and then order accordingly.

Another peculiarity of earthquake engineering has to do with the design criteria. The first step in determining the criteria is to estimate the intensity of ground shaking likely to be experienced during the lifetime of the structure, and of course this has a large uncertainty. The next step is to specify the shape of the design spectrum. This step often leads to prolonged discussions—should the spectrum curve be a little higher at this location, or a little lower at that location? It has often seemed to me that—in view of the large uncertainty associated with the first step—the second step hardly justifies such prolonged and detailed discussion.

Other Misconnections: Academics and Professional Engineers

Housner: I have also observed that practicing engineers do not always understand academics, and vice versa. These misunderstandings can lead to criticisms, each of the other. Thus, practicing engineers often allege that academics do research and publish papers that are not useful. I believe many academics would agree, although they would also point out that a professor and his students cannot be

expected only to do good, useful work—and that there are valid reasons for this.

First, since about 1960 a system has developed at the universities under which tenure and promotion decisions are heavily influenced by the number of papers a candidate has published, and the number of research grants obtained. Tenure is especially important to a young person who has just been appointed to the faculty, who knows that he has just seven years in which to demonstrate that he has enough publications and enough research grants to qualify. If he does not, he knows that after seven years he will be told that he has just one more year and then will be out.

Scott: The well-known policy described as "Publish or perish," "Up or out," and has many unfortunate consequences.

Housner: Yes. The official rules of the game specify this procedure, and the young assistant professor realizes that simply doing something *useful* will probably not count for much. Moreover doing something immediately useful to the engineering profession usually requires experimental research, and it is hard to get enough grant money or find enough time to conduct experimental work.

Scott: It almost seems like the decks are stacked against such work, at least early in person's career.

Housner: Second, practicing engineers also complain that, while there is probably a lot of valuable information in many of the published papers, it is very often not presented in a form that allows the worthwhile parts to be abstracted easily and put to practical use. This is certainly true. Clearly, what is needed is for

knowledgeable academics and/or practitioners to read though all the papers on a specific topic, for example, soil-structure interaction.

A summary can then be prepared—based on reading say some 100 papers—distilling out the significant new knowledge found, and explaining how it can be reliably put into practice. Such an effort requires a great deal of intellectual effort, however, and unfortunately does not earn many brownie points in the academic world.

Third, academics tend not to understand the needs of the practicing engineer. For one thing, an engineer is judged by the end product, and not by the process used to develop it. The academic, however, tends to think that the *process* itself is the most important thing. For example, if social or economic forces determine that a certain structure—an office building or a bridge—needs to be built, it will be built regardless of whether all the desired information is available. Thus, one of the practicing engineer's prime responsibilities is to use state-of-the-art professional judgment to bridge gaps in information, aided by the building code and by accepted design criteria. His role is quite different from that of the academic researcher who, in the presence of uncertainty always wants more information or a better theory.

Fourth, the building code specifies the strength that a structure should have in a way that is convenient for engineering use, but that does not necessarily represent the true physical performance of a structure. This difference between the code specification and true performance is not, however, always clear. For example, the way seismic code requirements are expressed can give the impression that the

earthquake performance of a building is represented by the forces prescribed in the building code. But that is misleading, as the building code merely prescribes the strength that the building should have.

Scott: So in several ways academics and practitioners are predisposed to misunderstand and be critical of each other.

Code Compliance vs. "State-of-the-Art"

Scott: I recall being on a committee several years ago when the Association of Bay Area Governments (ABAG) was doing one of their earthquake liability studies. I was a member of an advisory committee, along with several others, including Henry Degenkolb, and an attorney. I remember the attorney's insistence that to determine liability for earthquake damage, the courts would basically ask: "Did it comply with the code or did it not?"

Henry Degenkolb kept arguing that to be sure of safety—both in limiting property damage and protecting life safety—simple code compliance may not be sufficient. He argued that you also needed to exercise good "state-of-the-art" engineering judgment. Time after time, Henry and the attorney seemed to talk right past each other. At least the attorney could not seem to grasp the significance of what Henry was saying—that simply complying with the code was not enough.

Of course, Henry and the attorney were each speaking from their own disciplines. The attorney was basically insisting that in a court of law, code compliance is mainly what they will look for. But Henry was quite correct in maintaining

that in some situations the designer must go beyond the letter of the code for reasonable assurance that a building will have adequate earthquake resistance.

Housner: The lawyer was wrong and Henry was right. The Loma Prieta earthquake severely damaged the new Hyatt Hotel near the San Francisco airport. Even though the code requirements were satisfied, the owner sued the architect and engineer. I do not, however, know the outcome of that litigation.

Structural Steel vs. Reinforced Concrete

Scott: What are your observations and preferences regarding structural steel versus reinforced concrete frames, based on what we have learned from earthquakes?

Housner: In the past it was believed that welded steel frame buildings would be able to accommodate large plastic deformations better than reinforced concrete, but the Northridge earthquake raised serious questions about that. In any event, the quality of a building's design is a very important factor: A well-designed type A building is better than a poorly designed type B building. Or you could say that a well-designed concrete building is better than a poorly designed steel building, and vice versa.

In southern California the highrise buildings—those that are 30, 40, 50, or more stories high—have all been done with structural steel. With lower buildings, say 10 to 20 stories, it is common to make the beams and columns out of reinforced concrete. For 20 or fewer stories, if you make a careful dynamic analysis, design for ductility, and take into account the proba-

bility of earthquake occurrence, acceptable damage and so on, then I think you can do equally well with either steel or concrete. The very high buildings all use steel. Of course, the comparative cost of concrete and steel is a factor, and this changes over time. One problem with doing a taller building with reinforced concrete is that the columns tend to get too large as the building goes higher. The architect and owner object. They want the columns to be smaller. Big columns use up too much floor space.

Scott: And the contractor may also have trouble with the large columns and the dense reinforcing.

Housner: Under those circumstances, I would rather see a steel frame with the joints designed properly.

Scott: Up to about 20 stories they can be done in either concrete or steel. But above about 25 stories, you're saying the choice should be steel?

Housner: Yes, in earthquake country, the choice for taller buildings is always steel. Back in Chicago, of course, they go up 60 or 70 stories with concrete, but earthquakes are not a problem there. I do not know of that being done in earthquake country, although in Japan they do talk about building very tall buildings of concrete. But any such projected concrete building of 100 stories or more in height is not a simple beam and column structure.

Scott: With careful analysis, you could go up to 20 stories or so with either concrete or steel. For buildings of those heights, however, are there other considerations that would argue

against reinforced concrete design? This point is not often brought out in discussions, although may be well understood by many earthquake engineers.

Housner: Yes, there are such implications. For example, the question of cost enters the picture, as does speed of construction. The code just says in effect, "Use anything you want, but provide for these forces and these stresses." But what if the shaking is much stronger than you designed for, then what happens? If such strong shaking occurs, then probably the structural steel will accommodate the overload better than the concrete, providing the welded steel joints do not crack.

Scott: This is because of the basic nature of steel?

Housner: Yes, because structural steel members can undergo large ductile deformations. Concrete members, if properly designed, can also undergo large ductile deformations.

Scott: But concrete will crack at some point, won't it, even when they have taken many pains to make it ductile?

Housner: It is the structural member that behaves ductilely, rather than the concrete itself. The reinforcing bars imbedded in the concrete yield and the concrete cracks. The first sign of overstress is a crack in the concrete, which occurs when the reinforcing bar yields.

Scott: And once the concrete has cracked significantly, it cannot go back to its previous condition.

Housner: No, it cannot go back, but that of course does not mean it is going to fall down. If it cracks, however, it is damaged, and if the

damage is bad enough it would be a costly thing to fix. You would not have the problem in a steel building, unless the steel joints crack or the building ends up out of plumb. If a gross permanent deformation were to occur in a beam, it would be a major problem to repair. The cost of repairing earthquake damage is an important factor in designing a structure.

Portland Cement Association

Influential Research and Education

Housner: I should also mention the Portland Cement Association, which has had an influential role in earthquake engineering in California. The Portland Cement Association was a fairly small research group funded mostly by the cement manufacturers. When I was a student they had an office in Los Angeles, and offices in other principal cities. They published a little brochure on earthquake design of a small concrete building. In fact, they had a number of brochures that I remember the young people found very informative, and they played a good role in education in the practice of structural engineering.

They also did research in their laboratories in Skokie, outside of Chicago. When the engineers began designing multistory buildings for earthquakes, it was recognized that steel frame buildings could get overstressed and undergo some yielding, but without really jeopardizing building safety. The question came up about the performance of reinforced concrete frame buildings under similar circumstances.

Ductile Concrete Design

Housner: I am not sure about this, but my understanding is that at one stage the Los Angeles city building department put in a requirement that a concrete frame building must have the same ductility behavior as a steel frame building. This provision jolted the Portland Cement Association, which began doing research on the matter. The story about the Los Angeles requirement prompting that work was told me by Roy Johnston, Los Angeles structural engineer.

Anyway the lab came up with ways to reinforce beams and columns so they would behave in a ductile fashion. John Blume's name figures in again here. He co-authored a book with Nathan Newmark and Leo H. Corning on the design of multistory reinforced concrete buildings. Corning was from the Portland Cement Association, and they wanted John Blume and Nathan Newmark in part for the prestige of their names, but the basic work was done by PCA. The book presented the method of reinforcing for ductility.⁴⁴

Scott: It probably took some ingenuity to get concrete to meet the requirement that a concrete member should be as ductile as a steel member.

Housner: That's right. And it would have taken something like the Los Angeles requirement to get the concrete people and the research lab going on the subject. I also think it was a very important development in its own

right. At the time there was a lot of argument among the engineers as to whether reinforcing concrete in this way was a good thing or not a good thing. The San Fernando earthquake in 1971 convinced the engineers of the need for ductility in concrete. Now, nobody questions the desirability of reinforcing concrete in this way, although it does require some extra effort. I think it is important to note that the Portland Cement Association played a key role in this development.

Reduced Research Support

Housner: Then later the cement industry fell on hard times, and they reduced their support for the Portland Cement Association's research activities. The research lab is still going, and gets some funding from NSF, but is not as robust as it once was.

Scott: The industry itself has not been funding research the way they did earlier?

Housner: No. I think the cement industry suffers from competition from abroad, and thus the cement companies just don't make much money. That is about all I can contribute about the Portland Cement Association—they did play an important role back then.

Conclusion: The Past and Future of Earthquake Engineering

Housner: In concluding this section, I would like first to comment on the great advance in earthquake engineering practice from the early days to the present. Professor Martel once showed me some of his old correspondence with James MacElwane, Professor of Seismology at St. Louis University, St. Louis, Missouri.

44. Blume, John A., Nathan M. Newmark, and Leo H. Corning, *Design of Multistory Reinforced Concrete Buildings for Earthquake Motions*, Portland Cement Association, Chicago, IL, 1961.

Martel had written MacElwane in 1930 to inquire about the nature of ground shaking during an earthquake. Martel had never seen any record of such motion, and noted that some of the practicing engineers said the ground motion was sinusoidal. MacElwane replied that he had never seen any such record either, but that he was confident it was not sinusoidal. Despite the relative lack of data at the time, however, MacElwane had some very perceptive comments on the question of earthquake period:

I am inclined to believe from such information as I can gather from seismographic records that practically all periods are present from tenths and perhaps hundredths and thousandths of seconds to twenty seconds and over.... I am convinced myself that the idea entertained by some engineers that there is a single period which is predominant in all destructive earthquakes is an illusion.

That illustrates the state of ground motion knowledge 65 or so years ago. Nowadays structural engineers are familiar with strong motion accelerograms and spectra, and can even compute the spectra in their offices. They can also calculate the dynamic response of a structure. I think much of this tremendous advance in earthquake engineering has been made possible by strong motion accelerographs, improvements in engineering education, and the availability of the powerful modern computers.

Looking at the future of earthquake engineering I see a number of things that need to be

addressed. First, the seismic code needs to be revised in light of what we now know about the severity of ground shaking, and tightened up so that undesirable structures cannot continue to slip through. The code requirements should be studied and put into physically realistic form—then compared with dynamic analyses and recorded earthquake responses of buildings of various heights and materials. The post-earthquake performance of buildings should be reviewed, also.

I believe we also need a second level of research to reconcile the practice of earthquake engineering with the knowledge we now have about ground motions, performance of structures, performance of soils, potential earthquake-generating faults, etc.

Improved code requirements should be developed on the basis of these reviews. Special attention should be given to providing the necessary ductility in the resisting frame. Code specifications should take "acceptable damage" into account. Measures that only protect life and limb are not sufficient. It should not be considered acceptable simply to prevent building collapse if damage levels are nevertheless great enough to cause unacceptable economic loss to the community.

Some engineers propose that the design requirements be based on an energy analysis. An earthquake pumps substantial energy into a structure, and this must be dissipated by friction, cracking, yielding, etc. While this is a logical approach, we do not yet know whether it is practical. Some engineers are in favor of a "performance code," which specifies the performance of which a structure should be capable.

The answers to those questions need to be worked out.

The 1995 Kobe earthquake made it very clear that older, less-well-constructed buildings pose a great economic threat and life hazard. There are many such buildings in California cities, and something must be done about them. The seismic risk of industrial facilities is another big problem that confronts California. Any new industrial facilities should be designed for earthquake-resistance, but also older facilities should be retrofitted. Until now, earthquake research has not given this problem adequate attention. In addition, the Northridge earthquake demonstrated that the seismic design of wood buildings needs to be revised.

Clearly further research is needed on seismic design and analysis of all aspects of earthquake engineering. Back when the 10 percent g requirements were put in the code, we all thought the problem was solved. We thought

we knew everything we needed to know about earthquake engineering. Later, however, our confidence declined, as more information was obtained on ground motions, building responses, details of design, etc. We saw that the 10 percent g approach did not come to grips with many aspects of the problem. In my 1965 presidential address to the Fourth World Conference on Earthquake Engineering, I pointed out that it was very important to learn how to design for controlled damage, and I would still say the same now.

Scott: I guess we became somewhat less sure that we knew how to do really safe design, as we became more aware of the uncertainties?

Housner: Yes. As we learn more, the problem loses its simplicity. Each earthquake brings to light new information and new problems. Each earthquake expands our knowledge and, also, expands our ignorance.

Seismologists and Earthquake Engineers

"... the seismologists didn't understand the point of view of the engineers, or what engineers did, or what they were trying to do."

Scott: Bruce Bolt suggested several questions for me to address with you. The first one is, "What are your thoughts on the interaction between seismology and earthquake engineering over the years?"

Housner: That's kind of a tricky question. In the early years, when the old-timers Beno Gutenberg, Charles Richter, and Hugo Benioff were the seismologists at Caltech, we had friendly relations, but not really much interaction. I think mainly this was because the seismologists didn't understand the point of view of the engineers, or what engineers did, or what they were trying to do. And engineers did not know what the seismologists were doing, except for what was in the book by Gutenberg and Richter, *Seismicity of the Earth*.⁴⁵

Interaction

Housner: Later, in the 1950s, when Clarence Allen came into the picture, we had much better rapport between seismol-

45. Gutenberg, Beno and Charles F. Richter, *Seismicity of the Earth*. Geological Society of America, New York, 1941.

ogists and engineers at Caltech. Clarence made a point of attending engineering meetings, seeing what engineers thought, and what they were trying to do. Our relations were much closer. We learned a lot from Clarence, and from his ideas on faulting and the generation of earthquakes, about which the engineers had previously known almost nothing. There was nothing in our engineering education on faults, faulting, and the generation of earthquakes, and engineers did not learn any geology in their formal education. Then Bruce Bolt came to UC Berkeley, and he also interacted closely with the engineering community.

During the last 20 years or so there has been much more interaction between geologists, seismologists, and engineers. It was through people like Clarence Allen and Bruce Bolt, and Perry Byerly even earlier, that this change occurred. Byerly, a seismologist at UC Berkeley, was much interested in and contributed to engineering. He came to the engineering meetings and talked with us and saw what we were trying to do. Byerly retired probably a little before the 1971 San Fernando earthquake. In the 1971 earthquake we recorded a lot of interesting strong motion records, not only of ground shaking, but also of building shaking. We had seminars to explain all of this to the practicing engineers.

I remember we had a seminar in San Francisco at which we displayed the accelerograms that showed what the buildings did in the earthquake. Of course the engineers were much impressed. That was the first time they actually saw how buildings vibrated, and how strongly they responded. I remember Frank McClure asking Byerly, who was in the audience, "Perry,

these are the kinds of records we always wanted—why didn't you get them for us?" Seismologists, of course, were not really interested in the strong motion of earthquakes, and used very sensitive instruments that were useless for recording strong motions. So Perry responded, "Well, if I had gotten into the strong motion end of it, I'd now be *Assistant Professor Emeritus*." That was a good way of putting it. The engineers themselves were responsible for getting this information, not the seismologists.

Scott: That is an important point. The strong motions are the engineer's principal interest, but for the seismologist, who wants to understand what goes on at great depth, as well as everywhere else, the smaller motions and more sensitive instruments are crucial.

Housner: Seismology is a distinctly different scientific discipline from earthquake engineering. Seismologists cannot wait ten or twenty years between earthquakes that provide strong shaking data. They have to be doing something with the data they have, which is data from smaller earthquakes and distant earthquakes, recorded on sensitive seismographs. That has been their principal source of information, although in recent years seismologists have become more interested in the generation of strong shaking by fault slip.

People Who Were Effective

Housner: In retrospect, I would say that Clarence Allen, Bruce Bolt, Perry Byerly and Bob Wallace, and later, Walt Hayes, were the early people in seismology and geology who had a good interaction with engineers. Lloyd

Cluff also participated. We learned a lot from them because of that, and I think they learned from us.

Scott: I guess it was a two-way communication. But in the earlier days, the two sides did not understand each other.

Housner: They weren't too keen on making an effort to get together, either. So it was really the people I just mentioned who were the most effective in getting the interaction going.

Scott: What were the key things they did that made them effective? You have already put your finger on something—attending the meetings of those in the other discipline.

Housner: We had our meetings and heard papers, and seismologists would come and present things, but previously we never had much interaction. The advent of nuclear power forced many seismologists and geologists to give attention to earthquakes and ground shaking. I should also mention that in more recent years, Keiiti Aki and Hiroo Kanamori have been very helpful in interacting with engineers. They both came from the University of Tokyo Earthquake Research Institute. Keiiti Aki is a very eminent seismologist who came from Japan to Caltech, and then when Frank Press went to MIT, he took Aki with him. Then Frank got into the Washington, D.C. end of things. So a few years ago, Aki came back to the West Coast, and was at USC. He wanted to be where there was more action. In 1995 he moved to a new post in a French laboratory. Hiroo Kanamori came from Japan somewhat later than Aki. He also joined the Caltech faculty and has stayed here. He has also contributed knowledge to earthquake engineering.

I should mention that both Aki and Kanamori are in the EERI roster. In fact, all the names I'm giving you are in the EERI roster. In more recent years, many other seismologists and geologists, too many to mention, have come to EERI meetings and interacted with the engineers. Also, in the early days some of us engineers attended meetings of the Seismological Society of America.

Earthquake Engineering: An Interdisciplinary Field

Scott: Earthquake engineering—at least as the term is used in the name of EERI—really signifies something broader than structural engineering or civil engineering, doesn't it?

Housner: Yes. Originally, it was focused on structural and civil engineering—we had the idea that earthquake engineering was what the engineers did who designed a building. While that is still the essential element, in my opinion, we also have had to broaden the idea beyond the narrow definition of earthquake engineering. We began to see that you had to know something about the generation of earthquakes and the behavior of faults. Also the probability of the occurrence of earthquakes had to come into the picture, or else the engineers were working blind.

Also, when the nuclear power plants came on the scene, and other big projects, like big bridges and offshore drilling projects, there was no seismic code for them. The higher levels of safety that were essential for such facilities were way beyond what was needed for ordinary structures. The design engineers needed advice, and it became absolutely essential to

understand how faults generate earthquakes, and the frequency of occurrence, and the response of buildings. This knowledge subsequently affected the design of buildings, of course, but does not appear explicitly in the code. I have already mentioned how the City of Los Angeles adopted dynamic analysis for highrise buildings.

So I think it has been extremely helpful to the engineers to get an understanding of how earthquakes are generated, what causes them, what is the nature of the mechanism, and where and how often earthquakes occur. Often, we engineers served on consulting boards with seismologists, and that would help educate both sides. Geotechnical engineering also became important, as did risk analysis, insurance, disaster relief, and recovery.

A lot of different fields of knowledge come into earthquake engineering. There is structural engineering itself, structural dynamics, and soil mechanics, or what they now call geotechnical engineering. Seismology and faulting come into the picture. New forms of mathematics come in to handle the calculations. So if we were forming EERI now, I think we would use a more general name than "Earthquake Engineering Research Institute."

Scott: While earthquake engineering, broadly defined, seems almost intrinsically interdisciplinary, it still took the disciplines a while to figure that out. Was EERI one of the principal organizational and communication mechanisms that they used?

Housner: Yes. But it also depended on the right people coming in.

Scott: Were there others who played key roles? You have already mentioned Clarence Allen and Bruce Bolt, and a few more.

Housner: Yes. Then people like Joanne Nigg in sociology became involved. And now insurance representatives are interested. It is clear that you cannot stop at engineering design. You have to consider the impact on society. So it is now quite a broad, interdisciplinary study. Basically, however, I believe we must depend on the structural engineers to provide safe and cost-effective structures. All the other activities are aimed at making this possible.

Japanese Interdisciplinary Approach

Scott: Can you say anything more about the Japanese and their approach to earthquake engineering? About how their approach was different from ours, and about realizing that the subject was interdisciplinary?

Housner: I think in the early days they did have a better interaction in Japan. Anyway there was an interaction between seismologists, geologists, and engineers in the early days.

Scott: Did developments in Japan parallel what is now happening here?

Housner: No, they were different, because their culture and society are different, and they are differently organized. At the Earthquake Research Institute at Tokyo University there were seismologists like Professor Kawasumi and Professor Nobuji Nasu, along with Professor Kiyoshi Kanai and Kyoji Suyehiro, both professors of engineering. Today, also, there is a mix of seismologists and engineers at ERI.

Scott: When did they establish the institute?

Housner: The Japanese government set up the Earthquake Research Institute after the 1923 earthquake. The first director was an engineer, Kyoji Suyehiro, a very able man. He put together the first group. It strongly represented engineers, but also seismologists and geologists. Over the years since, it is clear that the engineering part of it decreased, and the seismological part increased. I think again this was for the same reason that Perry Byerly noted when he said that he would have been assistant professor emeritus if he had been concerned mostly with strong motion.

At the Japanese Institute, for a time the effort began getting strongly seismological and geological, and with fewer engineers, although in recent years the engineering part has again been expanding. But the Institute is really a government operation—it is in a government university. Some of the prominent old-time earthquake engineers in Japan were at universities, including Kiyoshi Muto, Shunzo Okamoto, Keizaburo Kubo, Hajime Umemura, Kiyoshi Kanai, Kazuo Minami, Ryo Tanabashi, and others.

I do not think they had anything comparable to EERI, and they still don't. They work mostly through the Japan Society of Civil Engineers, and the Architectural Institute of Japan. Of course, Japan is a smaller country. The reason we did not work through the American Society of Civil Engineers was because this is a big country, so the national society could not respond to the needs of the earthquake engineers in California.

Scott: Size is an important difference. Also in this country much of the seismic activity and interest has been in California.

Housner: Oh, yes, I'd say 90 percent in California, whereas in Japan there is interest in 100 percent of the entire country. Japan was formed by the Pacific crustal plate thrusting northwestward, and other plates in the area are also active. It is earthquake country throughout.

NSF Funding and NEHRP

Housner: After the National Science Foundation set up an earthquake engineering program with sizable funds to give out, interest in the problem developed in the Midwestern and Eastern universities. Doing work on earthquakes was a way of getting research funds, and the subject was an interesting one to study. I think earthquake engineering rejuvenated structural engineering at universities.

Scott: When did that really begin? Did it start with the National Earthquake Hazard Reduction Program (NEHRP) and the Cranston Act, which passed in 1977 and was named for California Senator Alan Cranston?

Housner: Yes, effectively with the national act. Even before that, NSF funded earthquake engineering research from the engineering mechanics section. But the national act was the real beginning—it was then that significant funding became available. I think at first the funding was all going to West Coast universities, because they were the ones who knew the problem. But then as our students graduated and went back and became professors at schools in the East, they carried their interests

back. So I would say maybe in the late 1970s considerable interest began developing back East. Nathan Newmark at Illinois and Glen Berg at Michigan were two who became active in the 1960s, and also Robert Whitman at MIT.

It is a very challenging subject for engineers, because it combines stress analysis, dynamics, and properties of materials, probability theory, seismology and geology—all these things. It is a very interesting subject, and then with the grant money available, that brought people in.

The seismology of the Midwest and East is also very interesting, and enigmatic, too, in a sense. Those regions are considered seismic regions of sorts, but the nature and mechanisms of the seismicity are not well understood. Circumstances seem to be quite different from those in California.

Otto Nuttli, professor of seismology at St. Louis University and a member of EERI produced some valuable studies of seismic hazard in the Midwest. But the big earthquake is like the sword of Damocles hanging over the Midwest.

As money became available, I think the seismologists went through a similar experience to what I just described. In their case I think money first became available in what they called the International Geophysical Year—in the mid-1950s. It turned out that when money

was available it certainly beefed up the seismological research a lot.

Scott: So the International Geophysical Year had an important influence on seismology.

Housner: Yes. Then also there was the nuclear testing, and the question of being able to identify an underground explosion by looking at the instrumental records. That concern put a lot of money into seismological research.

Scott: Research done to help in monitoring underground nuclear blasts?

Housner: Yes. To see whether they could determine whether a record was of an earthquake or an underground bomb explosion. I think the seismologists have solved this problem. It is surprising that money should be so effective, and of course it also has its drawbacks. The research tends to be driven more by the availability of funding than by the urge to do deep thinking. But the lesson here is if you want people to pay attention, all you have to do is provide the money.

Scott: And be sure that it gets spent on the right projects.

Housner: To a surprising extent, the right people do respond and the right projects do get done. You might think that would not be the case, but it is. So it does work.

Structural Engineers Association

“Without the association, the structural engineers would not have had any appreciable input into the codes.”

Beginnings in Southern California

Housner: I have already said something about how the association got started in southern California. In any event, R.R. Martel told me that several of the local engineers were interested in advanced structural engineering. Maybe they faced some tricky problem and would come and talk to him. He suggested to Oliver Bowen, "You ought to form a group of practicing structural engineers to meet regularly and discuss these things." So Oliver Bowen said they would do that. He unofficially organized a dozen of the practicing engineers who had enough interest to participate, and they began meeting for lunch regularly. I am not sure about the dates, but this went on for a number of years. I do recall that they called themselves the "Dirty Dozen."

Scott: Oliver Bowen was the one who helped kick it off, or who picked up the suggestion made by Martel?

Housner: Yes. Then at some stage, probably in the late 1920s, they decided to organize an official structural engineers association, just for the Los Angeles area.

Scott: I believe that was done in 1929.

Housner: That was when they started the Structural Engineers Association of Southern California in the Los Angeles area. Sometime later a similar one was formed up in the San Francisco area, and at a still later date they organized one in Sacramento. Also sometime later they organized the Structural Engineers Association of California (SEAOC), which had the three branches—Los Angeles, San Francisco and Sacramento. The three regional associations were called the Structural Engineers Association of Southern California (SEAOSC), the Structural Engineers Association of Northern California (SEAONC), and the Structural Engineers Association of the Central Valley (SEAOCV). Essentially that is the origin of the structural engineers association.

A Force for Good: The Building Code and the "Blue Book"

Housner: The structural engineers' association was a significant force for good. After the associations were organized, the members took a more active interest in the seismic requirements of the building code. The southern California and northern California associations were the more active ones—SEAOC, the state organization, was an umbrella, but was not active on code matters until later. And Sacramento was not active in the early days, because there were no severe earthquake problems there.

Scott: So in the 1930s the two really active associations were in the Los Angeles area and the San Francisco area?

Housner: Yes, at least that was true at the beginning. They gave a lot of attention to the code requirements. Without the association, the structural engineers would not have had any appreciable input into the codes. Later they began publishing what they call the Blue Book,⁴⁶ issued at intervals. Because it represented the consensus of the structural engineering community, the Blue Book has had an important influence on the people who put the codes together—the Uniform Building Code, the code of the City of Los Angeles, San Francisco, and so on.

Scott: The Blue Book's influence has reached far beyond California, hasn't it?

Housner: Oh yes. California has been the leader in all of this, and the rest of the country looks to them. So the structural engineers associations of California have been very effective organizations, I think. California's statewide association and the regional associations are quite different from the American Society of Civil Engineers (ASCE), a very large national organization, which did not play a significant role in earthquake design. It was the structural engineers associations of northern and southern California that really played a role. In recent years, however, ASCE has, through the work of interested members, become more active in earthquake engineering matters, an example being TCLEE, the committee on life-line earthquake engineering.

46. *Recommended Lateral Force Requirements and Commentary* (also known as the Blue Book). Structural Engineers Association of California, Sacramento, CA, 1996.

Importance of the Earthquake Problem

Housner: I think the earthquake problem is what made the California structural engineers' association something special. The members got interested and focused on the earthquake problem. Without the earthquake design problem, probably the structural engineers association would not have been so effective. Of course, the California association was concerned with other elements of the code, but this was also true of engineers in other parts of the country. The California engineers, however, made a specialty of earthquake engineering.

Scott: In short, they had a special intellectual interest in earthquake-related design questions, although they undoubtedly also have other organizational concerns?

Housner: Yes, they also have other problems, but the earthquake problem is special. After each damaging earthquake, they would realize that they did not know enough about earthquakes and earthquake design, and that the code needed improvement. I think that realization is what really got them to focus on the structural engineering problems of seismic design.

Scott: Also they no doubt saw that, in a seismic region like California, earthquake forces are in a sense the ultimate test of an engineered structure. There may be other important tests, but in California a strong earthquake is *the critical* test.

Housner: That's right. That is a test the engineer cannot hide from. I think that is a big item—the fact that they know the day of reck-

oning will come and that they ought to get ready for it. But not all of the engineers think this way—at least not 100 percent.

Contrast With Wind Engineering

Housner: It has been different with high winds, hurricanes, tornadoes, and so on. Somehow the engineering community has never really focused on the wind-related threats, or gotten organized to deal with them the way they dealt with earthquakes. Maybe earthquakes are seen as really something special, whereas the wind is something that we always have with us. If people in areas of severe wind organized the way we have here for earthquakes, I believe they could have brought about some big improvements in dealing with wind forces. In the last few years, however, the wind engineering community has organized a wind society similar to EERI.

Scott: Structures can be built to withstand much stronger winds than many existing structures typically are able to?

Housner: Yes. Especially residential houses. Here in California, when earthquakes showed that the average house of earlier times was not good, they worked on the code and put in requirements for bolting sills to the foundation, and putting in bracing, and other things that have greatly improved the resistance of residential houses. I think they could do something similar for wind forces, if they just organized to do it.

Scott: There does seem to be good organization for predicting hurricanes and their paths, and for evacuating populations when there is a clear threat from coastal hurricanes.

But it seems to have been a different matter to make homes safer, because we see major damage when very high winds or hurricanes strike populated areas.

Housner: Yes. I presume that cost is a factor, and that people are not aware that safer houses can be built. Engineered structures in general seem to have survived hurricanes, whereas the nonengineered houses have not. So the problem is not in the lap of the engineers, but in the lap of the building departments.

We Tried to Help: Caltech Conference

Housner: In the early days, when we recognized that we were not facing up to the wind problem, I remember getting funding from NSF, and at Caltech we had the first meeting on wind engineering. This led to organizing the Wind Engineering Research Council. But now WERC has reorganized into a membership organization, a counterpart to EERI.

Scott: Approximately when was that first meeting at Caltech, and were you instrumental in organizing it?

Housner: It was probably about 1970 that Don Hudson and I organized the conference. It was the first time that the wind types got invited to come and talk about wind and what their problems were, and what ought to be done. I recall that Alan Davenport and Leslie Robertson attended—both have become eminent in wind engineering. Also Jack Cermak at Colorado State University, and Anatol Roshko at Caltech. Les Robertson, a consulting engineer in New York City, was featured much later in an article in the *New Yorker* magazine, which gave a very interesting account of the wind-

design problems of the Citicorp Building in New York.⁴⁷

Scott: When you organized that 1970 conference, were you generalizing from your interest and work on earthquake design?

Housner: The conference was convened more with the point of view that in the earthquake problems we had people who were interested and were focusing, whereas they were not in the case of wind, although clearly there was a problem to be dealt with.

Scott: You thought this might be a way of getting something organized, but it took a long time to develop the way EERI did?

Housner: The Wind Engineering Research Council was really more of an academic type of organization. They met, reported, discussed research results, and that sort of thing. But WERC was recently reorganized to play a more active role, and was renamed the American Association of Wind Engineering. I think that was a good thing.

Scott: From its beginning EERI has always had strong participation by practicing engineers.

Housner: Yes. That's right. EERI has always been a forum where practicing engineers and academics could interact. The majority of the members are practicing engineers, and I think that is an important feature of EERI.

Scott: Analyses of the impacts of recent hurricanes seems to show that much of the damage was due to inadequate building design and con-

47. Morgenstern, Joe, "The Fifty-Nine Story Crisis," *New Yorker*. May 29, 1995.

struction. In short, apparently most of the damaged buildings could have been constructed to survive with much less damage if better codes and more rigorously enforced codes had been in place when the buildings were built. This appears to be leading to some significant work on code changes in a number of hurricane-prone areas.

Importance of Support Groups

Scott: One of the people who spoke at the EERI annual meeting in San Francisco in February 1995, John Birkland, EERI/FEMA Graduate Fellow, University of Washington, presented a paper entitled, "Politics and Policymaking After Large Earthquakes." In discussing how things get done in the earthquake field, he emphasized the crucial importance of a large and well-organized body of knowledgeable practicing professionals and academics who are actively trying to make progress on earthquake safety. Their knowledge, well-conceived recommendations and dedication make them especially effective after major earthquakes when public interest is high. This is what you are referring to in contrasting the effectiveness of the earthquake engineering people and the wind engineering people.

Housner: Yes, that is right. Wind engineering never had the kinds of support groups that earthquake engineering had in the form of EERI, SEAOC, and NSF, although recently the wind people may be moving a good deal in that direction. The wind problem is more diffuse, however, and the people involved are spread over a lot more of the country than is true of the earthquake people. This is true because in the past earthquakes have been

viewed as largely a California problem. I think that has had a significant effect. Anyway, the academics interested in wind did take the lead in trying to promote progress, but it was difficult. I also think the wind people suffered from a lack of interest on the part of the practicing engineers, whereas the practicing engineers have played a key role in dealing with the earthquake hazard. I think that is a very important factor.

Scott: I wonder why the practicing engineers were not more actively concerned with wind damage? We have had a long history of severe hurricane damage in the Atlantic and Gulf coastal states. Why did the engineers not get more involved?

Housner: For one thing, they never had a serious hurricane disaster on a big building. Wind has typically blown down one-story houses and trailer parks, but not the large engineered structures. A building in Lubbock, Texas was twisted or bent by a tornado, and there were cases in Miami where strong winds bent a building. But for the most part I think the engineers considered the code requirements for wind design satisfactory. I guess in a sense they were satisfactory at least to the extent that the larger buildings did not fall down in windstorms. One-story houses, however, are not designed by engineers, nor are trailer parks.

In addition, the wind people also suffered from the fact that they did not get a wind program established in the National Science Foundation. If such a program had been set up in NSF, that would have been a substantial help to the wind people. No such NSF program was estab-

lished for wind, however, whereas in contrast, NSF did get permission to set up an earthquake engineering program. This was really due to Mike Gaus, who was in NSF at the time and who pushed to get the program set up. Then when the NEHRP program was established, it focused on earthquakes—and wind was not included. Now, however, Elanora Sabadel at NSF does support some wind activities.

Scott: The failure to get a wind research program going in NSF may in turn have been

due in part to the lack of active wind support groups, as well as the lack of someone to play the Mike Gaus role for wind.

Housner: Or the role of Senator Alan Cranston. I judge however from the range of their stepped-up activities under the new name—American Association of Wind Engineering—that the wind people are now following EERI's example.

Earthquake Engineering Research Institute

“... it became clear that the Washington office was not listening to the advisory committee.... EERI was organized out of frustration with the Washington people.”

Scott: Can you give some of the history of the Earthquake Engineering Research Institute (EERI)? You were involved with EERI from its very beginning.

Housner: Yes, I was in at the beginning.

Origins: The Advisory Committee

Housner: After the war, I was a member of the Advisory Committee to the Seismological Field Survey that was, in a sense, the predecessor of EERI. Franklin Ulrich was chief of the Seismological Field Survey, which was a unit of the U.S. Coast and Geodetic Survey. Edward Hollis was Ulrich's assistant, and David Leeds was there later. There were several others who did the installation and maintenance of the strong motion instruments—I recall Richard Maley, Charles Knudson and B.J. Morrill.

In March 1983, a two-day conference was held at USC celebrating the fiftieth anniversary of strong motion instrumenta-

tion, dating from the first accelerogram obtained in the 1933 Long Beach earthquake. Don Hudson organized the conference, and all the old-timers showed up.⁴⁸

There was also another program dealing with seismological instruments, and it, along with the Field Survey, were both under the direction of Nicholas Hunter Heck, who was stationed in Washington, D.C., and had charge of the division of terrestrial magnetism and seismology. Heck also wrote a book on earthquakes.⁴⁹ The seismological program had been organized by the Coast and Geodetic Survey about five years before the Field Survey and its strong motion program were established.

As I mentioned earlier, after World War II it was clear that additional accelerographs should be installed in the western U.S. Ulrich recognized this, but was unable to get the Geodetic Survey people in Washington to listen. So he organized an advisory committee to lend more clout to the recommendations. I do not recall the names of all the members, but do remember Lydik Jacobsen, Harold Engle, R.R. Martel, John Blume and myself.⁵⁰

Scott: Ulrich wanted to get a jury of peers from the area concerned, who would verify the needs he expressed? "Look, what Ulrich is saying is valid—the strong motion program does need that kind of additional support."

Housner: Yes. We made recommendations to the Coast and Geodetic Survey on what

48. *Proceedings of the Golden Anniversary Workshop on Strong Motion Seismometry, March 30–31, 1983*, University of Southern California, Department of Civil Engineering, Don Hudson, ed., 1983.

49. Heck, Nicholas H., *Earthquakes*. Princeton University Press, NJ, 1936.

ought to be done for the strong motion program. We said there were not enough instruments, and urged that new and better instruments be developed, and so on. The advisory committee wrote several letters to Washington, but without any results—nothing ever came of it. I guess that in Washington they filed the communications in the wastebasket. Anyway there was no funding.

Finally, out of frustration, the advisory committee decided to form its own organization and work through it to raise funds to sponsor research projects.

Formation

Scott: The disappointing result prompted the group to begin thinking about forming the Earthquake Engineering Research Institute?

Housner: Yes. After trying for two years, it became clear that the Washington office was not listening to the advisory committee. I remember how angry Lydik Jacobsen got about this. He was a peppery type. Anyway we gave up on the Coast and Geodetic Survey, and EERI was organized out of frustration with the Washington people.

The idea was to wait no more for Washington to act, but to make an effort ourselves to get

50. The following information is based on John Blume's oral history: in April 1947, eight people met initially: John Bolles, Harold Engle, Harmer Davis, John Little, Lydik Jacobsen, Henry Powers, D.C. Willett, and John Blume. R.R. Martel, George Housner, and Col. William Fox were added in May. In September, 1947, those eleven, plus Professor Alfred Miller of the University of Washington, and Samuel Morris, chief of the Los Angeles Department of Water and Power, met in San Francisco for an all-day session.

something going—to raise money, get instruments put out, and so on. We resolved to organize our own nonprofit corporation, which we called the Earthquake Engineering Research Institute. That original intent of sponsoring research accounts for the use of "research" in EERI's name. EERI was formed in 1949 with about a dozen members.

Scott: That was a small beginning for an organization that later grew into a nationwide earthquake engineering society, with worldwide recognition. But of course at first you had that initial research-sponsoring role uppermost in mind. In some ways a small group can often move more effectively on something like that than a larger membership organization. At any rate, for quite a few years after EERI was formed, I believe its membership was limited and by invitation only.

Housner: Well, at the outset the people who were members were the principal ones who had evidenced some interest in the field of earthquake engineering. Afterwards others were added gradually by invitation. The membership was finally opened up after a couple of decades. I will say more about that a little later.

Scott: What about EERI's meetings and leadership?

Housner: EERI met annually, choosing San Francisco as the location because most of the members were in the Bay Area. Lydik Jacobsen, who had chaired the advisory committee and was instrumental in EERI's formation, was the first president and served for one year. Then I was elected president and served one year, after which Paul Jeffers, a Los Angeles consulting engineer, was elected as the third president.

After Jeffers, I was once more elected president in 1954.

Scott: Were there some special reasons why you were elected president a second time, rather than one of the other members who had not yet served?

Housner: Yes. Despite all the optimistic talk when EERI was formed, it turned out that nothing was being accomplished. Since I was probably the youngest EERI member, I was elected again with the understanding that I would spend some time trying to get something accomplished. I agreed to serve because I thought EERI had a great potential for good if we could once get things going. For quite a period no one else wanted to be president, and I didn't want it to die.

Getting Something Started: Conferences and Publications

Scott: So after being elected the second time, you served continuously from 1954 until 1965—eleven years. Presumably you were able to get something done during that time, but I gather it was fairly hard going.

Housner: Actually when I was president in 1951 I got the idea of organizing a conference on earthquake engineering. I talked to various persons and found general support, but it was not clear that the level of interest was sufficient to attract a sizable audience. We felt there were not enough people interested in earthquake engineering alone to have a successful conference. So we decided to add the topic of bomb blast on structures, which had been an active research field during the war and for some years afterward. In fact, a lot of people came to

the conference, so it was clear there was a great deal of interest in earthquake engineering. I think about 200 people attended.

Anyway in 1952 we held the EERI Earthquake and Blast Symposium at UCLA, and a total of 23 papers were presented. Martin Duke chaired the EERI committee that organized the conference, and his colleagues at UCLA did a good job of handling the conference and the proceedings. The proceedings volume was dedicated to the memory of Franklin Ulrich, who died of a heart attack shortly after attending the conference.⁵¹

When we published the 1952 proceedings it was the first time anyone had gotten out a proceedings on earthquake engineering. The 23 papers contrast sharply with the approximately 1,000 papers and ten-volume proceedings of the Tenth World Conference on Earthquake Engineering, held in Madrid in 1992, or the Eleventh World Conference, held in Acapulco, Mexico in 1996, which issued the proceedings on compact disk, with 1,440 papers.

So the 1952 conference was quite successful, but I must also tell a sad story that goes along with it. UCLA was then completing construction of their new engineering building, and Martin Duke and colleagues had begun installing strong motion instruments and strain gauges in the structure. But they got so busy organizing the conference that they stopped work on the instrumentation. Then, only three weeks after the conference, the 1952 Tehachapi

earthquake struck, producing moderately strong shaking at UCLA. Since the instrumentation was still not quite complete, no records were obtained.

Scott: That was both unfortunate and understandable. But looking back now, the 1952 conference can probably be seen as the beginning of all these conferences since. What did EERI do during your second term of office?

Housner: The first EERI brochure was published in 1951—I think it was organized by John Blume, who was secretary of EERI. In 1954 we published a bibliography on earthquake engineering and seismology that had been put together by Ed Hollis, who as I mentioned was with the Seismological Field Survey.⁵² Ed had bibliographical interests, and had showed me the manuscript of the bibliography. I told him to put it into shape and we would publish it.

About that time the EERI members in the San Francisco area decided that they would like EERI to sponsor a conference on earthquake engineering in 1956 to commemorate the 1906 San Francisco earthquake. John Rinne, an EERI board member from the Bay Area, suggested the idea and EERI approved. Rinne then became general chairman, and the engineers in the San Francisco area organized the conference, which was held at the University of California, Berkeley, in 1956.

It was called the World Conference on Earthquake Engineering, as they had decided to try to make it an international affair by inviting

51. Duke, C. Martin and Morris Feigen, eds., *Earthquake and Blast: Proceedings of the Symposium on Earthquake and Blast Effects on Structures*. Sponsored by EERI and the University of California, 1952.

52. Hollis, Edward P., *Bibliography of Engineering Seismology*, EERI, 1954.

foreign participants. This proved difficult in these early days, however, as we did not know people in foreign countries who had an interest in earthquake engineering. We were sort of in a vacuum. It took a number of years before interested people got drawn together. Anyway, invitations to the 1956 conference failed to reach some of the appropriate people in seismic countries. Nevertheless there were participants from Mexico, Chile, Colombia, Germany, Greece, Italy, Japan, New Zealand, Pakistan, and Turkey, and perhaps others. I believe about 140 persons attended, and 40 papers were presented and were later published in the proceedings.⁵³

Scott: So this 1956 event was the first time that earthquake engineers from many countries had met together? I suppose its success set the precedent for the other world conferences that followed?

Housner: Yes. After the 1956 conference, we began retrospectively referring to it as the "First" World Conference.

The International Association and the World Conferences

Housner: At the time of the First World Conference in Berkeley the Japanese said, "We'll host the next conference in Japan in 1960." Then probably in 1958 I got a letter from Dr. Kiyoshi Muto, who had headed the Japanese delegation to Berkeley, and was in charge over there, saying, "We think there ought to be an international association, and

we ought to form one." He then asked what should be the nature of the organization, that is, should it be made up of individual members, or be a federation of national societies?

I thought it should be made up of national societies. The earthquake engineering problem is not necessarily the same for every country that has seismic regions, and they each ought to have their own group organized. Encouraging the formation of national societies seemed likely to do more to promote earthquake engineering and seismic design in each country. So that is the way it went.

Anyway, the Second World Conference on Earthquake Engineering was held in Japan in 1960, at which time the International Association for Earthquake Engineering (IAEE) was officially established. The formation of IAEE was a very important development, certainly for the other countries, although maybe not so much for the U.S., since we had already formed our society before anyone else did.

EERI and the international association acted as a spur for other countries to form their societies and address the problem in their own countries. Now every country that has an earthquake problem has a national society or the equivalent, is a member of the international association, and is thinking about the problem. In the U.S., EERI is itself the national society, is affiliated with the international association, and the EERI president is the national delegate to IAEE. The New Zealand Society of Earthquake Engineering, which was formed early on, but after EERI, is a particularly active member of IAEE, and publishes a very good journal.

53. *World Conference on Earthquake Engineering*. Sponsored by EERI and the University of California, 1956.

Scott: Ever since IAEE's formation, the world conferences have been held every four years at various locations around the world.

Housner: Yes. The Third World Conference was in New Zealand (1964), the Fourth in Chile (1968), the Fifth in Italy (1972), the Sixth in India (1976), the Seventh in Turkey (1980). The Eighth was back in San Francisco (1984), and the Ninth was back in Japan again (1988). The Tenth World Conference was in Madrid (1992), the Eleventh in Mexico (1996). The Twelfth will be in New Zealand (2000). Each successive conference has drawn more people. We are reaching the interested people throughout the world.

Funding and Membership

Scott: Talk about EERI's own activity after 1956 here in this country.

Housner: Finances were a major concern, and figuring out what EERI might do with a small budget to promote earthquake engineering. Also the issue of the limited membership kept coming up. Let me first go back to the 1956 conference, which proved to be a big help in regard to EERI's finances. Putting on a conference like that calls for a substantial expenditure of money, and we in EERI had not done that before and had no precedent. So I prepared a proposal that we submitted to NSF, requesting \$25,000 to fund the conference, but nothing came of this. Next I got busy soliciting contributions from various corporations and so forth, and managed to raise enough money to cover the conference expenses.

Then on the very last day of the conference, I got a phone call from NSF saying they had

decided to fund the proposal. Later they explained that the long delay was because they had never funded such a conference before, so NSF's engineering division, probably Mike Gaus, had to do a lot of convincing to get approval. In any event NSF's approval meant that EERI had some money in the bank for the first time; we used NSF funding to pay for the conference. This nest egg helped strengthen the organization. So EERI activities after the 1956 conference included issuing a small number of publications, and promoting the design, construction and use of shaking machines. Then in 1965 I retired from the presidency.

Meanwhile, the membership issue was discussed repeatedly over the years. I mentioned how EERI had a closed, by-invitation membership. At first this was no problem, but as EERI became better known in the engineering community, more and more people wanted to join. I believed that it should be an open organization, but others on the board of directors did not think so. Consequently, the membership would be increased by limited increments every few years. Thus it went from 12 to about 15, and then to 25, and so forth. I kept pushing the matter, and other pressures to open the membership were felt.

Eventually, I think in the 1970s, the membership was opened up so that anyone was welcome who could demonstrate a continuing interest in solving earthquake problems. The membership now exceeds 2,000, and I believe EERI through its meetings and publications has played a very significant role in promoting better earthquake engineering and seismic safety. NSF has helped a great deal with this, having established the earthquake hazards miti-

gation program as part of the National Earthquake Hazards Research Program (NEHRP) in the late 1970s. Moreover NSF's efforts and support have played a major role in the development of earthquake engineering.

Developing New Shaking Machines

Scott: After the success of the world conferences had helped establish EERI as a going concern, what were some of its other activities? You have alluded to several.

Housner: We developed a new kind of shaking machine using funding from the California Division of Architecture. In the 1950s we still had a very limited knowledge of the dynamic properties of vibrating buildings, and saw the need for a radically new type of shaking machine to help in the study of patterns of vibration. The machines that this effort developed provided a great deal of information about the dynamic properties of buildings.

This was back about 1957. In discussing the shaking machines, I talked with the people at the California Division of Architecture to explain the importance of the proposal. We wanted some machines to vibrate buildings strongly and enable us to measure the natural periods, the damping, and the mode shapes. Jack Meehan was the person I principally contacted, and the division put up the money for the work.

The state people actually suggested that instead of working through EERI we should just do the work at Caltech, since Caltech had a better reputation than EERI. But I wanted the funds to go through EERI. My idea was that making the money available through EERI would help

establish its credibility, and then we would do the work here at Caltech. But then we ran into problems with that. When we wanted to order something through EERI, like an electrical control for the shaking machine from General Electric, they would not take the order, saying "We do not know EERI." So then we were forced to go through Caltech anyway. Caltech did the ordering, and then EERI paid Caltech.

Work at Caltech

Housner: A planning committee for the vibration generators (shaking machines) was appointed and included Lydik Jacobsen, Donald Hudson, Ray Clough, and perhaps one or two others. The actual project of design and fabrication was carried out at Caltech with Donald Hudson overseeing the project.

The superiority of these vibration generators was attributable to the contributions that Caltech professors Thomas Caughey and Dino Morelli made. Morelli, a professor of machine design, designed the actual vibration generators that exerted the forces. Caughey, a professor of applied mechanics, designed the electrical speed-control units.

A vibration generator comprised two counter-rotating metal baskets containing lead weights, whose novel feature was rotation about a vertical axis, whereas previous machines had rotated about a horizontal axis. The vertical-axis design made it possible for the forces to be applied closer to the floor of the building.

Four such force-exerting machines and two electrical control units were built. The force-exerting mechanisms could be operated synchronously—exactly in phase—or if desired

could be operated exactly out of phase. One of the control units was the master and the other was the slave. Each of them directly controlled two of the force-exerting units. These machines are now standard, and are used in other parts of the world. Kinemetrics Corporation made copies of the Caltech machines and sold them to a number of foreign countries. The original machines belonged to the State of California, so after a few years we gave them back to Jack Meehan and purchased machines from Kinemetrics. Jack gave one set to UCLA and one to UC Berkeley.

Scott: How were the machines used? How valuable was the new information they made it possible to acquire?

Housner: The machines were used on a variety of buildings to measure the natural periods of vibration of the first three or four modes, to measure the shapes of these modes of vibration, and to determine the damping in each mode. This work established the true dynamic characteristics of different types of structures, thus providing a reliable basis for making computer analyses. The vibration generators were also used to determine mode shapes and natural periods of several concrete dams, a number of earth dams belonging to the Los Angeles Department of Water and Power, the Santa Felicia Water District, plus a variety of other structures.

The measurements were made with relatively strong shaking—i.e., strong enough to be perceptible to occupants of a building being shaken. In fact, when the 10-story building of the Ralph M. Parsons Company was erected in Pasadena, we obtained permission to put two of

the shaking units on the top floor, with the stipulation that we would only shake the building after 5:00 p.m., so as not to disturb the occupants. But they had forgotten that the computer department worked until midnight. So the first time we shook the building, the computer people ran out shouting, "Earthquake."

When Caltech's 9-story Millikan Library building was under construction in the 1960s, in order to explore its dynamic properties we arranged to shake the building after 5:00 p.m. on weekdays, and on weekends, when the construction workers were gone. We put the machines on the roof and shook the building as hard as we could in resonance with the first mode of vibration, about one cycle per second. The roof of the building moved back and forth about one-quarter of an inch (double amplitude).

While this was going on, one day at lunch Clarence Allen said, "We have an odd problem at the Seismo Lab—all of our instruments pick up a one-cycle per second motion, and we cannot find out what is causing it. It comes on about 5:00 in the evening on weekdays, and on weekends. So we told Clarence, "We think we know what it is." We found that, sure enough, when we vibrated the building, the one-cycle per second seismic waves radiated out to the Seismo Lab, about four miles away. Even the sensitive seismograph on top of Mt. Wilson showed the one-cycle per second vibration. This was really the dynamic analog of Archimedes' Principle—as enunciated when he said, "Give me a base for my lever and I will move the world." Our version was, "Give us the right building on which to install our generators and we will vibrate the world."

Older Vibrating Machines

Scott: The much more sophisticated machines built at Caltech superseded the older shaking machines such as the one Lydik Jacobsen and John Blume built, and those that the Seismological Field Survey used. Would you say something about those older machines?

Housner: I think the first of the more primitive machines was the small one you mentioned, which Jacobsen and Blume designed in the early 1930s, when John was a student at Stanford. Then Franklin Ulrich and the Seismological Field Survey built a large shaker that had three rotating wheels on a horizontal axis, each about three feet in diameter. The center wheel had twice the eccentric weights of either of the other wheels, and counter-rotated. The result was that the vertical forces were canceled, and the horizontal forces were added, producing a horizontal force varying sinusoidally. This force was exerted about 2.5 feet above the floor.

Scott: How similar were the Jacobsen-Blume and the Seismological Field Survey machines. Were they virtually identical design?

Housner: No. Similar, but not identical. The drawback of machines of this kind was their lack of a speed control. When we began studying the matter, we found that the results obtained were not reliable. The way they were used was to rev the machine up to a high rotational speed and then shut the power off, allowing the machine to slow down gradually and to pass through the structure's resonance point.

Originally it was believed that as the machine slowed down, the amplitude of building vibration could be plotted as a standard resonance

curve, from which the natural period of vibration and the effective damping could be determined. We learned, however, that the run-down curve actually differed from the standard resonance curve. When the machine ran at a high frequency, the vibration of the building lagged behind the force, whereas at a low frequency, the force lagged behind the building vibration. Consequently, as the run-down speed approached the resonance point, there would be a change in phase. In effect, the force jumped across the resonance curve.

The peak of the actual recorded curve of building vibration was thus considerably lower than the true resonance peak. This gave incorrect values, especially for damping. With the new machines, we found that a building typically had about 5 percent damping, whereas the damping found with the SFS run-down machine was 15 percent. The drop from 15 percent to 5 percent made a big difference in the estimated response of buildings to earthquake shaking.

San Fernando, and "Learning From Earthquakes"

Housner: Martin Duke, UCLA professor with a specialty in soil mechanics, was president of EERI from 1970 to 1973. When the San Fernando earthquake occurred in 1971, Duke arranged with Leonard Murphy of the Washington office of the U.S. Coast and Geodetic Survey to prepare an earthquake engineering report for EERI, with funding from the agency. A substantial report was prepared, and it was published by the Survey in 1973, but EERI and Martin Duke got only a brief

acknowledgment in the preface, although they had done all the work.

Another initiative taken during the presidency of Martin Duke was his proposal that NSF fund a project called "Learning From Earthquakes," which would support investigations of and reports on damaging earthquakes. This was to have a long-lasting and important influence on EERI's activities, as both the project and the funding still continue, more than twenty years later.⁵⁴

EERI Monograph Series

Housner: Regarding my own activities in EERI, I also want to mention the monograph series that was published under the series title: *Engineering Monographs on Earthquake Criteria, Structural Design, and Strong Motion Records*.

Scott: The monographs appear to have been very successful and influential, judging from the numbers sold.

Housner: Yes. That series of seven books grew out of an earthquake engineering road show that EERI put on in 1977 and 1978, and that was funded by NSF. A team was organized to visit various cities and give seminars to acquaint engineers, building officials and members of government agencies with the basics of earthquake engineering. I was one of the speakers. Seminars were presented in Los Angeles, San Francisco, Washington, D.C., Seattle, Chicago, Puerto Rico, and Houston. In the course of the seminars, it became clear that a more thorough written presentation of each topic

would have been helpful to members of the audience, giving them something permanent to take home and study.

I recommended preparation of such monographs to the EERI administration, and as a result was appointed chairman of the monograph committee. The original project was funded by NSF, Mike Agbabian was principal investigator, and his office handled the business end. Authors of the seven monographs were Donald Hudson, Glen Berg, Anil Chopra, S.T. Algermissen, Harry Seed and I.M. Idriss, Nathan Newmark and William Hall, and finally, Paul Jennings and myself. Every EERI member received a copy of each monograph, and in addition quite a large number were sold. Especially popular was the one by Anil Chopra, *Dynamics of Structures: A Primer*,⁵⁵ which was used as a textbook in a number of universities and became a gold mine for EERI. In 1977, Paul Jennings and I wrote the monograph *Earthquake Design Criteria of Structures*,⁵⁶ which was republished in a second edition in 1982, so it was moderately popular.

Involvement With the Eighth World Conference

Scott: How did you get involved in the funding of the Eighth World Conference?

Housner: That happened in quite an odd way. The Seventh World Conference had been held in Istanbul in 1980, and unfortunately coincided with a military takeover of the Turk-

55. Chopra, Anil, *Dynamics of Structures: A Primer*. EERI Monograph series, 1981.

56. Housner, G.W. and P.C. Jennings, *Earthquake Design Criteria of Structures*. EERI Monograph series, 1977.

54. Duke, Martin and Donald Moran, *Learning From Earthquakes: Project Report 1973-1979*. EERI, Oakland, CA, 1979.

ish government. For one day, we were confined to our hotels, while the streets were patrolled by armed soldiers using tanks. That was the last year of John Blume's EERI presidency, and at the meeting of the national delegates in Istanbul, it was necessary to decide on the host country for the Eighth World Conference to be held in 1984. John took it upon himself to offer to hold the 1984 meetings in the United States, and the assembly of delegates accepted.

Scott: Had some of the EERI people already made some plans to do this?

Housner: My impression is that it was a surprise to everyone. John did not at the time have a plan for organizing the conference, and his term as president was soon over, Paul Jennings being elected to succeed him. Anyway by 1982 it was necessary to begin planning and making commitments, and to spend some money doing so. EERI had about \$35,000 in the bank, and Paul made it all available to the planning committee to get the ball rolling for the conference.

Roy Johnston headed the finance committee, EERI members were solicited for donations, and several of us worked at raising money from other sources. Susan Newman was the EERI executive director at the time, and she pressured me. Naturally, I approached NSF, and in fact approached them twice to obtain two different grants. William Butcher was the man at NSF, and he said that it was necessary to uphold the reputation of the U.S. I also contacted other organizations like the Electric Power Research Institute (EPRI), banks, engineering companies, and so forth. I put a lot of time into this effort. Finally, something over \$300,000 was raised.

Scott: Did that provide the 1984 conference adequate funding?

Housner: No, the total cost of putting on the conference was greater than that, and much of the money had to be spent ahead of time, before we knew how many participants would actually sign up and attend. So we spent some anxious times counting the pre-registrations and wondering how things would come out financially.

Scott: As I recall, the conference was a big success in terms of attendance and finances.

Housner: Yes it was. More than 1,000 people showed up on the first day, and later when the books were finally balanced EERI ended up with a substantial nest egg of surplus funds. Incidentally, one great hit of the conference was the blue cloth briefcase that was handed out to each attendee. The briefcase was well-made, had a neat embroidered logo, and was just right for carrying papers. Here more than a decade later I am still using mine. Also I still see others using those briefcases almost anywhere I go on earthquake business—Boston, Mexico, Japan, etc. I never found out who was responsible for selecting that briefcase, but whoever it was deserves our heartfelt thanks. I think it may have been the work of Loring Wyllie, or his wife. They co-chaired the special events committee.⁵⁷

Scott: Did your participation in the 1984 conference advance arrangements pretty much end your active involvement in EERI? I know of course that you still go to EERI meetings and are sometimes a speaker at a luncheon or dinner session.

Housner: You are right, however, that my active involvement did end then, although I was pleased to be chosen the first Distinguished Lecturer, and found it especially gratifying when they established the George W. Housner Medal awards. Looking back, it gives me great satisfaction to see how EERI has progressed through the years from that 1949 beginning with twelve members.

Seismological Society of America

Housner: Looking back over my career I feel I should also say something about my relations with the Seismological Society of America, which in earlier times had a relatively close relationship with EERI. I was a member for 45 years, profited from attending the annual meetings in the early days, and served as president. Thus, I have had a good opportunity to observe the society's development. In the early days, engineers attended the SSA annual meetings, and many earthquake engineering papers were published in the SSA *Bulletin*. EERI did not have technical meetings, so the SSA sessions were the only meetings to attend in order to learn about both earthquakes and earthquake engineering.

As the disciplines of seismology and engineering developed over the years, however, and

with more people working in each of the fields of study, there was a gradual separation. Whereas in the early days SSA and EERI shared an office and Susan Newman was the executive director of both societies. The two societies now have separate headquarters. I suppose this represents a natural law in the development of intellectual disciplines. As more people are involved, the subject tends to separate into specialties, and over time new societies are formed and new journals launched. In this particular case, however, I do feel that the increasing separation between SSA and EERI is a disadvantage for both seismologists and engineers. Both SSA and EERI meetings have become so large that each focuses on its own set of topics, and this has meant less interaction between seismology and earthquake engineering.

Evolution of Technical Societies

Housner: Observing technical societies over the years has led me to the following generalization about a certain evolution they seem to go through. The organization is originally set up to satisfy a particular need felt by a group of persons. Then as years go by and the membership grows, the organization of the society itself becomes of increasing importance, while the needs of the members become of lesser importance. When a society is mature, then the existence of the society itself becomes of prime importance, and the needs of the members are secondary. Or in any event, the activities and purposes of the mature society are often quite different from those for which it was originally set up.

57. The popular blue conference briefcase was specially designed by the steering committee, which was chaired by Joseph Penzien. Neville Donovan obtained sample briefcases from many different vendors for committee scrutiny, during which a new design was worked out in committee. Donovan took the design to one of the manufacturers, who tailor-made a supply for the 1984 conference. Loring Wyllie and his wife Beverly were both members of the steering committee.

Scott: Your last comment about the mature purposes being different certainly is true of EERI.

Housner: Yes. The American Society of Civil Engineers went through this kind of evolutionary process, and another good example is the National Geographic Society. The National Geographic Society was initially organized as a scholarly society to satisfy the needs of a group of people having special interests, and originally only qualified persons were elected to membership. As the Society matured, however, its very existence came to be of foremost importance, whereas the original purpose of serving the needs of the individual members tended to disappear.

Scott: I am not familiar with the history of the National Geographic Society, although I remember reading the *National Geographic* regularly in the 1930s. When the magazine became very popular and widely circulated, that development alone undoubtedly would have prompted substantial changes in the society's operation.

Housner: I think the history of National Geographic Society is a good illustration of what seems to be a natural law governing the evolution of such societies. It seems now the main purpose is to provide jobs for its staff.

Scott: Yes, I think we have all seen many examples of this kind of transformation in all

kinds of groups that are reasonably successful and long-lived. At first, as you point out, a small self-selected handful of people who are avid devotees of some subject matter found an organization to further their interest. Most or all of the work is done on a volunteer, unpaid basis, or by the staff of other sympathetic organizations, whose time can be made available to help with the affairs of the new society. That certainly characterized the earlier days of EERI.

Later, however, if the organization is successful and grows, the staff work gets to be too much for volunteers, and the enterprise manages to build up a budget that can support a paid staff. That is probably about when it really takes on a life of its own. The new paid staff, if they are eager hot-shots, probably have quite a few new ideas of things they and the organization can do. New tasks are taken on, new members join, and new money sources are sought. The original "old guard" is eventually relegated to honorific status, while others run the show. That does seem like a general pattern, although no doubt there are significant variations, depending on the organization's environment over the years.

Housner: Yes, when the paid staff appears, then the life of the organization becomes more important. This typical organizational life history is produced by what I refer to as the natural law governing the evolution of organizations.

UCEER and CUREe: Organizing Academic Researchers

“What I proposed ... was very specialized discussion of research that is needed, or being planned, or still under way. I believe such research-focused meetings would be very valuable.”

Scott: Having discussed EERI, would you now say something about the organization of CUREe (California Universities for Research in Earthquake Engineering), as I know you were involved?

It Started With UCEER

Housner: The story of CUREe really starts with UCEER (Universities Council on Earthquake Engineering Research). In the 1960s we began to see the value of having regular conferences where earthquake engineering researchers could report on their work, and jointly identify gaps in knowledge. UCEER was formed around 1967, when we thought it would be beneficial for each researcher in earthquake engineering to make a succinct presentation on his studies at a conference that all would attend. So with NSF funding, we held a series of

successful meetings for that purpose. UCEER was the brain child of Don Hudson and me.

Scott: Where were the meetings held?

Housner: They were held at different universities in California and elsewhere.

The initial meeting to organize UCEER was held at Caltech in December, 1965, under the leadership of Donald Hudson, and with representatives of nine universities attending. The decision to organize UCEER came out of that meeting, and Hudson prepared a report, *UCEER Information Report*, describing the proposed organization and the need for it, and published by Caltech in June 1967.⁵⁸

Scott: So this activity preceded by some years the establishment of the Earthquake Hazards Mitigation Program at NSF?

Housner: Yes, the NSF program you refer to was established by Congressional action in the late 1970s. I recall a 1970 UCEER meeting at the University of California, Berkeley, organized by Joseph Penzien, who issued a report on the meeting that compiled the abstracts of the talks given. After that, there were meetings of UCEER at the University of Michigan (1974), University of British Columbia (1976), Massachusetts Institute of Technology (1978), and University of Illinois (1980). These last four meetings were organized under the leadership of Wilfred Iwan, and a report issued after each one. After the 1980 conference, however, NSF turned down the request for

UCEER funding, because, they said, NSF had a policy against funding continuing operations. So that was the end of UCEER.

Scott: From what you say, the UCEER conferences were well-attended and valuable sessions. It seems a shame they were stopped.

Housner: Yes, although something similar may again arise.

CUREe: California Universities for Research in Earthquake Engineering

Housner: Some years later Bill Iwan and I sounded out the research community about resurrecting UCEER, but the reaction we got was so mixed that we decided to let the subject drop. After thinking the matter over for a while, I concluded that our California universities had a responsibility to serve the public by forming an organization among themselves. Others agreed, and at a special session during an EERI meeting in San Francisco, a group of concerned persons agreed to proceed with the organization of a consortium of eight California research universities. Bill Iwan took the lead in organizing and incorporating the group, and became the consortium's first president. He was responsible for the invention of its name and acronym: California Universities for Research in Earthquake Engineering (CUREe).

National Center for Earthquake Engineering Research

Housner: At about the same time, the early 1980s, NSF launched a program of setting up centers for scientific research that were to be

58. *Report on NSF-UCEER Conference on Earthquake Engineering Research : March 10-11, 1967, California Institute of Technology, Pasadena, California.* Universities Council for Earthquake Engineering Research, May, 1967.

given continuing funding of several million dollars a year. These were intended to work closely with suitable industries, and it was hoped that this cooperative and more concerted effort would lead to valuable results. At that time, Dr. Nam Suh, a professor at MIT, was the Director of the Division of Engineering at NSF. He came up with the idea that the earthquake engineering program at NSF should also establish a Center, independently of the official Center program, that is, using NSF earthquake engineering money, not Center money.

Scott: So this idea was for a special center separate from the general NSF center program?

Housner: Yes. Anyway, a number of universities submitted proposals.

Scott: Yes, and there was an effort to develop a coordinated California-wide proposal. And difficulties in doing that expeditiously and getting state government support prompted more urgent discussion of a better coordinating mechanism for California universities doing earthquake engineering research.

Housner: Yes. The California proposal was jointly submitted to NSF by UC Berkeley, Stanford, Caltech, and USC. To everyone's surprise, the Center was awarded to the State University of New York at Buffalo. It was called the National Center for Earthquake Engineering Research (NCEER), and through its administrative headquarters at Buffalo it provided research grants to researchers at Buffalo, Princeton, Rensselaer, and others. NSF gave NCEER \$5 million per year for five years, and the same level of funding for the second five-

year period, which ends in 1996. The State of New York provided the required matching funds of \$5 million per year. California researchers suffered because the NSF budget was reduced by \$5 million, consequently fewer of their research proposals were funded.

Putting the Center in a place that did not experience earthquakes led to a lot of comment, and the General Accounting Office in Washington was even asked to investigate the matter and report to the Senate. The GAO report stated that NSF had made a number of mistakes in handling this project. The awarding of the Center even appeared as a chapter in a book that discussed fraud, compromise, and political influence in scientific research.⁵⁹ An unfortunate consequence of the Center affair is its damage to the collegiality that had existed among earthquake engineering researchers. Moreover, in terms of urgency of need, California has an immediate earthquake problem, whereas the Midwest and East do not.

An interesting sidelight appeared in the *Los Angeles Times*, November 20, 1995, in an article headed: "A Hidden Engine For Southern California," by Stephen Sample, President of USC, and Charles Young, Chancellor, UCLA. The article's thrust is that the universities of southern California are valuable components of the economy. They refer to the universities as "an important hidden industry" in southern California that has attracted billions of dollars and millions of the world's best and brightest young people into the area. The article also comments: "Southern California and the state

59. Bell, Robert, *Impure Science*. John Wiley and Sons, New York, 1992.

have lost out in the past by lack of teamwork. By any stretch of the imagination California should have been the natural home of the federal earthquake research center established by Congress in the 1980s. Instead, the center went to New York, whose universities and Congressional delegation were a far more unified and effective team." Stephen Sample had been President of the State University of New York at Buffalo at the time of the NSF award, so he should know what was done then.

CUREe's Special Projects: Kajima and SAC

Housner: CUREe's first effort—the Kajima/CUREe Research Project, arranged by Bill Iwan—was funded by the Japanese engineering and construction company, Kajima Corporation in Tokyo. In a program that CUREe coordinated, researchers at the various California universities were supported in a number of special projects.

Scott: That was an important development, and I believe projects in the U.S. are still being supported under the Kajima program or its successors. I understand that Joe Penzien of UC Berkeley and Al Ang of UC Irvine made the initial contact at the Ninth World Conference in Japan in 1988. The request was made to Professor Takuji Kobori, executive vice president of Kajima. When the inquiry was well received, Bill Iwan and CUREe followed through.

Housner: Yes. In addition, Iwan also arranged for 14 Japanese construction firms to fund research for a special study of the Loma Prieta earthquake. CUREe also handled NSF funding

for the project on structural control on which Sami Masri and I were principal investigators.

Scott: CUREe is also playing a major role in the big research program to study the welded steel joint problem that the Northridge earthquake brought to light. Can you say something about that?

Housner: Yes. It was a big surprise to the engineering community when so many welded steel frame joints cracked during the Northridge earthquake. In the San Fernando Valley 200 steel frame buildings that suffered cracked joints have been identified. The engineering problem this poses is very serious, and affects not only Los Angeles, but also San Francisco, Tokyo, Kobe, and all other cities in seismic regions elsewhere.

FEMA provided some \$2 million to study and test steel joints, and to develop appropriate ways of retrofitting buildings with cracked joints. Since CUREe's participation was clearly appropriate, a three-way consortium called SAC was formed between the Structural Engineers Association of California (SEAOC), the Applied Technology Council (ATC), and CUREe. FEMA will provide additional funding of about \$9 million for research to solve the problem of designing new steel frame buildings with welded joints that will not crack during strong earthquake shaking. It is significant that similar cracking occurred in the Kobe earthquake, which demonstrates that this is a worldwide problem. I have also been told that cracked joints have been found in San Francisco buildings.

Still Missing: Discussions of Research in Progress

Scott: All in all, CUREe seems to have been pretty successful, at least so far, don't you think?

Housner: Well, yes and no. When I originally proposed such an organization, I had in mind a two-fold operation, only one of which has been pursued by CUREe. The two distinct activities I had in mind were 1.) cooperation in earthquake engineering research and carrying out special projects calling for more effort than a single university could handle, and 2.) facilitating discussions of work in progress, identification of knowledge gaps, and education of younger researchers. It is this second function that CUREe has not yet provided.

Scott: Of course, there are also other vehicles for such discussions of research in

progress, including EERI's annual meetings. But I take it you think something more is clearly needed?

Housner: The EERI meetings are very valuable, but for one thing, academic members are in the minority at the session, where most of the people attending want to hear about results—interesting research projects newly completed. What I proposed for CUREe, in contrast, was very specialized discussion of research that is needed, or being planned, or still under way. I believe such research-focused meetings would be very valuable. If this were undertaken it would essentially resurrect the functions of UCEER. We are working on organizing such a U.S. committee and Professor Kenzo Toki is arranging a committee in Japan. These are university researchers.

Major Subjects of Investigation

“Often the problem is so new... that we cannot answer all the questions and this leads to research.”

How Problems are Chosen

Scott: In your long professional career, you have dealt with a lot of problems. Would you say something about how some of these first came to your attention, as well as why you got involved in working on them. Are there any basic patterns in how this has come about?

Housner: In retrospect, I see three different ways in which I have been lead to study a problem.

Consulting Leads to Research

Housner: The first way problems came to my attention was as a consultant. A variety of special problems came into the picture that way, such as the problems we encountered with the Trans-Arabian pipeline and the pipeline across Lake Maricaibo in Venezuela (which I discuss later), or with suspension bridges. A number of our special research interests developed as a result of consulting. Someone comes in with a problem, you see that it is interesting and important, and you get to working on it.

Scott: You help people deal with what to them is a practical problem, but helping them find practical answers to their problems also gets you into some theoretical studies that you pursue further. Is that the way it goes?

Housner: Yes, consulting leads to theoretical studies involving how we work the problem out and explain the phenomena. Often the problem is so new to us as consultants that we cannot answer all the questions and this leads to research. An example is the design of the Trans-Arabian Pipeline. As consultant on that I was surprised to find that the above-ground pipe vibrated as a beam when the wind velocity was about 20 mph. This raised questions about the dynamic behavior of a pipeline containing a flowing fluid, and the consulting job presented me a problem "on a plate," so to speak. As consultant I had to try to solve it. It also demonstrates the great value to a professor like me with an academic base getting out in the field and doing consulting. It is an important way to identify and be made intimately aware of significant problems that need solution.⁶⁰

Scott: I can see how those experiences would provide a lot of intellectual challenge and stimulation.

Housner: Yes. My seismic consultation for the rapid transit district in the San Francisco Bay Area was another good example. Around 1950 the Parsons-Brinckerhoff engineering firm asked me to provide seismic design advice for the BART project, and especially for the

seismic design of the tube that carries trains under the Bay between San Francisco and Oakland. I believe this was the first time that the seismic design of a civil engineering project was based on knowledge of ground motions and dynamic performance. Previously the seismic criteria for a project like that—the San Francisco-Oakland Bay Bridge for example—simply specified that it be designed for a lateral force up to 10 percent of the force of gravity. No consideration was given to the ground motion or the structure's dynamic properties.

Someone Mentions a Problem

Scott: You noted a second way you have been led to study problems.

Housner: That can occur when someone simply mentions a problem. For example, one of my engineering friends once said he was designing the large water tanks to be installed at Marineland and said he did not know how to take into account earthquake forces and the motions of water in the tanks. At the time, I did not know how either, but the problem intrigued me, so I started analyzing the fluid dynamics of water in a tank. Later the problem and its solution became quite important for the design of water tanks for nuclear power plants, and for the design of large water or petroleum storage tanks in seismic regions.

Scott: Someone mentions an unsolved problem, and that stimulates you intellectually. You also referred to a third way problems engaged your attention—what was that?

60. Housner, G.W., "Bending Vibrations of a Pipeline Containing Flowing Fluid," *Journal of Applied Mechanics*. Vol. 19, Transactions of the American Society of Mechanical Engineers, 1952.

Reading the Literature

Housner: The third was from reading technical publications, historical documents and other such literature.

A good example was identifying soil liquefaction as an earthquake engineering problem. Over the years I have collected earthquake reports dating from the 1600s to the 1900s, long before the discipline of earthquake engineering developed. I saw several reports alleging that an earthquake had produced a water well. At first I dismissed this as impossible, but then later in 1958 I read a description of an earthquake in India, which stated that several minutes after the shaking stopped, water gushed up out of the ground and then subsided, leaving sand craters and sand boils behind.

I saw how this phenomenon could explain the water well description, and began wondering what was happening down in the ground to cause the ejection of water and sand. I published a paper on the subject, but at the time neither I nor anyone else in the engineering community seemed to grasp the practical significance of the phenomenon, which is now called soil liquefaction.⁶¹

Then when I visited the site of the 1964 earthquake in Niigata, Japan, I saw the tremendous damage caused by soil liquefaction. Because the damage was so spectacular, on my return from Niigata I recommended to NSF that the geotechnical engineers send a team of observers. They did this, funding a team headed by Ronald Scott, and including Harry Seed,

Robert Whitman and others. The Niigata experience aroused the concerns of geotechnical engineers in the United States, who clearly recognized the importance of the engineering problem posed by liquefaction. We now see sand boils in most large earthquakes, and sometimes soil liquefaction does great damage.

Scott: I remember the Niigata damage photos that Karl Steinbrugge showed me a couple of years afterward, when he was first helping me learn something about earthquakes. I also recall the liquefaction research that Harry Seed undertook after that earthquake. Now and then he would give progress reports to the Seismic Safety Commission, on which we both sat for a time. But now do you have any other comments on ways you have been led to identify and work on unsolved problems?

Housner: Only this—before you can do much real thinking about a problem, you need enough facts in your head to think with. In my youth I sometimes tried to think about problems when I just did not have sufficient knowledge. Later I discovered how important it is to read related information about a subject. You do that not necessarily to find a solution to a problem, but to build up enough knowledge to help you think about the matter. So just by "reading around" a problem you can stimulate your thinking. Three particularly stimulating writers were Theodore von Karman of Caltech, and G.I. Taylor and Lord Rayleigh, both of Cambridge University. The collected works of all three have been published. These three had a particularly stimulating way of looking at a problem and analyzing it.

61. Housner, G.W., "The Mechanism of Sand Blows," *Bulletin of the Seismological Society of America*. Vol. 48, no.2, SSA, 1958.

Consulting on Vibration Problems

Housner: I have been involved in consultation on many different projects, including nuclear power plants in the U.S., Japan and Italy, and some of the projects were special. They raised special problems that led to new horizons.

Liquid Storage Tanks: Marineland

Housner: For example, I got interested in the liquid storage tank problem because they were designing Marineland out here on the coast, on the Palos Verdes peninsula, and the engineer, John Driscoll, came to talk to me. They said there would be one large circular tank and one large ellipsoidal tank, both full of water and fish. They wanted to know how an earthquake would affect the water-filled tanks. What forces would the water exert on the tank walls, and how much sloshing there would be. They did not want the water to wash the customers away.

It was a tricky problem and their questions got me interested. I had to figure out what sort of dynamic fluid pressures there would be on the walls of those tanks. I told them what I thought initially, but then in working out the solution found there was more to the problem than I had first believed. Then I worked out a better solution. When lateral earthquake forces act on a storage tank, two types of pressures are produced. One pressure is due to the fact that when the tank wall moves, it is pushing against the water. The other pressure is due to the water itself sloshing.

Scott: The sloshing movement is a miniature seiche?

Housner: Yes, exactly. These movements turned out to be a very important problem. It was significant, not just for the Marineland tanks, but also for oil storage tanks and many other kinds of tanks. For example, severe sloshing produced "elephant-foot" buckling at the base of petroleum tanks when the tanks were rocking in the 1964 Alaska earthquake. The problem became extremely important when the nuclear power plants were built, because they have large tanks of fluid. So a lot of study has been done on that, much of it by former students of mine. The original work must have been in the early 1950s. My solution to the tank problem appeared later in a book entitled *Nuclear Reactors and Earthquakes*, published by the Atomic Energy Commission.⁶² I should add that Professor Medhat Haroun at U.C. Irvine, a former student of mine, has been particularly active on the seismic problems of tanks.

Tagus River Suspension Bridge

Housner: Consulting also got me interested in other special vibration problems, such as the dynamics of suspension bridges. Again, that came about when the Portuguese government was going to build a suspension bridge over the Tagus River in Lisbon, and I was asked to be a seismic consultant on the project. I got interested, and later one of my students, Ahmed Abdel-Ghaffar, became the big expert on the problem. He is now at USC. His work was especially interesting to the Japanese, who have been building a number of large suspension bridges, so he has spent some time over there.

62. Housner, George W., *Nuclear Reactors and Earthquakes*, Atomic Energy Commission, 1963.

Scott: What were the principal theoretical problems you have had to confront in dealing with suspension bridges, and what were the implications of earthquake motion?

Housner: Well, it is complicated dynamic analysis. Many modes of vibration are involved, and you could not analyze the problem without the computer. It is just too big. The questions you need to answer are about the modes of vibration of the bridge—horizontal vibration, torsional vibrations, vertical vibrations. The bridge is supported on towers that also vibrate, so it is a complicated problem of trying to understand the modes and their contributions to the forces. There are many modes of vibration, all of which have natural periods in a rather small region. Using the computer at UC Berkeley, Ray Clough calculated the mode shapes and periods for the Lisbon bridge project.

When we were looking at the Lisbon bridge, we also found out that the vibrations of the concrete piers on which the bridge rests were extremely important. In the direction of the bridge their width was 40 feet, and transversely the width was 80 feet. That is a big cantilever beam, but being 300 feet high, it is not as stiff as you might think. The towers had a natural period of vibration of about one second. So there was a big question as to how something like that would behave in an earthquake.

We figured that during a strong earthquake the piers would rock, raising up a few inches, but with the bridge in place its weight would prevent significant rocking. In short, the weight of the bridge is more stabilizing than its earthquake forces are destabilizing. The big piers in a bridge like that cost almost as much

as the bridge itself, almost as much as the superstructure.

Scott: Then while they were building the bridge, they must have just had to hope that there was no earthquake while construction was in progress?

Housner: Yes, exactly. We considered that possibility, and concluded that the piers would not fall over. They took a chance that the piers might rock a bit and end up a little out of plumb. One takes chances. A moderate earthquake did occur near the bridge shortly after completion, but before the accelerographs had been installed, another case of bad luck.

I will end these bridge-design comments on a light note, by recalling an unusual night on the town. The Tudor engineering firm in San Francisco was designing the concrete piers and the U.S. Steel Company was designing the superstructure. The Portuguese government sent over my friend, Julio Ferry Borges and a colleague to learn how we were approaching the seismic design. Ray Clough and I met with them at the Tudor offices, and we were asked if we would take them to dinner that night. We agreed, having in mind a quiet dinner in a Basque restaurant that we knew.

The steel company representatives had second thoughts, however, apparently deciding they would make a better impression by hosting the affair, so they took us all out to a fancy Chinese restaurant. Afterwards they took us to a strip-joint—it was the first time for Ray and me, and the last. But I guess it shows that earthquake engineering can be broadening in more ways than one. In those days the Kaiser Steel public

relations man was Mr. Babylon. I don't know what would have happened if he had hosted us.

Trans-Arabian Pipeline

Housner: These early consulting jobs I was involved in were a method of disseminating knowledge about earthquakes and earthquake engineering. The information went directly to the design engineers, rather than taking the slow, circuitous route through the building codes.

One interesting project I was involved in was the 1,000-mile Trans-Arabian Pipeline, which was built in the 1950s by a consortium of Chevron, Exxon, and Texaco, and which carries oil from the Persian Gulf across Arabia to the Mediterranean, coming out through Syria and Lebanon. I was a consultant on the design of the pipeline, and was interested to see how alert the oil companies were to politics.

They pointed out to me that the logical thing would have been to run the pipeline so it would come out to the Mediterranean at Alexandria, Egypt. But they decided against that because of the political situation in Egypt. The second choice would have been to come out through Israel, but again political considerations ruled that out. So the third choice was to come out through Syria and Lebanon. They have been proven right, at least so far. The pipeline has kept on functioning all the time, whereas lines that went through some of the other places got cut off.

The project was also interesting because of the design and manufacture process of the pipeline. The design was all done in California, the Chevron Oil Company being the design leader. The pipe was built here in Los Angeles by a

small fabricator called Consolidated Steel, which had developed some very efficient methods of fabrication. They had won the contract, convincing the oil companies that they could build the pipe and do it cheaper than the big companies like U.S. Steel. Of course one result was that before the project was done, U.S. Steel bought Consolidated Steel.

Scott: They bought Consolidated Steel, contract and all. After the contract was agreed to and the work in progress, but before the pipeline was completed?

Housner: Yes. To construct the pipeline, it was of course necessary to have information on the geology. Among other things they wanted to figure out where there was water. I remember a report on the geology that they showed me, which said they drilled down and found no water, but struck oil-bearing sand. Apparently, the whole place is just full of oil. I don't know if they ever found any water.

The pipe was 30 inches in diameter and 1,000 miles long. Since most of the Arabian Peninsula is rock near the surface, with little or no soil on it, they decided not to go underground. If there had been a deeper, softer soil, they would have gone underground. So the pipe is up above ground, and in structural engineering terms is a continuous beam with equidistant supports, 65 feet on center, and 1,000 miles long. Most of it was on a straight line, as there were no mountains in the Arabian desert.

The cost of the pipe was high, with 1,000 miles as the length multiplier. An increase in the pipe's thickness of only 1/16th of an inch increased the amount of steel needed by 50,000 tons. So they wanted to make it as thin as possi-

ble. On the other hand, the costs of a break would be considerable for repair, loss of oil and so forth. We were caught in between, which made it a very interesting design project to determine the optimum solution.

On that job I learned first-hand how important it is for a consultant to protect himself by putting everything in writing and on paper. One of my recommendations was to anchor the pipe at fixed intervals of a mile or two. If it were to break, that would limit the damage to that amount of pipe. This was discussed when I presented my report and agreed to by the oil company representatives.

Sometime later in the process, however, it was decided not to do that. Then while they were building the pipe they got a break, and 15 miles of pipe got torn out—it was just sitting on supports, which were located every 65 feet. So 15 miles of the supports got shoved down or torn out. My recommendation would have limited the extent of the damage to a mile or two. After that, they started following my recommendation, and the line does have the anchors at fixed points. I think they had put in only a couple of hundred of miles when the fracture happened, and of course the line was not operating yet.

Scott: So they fixed up what had already been done, and in the new line started putting in the anchors as they went along.

Housner: Yes. When the failure occurred, fortunately for me I had a copy of what I had recommended, so I was protected. When engineers asked me if I had let them build it without anchors, I could say, "No, no, I didn't, look here at my recommendation." So when you are consulting it is important not just to say things

by word of mouth. It is essential to keep some kind of written record. I had actually written up the report which I submitted to them. After that experience, whenever I gave some advice by word of mouth, I would always write a confirming letter covering the matter.

Scott: Say a little more about how the design process was organized by Chevron.

Housner: It was kind of an odd thing. There was an oversight committee chaired by a man from Exxon. Apparently there was a lot of infighting among the oil company people, and the chairman sort of lost confidence that he knew what was going on and whether or not they were doing it right. So he asked me to be an independent consultant and to review and evaluate what they were doing. The Chevron engineers did the design.

There was another strange thing. On the Arabian Peninsula there is a 100-degree swing in temperature between the hottest midday and the coolest part of the night. In a fixed pipe, a 100-degree temperature change corresponds to 20,000 psi (pounds per square inch) in stress. I pointed out that in building the line it was important to make the connections when pipe was at mid-temperature, not when the pipeline was coldest or hottest.

Another factor we considered in the design was wind influence. When the wind blows on a cylindrical body, vortices are shed. This is a common thing for tall chimneys—when the wind blows just right you get vortices and oscillating pressures vibrating normal (perpendicular) to the wind direction. So we wanted to make sure that the natural period of vibration of the pipe would not coincide with the shed-

ding of vortices in the kinds of winds that might blow. This is referred to as Karman Vortex Shedding, as Theodore von Karman was the first to analyze the phenomenon.

Even before the pipe was finished, however, they reported from the field that the pipe was vibrating when the wind blew about 20 miles an hour. That indicated that the natural period of the pipe corresponded to that frequency of vortex shedding, which was only half what it should have been. Then they told us that the section of pipe involved had not been connected at mid-temperature, but had been put in when it was colder. Then when it heated up that caused a large compression force in the pipe which changed the natural period. That showed up in the vibrations they had observed. We had them put on some dampers, which were connected to the pipe and stuck in the ground so they could move up and down and use up the vibration energy that way. That solved the problem, which they would not have had if the pipe had been connected when at the recommended temperature.

Scott: That emphasizes the importance of following plans and recommendations pretty meticulously.

Housner: Yes. But the contractor did it whenever they were ready, and did not wait for the recommended temperature. Of course it would have been easier to weld it at lower temperatures. I always keep alert for reports on that pipeline. It worked so well that they duplicated it. They built a parallel pipeline right next to that one. The two pump 600,000 barrels of oil per day.

Pipeline Across Lake Maracaibo

Housner: They discovered a big oil field in Venezuela, and had to cross Lake Maracaibo to get the oil out. It was 20 or 30 miles across the shallow lake, so they did something similar to the Trans-Arabian pipeline. They put the pipeline on trestle-work built across the lake. That work was also done in the 1950s.

Scott: In effect the pipe is laid as if on a low bridge across the lake?

Housner: Yes, the bridge is just there to support the pipe. They also had the problem of large temperature swings, which could cause the pipe to buckle. So they wanted to know what to do to keep the pipe in shape. Well, if you zigzag a pipe, then it can deform in the bends and not produce the high stresses. So that is what was done.

Offshore Drilling Platforms

Housner: I have been involved in quite a number of projects on the seismic design of offshore drilling platforms. Probably the first was the Chevron platform off Santa Barbara in the 1960s, and then later there was the big Hondo platform put out by EXXON, near Santa Barbara. There was also a drilling platform over by Indonesia.

Paul Jennings and I worked on these projects, which were interesting in part because of the two basic requirements that the platforms had to meet—resistance to wave forces and resistance to earthquakes. The solutions to the two problems ran counter to each other. To resist the waves, they wanted a stiff platform, but for earthquake forces, they wanted a flexible platform.

Scott: Was it a matter of seeking an appropriate compromise between the two?

Housner: Actually, the waves dominated the design, because offshore platforms often get large waves, whereas earthquakes are comparatively infrequent. So you design for wave motion, and then take steps to make it strong enough to take care of earthquakes. They asked us what kind of ground shaking they should take into account, and how to analyze the structure dynamically.

About three years ago an earthquake committee of the American Petroleum Institute got in touch with Caltech. One committee member, Jack Irick, an Exxon man, had been a young engineer on the design of the Hondo offshore platform, on which we had given a report some thirty years before. He contacted me saying, "We used your report so successfully that now we are coming back to Caltech on the American Petroleum Institute matter." We made up a team comprising Bill Iwan, Chair, Allin Cornell, Chuck Thiel, and myself.

We prepared model seismic design criteria for retrofitting existing platforms. Retrofitting has come to the fore because the platforms were done as long as thirty or so years ago, and now we know more about seismic design and earthquake response. Also some of those things were built without much thought to earthquakes.

Another offshore drilling matter involved the Mines and Mineral Management Agency, in Washington, D.C., which used to be a branch of the U.S. Geological Survey, and then was set up separately. One of their concerns is petroleum, especially offshore drilling platforms. In 1992 they contacted me and said they would

like to put on a workshop to address the seismic problems of the offshore platforms.⁶³ I said I would if I could have Bill Iwan to help, and they agreed. So Bill did most of the work. We had the combination workshop-conference here at Caltech in December, 1992, and got out a proceedings. It was a useful thing, because there is a seismic problem with the offshore platforms.

BART Tube Under San Francisco Bay

Housner: There was something similar with the BART tube under San Francisco Bay. I was seismic consultant on the original BART system, starting in the early 1950s. The system's most critical element was the rail traffic tube under the Bay between San Francisco and Oakland. I believe this was the first time that engineers had been confronted with the seismic design of such a project.

The question was, how would the BART tube under San Francisco Bay deform during an earthquake? The reinforced concrete tube is about 30 feet in diameter, with one-foot-thick concrete walls that are very stiff, and is embedded in the mud at the bottom of the Bay. Although this is like a large concrete beam, because of its length—some 20,000 feet—it behaves more like a piece of spaghetti than like a stiff beam. The question we faced was how the mud would deform during an earthquake, and to what extent it would bend the tube. I worked out a scheme for estimating the maximum tube curvature that could be produced by earthquake waves of varying wave length and amplitude.

63. *Seismic Safety Requalification of Offshore Platforms*, Panel on Seismic Safety Requalification of Offshore Platforms. Prepared for the American Petroleum Institute, 1992.

Another point of concern in the BART tube design was the fact that it was embedded in the soft clay under the bay, but at each end had to go into firm ground. The question was: how much relative motion should be accommodated between the end of the tube and the support at the firm ground? For this, a special flexible joint was provided, and we inspected it after the Loma Prieta earthquake. It showed evidence of some relative movement, but not very much. Of course, if there should be a repetition of the 1906 earthquake, the ground motion in San Francisco would be three or four times greater than in the Loma Prieta earthquake.

Scott: Data from even a relatively moderate shake would tell you something about how the tube might behave in still larger shakes. Presumably the 1989 Loma Prieta earthquake was one test of the tube's design. How did it fare during that event?

Housner: There was no evidence of bending damage. Also the flexible joint where the tube comes out of the mud and onto hard ground in San Francisco worked satisfactorily. Also the remainder of BART's system performed well in the earthquake. Unfortunately, however, the strong motion accelerographs that I had recommended to be installed in the tube did not function due to lack of maintenance. I thought that failure to get a valuable record was an engineering tragedy.

Scott: Could the BART tube instrumentation be incorporated into some other program for collecting such information?

Housner: The old program of the Coast and Geodetic Survey was merged into the U.S. Geological Survey, which is now based in Menlo

Park. Maybe it would be better to work it into USGS, or else into the California state strong motion program. One problem is that the BART tube does not fall under the state strong motion program, which is supposed to instrument only things that contribute to their funding.

Anyway, it probably should be the responsibility of some group whose principal interest is getting earthquake records, rather than an agency like BART, whose principal interests are quite different. On the other hand, I think USGS feels that there is only one such tube, and therefore it is not of a wide interest. So it sort of falls between the cracks, but I think it could be very embarrassing for BART if there is a very strong shake and they have to say, "We didn't get any record of it."

Scott: Would it be costly to put in a new state-of-the-art system to record earthquake motion in the tube?

Housner: No. The bare bones instrumentation might cost \$20,000. You could spend more if you wanted to. I think you would have to pay maybe \$1000 a year to maintain it, which is not very much.

Scott: Admittedly there are not many such tubes, but the matter should be of real concern to people who ride BART regularly. Also, the tube should be around for a very long time and experience several earthquakes. Knowing the motion and the tube's behavior might be very useful for analyzing future problems similar in nature.

Housner: Yes. The Loma Prieta earthquake would have been ideal for that. When we get a bigger shake, we really need to know how the BART tube behaved. Was there enough move-

ment to cause alarm? Should we do something to retrofit the tube?

Consulting on BART System Extensions Forty Years Later

Scott: In addition to helping with the earlier phases of the BART system design, particularly the trans-Bay tube, I believe you have also been involved more recently in the system's expansions that are now underway. This is the first expansion since the system was completed in 1974. Two new East Bay extensions are adding some 23 miles to the system's 71-1/2 miles of track, and other extensions are under active consideration, including a connection with the San Francisco airport in San Mateo County. What about the design of BART's extensions?

Housner: Engineering design work has been going on for the East Bay extensions, and also for the connection to the San Francisco airport. Joseph Penzien, Bruce Bolt and I have been the seismic consultants on this work.

Scott: You first worked on BART in the early 1950s, and here it is the mid-1990s. It must be kind of unusual for a consultant to have a 40-year interval in his work on two parts of the same system.

Housner: Yes, it is unusual. Of course, the recent recommendations were somewhat different, because a great deal had been learned about earthquakes and seismic design in the intervening years. Even so, I can say there is no serious defect in the original design, except that the structures have nonductile designs. They were, however, designed for larger seismic forces than were used for buildings.

I should add that samples of the mud (clay) underlying the Bay were tested dynamically here at Caltech by Professor Frederick Converse to determine whether the soil fabric would break down during dynamic strains. The material stood up well to the dynamic deformations imposed, and demonstrated that there would be no soil problems with the tube during an earthquake. Before the engineering design for the tube started, we had Bob Swain's company, Consolidated Geophysical, sink three small shafts in the Bay mud, and make velocity measurements of the stress waves at various intervals of depth, from which the dynamic properties of the Bay mud could be calculated.

Some seismic recorders were also installed in a shaft, to record earthquake motions. While many peculiar motions were recorded, there were no earthquake records. Unfortunately for our records, the only earthquake during this time was the Daly City earthquake in 1957, which came during a period when the instrument system had broken down and had not yet been repaired.

Scott: Ironically, after all the effort, the plan for the underground station at the San Francisco airport got caught in politics—between BART, the airport, the airlines, and a cost-cutting Congress. Things have been very controversial, especially as to whether BART would actually go into the airport itself, or instead have a station somewhere nearby.

Housner: That is true, but the BART airport extension seems to be proceeding nevertheless. It now looks as if the extension will probably be built, and the station will be at the airport.

Rocket Test Stands

Housner: Another consulting effort involved test stands for rockets. You no doubt recall how in World War II the Germans developed the V-2 rocket, using it to carry explosive charges across the English Channel in attacks on London. A few years later, in the 1950s a program was begun in the U.S. to develop much larger rockets with very large propulsion motors. Ground-based tests were first conducted, in which a rocket was fastened to a test stand and then fired. Larger test stands were needed as larger motors were developed, but figuring the requirements for such stands went beyond the experience of structural engineers.

My own study of structural dynamics under earthquake excitation had given me knowledge that was readily transferred to the test stand problem, so I served as a consultant for a number of those. One was a very large test stand to be built at the Air Force test center in Mississippi. The large concrete structure was to be used to test the Titan engine—not only the engine's operation, but also the operation of the controls for changing the direction of the thrust. So in this structure, instead of an earthquake force entering at its bottom, the rocket thrust force was applied at the top of the structure. This involved starting and stopping the thrust, as well as variations in the direction of the thrust, and any possible oscillations in the thrust that might develop.

Scott: The rocket motor is what shoots out the long burst of flame when we see a rocket launched.

Housner: Yes. The rocket is powered by combustible liquid fuels that are combined and

ignited. The combustion products flow out of the rocket at a high velocity. As the gas exits, it exerts a large thrust force on the rocket.

In any event, I learned a very important lesson while consulting on my very first test stand, which was also the first test stand constructed at the Mississippi facility. It was intended to test a rocket that was about 20 feet long and about one and a half feet in diameter. The test stand was a concrete tower about 15 feet square and about 50 feet high. The rocket was to be fastened to the outside of the test stand in a vertical position, and then fired.

Scott: How did that experience teach you a lesson?

Housner: The lesson related to the project specifications, which as originally written gave the magnitude of the thrust force to be used in designing the test stand, but then also said that the test stand should be designed for an oscillating thrust force having any frequency. Taken on its face value, this specification would have required the test stand to withstand the thrust force oscillating in resonance with the stand's natural period of vibration. As this was clearly unreasonable, I consulted with some of my Caltech colleagues involved in the combustion aspects of jet propulsion.

According to these colleagues, while small high-frequency oscillations in the thrust force had sometimes been observed, an oscillation of the total thrust would destroy the rocket motor, and that this sort of behavior had never been observed. So when writing my consultant's report, I explained this and went ahead to rewrite the specifications that I recommended to be used.

In short, the lesson I learned was that project specifications are sometimes written by people who do not know enough to prepare correct specifications. You have to watch out for this. As a result of that experience, I was able to recognize the same problem when it occurred in a number of earthquake projects, especially the seismic design of nuclear power plants.

Incidentally I should add that in the very first test involving the initial stand on which I consulted, the rocket motor's exhaust gases blew away the pavement of the street leading up to the stand. That demonstrated the very large forces that the exiting hot gases can exert.

Scott: What about the performance of the test stands on which you consulted?

Housner: I was not involved in the actual testing, but as far as I know the stands performed satisfactorily.

Scott: Did you have any other interesting experiences with rockets?

Housner: Yes. The engineering department of Aerojet Corporation approached me because I had done a stress analysis of centrifugal pumps for pumping the California Aqueduct water over the Tehachapi Mountains. Aerojet was designing a centrifugal pump for a liquid fuel rocket. It was to pump the fuel through a pipe to the combustion chamber at a very high velocity, which meant that the internal pressure was very high. When they tested the pump, its casing split in half.

From my previous experience I recognized that very high stresses were concentrated at the nose of the guide vein, which also held the top and bottom of the casing together. The casing

was made of a nonductile aluminum alloy, so I pointed out that by changing the composition of the aluminum alloy so it would perform in a ductile fashion, they would solve the problem.

Scott: Ductility had a beneficial effect there, just as it does in designing structures to resist earthquakes?

Housner: Yes. I was also peripherally involved in another kind of rocket problem, related to high-speed flow of liquid through pipes in a rocket. Previously, when consulting on the Trans-Arabian Pipeline I had analyzed the problem of the vibrations of a pipeline containing flowing liquid, and, as I noted earlier, in 1952 had published a paper in the *Journal of Applied Mechanics*. In the paper I had pointed out that the differential equation of the pipe indicated that the pipe would buckle sideways if the fluid velocity were sufficiently high.

About a year later, I got a letter from an engineer at the Wright Patterson Air Force Laboratory, enclosing photographs of a test he had run, pumping fluid through a pipe at high velocity. He had read my paper and saw its significance for rocket motors. His test and the photographs showed that the pipe did indeed buckle when the velocity reached a critical value. He said that his superiors had not believed my analysis, but had allowed him to make the test on a pipe of the diameter and length that would be used in the rocket. This experience shows how the analysis of one problem can have important applications to other problems. It also illustrates the importance of publishing technical papers.

Nuclear Power Plants

Housner: Probably around 1960 or a little before, when nuclear power plants were being considered, I had been a consultant to PG&E on their proposed nuclear plant at Bodega Bay. I could see that the degree of safety required went far beyond what is done for ordinary buildings, and that we really did not have the right information for that kind of design. Much more data was needed, and more precise data, that would enable you to determine the risk for a nuclear power plant subjected to strong shaking.

Scott: I presume that part of the need was for better strong motion data from earthquakes?

Housner: Yes, strong motion data was needed, and all sorts of other information for accurately assessing safety. They needed to answer questions that were never raised for ordinary buildings. You did not ask how an ordinary building would behave in the kind of earthquake that probably comes only once in 10,000 years. Nobody would listen if you did ask such a question. But it was clear that this kind of analysis would need to be done for the nuclear power plants.

I wrote a letter to the research department of the Atomic Energy Commission, pointing out the big need for more information, and urging them to realize that we were not prepared to answer the kinds of questions that would come up. Their letter of response said, "Thanks for your interest in the matter, but we have enough information to do the seismic design of nuclear power plants. There is no need to do any more research."

Scott: They in effect said, "We don't see it your way, and we do not need more information."

Housner: That is right, they said, "We don't need more information." And of course that got them into trouble, because they really did not have the data needed. It is another example of not being able to get key people's attention. It was also kind of annoying, because in the letter they said, in effect, "We know enough, and we're shortly going to be publishing a relevant document." They referred to a document on which I had been a consultant, and had prepared the earthquake engineering parts, so I knew that it was not really answering the questions that ought to be asked.

Scott: I guess they simply did not even know enough to realize what they needed.

Housner: I suppose it was something like that, I don't know. It certainly is a good example of how difficult it can be to get attention. Later on, of course, they came to realize the need, and quite a bit of money was pumped into the research. But by then they had waited until they had a real problem.

Scott: Would you say a little more about that?

Housner: It started with the PG&E plant that was proposed to go in at Bodega Bay.

Scott: That was the case where a hole was dug for the plant, a fault found there, and that led to the project being stopped?

Housner: Yes. The fault was not in itself a hazardous thing, but it gave leverage to the opposition, which essentially was the Sierra Club. Because of all of the fuss, there were special hearings, and finally, I guess at the suggestion of the Atomic Energy Commission, PG&E withdrew the application.

That of course also was a good example of what you could do to stop a plant if you were opposed to it. The same problem also showed up in the San Onofre power plant, and again for Diablo Canyon. It is also showing up back East. They now know more about the seismic hazard back East, and the question is that perhaps in planning for some of the plants already built there, they underestimated the kind of shaking they might get. Now they will have to go back and verify that the plants are all right.

I think they could have minimized the problem if they had gotten on it early. In retrospect, it clearly was handled quite wrong. The owner, the outfit putting up the money, the utility, did not know anything about earthquakes. They relied on their consultants. The opposition used the earthquake threat to try to stop the project. The two sides made their case before the regulatory commission, which did not know anything about earthquakes either. Of course, once you get a nuclear project started, you virtually cannot stop. So when the earthquake problems started coming up, the utilities said, "Well, O.K., we will make it stronger," and so on, in order to keep the process going and get approval. Because if you once get everything marshaled to go ahead, you can't stop a project. It is very costly if a project is stopped temporarily.

Scott: You mean because the funds, the project planning and the scheduling are all lined up and committed?

Housner: Yes, the funds, the engineering, everything—the whole process is all set up for a certain schedule, and you really cannot stop. You can't say, "Give us an extra year or two and

we'll study it all out." This should have been studied before they got started. I feel that the effect has been very bad. I think they were not doing the earthquake thing in the right way.

Scott: At first, this principally involved California or the West Coast, but later it has spread.

Housner: Yes, the concern showed up in other parts of the country. Several years ago a number of us, academics as well as engineers from Rockwell Corporation, wanted to undertake a project to draw up a model seismic code for nuclear power plants. We prepared a proposal, went back to Washington, and made a presentation to the Department of Energy, but were unsuccessful.

Nuclear Tests and Some Unexpected Results

Housner: I will mention a couple of interesting experiences in connection with seismic consulting on nuclear testing. These illustrate how even the most careful plans are not always carried out successfully. In the 1950s, Nathan Newmark and I were consultants to the Atomic Energy Commission and advising James Reeves, who was responsible for safety during nuclear tests.

At the time, preparations were underway for the first underground nuclear detonation at a site in New Mexico. The nuclear explosive device was installed in a chamber at the bottom of a hole so deep underground that the explosion would be contained. But questions had been raised about the effect of seismic waves from the underground blast on a mine about 25 miles away. We were able to show, however, that the intensity of shaking would be so attenuated that it would not be a threat.

On the day of the first underground test the observers—including Newmark and me—were stationed about one mile from ground zero. When the detonation occurred, the ground where we were standing got quite a jolt. The really alarming thing, however, was that coincident with the jolt, we saw a cloud of dust fly up from ground zero. At first I thought the nuclear explosion had broken through to the ground surface, but we learned that this was not the case. The explosion causing the cloud of dust was from a dynamite charge placed on the surface, which they intended to set off about five minutes after the nuclear explosion. The dynamite explosion was intended to help calibrate the attenuation of seismic waves, but somehow the underground blast had set off the dynamite prematurely. This event was unexpected and was never explained.

Another curious case was encountered when I attended a special meeting on another AEC calibration test. The project's purpose was to provide calibration permitting comparisons between underground nuclear explosions and

underground non-nuclear detonations. An underground spherical cavity was prepared, and a large sphere of ordinary explosives was built up brick by brick, with a detonator at the center. After the sphere was completed, some one asked "What detonator was used?" The question was important because one box of detonators had been deactivated in another project, and no one knew for sure whether a good or bad detonator was sitting in the middle of the sphere of explosives. At that point there was nothing to do but proceed with the test, hoping that the detonator was good. It was.

Fortunately Howard Hughes did not learn about this particular incident, as he was then trying to stop the whole nuclear testing program at the Nevada test site, because he feared for the safety of his hotel buildings in Las Vegas, where he himself lived. For a time, Hughes employees were contacting everybody they could who was knowledgeable, including Clarence Allen and me, trying to find evidence that would help their campaign against testing. They never found anything significant.

State Water Project, Canals, Dams

“There are something like a 1,000 dams in this state, most of which were built before they had a good idea what the earthquake problem was.”

Housner: The State Water Project was started in the 1950s, when I was president of the Earthquake Engineering Research Institute. It was an enormous project, with 20 dams—including Oroville Dam—and a couple of power plants and pumping plants. The project brings water from the Feather River down to southern California, with much of the water coming down almost alongside the San Andreas fault. The aqueduct crosses the San Andreas fault three times.

Consulting on the California State Water Project

Housner: My long-standing interest in the performance of dams first developed out of being a consultant to the Department of Water Resources on the Feather River project. The first seismic consulting board was established in 1962. That experience is an excellent example of how helpful consulting work can be to a professor. It is very doubtful whether I would have thought of those problems without the stimulus of the consulting work.

Scott: A practical problem is presented, to which you are asked to apply theoretical analysis, as well as engineering judgment.

Housner: Right. The original board advised on the seismic design of the whole project, or at least the part constructed after the group was formed. This included the seismic design of the canals and dams, the pumping plants, the power generating stations, and the tunnels through the mountains. There have been other such boards at DWR later, but they usually involved the same people, in different combinations, depending on the specific budget involved.

As president of EERI, in the late 1950s I remember writing to Harvey Banks, director of the Department of Water Resources, pointing out that special attention should be given to such a major project in highly seismic regions. I said I recognized that much of the water project was coming down right along the San Andreas fault, and through other seismic areas, and urged that some special consideration be given to the earthquake hazard. At the time, I was thinking more about special studies that ought to be made.

After writing Banks, I got word back from Larry James, the chief geologist, saying that they would like to come down and talk to me. So we met them here at Caltech. The EERI group included Don Hudson, Samuel Morris, head of the Department of Water and Power of the City of Los Angeles, and myself. Morris Dam here in this area was named for Sam. I also remember Larry James, Bob Jansen, and Don Thayer being at the Caltech meeting—they were all from DWR. We discussed the matter and presented the case for special attention to seismic concerns. After the meeting, we

did not hear anything more about the matter until several years later, after Harvey Banks was gone. He was replaced by Alfred Golze, from the U.S. Bureau of Reclamation. Then an advisory board was established.

For a long time I assumed that Harvey Banks simply had not been convinced of the need. Much later, however, I learned in talking to Vernon Persson in the Division of Safety of Dams that I was wrong in assuming that Harvey Banks was not interested. He had been keen right from the beginning in wanting to have an advisory committee. But it was a very slow process to get the thing accepted and organized. Consequently the board was not formed until after Harvey Banks had retired. The delay had just been a matter of the bureaucratic wheels turning slowly, very slowly.

We saw another example of that kind of delay after the Loma Prieta earthquake. The earthquake was in October, 1989, and our report on it came out in June of 1990, recommending that Caltrans set up a seismic advisory board, among other things. At the end of November, 1990, Jim Roberts said he would appoint the board, and that we would have our first meeting in January, 1991. We did have the first meeting as scheduled, but did not get our official appointments for another eight months. So that is the way the administrative machinery grinds slowly.

As I recall, the initial membership of the State Water Project's seismic advisory board included Hugo Benioff, chairman, Nathan Whitman, a structural engineer, Harry Seed, and myself. Later Bruce Bolt and Clarence Allen were members, as were Jim Sherard, John Blume, and I.M. Idriss. When Hugo

Benioff retired from the board, Clarence Allen became chairman, and when Clarence retired as chairman, I succeeded him.⁶⁴

By the time DWR formed the seismic advisory board in 1962, the State Water Project had been under way for some years. The project's main dam, Oroville Dam, had already been designed and the dam was being built. So the board's recommendations applied to all of the project except Oroville Dam.

The board recommended appropriate instruments for recording strong ground shaking, and also made recommendations on the analysis and design for the project. To my knowledge, this was the first time such a thing was done for a big project. Later, another look was taken at the seismic resistance of Oroville Dam, which was found to be okay, although some auxiliary structures and other items were found to be deficient in resistance and were strengthened. The board also made recommendations pertaining to matters of seismology and geology.

Scott: The board helped set design criteria, or at least it established a methodology for the design criteria?

Housner: Right, and we met several times a year to address specific problems.

Scott: The board's policies and decisions were incorporated in the work on the project?

64. The official record is unclear on the initial board membership, as the 1962 annual report lists only Hugo Benioff as chairman. A total of eight people served between 1962 and 1974, five at any one time. The eight are: Clarence Allen, Hugo Benioff, John Blume, Bruce Bolt, George Housner, Harry Seed, James L. Sherard, and Nathan Whitman.

Housner: Yes. When the State Water Project part of the department's work was finished, that initial committee was retired, and another committee—comprising essentially the same people—was formed to advise on the safety of the existing dams in the state. In the Department of Water Resources there is the Division of Safety of Dams, which Vernon Persson now heads. It was formed after the 1928 collapse of the St. Francis Dam in southern California. Their responsibility is to see that existing state-owned and privately-owned dams in California will be safe against earthquakes. There are something like a 1,000 dams in this state, most of which were built before they had a good idea what the earthquake problem was. I believe that in the Division of Safety of Dams it has been called the Consulting Board for Earthquake Analysis, but before that a different name was used, depending on whose budget was being charged. DWR also utilized a peer review committee of experts for major dams. The peer review committee was appointed at the beginning of the project, which I think was a good procedure.

Auburn Dam: Safety Issue

Scott: The state also played a key role in reviewing the seismic safety of a proposed federal dam—Auburn Dam. Would you discuss the controversy over that dam, which was to be built by the U.S. Bureau of Reclamation. The process of getting ready to build the dam was well along, and major preliminary excavation work at the site had already begun. Then in the late 1970s and early 1980s two or three state bodies, including the Seismic Safety Commis-

sion, played a role in reviewing the Auburn Dam's safety, although it was a federal dam.

Housner: Yes. The Bureau of Reclamation was going to build a big federal dam near Auburn, a very long and high thin-arch dam. A question arose whether they had done the earthquake analysis and design correctly. In fact, we thought their original analysis could have been better. The 1975 Oroville earthquake was mainly what touched that issue off. A system of faults runs essentially from Oroville down south beyond Auburn. People had thought those faults to be inactive, because there had been no significant earthquakes there in historical times. But then the Oroville earthquake came, and that of course *was* a significant earthquake in this foothill region. If it happened in the Oroville area it might also happen at Auburn. The first information I had on that was when I received a letter from a retired Bureau of Reclamation engineer, saying he thought the seismic design was not adequate. I sent it to Vernon Persson. Word got out and considerable discussion followed.

I think the Seismic Safety Commission wrote a letter to the Bureau of Reclamation that said, in effect, "Unless your dam has the approval of the Department of Water Resources, we will object to it." So the Department of Water Resources then set up a special committee on Auburn Dam, of which I was chairman. Harry Seed was a member, Douglas Campbell, Bruce Bolt, and somebody else—I have forgotten who, as there were so many groups advising on the project. I recall that Clarence Allen and Ray Clough were on an advisory committee to the Bureau of Reclamation.

We listened to the presentations of the Bureau of Reclamation engineers, telling what they had done and what they thought ought to be done, etc.. The critical thing turned out to be the possibility of a fault displacement—actual fault movement—directly under the dam.

Scott: The Bureau of Reclamation staff also gave the Seismic Safety Commission a number of presentations, reporting on their progress in getting more geologic information on the dam site and vicinity. They also reported on their effort to provide information to the committee you just mentioned.

Housner: Yes. In the Oroville earthquake there had been actual movement along a fault which showed up at the ground surface. There was concern about the thin-arch feature of Auburn Dam. If movement occurred under the proposed dam, it might crack. I should say that the probability of faulting under the dam was exceedingly small, but the consequence of a failure would have been to wipe out a portion of Sacramento, and this was an important part of the problem. So the Bureau studied the thing again, and later came back with another design. Instead of a thin-arch dam, they proposed what they called a fat-arch dam, with a much wider, thicker section. That seemed to be safe, but since then, they have not been able to get funding for that project.

Scott: The thin-arch dam was essentially withdrawn. Was that on the basis of your advisory body's recommendations?

Housner: The committee told them that the dam ought to be able to withstand a fault displacement of so many inches, and that requirement sort of ruled out the thin arch.

Scott: The proposed thin-arch design really would not withstand that much displacement?

Housner: Well, maybe yes and maybe no. At least they could not show that the dam could withstand the specified amount of displacement.

Scott: After that precedent, when a major dam is considered almost anywhere in the world, a more comprehensive look is taken at the consequences of building the dam, and especially possible effects of seismic shaking on the dam?

Housner: Right. Of course, in the developing countries, where you might not see up-to-date engineering, they borrow from the World Bank, since they don't have the money themselves. As a condition of the loan, the World Bank insists that the earthquake hazard be considered. That is extremely important. Many of the people on our general advisory committee to DRW, like Harry Seed and Jim Sherard, were also consultants on dam projects all over the world. I think that was a very important activity. I retired from the DRW committee in 1994, after about 32 years of service.

Scott: The State Water Project is pretty well completed, at least for the time being.

Housner: Yes. They are still thinking, however, about expanding it to the north, including the Eel River. They are also looking all the way up to the Columbia River. I'm sure eventually they will have to do something if they want water for the growing population. The question of building the peripheral canal around the Delta is still being debated.

In closing my discussion of the State Water Project, I want to mention a very interesting book on where California water comes from and where it goes—*The California Water Atlas*.⁶⁵ Although the book has nothing to do with earthquakes, I mention it because I consider it an outstanding effort. I know that copies went to China, and it served as a model there, and possibly also in other countries as well. The atlas was prepared by the Department of Water Resources and the Office of Planning and Research, with William Kahrl serving as the project director.

65. *The California Water Atlas*, Governor's Office of Planning and Research, State of California, 1979.

Inquiry into the Loma Prieta Earthquake

"The problem was obviously broader than Caltrans.... We included those other things...and felt we were justified, in that the board was an independent body and serving without remuneration. So we could say what we thought."

Scott: Will you discuss the board of inquiry into the Loma Prieta earthquake disaster, which you chaired and which reported to Governor George Deukmejian in the summer of 1990? After the earthquake, the Governor seemed to dedicate himself to doing something about the earthquake hazard, and supported your inquiry very strongly in his executive order. After the Loma Prieta earthquake he seemed to support earthquake preparedness more than almost anything else in his whole administration.

Housner: Yes, the earthquake obviously was a great shock to him, especially the collapse of the elevated Cypress Freeway in Oakland. I was watching TV the evening of the earthquake, and at about 7:00 p.m. they said, "We have been trying to get hold of the Governor, who is in Germany, and now we have finally reached his hotel." It was 3:00 o'clock in the morning there. The earthquake had occurred when it was 1:00 o'clock

in the morning in Germany. He was in bed when he answered the phone and said, "Hello." They said, "Hello, this is station so-and-so, there has been a terrible earthquake in San Francisco, what do you plan to do about it?" In the circumstances, I thought he showed remarkable restraint.

In appointing the Board of Inquiry, Governor Deukmejian said he wanted to know what happened, why it happened, and what should be done to prevent it happening again in the future. He said he wanted a *fact-finding* report, not a *fault-finding* one. So that set the tone, and in my opinion the board's effort was very successful. Everyone from the state agencies that we had contact with, and who testified—from Caltrans, the General Services Agency, etc.—all made a very good impression on me. They knew what they were talking about. The testimony we got from non-state agencies also made a very good impression. They all knew what they were doing. I had expected more people to come in and try bluffing.

In 1971, following the San Fernando earthquake, I testified at a hearing of the Assembly Committee on Transportation on the collapse of the freeway structures. I felt that the Caltrans people who testified were not very open in their remarks. But in 1990 it was quite different.

Scott: So everything was all pretty straightforward in the board's investigation work and the responses to it?

Housner: Yes it was—straightforward.

Relations With the Governor's Office

Scott: Describe your relations with the Governor or the Governor's staff in setting up and operating the board. Were there many contacts in the process, or were there initial contacts and then you were pretty much on your own until the report was turned in?

Housner: The first contact was when I received a telephone call from the Governor's office when I was in Washington at a meeting of the National Academy of Science earthquake committee. Before I was officially appointed, the Governor's office asked me to come up to Sacramento. Obviously, they wanted to see who I was and what I looked like. I had to pass inspection by the Governor's key men. After that, I did everything with Ben Williams.

Scott: When you went to Sacramento to "pass inspection," how long did that take?

Housner: It was about one hour. My second visit involved recommending who should be on the board and getting the members appointed. I sat down with Ben Williams and indicated what I thought ought to be done. Then he went off to the Governor's office, and got the appointments made. In that sense we did have a close relationship. It was clear that he was also talking to other people in the Governor's office.

Scott: Ben Williams was the key contact with the Governor's office?

Housner: Yes. Ben Williams was a very able man from the Governor's office, the Office of Planning and Research, who was assigned as the board's administrative officer. Peter Milne was his assistant. Those two were of great help.

I recall Chuck Thiel saying, "You really lucked out, getting Ben Williams."

Constituting the Board

Scott: Could you start by discussing how you went about constituting the board and getting started at the beginning of the inquiry?

Housner: It was clear, of course, that the board and report must be above suspicion—that was an essential feature. You had to have knowledgeable and competent people.

Board Membership

Housner: There were nine California members, plus two ex-officio members, for a total of eleven. One ex-officio member was Lee Dickinson, from the Transportation Safety Board, and the other was Walter Podolny from the Federal Highway Administration. The two were ex-officio in the sense that they represented their agencies.

Scott: Those two ex-officio representatives were actually formal members of the board?

Housner: Yes. The term "ex-officio" does not show up on their appointment. I use the term to indicate that they were there to represent those two agencies. They certainly contributed, but did not participate in writing the report.

The Transportation Safety Board is the outfit that sends in investigators when there is an airplane crash, or a bridge collapse, or anything of that sort. The Federal Highway Administration plays a very important role in highways and bridges, which I wasn't aware of before I got involved in this. When we buy gasoline we pay a federal gas tax that goes to the Federal High-

way Administration. It is that agency's responsibility to see that the national network of roads is right. I think most of the money we pay in the federal gas tax comes back to Caltrans. A very sizable fraction of their operation is funded from the Federal Highway Administration. The administration also has a technical arm, because many of the states, at least the smaller states, do not have much technical competence in their highway departments. So the Federal Highway Administration has technical people to help the state people. The federal people have a high opinion of Caltrans.

The other nine board members included Mihran Agbabian of USC, Paul Jennings from Caltech, Robert Wallace of the Geological Survey in Menlo Park, Joe Penzien, a consulting engineer, Eric Elsesser, a consulting structural engineer in San Francisco, I.M. Idriss from UC Davis, formerly a consulting geotechnical engineer, Alex Scordelis from UC Berkeley, Christopher Arnold, a consulting architect in the Bay Area who is much interested in earthquakes, and myself. I served as chairman.

Charles Thiel was the general editor of the report, and also wrote initial portions of the text. Gail Shea and Laura Moger assisted in formatting and editing. Those three were responsible for the appearance of the report. John Hall of Caltech served as technical editor. I was fortunate to have such able co-workers.

Several Constraints

Housner: It was not so easy to put the board together. For one thing, the Governor said that nobody from any state agency should be on the board. Then he said that no one who had done any work for Caltrans should be on the board.

Of course, many of the structural engineers in San Francisco had done jobs for Caltrans. The board was to be quite free of Caltrans. So that limited the number to choose from. Also, of course, you could not have all the board from the Bay Area, or all from southern California. There had to be geographic balance. It was also necessary that the members of the board represent all relevant fields of knowledge. And they had to be people who were not going to get in there and rock the boat. And nobody did. It was a very good board. They were all very knowledgeable and interested, and did their work. The board members wrote the technical chapters of the report.

Holding Public Hearings

Housner: We had seven public hearings at which 72 individuals testified. The hearings took a total of ten days. Three of them were double days. We thought it very important not to end up with a situation where the report comes out and then somebody comes along and says, "Well, I knew something, and did not have a chance to tell you." That added up to a lot of people testifying, and the hearings turned out to be a big job.

It was the first time I had gotten involved in such hearings. You have to do a lot of arranging ahead of time. We had the first hearing in Sacramento, the second in Oakland, the third in San Francisco, then in Pasadena, then in Sacramento again, and so on. That all had to be arranged weeks ahead. You had to identify who should come to those hearings, see that they got invited, and try to see that they came.

John Hall and I sat down with Ben Williams and Peter Milne and agreed—"Well, we will

have the meeting on this day and hear these people." Then they got it all done. We were fortunate. I got a good impression of the people from the state government. I had learned earlier that the success of such a project depended on getting knowledgeable and reliable participants.

We got a lot of information from Caltrans. They were very cooperative. Of course we knew the people—Jim Roberts, and Jim Gates. They felt happy with the board, in that the board was not out to scalp them, but would be fair. And they provided a lot of information in testimony, reports and papers.

Scott: The background provided by Caltrans laid the basic groundwork?

Housner: Yes. They made presentations at each of the seven hearings. When they made one presentation, that would bring up other questions that would be answered in subsequent hearings. Also, we could not have just Caltrans testifying, so we found a consulting bridge engineer who had designed bridges for cities and counties, but not for Caltrans, and got him to testify. We tried to pick everybody we thought might have something to contribute, so as to hear them, or else make sure that they didn't have anything to contribute, if indeed they did not. For example, we had testimony from the Oakland city engineering department, and from the San Francisco building department. We got all sorts...everybody. Of the 72 people who testified, half did not contribute anything new, but by testifying established that there was nothing new for them to contribute.

We tried to cover everything. We even located a couple of the engineers who had worked on the design of the Cypress structure. They were retired. We thought, "Well, we ought to talk to them, there might be something they know that they would not want to talk about in public." So Alex Scordelis and I had them to dinner in Sacramento and chatted. It was clear that it was a terrible shock to them when the structure went down.

Scott: Did they testify later?

Housner: No, they were reluctant to come and testify. But I had wanted to be sure they did not say, "Well, something was not done right," and that sort of thing. But there was nothing of that sort that they wanted to say. And, of course, they were not really in a position to know too much. Forty years ago they were young guys, and were not then in the position of decisionmakers. The decisionmakers of 40 years ago were not available.

Preparing the Report: A Tight Schedule

Scott: You divided the writing up among the board members?

Housner: Yes, the drafts of technical sections of the report were done by the board members, but I am not going to tell you exactly who did what. We would say, "What would you like to do?" "Oh, I would like to do that." And so on. But that of course was only part of it, there was also the big job of making a consistent report—we were fortunate that they made a contract with Charles Thiel and Gail Shea.

Chuck Thiel is an independent engineering consultant based in the Oakland area, and Gail

Shea is an editor living in the East Bay area who works on earthquake-related manuscripts.

Thiel is very knowledgeable in earthquake engineering, and is also very good at writing, at the use of language. So the tone of the report is his contribution. He and Gail Shea had to edit the report to be a consistent whole, and they had to get the photographs and the drawings. We were very fortunate to work with them. Chuck put together the Findings and Recommendations part of the report in a form that could be read and understood by non-engineers.

When the Governor appointed us, he wanted to know, "When are you going to give us the report?" Well, I said June 1. It would take at least that long. And of course it would not do to say a year or more. So we did the hearings. Then the drafts had to be written and given to Chuck Thiel. So in a sense Chuck had only about two months—a real busy time. In putting the report into final shape, I read every word of the draft as Chuck Thiel submitted it to me, and made revisions.

Ben Williams went to the State Printing Office and asked if they could do it. They would have been glad to do it, but said they could not do it in the time we could give them. So they said, "Go to this private company." Chuck and Gail had to end up with hard copy, with everything laid out. They turned it over to the printer about nine days before June 1. The printer worked overtime and got it out. The first printed copies were put in our hands the day before we gave the report to the Governor. So we were very lucky. All along the line, there were possibilities for getting fouled up. But in editing it, and in printing it, everything worked out just right.⁶⁶

Scott: Getting such a report out on such a schedule must have taken a lot of intensive effort and attention.

Housner: Yes, it absorbed most of my time for six months.

It was also a very educational experience for me, and I learned a lot about Caltrans. My idea of Caltrans was as the agency designing free-ways and bridges, but when we got to looking into the matter, we of course saw that it was a bigger problem. There was more involved than just Caltrans and the bridges. Jim Roberts and the engineering is actually only a part of Caltrans. They for example also have an enormous maintenance crew handling things even to the signs on the freeways. There must be millions of them, so there must be a big sign department. And there is a real estate department, and so on. It was clear that engineering was only a part of the operation.

Caltrans Budgetary Process

Housner: Consider the budgetary process. Since the engineering is only part of Caltrans, when its engineers put in their budget request, it then must compete with the budget requests of all the other Caltrans units. Then above Caltrans is the Department of Transportation, which has other units and its own budget review. After that it goes to the Transportation Commission, then to the governor and to the legislature.

Scott: The budget-making has several steps from the time the engineers make their recommendations to the final adoption of the state budget. And budget items can be trimmed in each of those steps.

Housner: Yes, and I suppose that in each review they feel they ought to take a whack at it. Anyway, you can never be too confident about the engineering budget. For example, after the San Fernando earthquake when Caltrans decided to do retrofitting, they started to do a first phase of tying spans together with cables. Chiefly because of budget constraints, this took 17 years to complete. That is an unsatisfactory rate of progress.

At one point, in fact, the budget crunch forced the elimination of the earthquake engineering group. About 1975 Caltrans experienced an extreme drop in program funding due to the oil crisis, and *all* the young engineers in the then-budding seismic analysis unit were laid off, because they had very low seniority. Very little seismic work was done that year. After 1976, the seismic unit built back up to the point where in 1989 about six persons were working full-time on seismic matters, and by 1995 approximately 30 people in the Office of Earthquake Engineering were working exclusively on seismic issues.⁶⁷

Scott: So the Loma Prieta hearings brought home to you the size and variety of all the Caltrans activities? You were struck by the very magnitude of the Caltrans operation, and also the impact of size on the budget process?

66. *Competing Against Time: Report to Governor George Deukmejian from the Governor's Board of Inquiry on the 1989 Loma Prieta Earthquake.* Governor's Office of Planning and Research, State of California, 1990.

67. Information based on Housner telephone communication with James H. Gates, Caltrans 1995.

Housner: Yes. For example, they have 12,000 bridges. Usually, when you want to retrofit a bridge you get the plans, see what is involved, then go out and look at the bridge—maybe spending three or four days in the process. When you think about doing that for 12,000 bridges you are talking about something like 50,000 man-days. So dealing with the size of the operation alone is clearly a big problem.

Scott: You are describing the process of trying to establish appropriate priorities for retrofitting? What should be done first, and what next, and so forth?

Housner: Right, given the enormity of the operation and all those bridges. In addition, the cities and counties also have about an equal number of bridges. Caltrans also has some responsibility to help the cities and counties on that problem. I believe, however, that the passage of the ballot proposition at the election of June 1990 raising the gasoline tax has really helped Caltrans engineering. But budgeting remains an uncertain process.

Recommending Outside Help: Advisory Board and Peer Review

Housner: When we looked at the Loma Prieta damage, it was clear that the Cypress structure and the Bay Bridge were of very good quality. That is, the quality of the materials was very good, and the quality of the construction was very good. Everything was good, except the earthquake resistance. How did that happen? One weakness of early Caltrans work was that earthquake engineering research was lagging behind application. They were designing the Bay Bridge and Cypress structures before

the requisite knowledge was available from research. The research was always lagging behind the need. The research on how to reinforce concrete members so they will not fail in a brittle fashion was done after the Cypress structure was designed.

Scott: You are now referring to research in earthquake engineering and seismic design, generally, and not just research done by Caltrans?

Housner: Yes, earthquake engineering research in general, not just Caltrans.

Scott: What did you recommend to help with such problems of keeping up to speed?

Housner: One of our Board's recommendations was to set up a seismic advisory board. Caltrans actually set up two kinds of committee—a permanent earthquake advisory board, and peer review committees for specific projects. I think these have been proving very helpful. The Seismic Advisory Board, which reports to the director of Caltrans, is an independent committee of outsiders. The advisory board met oftener the first year but twice a year thereafter. The meetings are organized by the Division of Engineering, but the committee reports to the Caltrans director. This provides a way for the engineering questions to be brought to the attention of the director.

Scott: What was the advisory board's composition?

Housner: I was chairman, and the other members were Joe Penzien, Bruce Bolt, Nick Forell, Joe Nicoletti, Alex Scordelis, Ed Idriss, and Frieder Seible. I think we helped Caltrans, answering questions on seismic problems, and

otherwise advising and supporting them. One example was helping them with their response to the Governor's directive that they check all the bridges in the state.

Caltrans has about 12,000 bridges, too many for them simply to go out in the field and check. But they had the bridge data in their data bank, and worked up an algorithm, using the date a bridge was built, the number and length of spans, height above ground, the importance of the road, and such factors. Then they worked out a scheme for ranking the bridges. We helped them get that together.

Scott: You mentioned the peer review committees that are set up for specific major projects. What about this comment Joseph Nicoletti made in 1995 regarding a remark by Bill Moore in an oral history interview. Moore had suggested that he thought Caltrans was unduly limiting the scope of peer reviews:

Bill [Moore] is right about Caltrans limiting the scope of the peer reviews. I believe that Jim Roberts takes the position that peer review is required only for unusual projects, and to date it has been performed primarily for retrofit projects. Even for those projects, the review usually is initiated after concepts and schematics have been approved, and it is usually terminated before final design is completed.

Housner: I agree with Joe Nicoletti's comments. The Caltrans Seismic Advisory Board recommended that Caltrans plan peer review for selected representative projects, ranging from new construction of a simple one-span or

two-span bridge to the retrofit of a complex interchange. The Board recommended that the review include the whole process from concept to completion of final design. I believe that in due course Caltrans will follow the board's recommendation.

Other State Facilities

Scott: *Competing Against Time*⁶⁸ was aimed primarily at Caltrans, but it did have other implications beyond Caltrans. Say a little about that, and about the responses since.

Housner: Yes, the problem was obviously broader than Caltrans, and other state facilities were involved. We included those other things in the board's report, and felt we were justified, in that the board was an independent body and serving without remuneration. So we could say what we thought. The Department of General Services operates thousands of state buildings. Some they built and own, and some they rent. Earthquake hazard had never really been a consideration. We thought that a lot of the buildings might be hazardous, and need to be looked at.

There were also the University of California system and the state university system. Those systems have lots of facilities, and we have known that they have some serious problems. Things have happened telling us that something was wrong. And the problems are not limited only to the old buildings. There have been cases of university buildings—new buildings—in which mistakes were made.

68. *Competing Against Time*. State of California, 1990.

Scott: Some of their design processes and use of technical expertise seem to have been quite faulty, at least in the past.

Housner: Let me give you one example. There was Norris Hall, a three-story residential building at the University of California, Santa Barbara, a long, narrow building. One of the local structural engineers who visited the building thought there must be something wrong. He happened to be in the building and began wondering, "Where is its resistance?" He looked up the calculations and found that the engineer had slipped a decimal point in figuring the earthquake forces.

Scott: So the design error was discovered, but only after the building had been built?

Housner: Yes. The designers had slipped by one decimal point when they made their calculations, and greatly reduced the earthquake forces they designed for. With those forces so small, the two end walls would not provide enough resistance. After discovering the error, the University put in additional cross-walls. Then came the 1978 Santa Barbara earthquake, and some of the walls cracked. If those walls had not been added, however, the whole building probably would have gone down in the 1978 shaking.

Our report had also recommended that the state universities should make sure that everything is thoroughly checked. The public universities are not subject to the city building codes, and their designs are not checked by the local building department, one of whose main functions is to catch mistakes in designs before construction is started. In the past there obviously were cases where, because such checking

was not done, mistakes were made and were not caught.

Scott: The University of California and the state universities have needed the equivalent of a good, thorough municipal plan check, or of the Field Act plan check that public schools get. And then in addition for major projects they also need a peer review—a higher level of review.

Housner: If a strong earthquake hit UCLA or Berkeley, it would knock some of those old buildings down. It could do a lot of damage and cause a lot of casualties. I believe that now, in 1995, the University of California does have a program of retrofitting old hazardous buildings. The California State University system also has such a program—Chuck Thiel chairs the earthquake advisory committee that guides it. I thought it was a very good thing for them to form such a committee before the Northridge earthquake.

Vulnerability of the San Francisco Bay Area

Housner: Our Loma Prieta report also emphasized the vulnerability of the Bay Area. It is clear that the Bay Area, between the two major faults—San Andreas and Hayward—is between the jaws of a nutcracker. The Loma Prieta earthquake was just a warning, and we put that in the report.

Another influential publication following the Loma Prieta earthquake was the tabloid-type report the U.S. Geological Survey put together and issued in 1990. It reached some two million newspaper subscribers in the Bay Area, and was also distributed in foreign language editions.

Sometime afterward a survey was made. They asked people in the Bay Area, "What about future earthquakes?" Two-thirds of the people said, "Oh yes, we think there is a strong likelihood of getting a magnitude 7 or bigger in the Bay Area in the next 30 years." They have educated a lot of people. Circulating that brochure was a very worthwhile thing to do.

Scott: That publication was entitled *The Next Big Earthquake in the Bay Area May Come Sooner Than You Think: Are You Prepared?*⁶⁹ USGS staffer Peter Ward was the man behind it, and an excellent job was done. It was made available in English, Spanish, Chinese, Braille, and Recordings for the Blind. It was distributed very widely, is still available, and has been quite effective in reaching a lot of people.

Housner: Improved awareness of the hazard is very important. It is clear that when the big earthquake comes in the Bay Area, instead of being like Loma Prieta, which was 40 or 50 miles away from the main urban centers, it will be much closer. Instead of \$6 billion in damage, it will be more than \$100 billion, unless we do something. So it is really important that the Bay Area get going, because they have many old buildings in that region.

Executive Order and Initial Response

Housner: The Governor issued an executive order within a week after we turned in our committee's report. He put into effect our rec-

ommendations relative to the state agencies—that Caltrans should set up committees, that the universities and General Services should report on what they had done. I think that was very helpful. Caltrans did not need any prodding—essentially Caltrans did everything the report recommended. But I got the impression that the university systems and others needed some nudging. The Northridge earthquake was a very strong nudge.

Scott: The people associated with the Seismic Safety Commission were very much appreciative of the way the Commission was "written into" your report to the Governor.⁷⁰ Essentially the Commission was asked to monitor the agencies to see if they implemented the recommendations of the executive order. The Commission viewed that responsibility extremely seriously, and took a very considerable interest in the way the university systems responded to your Loma Prieta report.

Housner: Yes, in addition to the bridges, there are also the universities and state-occupied buildings. And after Loma Prieta, the Seismic Safety Commission did pick up on that aspect of our report. As I noted, UCLA and UC Berkeley undertook retrofitting programs, starting with their most hazardous buildings. That was an important step for UC Berkeley, which had some very old buildings.

Scott: Yes. It took a long time to get something going, and there was a lot of resistance to undertaking an active program.

69. Ward, Peter, *The Next Big Earthquake in the Bay Area May Come Sooner Than You Think: Are You Prepared?* U.S. Geological Survey, Menlo Park, CA, 1990.

70. Scott was a member of the Seismic Safety Commission from 1975 to 1993.

Housner: I think the Seismic Safety Commission was very influential in helping get something going at the University of California and also the California State University system.

Scott: Would you talk about the response immediately after the publication and the executive order? What was your experience then? Did your phone ring off the hook? What has happened since?

Housner: One observation is that we never got any criticism of the report. It is unusual not to have someone say, "Well, you did not have it right here, or there." But reactions to the report unanimously praised it, and we got a lot of compliments. When we put the report together we had in mind that it should be an educational thing. We hoped that students would be reading it in the future, and it was written in that way. We did not write it for the practicing engineers, and did not write it only for the Governor. We tried to explain things so that future students could read it and profit from it. That turned out well. I think students are able to read it and understand what happened. Also, members of the public can read the front part of the report and understand the nature of the problem.

Scott: So it turned out to be a very worthwhile effort, although I know the early deadline imposed a heavy burden on you.

Housner: Oh, yes, the exercise of the Board of Inquiry was very worthwhile. But it did take an awful lot of time and was a very heavy burden.

Scott: It was a remarkable accomplishment and has had a significant influence. It will prob-

ably continue for a long time to have some very considerable influence.

Housner: Hopefully it will also have an influence in other places—other countries.

Scott: It was quite widely distributed, wasn't it?

Housner: Yes. More than 5,000 copies were printed.

Another Kind of Report Needed: "What Does It Mean?"

Housner: I recall going to Sacramento one afternoon after I was officially appointed and meeting with a right-hand man—one who was implicitly trusted—who decided who the Governor would see and recommended what he should do. I told him that I thought the state ought to do another report, in addition to the one the board would do.

There would be engineering reports, such as by the EERI people. In addition, however, I said there ought to be a report at a higher level saying, "What did this earthquake do and what should it mean to the people of California and the state government?" I thought they needed a report telling the Governor and his office and the state agencies what to think about the earthquake.

The reports that EERI gets out are interesting mainly to practicing engineers and people like that. And the Board of Inquiry report was too narrowly focused for what I was talking about. After each major earthquake there ought to be a report telling the state government and the agencies what to think of it. What is the impact? What should we do?

Scott: The Governor's executive order issued after the Board of Inquiry report asked the Seismic Safety Commission to monitor and comment on the implementation progress reports of each of the state agencies and the two university systems.

Housner: Yes, but that was still not what I was talking about. I thought there ought to be another report. The report of the Board of Inquiry did not look at the overall problem of earthquakes, but was more narrow. I thought that in addition there ought to be a report that said, "This is what the earthquake did, and this was its effect on the state government." That kind of a report did not come out on the Loma Prieta earthquake. But such a report did come out after the Northridge earthquake. The Seismic Safety Commission's report, *The Northridge Earthquake: Turning Loss to Gain*,⁷¹ is the kind of report I had in mind.

Scott: Preparation of the Seismic Safety Commission's report on the Loma Prieta earthquake was basically contracted out to a consultant. After Loma Prieta, the Commission held a lot of public hearings and heard a lot of testimony, much of which was very valuable. The hearings were recorded in court-reporter fashion and the transcripts made available to researchers and the public in at least a limited supply. When it came to writing it up and saying what it all meant, however, work was mostly turned over to a contractor who met with the Commission periodically in workshops that reviewed what she was doing. Then

the report was issued, Seismic Safety Commission, *Loma Prieta's Call to Action: Report on the Loma Prieta Earthquake of 1989*.⁷² But the Commission had not been involved in the report preparation process in anything like the thorough way it was after the Northridge earthquake.

After Loma Prieta, one of the Commission's main efforts—assigned it by the Governor—was to follow up and report on the implementation of the recommendations of the Board of Inquiry, *Competing Against Time*. Also I believe the level of financial support available for the Commission's post-earthquake report was far smaller after Loma Prieta than it has been this time after the Northridge earthquake. A much more thorough job was done in preparing *Turning Loss to Gain* after the Northridge earthquake than the Commission had done after Loma Prieta.

Housner: Yes, the Seismic Safety Commission's report after the Northridge earthquake is the kind of report I had in mind.

Conclusion

Scott: How would you sum up this discussion?

Housner: The post-earthquake efforts went very well. As I noted before, Caltrans did everything we recommended. I think our report was very useful for Caltrans, and I also think they have been doing a very good job. With respect to bridge retrofitting, of course with so many bridges it is a very big effort.

71. *The Northridge Earthquake: Turning Loss to Gain*, Seismic Safety Commission. State of California, 1995.

72. *Loma Prieta's Call to Action: Report on the Loma Prieta Earthquake of 1989*, Seismic Safety Commission, State of California, 1991.

After the 1989 earthquake, by the time of the Northridge earthquake in 1994, they had retrofitted 250 bridges—which is a lot of bridges, so that is pretty good. Unfortunately, however, they had not retrofitted the bridges that collapsed in the Northridge area, although they were on the books to be retrofitted.

You can understand that with all the excitement after the Loma Prieta earthquake, and concern about a big one on the San Andreas fault or the Hayward fault, a lot of attention was paid to bridges in the Bay Area. And of course the 1994 Northridge earthquake was completely unexpected by the seismologists and geologists. So there would have been no reason for Caltrans to have singled out for earlier retrofitting those particular bridges that were damaged in Northridge.

To sum up, I think all this has been very successful, and Caltrans is going to continue with the advisory board—from which I retired in

1995. The board serves the purpose of bringing new thoughts and ideas in from the outside. That was one of the deficiencies before—they were not getting enough new viewpoints coming in from outside of Caltrans.

Scott: It is very important for a bureaucracy like Caltrans to be able regularly to get new ideas from sources beyond their own staff. Do you now feel reasonably confident that the changes will become a permanent part of the state system?

Housner: I think so, but uneven funding can be a major problem, as I noted earlier, and it does depend strongly on the chief of the Engineering Division. Jim Roberts has been very effective.

Scott: So in general you think the Caltrans record was pretty good in the years after the Loma Prieta earthquake?

Housner: Yes, they responded very well.

The Northridge and Kobe Earthquakes

“We know the cracking was associated with the welding, but we cannot say how a joint should be fabricated to make it 100 percent safe against cracking.”

Comparisons and Contrasts

Scott: In a remarkable coincidence, the Northridge and Kobe earthquakes were exactly a year apart. The January 17, 1995 earthquake in Kobe occurred precisely one year after the January 17, 1994 earthquake in Northridge, California. Both occurred early in the morning. Both appear to be very significant events in the field of earthquake engineering. Would you discuss their significance and what you think we ought to be learning from those earthquakes.

Housner: In addition to the calendar coincidence—occurring one year apart—there were also other close similarities. There was a near-coincidence in magnitude—the Northridge earthquake was $M6.7$ and the Kobe earthquake was $M6.9$. The Kobe earthquake was rated $M7.2$ on the Japanese magnitude scale, which is calculated differently from ours. Kobe's population is about 1.5 million, and the San Fernando Valley's about the

same. Roughly the same number of people were affected by the damaging ground shaking of each earthquake.

There were, however, also significant differences. Kobe had large areas of soft, weak soils along the waterfront, which the San Fernando Valley did not have. The two areas were unlike in age and degree of development. The Northridge area is relatively new and lacks a large commercial and industrial component, whereas Kobe has a large commercial and industrial development. A large part of Kobe's buildings are relatively old—pre-1981, when the Japanese code did not specifically include ductile design. The large life loss and monetary loss in Kobe was caused by building damage and collapse attributable to the many older buildings. Before that earthquake, there had been a general feeling in Japan's engineering community that structures in Japan were stronger and more earthquake-resistant than structures in the United States.

Scott: Yes, I had heard that the Japanese considered their engineered buildings to be somewhat safer. But you are now saying that the Kobe earthquake demonstrated otherwise. How do you think this misunderstanding arose?

Housner: In the early days—following the 1923 Tokyo earthquake and the 1933 Long Beach earthquake—the seismic codes in each country specified the 10 percent *g* design. In Japan this was later raised to 20 percent *g*, and it was believed that lowrise buildings in Japan were twice as earthquake-resistant as U.S. buildings built to seismic codes.

This assumption was not, however, necessarily accurate. Admittedly, other things being equal, it would take stronger ground shaking to produce the first crack in a Japanese building than it would take to produce the first crack in a California building. But this difference does not indicate the actual comparative earthquake resistance of the buildings. The ground shaking in Kobe would have produced seismic forces three or four times as great as their 20 percent *g* design envisaged, so a building's resistance would have to depend on its ability to survive ductile deformations. But ductility did not appear in building codes until 1971 in the U.S. and in 1981 in Japan.

Northridge Earthquake: Surprising Damage

Scott: There were some real earthquake engineering surprises in the Northridge earthquake.

Housner: Yes, the Northridge earthquake caused a lot of excitement on the part of earthquake engineers and researchers. The engineering community was quite shaken by the event, especially because of the steel-joint problem it disclosed. With respect to the other things that happened in the Northridge earthquake, we can say, "Oh, yes, we can understand that." But the engineers could not say that about the welded steel joints that cracked.

Understandably the steel damage captured most of the engineers' attention, and I will discuss that in more detail here. But we should not overlook the fact that the Northridge earthquake also damaged many wood structures, and this is leading to revision of the building code.

John Hall is editor of a report on the performance of wood buildings.⁷³

Cracked Steel Joints

Housner: The Structural Engineers Association of Southern California, the City of Los Angeles, and other organizations have convened many meetings and workshops. Their principal topic of concern was the cracked joints. In the valley area, some 200 steel frame buildings were identified as having cracked joints. In none of those cases, however, was anything visible from the exterior that indicated cracked joints.

The Getty Museum building was under construction and unfinished at the time of the earthquake. So when it cracked, the cracking could be seen. Also the Auto Club building was a steel frame structure, which for architectural purposes had a one-brick-wide facing around the steel columns. So when the steel moved, the brick broke and exposed the cracking. From then on, it sort of snowballed, with more and more owners checking their buildings and finding cracks in the steel joints. In February 1995 Los Angeles sent letters to the owners of about 400 steel frame buildings in the San Fernando Valley, directing them to have their buildings checked for cracked joints.

Surprisingly, none of the buildings identified with cracked joints in the Northridge earthquake had been instrumented by Tony Shakal under the strong motion program. It would have

been extremely helpful to have those records, which would give a clear idea of the forces at work when the joints cracked. But the program's advisory committee of engineers had said, "Do not instrument any steel buildings because that is a solved problem." I think this illustrates the difference in viewpoints between practicing engineers and academic researchers. Researchers would have said, "Let us measure the response to see if the problem is really solved."

A Broader Problem

Housner: The City of Los Angeles has issued an order that all steel frame buildings in the area of very strong shaking should be checked to see if the joints had cracked. In addition, however, there are a lot of steel buildings in downtown Los Angeles, which did not experience such strong motion. The city has been unsure what to tell the owners—should they ask the owners to check every joint, or to check some kind of sample? One idea is to check one-tenth of the joints, and if a crack is found, then another tenth of the remaining joints would be checked, and so on. It is a big problem.

Apparently, however, the city has decided not to require steel building owners in the rest of Los Angeles to check their joints, which would be difficult and expensive to do. In the construction process, the steel is covered by some kind of fireproofing and insulating material, which used to be asbestos. So to inspect joints, the asbestos or other insulation first has to be opened up or removed, and that raises another big problem. Some council members were quoted as saying that it might cost building owners \$4,000 per joint to do the checking. So

73. Hall, John F., ed., "Northridge Earthquake of January 17, 1994 Reconnaissance Report," *Earthquake Spectra*, Supplement C to Vol. 11, Chapter 6: Wood Buildings. EERI, Vol. 2, January 1996.

basically what they are saying is, "We don't know how to handle the problem."

Scott: The downtown buildings in Los Angeles were outside the area of very strong earthquake shaking, but are quite similar to the buildings that were damaged?

Housner: Yes, they are the same, really, although some in Los Angeles are much taller. And they did get a good shake during the Northridge earthquake, but not a severe shake.

Scott: And in future earthquakes they could be shaken as strongly as those in the Northridge area were this time? Also some of them could have been damaged by the Northridge shaking, but the damage is not visible?

Housner: Oh yes, the downtown buildings could be shaken as strongly in the future as those located in the Northridge area. The seismologists tell us that a fault similar to the one in Northridge goes under the central part of the city. So there could be a repetition of the Northridge shaking right in downtown Los Angeles. I guess we should say, "There *will* be such shaking, but we do not know when." So it is a big problem to figure out what to do about those buildings. One possibility is that some form of structural control could prove useful in limiting building response.

Scott: Regarding steel building damage in the Northridge earthquake, I take it none of those involved a serious life safety problem, as none of them collapsed. So at that level of shaking, at least, it did not involve a serious life safety problem?

Housner: Nothing collapsed, but we are not sure what might have happened if the earth-

quake had lasted longer. In general, engineers would point to unreinforced masonry buildings as the number one seismic life safety problem, and reinforced concrete frame buildings built before the 1971 San Fernando earthquake as number two. The steel frame problem would be rated as number three. Still, the collapse of a highrise building—perhaps in an earthquake that lasted longer than Northridge—would be a real disaster.

We talked with our colleagues in Tokyo, where they have many similar highrise buildings. The companies that build them said, "There is no problem here, we do it better in Japan." They said their welding is better. But steel joints were cracked in the Kobe earthquake, and even before that earthquake some of their academics were saying, "We think we do have a problem."

There are of course significant cultural and legal differences. In California, when an engineer designs a building and a contractor builds, if everything is done right according to code, and then later if the welds crack, the owner would have to fix it. The owner would not be able to go to the engineer and contractor and get them to do it at no cost. In Japan, however, if a big engineering company puts up a building, and say 20 years later there is some kind of trouble with it, the owner of the building could go back to the engineering company. "We bought this building from you, and it's no good—you fix it."

Scott: I take it that even if the Japanese engineering company did everything right, they would still be expected to make good on a building that later developed some trouble?

Housner: Yes, but perhaps the Kobe earthquake will be changing that. I have been surprised, however, that we have not heard of more lawsuits coming out of the Northridge earthquake. The joint design and welding procedures used followed the requirements of the American Institute of Steel Construction and the Lincoln Electric Co. The Lincoln Electric Co. makes the welding rods, and they recommend how the welding should be done. So there are some "deep pockets" there. Recently, I did hear that a class-action lawsuit had been brought against the Lincoln Electric Co.

Need for Research

Scott: It will take quite a while for this all to work out, won't it?

Housner: Yes. We still don't know specifically what caused the cracking to occur where it did. We know the cracking was associated with the welding, but we cannot say how a joint should be fabricated to make it 100 percent safe against cracking. Meanwhile, retrofitting of the buildings is proceeding slowly because of both technical and insurance problems. A lot of the buildings with cracked joints are standing there vacant, the owner just waiting until there is some consensus on what to do. Other owners, however, are going ahead with retrofits. The first thing the City of Los Angeles did was delete the code detail on how to do a design. Instead the city said that with a proposed building retrofit, the engineer should go talk to the building department and satisfy them about the retrofitting that is planned. If the city staff agrees, they can go ahead. The ones that are going ahead are saying that they are making the buildings at least as good as and probably bet-

ter than before the earthquake. That is presumably defensible, particularly as there are hundreds of other steel buildings in active use, and which are in the pre-earthquake condition. Obviously we are not going to solve the longer-term problem in a year.

Scott: Until we know more definitively what fundamental changes in the design and construction of steel buildings ought to be made, putting a building back so it is at least as good as its pre-earthquake condition may be about all we can ask?

Housner: Yes, a damaged building retrofitted to that level would be as good as the many other such buildings in the Los Angeles area that were not damaged.

Scott: The research will need to be completed, or at least well along, before we will know what to prescribe for the longer term?

Housner: Yes. When the SAC project issues its reports, that may help clear the air.

Evidence From Past Tests

Housner: In the past, very little testing was done on joints, and what was done investigated the ductility of the beam on the assumption that the welding was satisfactory. The tests were made to see how the beam would perform in a ductile fashion. There would be buckling of the flange of a beam, but if a weld broke, they would say, "Well, it was a bad weld." Their testing was looking at other things, mainly the deformation.

Professor Michael D. Engelhardt at the University of Texas, a student of Egor Popov and Vitelmo Bertero in Berkeley, had been involved

when they were doing the steel beam testing. Later he got some money and did his own testing. In reviewing the various test records, it dawned on him that cracked joints were noted in many of the tests. Tests by Popov, Bertero, and Engelhardt all showed cracked joints. So he wrote a paper on the subject, which was published by ASCE in December, 1993⁷⁴. Engelhardt pointed out that a significant proportion of the joints had failed because the welding cracked. He argued that such welding of the joints did not represent a reliable way of doing things.

Scott: At the time when the tests in question were done, probably the researchers were focusing on matters other than the welded joints, which I presume were incidental to the subject of the tests?

Housner: That is right. But then Engelhardt thought to look at the implications of all those cracked welds that were observed. I am told that his article caused consternation in the steel industry. Publication of his December, 1993, paper based on reviewing the past tests made him an early whistle-blower. His paper showed that in a sizable fraction of the tests the joints did not perform. So his December paper was throwing doubt on what was being done, and then immediately afterward the January 1994 earthquake provided evidence of damaged joints, which corroborated him.

Scott: So the January, 1994 Northridge earthquake provided immediate confirmation that his concerns had some justification.

Housner: Yes. When the Northridge earthquake hit, the Getty museum suffered cracked joints. The engineer on the job was Robert Englekirk, of Englekirk and Sabol in Los Angeles, who then got \$50,000 from Getty to design ten joints—five pairs—have them made in Los Angeles, and ship them to Engelhardt in Texas for testing.

Englekirk had sections made here in Los Angeles—a piece of column and a piece of beam. The tests were set up so sections made somewhat different from each other could be tested to see what worked best. Each pair of joints was identical in design, but welded by a different firm. The first pair was designed according to the pre-existing code, and those failed and cracked right away. The other pairs involved designs intended to make a stronger joint, adding plates to the top and bottom of the flange, welded to the column and the beam, different things like that. But the test results were ambiguous, as some joints cracked and some didn't.

A Challenge for Researchers

Scott: It is a fascinating and challenging problem for the researchers.

Housner: Yes, they will have to do a lot more tests of joints. In my opinion, the problem will require many tests of big beams and of various kinds of welding and welding configurations. So far they have not done enough tests to provide any statistical basis for making decisions. After enough tests have been done, perhaps we will be able to say something like, "When

74. Engelhardt, M.D. and A.S. Husain, "Cyclic-Loading Performance of Welded Flange-Bolted Web Connections," *Journal of Structural Engineering*. Vol.19, no. 12, Dec. 1993.

things are done according to this design and procedure, 95 percent of the joints will perform properly." If so, we can then go ahead with the design of the building, keeping in mind that maybe 5 percent or so of the joints may fail. But we cannot say anything like that now. It may take years to work this out. Various improved ways of making a joint have been proposed, and presumably will be tested.

Scott: Meanwhile this puts a big cloud over the design of new steel buildings not yet built.

Housner: Yes, also in other seismically active regions such as San Francisco and Tokyo, where it is also a big problem. The problem is only severe, however, in areas where strong ground shaking may occur.

FEMA is putting up money to study the Northridge earthquake, particularly the steel joint problem. I mentioned that a joint venture partnership called SAC has been formed of SEAOC, the Applied Technology Council, and CUREe (California Universities for Research in Earthquake Engineering). The goal of SAC is to develop professional practices and recommend standards for the inspection, repair, retrofit and design of steel moment frame buildings, to provide for reliable and cost-effective seismic performance of new construction.

Steve Mahin, president of CUREe at the time, was appointed to be the lead person in the joint venture, which he will do full time, having taken a year off from school at UC Berkeley. FEMA has employed Bob Hanson as its person to supervise the various activities—he has taken time off from the University of Michigan and is now out here. It was a very good step to get

Hanson in, because FEMA does not have any technical competence in earthquake research.

Northridge Earthquake Reports: Caltrans and SSC

Caltrans Report

Housner: The Caltrans report on the Northridge earthquake was written by the Caltrans Seismic Advisory Board, and was really a follow-up to *Competing Against Time*, except that it was done by and for Caltrans.⁷⁵

The subject of *The Continuing Challenge* was Caltrans and the Northridge earthquake—what happened, why, and what we should do. The Director of Caltrans, Mr. James van Lobensels, asked the Board to prepare a report on the effects of the Northridge earthquake on Caltrans structures. This was essentially a continuation of the report put out by the Board of Inquiry. Four of the members of the Board had also been members of the Board of Inquiry. Charles Thiel, Gail Shea and Laura Moger again helped with the preparation of the report. All of the recommendations in the report *Competing Against Time* were still relevant to the post-Northridge situation, and *The Continuing Challenge* emphasized the same recommendations again and added a few new ones.

The main new finding that came out of the Northridge earthquake was the possibility that destructive earthquakes could be generated on a so-called "blind" fault that does not have

75. *The Continuing Challenge: Report on the Northridge Earthquake of January 17, 1994*, Caltrans Seismic Advisory Board, State of California, 1994.

obvious surface traces left from previous earthquakes, and is thus not recognized.

The earthquake in Kobe, Japan collapsed some of the elevated freeway structures, so that some of the recommendations in *The Continuing Challenge* were also relevant to the concerns of Japanese engineers. Mr. Saporu Ohya, President of OYO Corporation in Tokyo, got permission to translate the report into Japanese, and had several thousand copies printed and distributed them to interested engineers. He said that he first printed 1,000 copies, but got so many requests that he printed 2,000 more. OYO Corporation has purchased Kinometrics and Agbabian Associates, so it is a major player in the earthquake community.

Seismic Safety Commission's Northridge Report

Scott: Earlier when discussing the Loma Prieta earthquake, we mentioned the Seismic Safety Commission's Northridge earthquake report, *Turning Loss to Gain*, on which the Commission did a very thorough job of preparation.⁷⁶

Housner: Yes, the Seismic Safety Commission's report after the Northridge earthquake is the kind of report I had in mind earlier when I said another kind of report was needed.

Scott: Northridge occurred after I left the Commission, but I am aware of the outstanding work of executive director Tom Tobin, the Commission, the staff and the consultants in analyzing the implications and significance of that earthquake. I think several factors were

involved. To start with, the Commission had the example of what had been done before on earlier earthquakes. Especially important was its having, at Governor George Deukmejian's request, followed up on state agency responses to the Loma Prieta Board's recommendations.

When Northridge came, the Commission had some seasoned leaders at the helm, both Commission members and staff, and was soon armed with an executive order from Governor Pete Wilson to do a comprehensive investigation, and got a substantial amount of state and federal funding to do the job. The result was that they did the kind of job I think you had been looking for all along. Tom Tobin, who had been thinking of other employment after many years with SSC, stayed on until the Northridge report was complete.

Tom Tobin and the Seismic Safety Commission

Housner: Yes, after ten years on the job, executive director Thomas Tobin left office in July 1995. During Tobin's tenure the Seismic Safety Commission played an important role in California, as well as in the United States.

Scott: I know Tom Tobin rather well, having been a SSC Commissioner from 1975 until 1993, and so having the opportunity of observing and working with him fairly closely during eight of his ten years as executive director. He was a remarkably effective public servant, and I think a lot of progress was made while he was with the SSC.

Housner: The Commission's vigorous approach under Tobin's leadership has done much to focus public attention on earthquake

76. *The Northridge Earthquake: Turning Loss to Gain*, Seismic Safety Commission. State of California, 1995.

hazards in California—particularly the hazards of state facilities, and those within cities. On returning to private life, Tobin established a consulting firm, Thomas Tobin Associates, which should find Tobin's government experience a most valuable asset. During the past ten years SSC has issued over 40 substantial reports on earthquake safety in California. Their continuing program and publications called "California at Risk," annual reports on the state's URM building law, and the landmark reports on the Northridge earthquake are excellent examples.

Scott: The Seismic Safety Commission also prepared a report for FEMA to help other states in seismic regions organize units similar to SSC.⁷⁷ Another very interesting report growing out of the Northridge experience is *Public Safety Issues*,⁷⁸ a collection of individual statements by Commission members, expressing their thoughts on matters that did not get a sufficient consensus to appear in the main Northridge report, *Turning Loss to Gain*.

Panel on Retrofitting the Los Angeles City Hall

Scott: While you still are on the subject of Northridge, would you say a word or two about the City Hall project?

Housner: In 1995 I was put on an advisory panel appointed by Los Angeles Mayor Richard Riordan to advise on retrofitting the City Hall, which was damaged by the Northridge earthquake. Charles Thiel, Allan Porush, and I

were the engineers on a 14-member panel. The building is a large structure built in 1928, with a tower projecting above the tenth floor up to the 28th story.

The project involves both a seismic retrofit, and a nonseismic rehabilitation. When the cost escalated from \$90 million to \$242 million, the City Council was alarmed, and asked for a special examination to explain the situation and make recommendations. The \$242 million covered the seismic retrofit, plus a complete upgrading of the 70-year-old building. Among the project's remarkable features is the estimated cost of \$52 million to evacuate the building for two years and then reoccupy it. The building has a steel frame that was designed for wind forces only, and its masonry walls have been cracked by five earthquakes: Long Beach, Tehachapi, San Fernando, Whittier, and Northridge. The damage to the tower in the last earthquake was enough to alarm the city administration.

Scott: What did the advisory panel conclude?

Housner: The design of the retrofit had been completed before the Northridge earthquake. The design called for placing the building on base isolation, and we asked the engineer, Nabih Youseff, to calculate the response to the Rinaldi accelerogram, which was very severe, to verify that the isolation clearance was adequate. The panel concluded that everything was all right, but that the seismic retrofit should be kept separate from the upgrade. In November 1996 the city had found sufficient funds to do the \$242 million job, both retrofit and upgrade.

77. *Creating a Seismic Safety Advisory Board: A Guide to Earthquake Risk Management*, Seismic Safety Commission, State of California, 1993.

78. *Public Safety Issues*, Seismic Safety Commission, State of California, 1995.

Ending My Thirty Years of Service

Scott: With the Caltrans report on Northridge wrapped up, I believe you discontinued Caltrans responsibilities?

Housner: Yes. I had planned to do that earlier, but then the Northridge earthquake came. I have also stepped off of the advisory board of the Division of Safety of Dams. In retrospect, I feel that serving on these committees—whether Water Resources, or Division of Safety of Dams, or Caltrans—was really helpful both to the agencies and to me. The committees brought the agencies a lot of thinking on what could be done, and the experience was also educational for me.

Scott: You were able to help bring in other people and their ideas. Also I think your willingness to stick around was important—just staying there consistently and being available. Also the kind of respect you have nationally and internationally no doubt was an important factor that made your help especially valuable.

Housner: The big water project was the first such major project in which earthquake considerations got in at the beginning, or almost the beginning. Previously, that was done after a project was designed, and then they would try to figure out if there was some problem with earthquakes. That was a first time for considering earthquakes from the beginning, which is a good policy. I believe it set the standard for many other major projects. (I have already discussed the San Francisco area's BART project, which took the earthquake problem into account from the beginning. The BART project was not as large as the Sate Water Project, however, and was done by private

engineering companies—Bechtel, Tudor, and Parsons-Brinckerhoff.)

Kobe Earthquake

Initial Mixed Reports

Housner: Many of the traditional wood buildings in Kobe collapsed, so wood structures are also a problem there. When I was there I saw cracks in the steel frames of some of their buildings, but at that time not many buildings in Kobe were known to have cracked joints. Recently, however, I have seen reports saying that many steel frame buildings in Kobe did have cracked welded joints.

I saw photographs of one building in which square columns were made of plates about 2-1/2 inches thick. At the middle of the first story, the column cracked right across. During the earthquake when the building was vibrating back and forth, the column was pulling up and down on the foundation, and when pulling up and in tension it must have cracked right through. That would have been different from the cracking of the joints.

Scott: Both the Northridge and Kobe earthquakes have posed a number of yet-unanswered questions, haven't they?

Housner: Yes, we have seen some strange things happening. In quite a number of the Kobe buildings, a story collapsed up in the middle of the building. In our earthquakes we have seen first-story collapses, but these collapses occurred farther up. We think the collapse occurred where there was a change in the type of construction. We have been told that there was a 24-story building in which the fifth

story collapsed. So above the fifth floor there are 19 stories still sitting there. It seems unbelievable. In Kobe they apparently used a system in which the first four stories were made of steel, and then there was a transition to reinforced concrete. So this fifth-story collapse was right at the transition.

In some of the other buildings, however, the collapse occurred at other levels. Anyway there will be a lot of things to learn from the Kobe earthquake. A lot of people went over right after the earthquake, and Caltech sent a team over. Some months afterward, Sami Masri and I were in Japan and made a side-trip to visit Kobe. Now we are hoping that the evidence will begin to be somewhat more sorted out.

Scott: It was a very damaging earthquake. Was this because it was an unusually severe earthquake, or was it more a matter of weak structures?

Housner: Kobe got some very strong shaking in that earthquake, comparable to the Northridge shaking, and the fault went directly through town. Nevertheless the severity of the damage can better be attributed to weakness in the buildings or in the soil. New buildings in Kobe designed after their 1982 code revisions performed satisfactorily.

Obtaining Information Promptly Via the WWW

Housner: An interesting thing that has come out of the Kobe earthquake is the use of the World Wide Web. About a week after the earthquake we discovered that the Japan Railways had put on the Web a list of peak accelerations taken from 20 of their stations. We

realized right away that this was a solution to our problem of quick access to such data, immediately following an earthquake.

In the past, it has always been difficult to get such information, except for earthquakes occurring in California. As I noted earlier, for California earthquakes, we have been getting data promptly through Tony Shakal and the state's Strong Motion Program. But we have not been able to get the data from other countries. There has been a reluctance to give out information. If, for example, the Japanese have an earthquake and we try through official channels to get copies of their ground motion records, nothing happens. The reluctance to give out data and accelerograms is partly due to those who are in charge of the data hoping to publish research based on the information. Later the data appeared in official reports.

But I believe things are changing. Thus, from the Japan Railway people I received a report on the earthquake on Hokkaido Island. It was entitled "Prompt Report," very like the "Quick Report" title used by Tony Shakal and the California Strong Motion Program. That was at least a good start, and they probably got the idea from seeing what the California program had done.

Regarding Web use, I wrote Dr. Tsuneo Katayama the secretary and Professor Sheldon Cherry, the president of the International Association of Earthquake Engineering, pointing out how the Web offers an opportunity for quick dissemination of information. Bill Iwan consented to provide leadership in developing a scheme that all countries could agree on and follow, so the information could be presented in a uniform and consistent manner. For exam-

ple, give the peak accelerations, include a typical accelerogram, a map, and indicate how to locate the material on the World Wide Web. At the end of 1996 this had not yet been accomplished.

Scott: That would ensure some consistency in both content and format—what is included and how it is presented—and provide the information quickly.

Housner: Yes. With that arrangement set up and accepted by the association, the national delegates from each country could be responsible to get information for that country put on the Web. Then IAEE could notify its membership worldwide as to the information's availability. It is important that people be alerted to what is available, and where. The first time we found information on the Web about the Kobe earthquake we just happened on that data. Apparently other things about the Kobe earthquake were also on the Web, but we never found them.

So it is important to get the appropriate information on the Web and do it quickly, let interested people know that it is there, and indicate where they can find it. I believe this will solve our past problems of earthquake information access. You can get the information from the World Wide Web and have it printed out. What is given out would give a good picture of the earthquake, but would not in itself be good

enough for someone to do an analysis from it—that would come later.

Scott: Enough information can be given out to enable everybody to understand right away the basic information about the earthquake, but not enough detailed data is divulged to "give away the store" so to speak?

Housner: Yes, that is about it. And the staff people who collect the information can put it on the Internet without having to get permission from higher ups, they can just do it. It seems like a good logical solution to the problem of quick access to basic earthquake data. After an earthquake, interested people everywhere can get a prompt update so they understand what the general situation is in terms of the basic seismic data. I certainly think that such quick and widespread distribution greatly outweighs any losses from giving out one's own information.

In the old days it was very difficult to get the information, and while we waited we would get all sorts of stories and misinformation, often quite wrong. This could be pretty serious in the area and country affected, because a lot of decisions are made in the first few months after an earthquake. It also affects other countries that may have to make some decisions. So it is important to have correct information available as soon as possible.

National and International Activities

"... a good report can influence projects in many parts of the world."

Scott: Your interest in and dedication to earthquake engineering has taken you almost literally all over the world for the past half century. Would you cover the highlights of your activities of a national and international scope?

Housner: You are right about earthquake engineering having taken me to many places. I was musing just the other day about how earthquake engineering led to my shaking hands with three Presidents. When Don Hudson and I were in India in 1959 to help the University of Roorkee start its earthquake engineering work, we shook hands with President Nehru in New Dehli at the celebration of Independence Day. Then at the Sixth World Conference on Earthquake Engineering in India in 1978, we shook hands with Indira Gandhi, President Nehru's daughter and President of India at the time. Then when Ronald Reagan was U.S. President, I shook his hand when I was awarded the National Medal of Science at the White House in 1978. When he shook my hand, Reagan said, "Ah, from California."

Scott: Among the most important international earthquake engineering activities has been the interaction between the U.S. and the Japanese. Bruce Bolt especially recommended

that I ask you to comment on these developments, whose roots go back many years.

Before World War II

Housner: In the previous generation, well before World War II, and especially in the late 1920s and early 1930s, there had been significant interaction between the U.S. and the Japanese. In those days we were getting guidance from a few of the Japanese engineers in making a start in earthquake engineering. R.R. Martel and John R. Freeman were particularly active on the U.S. side, and Kyoji Suyehiro and Tachu Naito on the Japanese side.

I noted earlier that Freeman wrote an important 1932 book on earthquake engineering. He was also instrumental in bringing the work of the Japanese to the attention of U.S. engineers. Naito, of Waseda University, wrote a book on lowrise earthquake-resistant design shortly before the 1923 Tokyo earthquake. Also, some buildings Naito had designed by the 10 percent method survived the earthquake, and for that he became famous. Kyoji Suyehiro was the first director of Tokyo University's Earthquake Research Institute, and late in 1931 gave earthquake engineering lectures in the United States.

At that time, the early 1930s, the Japanese were ahead of us. Later, however, the Japanese engineers were rather disorganized by World War II and its aftermath. So they were behind us in post-war work in spectrum analysis, computing, dynamic responses, and accelerographs, but have now caught up and are doing more in experimental research than the U.S. is doing.

Post-War Era

Housner: We had our first post-war experience with the Japanese in 1955 when Don Hudson and I went over there to spend a month. We were the first contacts on earthquake engineering to visit Japan after the war. We went around talking to the Japanese earthquake engineers and sightseeing. We met professors Tachu Naito, Kiyoshi Muto, Hiroshi Kawasumi, Shunzo Okamoto, Yukio Otsuki, Kyoji Nakagawa, Kazuo Minami, Kiyoshi Kanai, Nobuji Nasu, Toshihiko Hisada, Ryo Tanabashi, and assistant professors Keizaburo Kubo, and Takuji Kobori. After this initial trip in 1955, our contacts were greatly expanded by the Second World Conference on Earthquake Engineering, held in Tokyo in 1960.

Scott: They were behind us when you visited Japan roughly 10 years after the war?

Housner: Yes, they had not gotten organized after the war, whereas before that, they had been well organized. Now they are again well organized and are ahead of us in experimental research. They only developed their accelerographs and shaking machines after we had ours and showed how important that was. I would say they were trailing behind us until about 1965. More recently, however, say in the last 10 years, they have gotten ahead of us in some ways, especially in experimental things. They have put a lot of money into earthquake engineering experimentation. They have enormous shaking tables and reaction walls, shaking machines, all sorts of things.

They built a big shaking table facility on Shikoku Island, which as near as we can make out cost maybe more than \$200 million. In the

city of Tsukuba, their science city, the government built a civil engineering laboratory—two big experimental facilities for civil engineering—which are mainly devoted to earthquake problems. They told me they cost about \$350 million. When they were building their big suspension bridges, they also built a big shaker with an enormous mass that they could put on a bridge and cause it to vibrate.

Scott: They began experimenting on the bridge with the big shaker?

Housner: Yes, they are way ahead of us in that game. They seem to have a lot of money available for earthquake studies, which makes a big difference.

Scott: Because of their vulnerability, the Japanese nation naturally has a deep concern about earthquakes.

Housner: Yes, their big 1923 earthquake essentially destroyed the capital of the country and killed 100,000 people. They do not forget, and they get many reminders of the shaking.

We Maintain Close Relationships

Housner: We have close relationships with Japanese researchers. There is now a lot of concern in this country that the Japanese are moving ahead of us on the experimental side, which is the source of the basic data needed for analytical studies.

Scott: Are we able to learn from what they learn? Or does a good deal of the knowledge become a proprietary matter not generally available?

Housner: That is another problem. The language barrier is also an important consider-

ation. They read all our things, but we cannot read theirs. We tried once to set up a program to translate, but that fell through. Professor Masanobu Shinozuka, then at Columbia University and now at the University of Southern California, had that project, but he said when he tried to translate the papers, he found them so condensed and ambiguous that he could not do it. I think the style of writing a paper in Japan differs from ours. The only way to do it would be to sit down with the author and then translate. So language is a difficult problem. The Japanese researchers can all read English, but most U.S. researchers cannot read Japanese. I think closer cooperation and communication between researchers in the U.S. and Japan should be encouraged. It would help both sides. We are now trying to develop closer relations between university researchers. We are doing this primarily with Professor Kenzo Toki, who is at Kyoto University.

UNESCO-JAPAN International Institute

Housner: In 1965, when I was at the Third World Conference on Earthquake Engineering in New Zealand, Dr. Fournier d'Albe of UNESCO asked if I would serve on the newly formed Board of Directors of an earthquake school in Tokyo. Fournier d'Albe was very active in earthquake concerns, and particularly in promoting attention to the subject in developing countries. UNESCO and the government of Japan had agreed to the joint establishment in Japan of the International Institute of Seismology and Earthquake Engineering (IISEE). Each party appointed two persons to the Board of Directors.

Scott: Since there were only two parties, UNESCO and Japan, this was a four-member board?

Housner: Yes, plus some ex-officio members. The Institute funded twenty individuals—ten seismologists and ten earthquake engineers—to spend a year there.

Scott: This arrangement to bring twenty each year was to be continued on a long-term basis?

Housner: Yes, and it is still in operation. In addition, two foreign professorial visitors were invited to spend a year in Japan at the Institute. Joe Penzien spent a year there as visiting professor, as did Norby Nielsen, who was there at the time of the 1964 Niigata earthquake, as well as others whom I knew. The school celebrated its thirtieth anniversary in 1995, and invited Don Hudson to be a speaker at the ceremonies. The school is now located in the city of Tsukuba, at the Building Research Institute. In the beginning, Dr. S. Omote was director of the school.

When I went to the Board of Directors meeting following the Niigata earthquake, I asked if I could visit the city, to see the damage firsthand. I visited Niigata with Professor Robert Stonely, who at that time was the other UNESCO-appointed director. He was the discoverer of the Stonely waves in seismology. Our visit to Niigata was how I got to view the extensive damage caused by soil liquefaction in that earthquake.

First Report on Earthquake Engineering, 1969

Scott: In addition to your international activities, you have also done a lot with the fed-

eral government in Washington, some of which you have already touched on in passing. Would you take a little time to treat some of those activities more thoroughly?

Housner: A little after the Alaska earthquake, which I discussed earlier, back around 1965, the office of the President's Science Advisor formed a seismological advisory committee to make recommendations on needed research. Frank Press chaired the committee, and asked me to serve on it. After attending a meeting, I realized that appointment of a "token" engineer had been an afterthought, and that the committee's final draft had to be delivered in a few weeks.

Scott: I take it you were appointed after the committee had already been active for quite a while and had its report nearly ready to go?

Housner: Yes. So Don Hudson and I quickly got to work drafting a short report on earthquake engineering, which was appended to the seismological report. We realized, however, that this was not a suitable presentation of earthquake engineering, and a year or so later I submitted a proposal to the National Academy of Engineering that they form a committee to prepare a report on earthquake engineering and needed research. After some time the National Research Council (NRC) organized a 13-member Committee on Earthquake Engineering, which I was asked to chair. This time I arranged to do a thorough job, and involved many people concerned with earthquake engineering. I believe the NRC got funding from NSF. Robert Cliffe was the NRC man in charge of the project.

The report⁷⁹ was divided into eleven chapters, each addressing a different topic, such as performance of structures, strong ground motions, geotechnical engineering, etc. The writing was done by seven panels that involved a total of 43 individuals. After a lot of hang-up time in NRC, a 213-page report was published in 1969 and given a wide distribution. I think it was very effective, because each chapter explained the problems of earthquakes and engineering, as well as recommending needed research. This was the first time earthquake engineering had been described as a discipline.

Scott: This was a quite separate and distinct project from the engineering volume on the Alaskan earthquake?

Housner: Yes, it was a different type report and was not on the Alaska earthquake, although that earthquake triggered my thinking. An embarrassing thing happened with that report. When the report was printed I saw that somehow Bob Whitman's name had been left off the list of participants, although he had been an active contributor, so it was a most regrettable oversight.

Committee on Natural Disasters, 1967-1993

Housner: Several things were going on then at about the same time, including the work of the seven panels on the Alaska earthquake (including my panel on earthquake engineering), which I discussed earlier, and the report on earthquake engineering research, noted

above [see Chapter 6, "Earthquake History and Reporting"]. In addition, I should also mention a third effort, which was on natural disasters in general. All three of these were committees of the National Research Council.

After the Alaska earthquake it was clear that a lot of misinformation was showing up in the newspapers, and this was affecting the decision-makers in the cities. There ought somehow to be an accurate factual report available that would exclude that kind of misinformation. Again I wrote to the Academy of Engineering, proposing that a committee be set up to look at earthquakes and other natural disasters, and come out with such a report. It did not necessarily need to be a big report, but at least a factual document to help correct misinformation or circumvent its becoming part of the permanent record. The National Research Council set up the Committee on Natural Disasters in 1967, and the first project was to report on the 1967 Caracas, Venezuela earthquake.

I recall that I asked Mete Sosen to visit Caracas, and when he arrived he called me, saying that it was an important event, so Paul Jennings and I went there. That committee now comes under the NRC Board of Natural Disasters. I was chairman of the initial committee, which issued a number of reports on natural disasters: earthquake, flood, wind, and volcano.

Origins of NEHRP

Housner: In the early part of 1969, before the San Fernando earthquake, I got a telephone call from Ann Wray, a young woman in Senator Alan Cranston's office. Cranston was then a newly elected U.S. senator from California. She said that Senator Cranston was interested

79. *Earthquake Engineering Research: A Report to the National Science Foundation*. Prepared by the Committee on Earthquake Engineering Research, National Research Council, 1969.

in preparing a bill to reduce damage from natural hazards, earthquakes, wind, flood, and so on. She asked me about each of them. "Is some government agency involved with winds?" Yes, there are two agencies involved in winds. Then she asked, "Well, what about floods?" The Corps of Engineers deals with floods.

Each time I named an agency as being responsible for a hazard, she said, "We won't include that." Apparently they did not want a bill that would step on somebody's toes. I suppose it would be more confusing and difficult for the bill if you got some established agency saying, "Well, we don't want that." And that left only earthquakes. After talking with Ann Wray, I sent her a copy of the 1969 National Research Council report on earthquake engineering research, which laid out the problems and made recommendations, and I talked with her several times when I was back in Washington.

Scott: So Cranston's office excluded hazard topics from the legislation if they found existing agencies dealing with those topics, and that process trimmed the bill down to just earthquakes?

Housner: Yes. Earthquake disasters were the last item left, since no government agency was dealing specifically with them.

Scott: That was the beginning of the legislation that Senator Cranston and California Congressman George Brown pushed through Congress in 1977, setting up the National Earthquake Hazards Reduction Program?

Housner: Yes. The San Fernando earthquake occurred in 1971 and got a lot of attention. The event apparently had significant repercussions at the National Science Founda-

tion, which already had a modest earthquake engineering research program going.

Scott: With San Fernando, earthquakes had suddenly achieved a higher level of significance.

Housner: Yes, and since NSF already had a program, Mike Gaus was able to get it enlarged to fund substantially more earthquake engineering research.

Scott: That also laid more groundwork for passage of the NEHRP legislation.

Housner: Yes, the National Earthquake Hazard Reduction Program, established by the Cranston bill passed in 1977, has been extremely helpful in developing earthquake engineering and funding earthquake engineering research. Certainly without the contributions of NSF, progress in earthquake engineering would have been much slower.

I should mention here some of the persons involved in the NSF program over the years and with whom I had good relationships. Originally there was Mike Gaus and Chuck Thiel, and later Jack Scalzi, Fred Krimgold, Cliff Astill, S.C. Liu, Bill Anderson, Nora Sabadell, and William Hakala. In the early days the NSF staff members would visit the universities to discuss the research that was going on, although as the program expanded they were unable to keep this up. Also, an academic would take a year or two leave of absence from his home university and spend it at NSF. An example was Henry Lagorio, from UC Berkeley.

Delegation to China, 1978

Housner: In 1978 I had a very interesting experience when I led a twelve-member team

to China, the first U.S. earthquake engineering team to visit that country. It was organized by the National Academy of Sciences as part of the U.S.-China Cooperative Program that was agreed upon when President Richard Nixon visited Chairman Mao in 1970. Apparently, the only subject that everyone on both sides felt agreeable about undertaking jointly was earthquakes, i.e., seismology and earthquake engineering. A seismological team had visited China after the Hai-Cheng earthquake in 1974.

Later, in 1976, the catastrophic Tangshan earthquake occurred, and was the main reason that the U.S. team wanted to visit China in 1978. It was a three-week visit, during which we started at Beijing, went to Harbin, where we visited the Institute of Engineering Mechanics (IEM), China's big earthquake engineering research laboratory, and returned to Beijing. Next we flew to Cheng-Du, the capital of Sechuan province, which is next to the foothills leading to Tibet. Then we flew back east to Guilin, then to Canton, and Hong Kong, and returned home. While in Beijing, Paul Jennings and I met with Caltech alums H.S. Tsien and C.M. Cheng, who successively occupied the post of director of the Institute of Mechanics in Beijing. Cheng's son also received his Ph.D. from Caltech, and is now at the General Motors Research Laboratory.

The 1978 team was composed of well-known names in earthquake engineering and earth sciences. Members of the delegation included Paul Jennings, reporter, Ray Clough, Genevieve Dean, Henry J. Degenkolb, William Hall, Liu Shih-Chi, R.B. Matthiesen, Joseph Penzien, Teng Ta-Liang, Robert Wallace, Robert V. Whitman, and myself as chair. The team

prepared a report that was published in 1980 by the National Academy of Sciences.⁸⁰

I recall that during the tour, team member Henry Degenkolb made a great impression on the Chinese engineers. He impressed them with his knowledge of earthquake engineering, and with his authoritarian pronouncements as to what he thought.

Scott: Henry was an impressive person in many ways. He knew a lot about practical earthquake design, and made a career of visiting earthquake sites and giving his interpretation of what he saw. He could also be pretty forthright and blunt in saying what he thought. But I take it he impressed the Chinese engineers in yet other ways?

Housner: Well, one other way was when they tried, unsuccessfully, to drink him under the table. Prior to our last banquet in Beijing, before departing for eastern China, Dr. Hui-Xian Liu told me that they had arranged for their best drinker to sit next to Henry. Toasting with maotai, he would try to drink Henry under the table. But at the end of the evening I noticed that the big drinker had to be helped out of the banquet room, whereas Henry was still himself. Dr. Liu admitted defeat, and said they would establish the Degenkolb scale, with the degen as the unit of measurement.

80. Jennings, Paul C., *Earthquake Engineering and Hazards Reduction in China; A Trip Report of the American Earthquake Engineering and Hazards Reduction Delegation*. National Academy of Sciences, CSCPRC Report no.6, 1980.

Second Report on Earthquake Engineering, 1982

Housner: After finishing the earthquake engineering report, the committee was disbanded. About ten years after publication of the 1969 report, Robert Cliffe, the man at the National Research Council who was the executive secretary for the committee on natural disasters and for the committee on earthquake engineering retired and was replaced by Allen Israelsen, who was very helpful to the program.

Israelsen got hold of me and said, "It's about ten years since that first report on earthquake engineering came out. Isn't it now time that you looked at the field again to see what's happened, and make further recommendations?" I agreed, and he raised the money from NSF. In those days, I wasn't clear on how it was done, but now I understand what he would have done. He would have written up a proposal, which was then approved by the National Research Council administration, saying what they wanted to do and how much money it would take. Then he would go to NSF and say, "Will you give us some money?"

He proceeded that way and got the funding. We set up another committee and got out a report called *Earthquake Engineering Research, 1982*.⁸¹ Both that report and the previous report have been given wide circulation, and I think have been very influential.

Two Workshops on Strong Motion Instrumentation—1978, 1981

Housner: An international workshop on strong motion instrumentation was held in Honolulu in 1978, with funding from NSF. The workshop was organized by Bill Iwan of Caltech, and produced a very influential report that helped people in many countries to install instrumental arrays.⁸² It shows how a good report can influence projects in many parts of the world. The success of such a project depends, of course, on having a capable organizer, on bringing together the right mix of people, and being adequately funded, in this case by NSF.

Bill Iwan had a very efficient arrangement. The bus picked us up at the hotel and took us to the university for breakfast, where we met at the East-West center. Lunch was served there, and we worked until 6:00 p.m., when the bus took us back to the hotel. Some participants complained that they never saw the beach.

A similar national conference on strong motion instrument arrays was held in Santa Barbara in 1981, again organized by Bill Iwan and supported by NSF.⁸³ One of the recommendations of the 1981 workshop was to set up an oversight committee on the strong motion problem in the United States. So Bill and I talked about

82. *Strong Motion Earthquake Instrument Arrays: Proceedings of the International Workshop on Strong Motion Earthquake Instrument Arrays, May 2-5, 1978, Honolulu, Hawaii*. California Institute of Technology, Pasadena, CA, 1978.

83. Iwan, W.D., ed., *U.S. Strong Motion Earthquake Instrumentation: Proceedings of the U.S. National Workshop on Strong Motion Earthquake Instrumentation, April 12-14, 1981*. California Institute of Technology, Pasadena, CA, 1981.

81. *Earthquake Engineering Research—1982*. National Research Council, Committee on Earthquake Engineering Research, National Academy Press, 1982.

it with people back at the National Research Council, and at NSF and USGS, saying, "We ought to do that—there's a big need for that sort of thing." But we never did get an effective oversight committee established.

Standing Committee on Earthquake Engineering

Housner: Probably around 1982 or 1983 a new Committee on Earthquake Engineering was established at the suggestion of Al Israelsen, with the idea that it would be a standing committee periodically holding meetings, providing advice, etc. Funding came from the Earthquake Hazards Mitigation Program of NSF. I agreed to chair the committee, and we arranged for it to meet twice a year. At the second meeting each year, representatives of all concerned government agencies appeared to review the earthquake situation in each of the agencies, after which the committee would comment and make recommendations. I recall representatives from the Corps of Engineers, the Bureau of Reclamation, the Navy, the Air Force, the Department of Energy, NIST, the Nuclear Regulatory Commission, NSF, USGS, FEMA, Defense Nuclear Agency, Federal Highway Administration, Department of Housing and Urban Development, Veterans Administration.

On behalf of the Committee on Earthquake Engineering, particularly, I had to make frequent trips back to Washington. I learned that it was harder than I had expected to get a standing committee of that sort organized and its credibility established. The difficulty in getting it established kept me from stepping down as chairman at the end of the three-year term

of office. Al Israelsen said, "Don't go now, because we're not really established yet, after only three years." So I agreed to stay on as chairman.

The committee undertook a number of projects, one major effort being to convene a workshop on earthquake problems in geotechnical engineering. Bob Whitman organized the two-day workshop, held at an MIT facility in Boston, with 35 attendees. This included most of the U.S. geotechnical engineers who were particularly interested in soils and earthquakes, as well as Liam Finn, University of British Columbia, Andrew Schofield, Cambridge University, England, and some representatives from other countries. One of Schofield's contributions was an early poem by the English poet Robert Herrick, who lived in the 1600s. The poem, entitled "Upon Julia's Clothes," has an early mention of liquefaction.⁸⁴ It reads:

When as in silks my Julia goes
Then methinks how sweetly flows
The liquefaction of her clothes.
Next, when I feast my eyes and see
That brave vibration each way free;
Oh how that glittering taketh me.

I believe this was the first time geotechnical engineering and poetry interacted.

A 240-page report of the workshop, *Liquefaction of Soils During Earthquakes*, was published in 1985.⁸⁵ The workshop was followed by three

84. Herrick, Robert, "Upon Julia's Clothes," *English Verse*, Oxford University Press, 1940.

85. *Liquefaction of Soils During Earthquakes*, Committee on Earthquake Engineering, Commission on Engineering and Technical Systems, National Research Council, National Academy Press, 1985.

seminars on geotechnical engineering held in Washington, D.C., Denver, and San Francisco, and each attended by 100 to 200 persons, and by the committee report. I think this was the first time geotechnical problems of earthquake engineering were clearly presented to audiences of engineers and geologists.

Shortly after the 1985 workshop, Al Israelsen retired and was succeeded by Riley Chung. The organization of NRC was later revamped, the Board on Earthquake Engineering dissolved, and a Board on Natural Disasters organized. I was not involved in that committee, but Bill Iwan was a member and chair. (I recall Chuck Thiel, with his Washington experience, once telling me that NRC identifies a willing horse, and then works him to exhaustion. I suppose in my NRC committee work I was an example of that.)

Given this earlier Washington, D.C. activity, I was surprised when neither the 1989 Loma Prieta nor the 1994 Northridge earthquakes appeared to have an appreciable effect on Washington. While both earthquakes had a strong influence on California, they did not seem to prompt any significant additional Washington effort to face up to the earthquake problem, despite the substantial earthquake risk that prevails in many parts of the United States.

Administration of NEHRP: A Major Problem

Housner: There have been complaints about the National Earthquake Hazards Reduction Program (NEHRP). The central problem is that the four government agencies involved in the program have no common interests. Also,

when the program was set up, FEMA was designated lead agency, but has no particular competence in engineering or seismology, or research generally. In administering the program, FEMA did not try to coordinate it or tell the other agencies what to do. Each agency went its own way. FEMA's agency objective was to provide relief and recovery from natural and man-made disasters.

Then several years ago they appointed an advisory committee to FEMA—advisory on the NEHRP program. The original idea was to have a committee composed of people so eminent that the agencies would all be likely to go along with what they advised. But they did not do that, instead they made the committee much larger, ending up with about twenty people, selected seemingly at random from a variety of disciplines.

Finally, the FEMA committee got out a report that was very critical of the program.⁸⁶ As a consequence, Congressman George Brown, who had a big role in establishing NEHRP, was joined by six other representatives in sending a letter to President Clinton saying that the program is not working right, and recommending that he appoint an expert, high-level committee to look at the program and see what should be done.

The way I heard it, there was no response to the letter to the Clinton Administration. Then early in 1994 the congressmen began suggesting they would set up a workshop and try to come up with something. When word of that

86. *Report of the Advisory Committee of the National Earthquake Hazards Reduction Program.* FEMA, Office of Earthquakes and Natural Hazards, January, 1993.

went to the President's science advisor in the Office of Science and Technology Policy, that office undertook to do something.

As near as I can make out, they did not appoint a high-level committee, but had a workshop with about 40 people invited. In reading the report⁸⁷ that came out of the workshop, you get the impression that the problem will be solved by seismologists and social scientists, while engineering seems sort of incidental. They say that a seismic code ought to be developed.

Scott: That does not sound very encouraging.

Housner: Then following the Northridge earthquake, James Witt, the new Director of FEMA, changed that agency's approach to earthquake disasters. He saw that just cleaning up after disasters did not constitute progress, and he also made disaster-reduction a goal. Thus, to avoid a future disaster, FEMA has been funding research on the cracked welded joints caused by the Northridge earthquake. Witt has been a positive influence on FEMA.

Scott: Yes, he has. But, there is a lot of internal resistance in the agencies to any significant change in the organization and management of the program. They cherish their independence, and any real change in the NEHRP program would mean a significant modification of the relative autonomy with which they administer their own shares of the program. As it is, the NEHRP program actually does not have an effective head.

Housner: Some have been saying that the program should be run by an earthquake commission. That way the money would go to the commission, which would then deal it out to the agencies, which of course do not like that idea.

Scott: It is a tough inter-organizational problem, but as you suggest, there ought to be a way to work with the agencies by means of a commission or structure capable of giving overall direction and leadership. That would be politically difficult to put in place, but it could work, at least work for a while.

Housner: Yes. But eventually, I think such a body would go the way of the Nuclear Regulatory Commission. That is, pretty soon they are hiring their own people to do things, and then you've got just another player in the game.

Scott: You are watching this NEHRP business, but are not directly involved?

Housner: Yes. I am watching it from a distance. The latest development occurred when President Clinton's Science Advisor, John Gibbons, announced the formation of the National Earthquake Loss Reduction Program (NEP). The stated intent of this move is to better focus research funds on effective means of reducing future earthquake casualties and losses. Robert Volland will serve as the NEP program office director. I hope NEP will try to encourage the earthquake-related activities of NSF, USGS, and NIST.

Seismic Zones for Earthquake Engineering

Scott: At its July 1993 meeting, the Seismic Safety Commission had virtually an all-day ses-

87. *Strategy for National Earthquake Loss Reduction*. Report Prepared by the National Earthquake Strategy Working Group for the National Science and Technology Council and the White House Office of Science and Technology Policy, 1995.

sion on concerns with the new map that FEMA and the Building Seismic Safety Council (BSSC) proposed to include in the NEHRP seismic code. California engineers were concerned that the map proposed, which was a new version of earlier ones based on Ted Algermissen's work at USGS, was inappropriate for earthquake engineering purposes, and would result in some drastic and undesirable changes in the seismic codes used for building design. That subject has been a source of continuing debate. Would you care to comment on it?

Zoning Problem: A Misunderstanding

Housner: Seismic zoning—the large-scale zones in the earthquake code—has always been a problem in the earthquake business. The use of Algermissen's map probably resulted from an effort by USGS and FEMA to show that they are accomplishing things. But they should also have involved California engineers who are doing things in earthquake engineering.

There is a misunderstanding between the geologists and seismologists on one hand, and engineers on the other. There is no clear distinction between the inputs the earth scientists can make, and the decisions for which engineers use those inputs. Here is a seismic zone map from the 1991 Uniform Building Code. This has to be prepared by engineers on the basis of information from the seismologists, as well as practical considerations. In their mapping, the engineers can make only broad rounded curves, and do not know how to make details like this (pointing to a pronounced, rather sharp jog or neck in the line between Zone 3 and Zone 4 in southern California). Probably some seismologist or geologist was involved. That is bad—the

1992 Landers earthquake occurred right there (in the Zone 3 portion of the jog) and that demonstrated that it should be in Zone 4. An engineer would have drawn the line straight across, without the jog.

Here is another example in the same zone map, the portion up in Idaho and Montana. This area is in Zone 2B, yet at one point it comes very close to an area that is in Zone 4. That looks very odd, and I can tell you how it got in the map. Originally, the line made a broad loop. Then the Department of Energy sent word to all their labs to be sure that their facilities are built according to the seismic code. The Department of Energy laboratory in Idaho, which is located about here, apparently decided to circumvent this. They hired a consulting geologist who prepared a report and convinced the review board for the building code that the area where the lab was located was less hazardous. So the line was changed and the zone reduced from a 3 or a 4 to a 2B.

Scott: In response to that special report by the consulting geologist, they put a major jog in the line to extend the 2B area further north-east, up close to the small area in Montana zoned 4?

Housner: Yes. But soon after they did that the [1983 Borah Peak] Idaho earthquake occurred just north of the lab, and they had to change the line again.

Scott: Then they were really gerrymandering the zones.

Housner: Yes. It was done on the wrong principle. They got a report by geologists who said, "We consider that there won't be any earthquakes here. It is up to somebody else to

prove that we are wrong." That is a poor way of going at it. Anytime you see that kind of fine detail in a seismic zoning map, you can be sure a non-engineer got into the picture.

I once compared the Canadian building code map with that of the U.S. It is considered improper for one country to zone territory in an adjacent country. So our zones go up to the border and stop, and theirs also stop at the border. When I put the two maps together to compare the border region, I found a very bad mismatch. I showed that at a couple of meetings. Now, however, the zones have been made congruent, although it is not known whether this has improved the situation.

Scott: So they have become aware of the inconsistency and have changed the lines. This does illustrate how factors other than high technical expertise or detailed scientific knowledge are involved in zoning map judgments.

Housner: Well, they simply do not have the kind of knowledge needed when the judgments are made. A good zoning map is one in which a big earthquake does not occur in Zone 2 or 3, but does occur in Zone 4. If a big earthquake never occurs in Zone 4, the zoning map is no good.

Failure to Understand Key Words

Housner: The non-engineers do not understand clearly what the word "design" means to an engineer, so they keep misusing the word, and that in turn leads the engineers to misunderstand what the non-engineers are trying to say. I guess it works both ways, with the engineers misunderstanding the other disciplines' use of words. For example in the early days

there was a very good case of engineers—myself included—misunderstanding the word "epicenter." The engineers said, "Oh, the epicenter is the earthquake's center on the surface." Of course, it is not at all the center on the surface. Instead it is the point on the surface of the earth directly above the place where the earthquake slip began. But the point where it starts may be at one end of the slip, and the other end may be 50 or 100 miles away. Because of that misunderstanding, for a time things got very confusing for the engineers.

There was an earthquake in Mexico, and some of our engineering friends went down. They said, "It is very odd, we looked where USGS told us the epicenter was, and there was hardly any damage. Over some 15 miles away from the epicenter, however, there was very heavy damage." They were learning that the location of an earthquake's epicenter does not necessarily indicate where the strongest earthquake shaking occurs at the surface. They also learned that the location determined for the epicenter can be subject to considerable error. We have seen how the seismologically-announced epicenter location can include greater or lesser errors. I recall Perry Byerly telling me that the only correct definition is this: "An epicenter is a mark made on a map by a man who calls himself a seismologist." That definition cannot be faulted.

The word "epicenter" got into the picture when a British engineer, Robert Mallet, went down to Italy around 1850 to look at a small earthquake. He did not know anything about the faults, but looked at the damage, said that was where the earthquake's center was, and referred to it as the "epicenter" of the earthquake. Then some geologists and seismologists

used the word, but with a different meaning, that is, to indicate where the slip started, because that is the point determined by seismograph records. This is now the accepted meaning of the word.

Recent Activities and Observations

“Typically, the design of a big project like that goes ahead, and then later they ask, well, what about earthquakes?”

Port of Los Angeles Project

Housner: The Port of Los Angeles has been expanding and upgrading. The port facilities down in that area are split between the City of Los Angeles (San Pedro) and the City of Long Beach. Los Angeles has started on Project 2020, a vast, multi-billion dollar expansion and rebuilding of its port facilities.

In contrast to how it is usually done, they considered the earthquake problem ahead of time. Typically, the design of a big project like that goes ahead, and then later they ask, “Well, what about earthquakes?” But at the outset, the Port of Los Angeles set up a committee to advise them on the earthquake problem—this was back in 1988 to 1990. I believe the committee was formed at the suggestion of Ed Idriss and Geof Martin, who had been geotechnical consultants. Now they are at UC-Davis and USC, respectively.

I agreed to chair the committee. Other committee members included geologists and engineers whom we know. The committee's assignment was mainly to help the port prepare seismic design criteria to deal with the problem posed by the very

soft ground on which the port facilities are built. Port engineer Richard C. Wittkop was the staff person in charge of Project 2020.

We organized a workshop, had a lot of people come in, and had the proceedings of the workshop published.⁸⁸ That took time, but I thought it was important to participate and help set a precedent for looking at the earthquake problem ahead of time, and for taking seismic concerns into account in making the plans and laying out the project.

Scott: Was that something of a first for such port studies?

Housner: Probably, except maybe for Japan. Although I would guess that their ports were started earlier, before they would have given earthquake concerns any thought. The 1995 Kobe earthquake demonstrated that the port facilities there were not sufficiently earthquake resistant. On the other hand, the Japanese have been more aware of the earthquake hazard to ports. The Japanese government has a ports and harbor agency whose earthquake section has put out strong motion instruments, and is knowledgeable about earthquakes and seismic design.

Translation of Tangshan Earthquake Report

Housner: In 1993 and 1995 I again made visits to Harbin, and could see that the economic and social situation in China had improved tremendously during the years since our 1978 visit. I went back to Harbin because we had a cooperative program to translate the big Tang-

shan earthquake report that the Institute of Engineering Mechanics at Harbin prepared after the 1976 earthquake, and which was published in Chinese in 1986.

The joint translation project involves Caltech and IEM, and our efforts have been funded by a grant from the National Science Foundation. The original agreement was between myself and Dr. Hui-Xian Liu former director of the institute, who founded it back in the early 1950s. His successor as institute director is Dr. Li-Li Xie. The institute's name is somewhat misleading, as the 400-member organization is now mostly devoted to research on earthquake engineering, and is in the process of building up its experimental facilities.

That disaster was the classic example of a great earthquake striking very near a major city that was completely unprepared for such shaking. Most of the city of Tangshan ended up in total or partial collapse, and the earthquake killed somewhere between 250,000 and 500,000 of the city's approximately one million inhabitants. There are also cities in the United States and in other countries that are similarly unprepared, and that could potentially be hit by a big earthquake.

I felt that it was important to get the report out in English for a wider readership, and have been very busy recently working on translating the IEM report from Chinese to English. The example might spur cities whose circumstances resemble Tangshan's into taking their problem more seriously. Tangshan was mainly of brick buildings, like Memphis and St. Louis, which in that way are counterparts of Tangshan, and until recently were built with no earthquake design requirements.

88. *Proceedings of the POLA Seismic Workshop, 21-23 March, 1990*. Port of Los Angeles, 1996.

In the U.S. Midwest, just as in Tangshan, there is the probability of a great earthquake, which people tend to write off, to ignore. Tangshan is also a good example of the problem that can be caused when the seismologists draw up the zoning map. They drew up the zoning for the China building code, and put Tangshan in a zone that did not require any earthquake design.

Scott: Having detailed information on what happened in Tangshan more readily available and more widely distributed might encourage our counterpart cities, and those in other parts of the world, to come to grips with their earthquake hazard.

Housner: The report on the Tangshan earthquake was prepared mainly by IEM, with geological and seismological assistance from other agencies. It required about ten years to prepare and publish the profusely illustrated four-volume report, and I was probably the only person or one of a very few outside China to receive a copy. Shortly after we agreed to undertake the project, the Tiananmen Square incident occurred, whereupon all official connections with China were canceled.

After a few years, however, it again became possible to have joint projects, and the translation was officially undertaken. We agreed on the following procedure. 1.) The original authors of the chapters made the first attempt at translating into English. 2.) Then an English-language expert at IEM went over the translations with the authors, and tried to put the material into acceptable English. 3.) The draft was sent to me and I circulated it to a committee of four Chinese-Americans who were experts in the technical subjects as well as fluent in Chinese

(Frank K. Chang, Nien-Yin Chang, Zhikun Hou, Moh-Jian Huang). 4.) The marked-up draft then came to Caltech, where we worked to put it into good English, and prepared it for publication by Caltech. The word-processor and computer have been absolutely essential to this project.

A preliminary printing of a limited number of copies of volumes 1 and 4 was prepared and taken to Tangshan for the earthquake's twentieth anniversary ceremony, July 28, 1996. We had expected the final printing of the full four volumes to be completed by the end of 1997, but there has been a setback. Unfortunately, in May 1996 a fire broke out in IEM and all their materials and equipment for the translation project burned up, so this will delay its completion.

The entire process has been extremely time-consuming for me. I conclude that nature simply does not want Chinese translated into English. Despite all the difficulties, still I think it was a worthwhile project. The earthquake was an even greater disaster than I had previously been aware.

Harbin Proposal: An International Seismic Code Workshop

Housner: I suggested to IEM in Harbin that they organize an international workshop on seismic codes, and they would like to get the U.S. and Japan to cooperate. It has been agreed to hold the workshop at a suitable location in China. The workshop would discuss the deficiencies in seismic codes, and how to improve the codes. I think enough problems on code development have arisen over here to make such a workshop useful to us as well as to the

Chinese and others. There are a lot of problems with our Uniform Building Code, and with the other codes used in the U.S., too. There are a lot of engineering problems on which our code does not do as good a job as it might.

Scott: Would you discuss those problems a bit?

Housner: The basic problem is that the code is a legal document, so it is not enough just to say, "You must do a good job." It has to be specific in saying what a good job is. Having to specify this in such a legal document, when the code is a simplification, leaves things open to judgment. The engineer may, or may not, do those things right. The observation is not new to me—the engineers are quite aware of this.

Of course the code does not prevent a good engineer from doing the right thing. You can always do the right thing. But it is very difficult to write a code that will always prevent the designer from doing the wrong thing. I do think, however, that there are a lot of specific points on which the code could be tightened up. The Northridge and Kobe earthquakes emphasized this, which helps demonstrate why such a workshop would be beneficial.

Since the Northridge earthquake I just have not had time to think about the workshop, but I did conclude that the U.S. side of the workshop should not be organized by a university, but by something larger. I proposed that CUREe do that for the American side and CUREe agreed. The Chinese were keen on proceeding with the workshop, and it was held in China in December 1996, although I was not involved.

The idea of the workshop was to bring the knowledgeable people together to talk over the problems—discussing the defects in the codes and where they need strengthening. The Harbin people had the idea that on the basis of this they would prepare a kind of model code. In the past, each Chinese agency has had its own code, but it might be better to have all of them at least guided by one model code. Of course, we have the same problem here, where our government agencies each have had their own codes, although they are now trying to get together. In China, everything is done by government agencies, making it that much more difficult. Also the academics there are not as prominent in the picture as they are here in the U.S.

High-Speed Rail Line in Taiwan

Housner: I get involved in other things, too, such as in Taiwan, where they are planning a high-speed rail line, like the one in Japan. It is to run from Taipei in the northwest corner of the island south to a city in the southwest—Kaohsiung. The western part of the island is the only flat part, and the rest is all mountainous. They decided to elevate the track. The earthquake research center at the university was assigned the task of preparing the earthquake design criteria. In my opinion that was putting a big burden on those people at the center. Around 1990 they asked Joseph Penzien and me, and Professor Keizaburo Kubo to be an advisory committee. That required us to go over there several times to meet with them and advise them, over a period of a couple of years.

Scott: What kinds of problems did you deal with?

Housner: One question was what to prescribe for the design engineers. We wanted to be sure they had the correct zoning map—one prepared for engineers, not one done by seismologists. Other questions included how to specify appropriate design spectra, and how to specify allowable strain. In designing almost anything—except nuclear power plants—you recognize that the worst shaking has a very small possibility of occurring during the life of a facility. So you design to permit some overstressing. You need to decide how much damage you would be willing to accept in the event of the worst shaking.

Scott: A structure can take a good deal of overstressing without being destroyed, or even without necessarily being damaged beyond repair.

Housner: Yes. If all you are interested in is life safety, you can accept a lot of overstressing, while still making sure the building won't collapse. But of course in a transportation system like BART with its elevated structures, while life safety is of course a chief concern, you also want the system to survive the earthquake in a condition to run again. You want to be able to repair it quickly. Those considerations all have to enter into the decisionmaking process.

Scott: Your committee has advised them in dealing with these issues. Is that part of the work completed?

Housner: Yes, that part of our job ended with the earthquake research center putting out their report to the agency.⁸⁹ When the rail project actually starts, however, my guess is that we will be involved again, because other

questions on the part of the design people will come up.

Scott: When is that likely to be?

Housner: When we met with them, the feeling was that they would go ahead quickly, but since then I read that financial concerns may delay the rail project. So we don't know.

International Decade for Natural Disaster Reduction

Housner: At the Eighth World Conference on Earthquake Engineering, held in San Francisco in 1984, EERI President Paul Jennings invited Frank Press to be keynote speaker. Frank had been a professor at Caltech, and then moved to MIT. He was a seismologist, or as he would probably put it, a geophysicist, and in 1984 he was President of the National Academy of Sciences. His keynote address proposed establishment of an International Decade of Natural Hazard Reduction. When his speech circulated around the world, it aroused great interest among geologists, seismologists, earthquake engineers, wind engineers, etc. Many wrote to Frank urging that steps be taken to establish the Decade.

Seeing so much interest expressed, and from many countries, Frank felt we ought to do something. So Frank appointed an 18-member advisory committee and asked me to chair it. In addition to the 18 committee members, there were also 18 U.S. liaison representatives, and

89. *Seismic Design Criteria For High Speed Rail Project*, Report to Provisional Engineering Office of High Speed Rail. National Center For Research on Earthquake Engineering, Ministry of Transportation and Communications, Taiwan, R.O.C., November 20, 1992.

four international liaison representatives. The committee prepared a report that was published in 1987 and distributed widely. The report focused on sudden-impact disasters, including earthquakes, floods, hurricanes, tornadoes, tsunamis, landslides, and wildfires.⁹⁰

I accompanied Frank to the United Nations in New York for a presentation on why the U.N. should be the lead agency for the Decade. A proposal to put that into effect was presented to the U.N. General Assembly and was approved, providing that the name be changed to the International Decade for Natural Disaster Reduction—that is, changing "hazard" to "disaster." The IDNDR was supposed to begin in 1990 and be completed in the year 2000.

Anyway, the General Assembly resolution endorsing the decade authorized the U.N. administration to get in the picture, and they appointed a so-called committee of experts representing many different countries, which Frank agreed to chair. He worked with the committee to recommend the direction it ought to take.

Most countries already had people who were interested in each of the hazards, such as doing something about earthquakes, or winds, or floods, but they found it difficult to get their governments to listen. We recommended that each country form a national committee, so the people interested could come in from below, and then the U.N. could present the ideas to

the national governments from above, so to speak.

We thought the national committees would really be responsible for getting work done. But it was hard to get responses from their own governments. And a project that involves the U.N. and many national governments has proved really too unwieldy, so it has been difficult to make progress in science and engineering on disaster reduction. In any event, the Decade was slow getting started, although now has certainly exerted a beneficial influence.

Scott: In light of the slow start, do you have any suggestions as to better ways to proceed?

Housner: In retrospect I think a program involving so many different countries and kinds of disasters is just too cumbersome. I believe it could have been better managed if the effort has been focused on a single major type of disaster—perhaps earthquakes, or hurricanes. Even just in Washington alone there are too many agencies involved in the IDNDR agenda. The involvement of so many agencies makes it difficult to get concerted action.

There are also too many conflicting aims among the interested parties. For example the objectives of people mainly concerned with disaster *mitigation and relief* are different from the objectives of those who focus on *disaster prevention*. I have learned a lesson from the IDNDR experience. You need to focus attention and concentrate energies on a single clearly defined objective, and to limit participation to two or three key countries. Then, if everything works out, bring other countries into the picture. If the effort were successful with earthquakes, then it could serve as a model

90. *Confronting Natural Disasters: An International Decade for Natural Hazard Reduction*, National Research Council. Advisory Committee on the International Decade for Natural Hazard Reduction, National Academy Press 1987.

for the other disasters. Anyway it remains to be seen how the IDNDR will turn out.

Conference on Natural Disaster Reduction, 1996

Housner: I became involved in the International Conference on Natural Disaster Reduction, held in Washington, D.C., December 1996, and run by the American Society of Civil Engineers. The ASCE office has responsibility for the detailed work of organization and publication, although the international conference is under the auspices of the United Nations International Decade for Natural Disaster Reduction. They tried to get everything organized well ahead of time, and I chaired the organizing committee, which started having regular meetings in June, 1994. As vice-chairman, Riley Chung has done most of the work.

Scott: So something is really going to develop out of the International Decade? Previously it had begun to look as if the Decade might not get much done.

Housner: Yes, previously the IDNDR meetings had been all talk and no action, and engineering did not play any role. There was a big IDNDR conference in Yokohama, but it was all talk about how important the subject is, and repeating observations on the kinds of things that ought to be done. There was no meat in it. So the engineers took things into their own hands with this conference, in hope of getting something valuable under way. The idea was to emphasize engineering as the central subject, but will also include the other concerns, such as mitigation, disaster response, and so on. Originally Frank Press was scheduled as keynote

speaker, which was appropriate because he was the originator of the IDNDR, but when he was unable to do this, Paul Jennings agreed to give the keynote address.

We have been pinning our hopes on this conference. We allowed plenty of lead time, and tried to get everything organized ahead of time so we would know what we are doing. I did not spend a lot of time on this, and only agreed to do it working jointly with my friend Riley Chung, with whom I had worked before when he was at the National Research Council. He agreed to do all the work required.

The conference brought together people from many different disciplines with an interest in disaster reduction. There was an enlightening exchange of information, although it will take a year or so to see the effect of the conference. I believe we will develop closer cooperation with Japan.

World Seismic Safety Initiative

Housner: In addition to planning for the ASCE-sponsored 1996 conference, we have also organized the World Seismic Safety Initiative. That effort grew out of our discouragement with the International Decade's lack of positive action on seismic safety. The first thing was to try to get the International Association for Earthquake Engineering involved. It is a federation of national societies. Each country that has a national society—or at least national committee—is a member of IAEE. IAEE has a board of directors with some 15 countries represented, and there are national delegates from 38 countries. But since IAEE itself is not set up to do projects, I talked with IAEE Secretary General Tsuneo Katayama about IAEE setting

up the World Seismic Safety Initiative. EERI helped with this and put up some money. EERI set up a committee to prepare a report on how this should be done. I recall that Haresh Shah chaired, and Chuck Thiel, Bill Iwan and I were on the committee.

The World Seismic Safety Initiative (WSSI) is a non-profit organization whose only function is to do things—arrange things. So WSSI can raise funds and initiate projects. It is an independent organization under the umbrella of IAEE. Haresh Shah of Stanford is chairman. Now the idea is to set up three offices or branches, one in the United States, one in Europe, and one in Asia. Each would try to do things in their areas. It remains to be seen how successful this will be.

International Association for Structural Control

Housner: Another of my current involvements relates to structural control of seismic response. Several years ago the NSF asked me to help set up a panel on structural control, which is an interesting topic. "Active" control does something—it exerts force—whereas a method like base isolation of a building is an example of "passive" control. There are many different ways of controlling response, and it is our purpose to investigate which is best.

The idea was to put on the workshop and write a report with suggestions for future research. That is, we would lay out what a program in structural control ought to be. I considered the topic important and was interested, but was reluctant to get into it. It so happened, however, that Professor Sami Masri was also inter-

ested. He is a former Caltech student who got his Ph.D. degree here 28 years ago and is now at USC. He was interested in active structural control, and while spending time here at Caltech on a visiting basis said, "If you will agree to set up the panel, I'll do all your work." What that means is *most* of the work. So we organized a workshop in 1990 and published a proceedings.⁹¹

NSF then established an initiative on structural control research. They got \$1 million per year for five years, for unsolicited individual research proposals in structural control. That kind of unsolicited proposal funding suffers from the fact that it is split up so much. If \$1 million went to one research project, it might get something done with a larger program, but that is not possible when it is given out in individual grants of maybe \$50,000 per year. Overhead must be taken out, and with the rest a professor supports his student to work on a project for a year, and that's the end of that small project—it is over. So that is not a good way to make progress on a larger program.

Anyway the 1990 workshop showed that there was a lot of interest in the United States and also in Japan—a number of Japanese wrote and asked if they could come. We agreed that they could, and in due course their number added up to about twenty. Dr. Takuji Kobori attended and said he would organize a counterpart panel and a workshop in Japan. Then in 1992 several sessions were devoted to structural control at

91. *Proceedings of the U.S. National Workshop on Structural Control Research: 25-26 October 1990, University of Southern California, Los Angeles, California*, U.S. National Workshop on Structural Control Research. University of Southern California, 1990.

the Tenth World Conference on Earthquake Engineering held in Madrid, Spain. Those sessions also involved people from Europe and other places. They were looking to us in the U.S. for leadership, and it was clear that an international association was needed, and so we got started on setting up the International Association for Structural Control.

A committee was appointed to take the initial steps: myself, Professor Masri, Professor Takuji Kobori of Japan, Professor Fabio Casciati from Italy, and Dr. Li-Li Xie, director of the institute of engineering mechanics in Harbin, China. But the main burden fell on the U.S. to get it going. It took time—we had to prepare model statutes, bylaws for the association, etc.

Scott: So this effort has extended way beyond Professor Masri's initial stay at Caltech?

Housner: Yes, and he is still doing the work. Fortunately, he lives only two blocks from Caltech. In 1993 we held a second workshop in Honolulu, with funding from NSF and Japan, and a proceedings was published. The whole thing has been a lot more work than we had expected. The International Association for Structural Control, a nonprofit organization, became official shortly before it took its first action, holding the August 1994 structural control conference in Pasadena. I served as the first president of IASC. Professor Kobori is the second president, 1996-2000, and Professor Masri is secretary-general, and Professor Akira Nishitani is executive secretary.

The 1994 conference had funding from both NSF and Japan. Some 235 papers were presented, the greatest number coming from the U.S., with Japan second, and also papers from

Canada, European countries, and others. It was a very successful conference, with participants from 15 countries. The previous proceedings were put out by the U.S. panel, and the 1994 proceedings were put out by the International Association.⁹² Already the Japanese are planning to hold the 1998 Second World Conference on Structural Control in Kyoto, Japan. In between the two world conferences, a 1996 workshop was scheduled and held at the new University of Science and Technology in Hong Kong in December, 1996.

International Infrastructure Research

Housner: Another thing I have gotten involved in with Sami Masri is trying to organize the International Initiative for Intelligent Infrastructure Research (IIIR, or I-4-R)—at least that is the name we have adopted for the time being. We did not want to make the name too specific, such as "earthquakes and steel buildings," because if the effort is successful, there are other related problems to consider, and this title fits with the infrastructure program at NSF.

It started when we realized that there was no established program for international cooperation in earthquake engineering research. We saw better cooperation between researchers as a very promising approach to getting more out of the experimental research that is done, as well as to securing broader support. But in the past, things have usually been done on a strictly

92. *Proceedings of the First World Conference on Structural Control, August 3-5, 1994.* International Association for Structural Control. Edited by G.W. Housner et al., Pasadena, 3 vol., 1995.

ad hoc basis—two people would simply decide to work together—that was the way joint research was done.

We thought there should be a standing oversight committee to identify problems, and in each country also identify people who might be involved, and get something going. The oversight committee would be an advisory group and would try to stimulate others. It would initiate, facilitate and coordinate. Our thinking has been to begin with Japan, and our colleagues there are interested. Eventually we would like to have representatives from the various countries that have earthquake or wind problems on which they do experimental work.

For example, Japan has a big shaking table, and somewhere else they have other facilities, and so on. The idea was to get together so that if one of these facilities had a project, others might be able to piggy-back on the work.

Thus, at Hong Kong, where they have frequent high winds, the university is in the process of setting up an experimental facility—a two-story or three-story building—big enough to be a building and not just a model. When heavy winds blow they will measure the forces on the test building. At the same time other people may, for example, be interested in measuring the distribution of wind pressures. The thought was to organize so as to optimize what can be gotten out of one of these projects.

The Japanese have been doing research on structural joints in the past and are much interested in such activities. The steel-joint damage observed in the Northridge and Kobe earthquakes has also highlighted the matter. So we thought we could coordinate with them on that kind of work—they would experiment with

some joints, and the U.S. would experiment with others. We already have support from interested Japanese people, such as Professor Kenzo Toki, Professor Hiro Iemura, Professor Heki Shibata and Professor Makoto Watabi.

Scott: Talk a bit more about how I-4-R got started.

Housner: In working on this idea, Sami Masri and I talked first with the Japanese. So far it is just talk, but if it goes ahead I think it could have a very beneficial influence. The cracked steel joints are a good example of the sort of problem that can be dealt with this way. The cracked joints seen in Northridge are such a big problem that support has been forthcoming on that score, and the observation of cracked joints in the Kobe earthquake makes cooperation even more interesting.

Scott: This seems potentially a very significant development.

Housner: Yes, but so far it is just talking. Whether or not we can get both sides involved is not known yet. The important thing is to set up a mechanism for continuing cooperation. With Professor Toki we are first to set up cooperation on university research in earthquake engineering. If that can be done, other research can be brought in. He thinks Japan can set up its cooperative program, so it is up to us to see if the U.S. can set up its own program.

U.S.-China Cooperative Program

Housner: Something else that had gotten me concerned about international cooperation in research was our experience with the official U.S. cooperative research program with China on earthquake problems. That whole thing

goes back a long time. When President Nixon first went to China about 25 years ago, he and Chairman Mao agreed that there should be scientific cooperation between the two countries. About the only noncontroversial topic that could be found, however, was earthquakes. So it was decided that earthquakes would be appropriate for the cooperative program.

Arrangements were made for cooperation in seismology, and later a protocol was drawn up that included earthquake engineering research. On our side, NSF was to fund cooperative research projects with China. This went along for a number of years in a rather disorganized way, because they just waited for unsolicited research proposals to come in. Of course the people over here did not know what was going on in China, and vice versa.

There were other organizational problems. USGS was in it, working directly with the corresponding organization in China, the State Seismological Bureau. But NSF-Earthquake Engineering ended up working with the Ministry of Construction. Well, NSF represents the academic research community here, but the Ministry of Construction does not represent the research community over there. So it has been a mismatch.

Then NSF asked me to organize an NRC committee and try to get the program straightened out and moving. That seemed reasonable, and we formed a committee. Before we could get going, however, along came the Tiananmen Square incident, after which the word was "Out," so we were out, and nothing was done. After some time elapsed, interest revived once more: "There ought to be a meeting between

the appropriate people in China and those in the U.S."

In 1992 we met in Canton, China on the matter, trying to lay out a cooperative program of research so it will not just be a random group of projects. It was a good meeting and we drew up a report on what ought to be done.⁹³ The report explains what we thought ought to be done, and the kinds of projects that ought to be undertaken. It also contains a good deal of other useful background information on the cooperative efforts. Bill Iwan took a leadership role in this.

At our meeting the representative from the earthquake engineering research laboratory in Harbin, which is attached to the State Seismological Bureau, said, "Well, the Ministry of Construction sends research ideas along to me, saying they are good projects and we ought to do them. But they do not give us any money." The recommendation should come via the State Seismological Bureau, along with some budgetary provision. So we have had a cooperative program, and there have been some useful projects, but there was almost a complete mismatch in the way the program was organized for earthquake engineering in the two countries.

It is awkward for our U.S. people to pose formally the issue of the way things are organized on the Chinese side. NSF is not likely to tell the Chinese, "You have us talking to the wrong people." But the mismatch was nevertheless a real problem. We thought that some kind of

93. *Workshop on Prospects for U.S.-P.R.C Cooperation on Earthquake Engineering Research*, National Research Council, U.S. Panel on the Evaluation of the U.S.-P.R.C. Earthquake Engineering Program, 1993.

independent oversight committee could raise issues. It could say, "The Ministry of Construction ought to be talking with NIST (National Institute of Standards and Technology), NSF ought to be talking with the earthquake engineering research lab and the universities." My thinking is that in each country there should be a small panel of people representing the oversight committee. In China, that panel could,

for example, suggest to the Ministry of Construction that it ought to work with NIST in the U.S.

Scott: A recommendation coming that way would be more acceptable and more persuasive.

Housner: Yes, they would not listen if the suggestion came from an individual.

Discussion of Selected Publications

“... while it is difficult to come up with a new idea, and its acceptance is resisted, once the new idea has been enunciated and explained, it then becomes quite obvious to just about everybody.”

Scott: After reading a draft of your oral history, Clarence Allen recommended that we work on a chapter that would deal with a selection of your publications, chosen to include those you consider particularly significant, and also to illustrate the range of your publishing activities. I see that your curriculum vitae lists a total of over 190 publications, which is a pretty large number.

Housner: Yes, but of course you have to realize that the 190 were spread over a great many years, my first publication being in 1941, and my last in 1995—a total of 54 years.

Scott: That still seems like a lot. It averages well over three publications a year—about 3.5 in fact.

Housner: That is true, but averages do not tell the whole story, because as you know, different kinds of papers require very different amounts of time for their preparation. I should also note that many of my papers have joint authorships of the kind that develop when two colleagues get interested in a

topic. Sometimes the co-author has been a graduate student who has been working with me on a particular research project. I was pleased that all of the twelve former students who co-authored papers with me have had very successful careers.

As I look back over my publications I see that when I was a young man my papers dealt with specific technical problems. Then as I grew older my interests broadened. Now I do not publish any papers on specific technical problems, because I cannot compete with my young colleagues. I am reminded of what the composer Franz Joseph Haydn is reported to have said when in his 70s: "I still have good musical ideas but I do not have the stamina to work them out." I now understand just what he meant.

Scott: When you say you cannot compete with younger colleagues, is it mainly a matter of stamina? Certainly it takes a lot of time and effort to keep up with the ever-growing literature, find funding, and keep doing innovative research. But is there more to it than that? Are there other life-cycle factors such as a shift in your basic interests and motivations?

Housner: In my youth, I would get interested in a technical problem and work on it intently. I would work until 2:00 in the morning, as I was a "night" person then, never going to bed until after midnight. Now my lifestyle has switched—I go to bed early and get up early. But I no longer feel like working out the details and the computing on specific technical problems. While I am still interested in reading the papers published by my younger colleagues, for my own activities I am more interested in promoting the field of earthquake

engineering, both research and implementation. I feel that these activities will save lives in the future.

Four Kinds of Writings

Scott: Say a little more about the different kinds of papers you have written.

Housner: At the outset I should emphasize that in my research and publications, I always tried to do something that would have a bearing on a practical earthquake engineering problem or some other kind of engineering problem. That said, I identify four main categories of papers, which I will discuss in turn. They are: 1.) research papers, 2.) educational writings, 3.) policy papers, and 4.) papers done for workshops and conferences.

The first category includes the most important papers, particularly for an academic or a researcher. These take a lot of time and thought because they present new results—new ideas and new concepts, or new information resulting from research. Or such a paper might present new results based on new analyses of old data.

Scott: Those papers would nearly always be published as part of the regular literature of the discipline, usually in a technical journal or report?

Housner: Yes. The second type of paper is written primarily for educational purposes. If done right, these educational writings also require a considerable amount of time and effort. One example is a chapter in a handbook. Others are papers intended to educate certain people or groups of people, such as bringing

new research results to the attention of practicing engineers.

The third type of paper or report is also educational, but is more explicitly devoted to policy, setting forth policies that the writer believes should be followed in earthquake engineering research and implementation, or disaster mitigation. Such papers can bring features of the earthquake problem to the attention of legislators, other public officials, and the public generally.

Scott: Educational and policy papers are both quite important and do take a good deal of thought. The policy paper in particular requires a researcher or practitioner to think about a familiar subject matter in a different way, answering questions such as: "What ought to be done?" and "How might that be accomplished?" With luck, some policy papers do help influence the future course of events. When that happens, it can be pretty gratifying for the author or authors.

Housner: Yes, that has happened a few times with publications I was involved with—mainly publications issued by the National Research Council.

A fourth type of publication is the "filler" papers we do when requested to write something on a particular topic for the proceedings or a workshop or conference. While these have educational value because they disseminate knowledge, they can be written without undue effort, and I must admit that a number of my publications are of this type.

Scott: The conference or workshop paper is normally built on what the contributor already knows well and probably has already written up in the literature. The material is then recast in

somewhat different format for a conference-type audience.

The Difficulty of Presenting New Ideas

Scott: Having outlined your four main kinds of writings, would you go back and comment a little more on the first type of type of paper you mentioned—one that presents new findings, concepts and ideas?

Housner: I might say a word or two about the process of developing and disseminating new ideas. I have had a few new ideas in my career, and can say that the entire process is not easy. Apparently we have been brain-washed by our educational system in a way that makes it difficult to think outside of the system. Thus, it is very difficult to think of a completely new idea.

Moreover it is said that every new idea tends to go through three phases. At first, people tend to say, "It cannot be correct," or "It is not relevant." Next, they may say, "Others have already thought of that." Then when the new idea becomes generally accepted, they say, "Well, that is obvious." So in my experience, while it is difficult to come up with a new idea, and its acceptance is resisted, once the new idea has been enunciated and explained, it then becomes quite obvious to just about everybody.

Scott: Although this may be less so in earthquake engineering, in some fields a new idea or theory may be quite controversial at first. For a time it will undergo a somewhat skeptical scrutiny and testing. Then if it proves out, it will begin to be accepted generally. Of course, if it does not survive the initial testing, well, it is "back to the drawing board" for the researcher.

But you are certainly right about the reluctance to accept new ideas at first, and then their coming to be seen as more or less obvious as people get used to them.

Housner: When you publish a paper, you sometimes wonder whether anyone reads it. But then you receive a letter asking for more information or explanation—a letter that might be from the U.S., or from South America or India. So then you know that someone did read your piece and think about it.

Scott: The English-language earthquake engineering literature circulates worldwide, so it is understandable that you might get responses from faraway places.

Housner: When I was in China in 1978, I was surprised that so many engineers that I met knew about my publications, particularly since communication between the U.S. and China was not good at that time. Then I found that the Chinese government translated our journals, reports, books, etc., into Chinese. For example, the 1970 book *Earthquake Engineering* that Robert Wiegel edited was printed in China, in Chinese.⁹⁴ I often thought that we in the U.S. should have a program of translating interesting publications into English from Japanese, Chinese, Russian, etc., but this seems not to have been in the cards.

Scott: Such a translation program would be extremely valuable, but it probably would be hard to convince some of the powers-that-be of the need to provide financial support for such a program, especially considering the rather

insular nature of some of our thinking. In a lot of ways, this country tends to focus most of its attention internally. While that is not necessarily true of researchers and academics, it certainly affects the budget-makers.

Housner: In terms of earthquake concerns worldwide, while many countries confront severe earthquake problems, unfortunately there is not very much cooperation in earthquake engineering research and implementation. There is not enough coordination between research done in say Japan and the United States. Eventually, of course, such information does become disseminated, but time is of the essence in earthquake disaster reduction. The longer we wait, the more [buildings] there will be to suffer damage.

The earthquake problem in the U.S., Japan, and China, is very similar as regards seismicity and exposed cities. Research and implementation is under way in each country, but it is clear that these efforts are not synchronized. I have tried to help improve coordination, but the great differences in the administrative setups in the three countries have prevented us from finding a way to achieve this. I am now in communication with Japanese colleagues to try to establish better cooperation in university earthquake engineering research.

I see the *practice* of earthquake engineering as particularly deficient in international coordination and cooperation. While I am aware that there are various contacts between the three countries, I do not believe these achieve the coordination and cooperation that is desirable.

Scott: You are referring to such contacts as the joint U.S.-Japan seminars that have been

94. Weigel, Robert, *Earthquake Engineering*. Prentice-Hall, NJ, 1970.

going on regularly for some time? They are helpful, but do not fully meet the need?

Housner: Yes. Those joint UJNR [U.S.-Japan Natural Resources] seminars come out of an agreement between NIST and the Ministry of Construction in Japan. The meetings alternate annually between Japan and the United States. To each meeting the Ministry of Construction and NIST invite half-a-dozen researchers each to present papers. Each meeting is thus a micro-conference, and there is a feeling in both countries that the seminars do not accomplish what needs to be done for university researchers.

We are actually not even well coordinated within the United States, and do not really know what is going on in earthquake engineering research at the various universities and laboratories. One possibility that I have proposed is that in the U.S. we set up a standing committee charged with trying to keep track of what people are doing in earthquake engineering research, and that a similar committee be set up in Japan.

Each committee should find out what research is under way in its own country, determine if gaps in knowledge exist, and identify research projects that could profit from cooperation. The two committees could then form a joint committee to transmit information between the two countries, so that researchers in each understand the complete picture. In one sense this would be trying to do on an international scale what the Universities Council on Earthquake Engineering Research accomplished while it was active.

Selected Textbooks and Educational Publications

Housner: I reviewed my publication list and chose roughly 30 items that seemed to me to have been important in my career, and also that I thought would best give an idea of my activities. We can discuss these publications in general, I can pick out a few for special comments, and we could also list the selections here.

Scott: We will include your lists of nine books and twenty-two research papers at the end of this chapter. Could you start by talking about the nine books you singled out?

Three Textbooks

Housner: The first three on the list of books are textbooks, the two on applied mechanics co-authored with Donald Hudson, and the third, on the analysis of stress and deformation co-authored with Thad Vreeland.^{95, 96, 97}

We started writing the applied mechanics books in the late 1940s because at the end of the war it was clear that education in engineering thereafter would be quite different from what it was like pre-war. There was a need for a fresh look at mechanics. An acquaintance of mine, Harvard professor Howard Emmons, once said to me in passing, "Your book on dynamics is not bad," which I took to be high praise. About 20,000 copies of each mechanics book were sold in the United States, and they were reprinted in a number of foreign countries.

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95. Housner, G.W. and D.E. Hudson, *Applied Mechanics-Static.*, Van Nostrand, 1950.
 96. Housner, G.W. and D.E. Hudson, *Applied Mechanics-Dynamics.* Van Nostrand, 1951.
 97. Housner, G.W. and T. Vreeland, *Analysis of Stress and Deformation.* Macmillan, 1966.

The book with Vreeland on stress and strain sold quite a few copies, and I feel that all three of the books lifted the intellectual level of teaching of the subject matter.

It is not easy to write a good engineering textbook, and it is particularly difficult to prepare homework problems that will be intellectually stimulating to the students. Don Hudson and I put a lot of thought into the homework problems, and occasionally we would get a frantic telephone call from an instructor asking how to solve a certain problem. So the books had an educational influence on teachers.

Nuclear Reactor Handbook

Scott: I see you checked something on nuclear reactors and earthquakes—an AEC handbook.⁹⁸ Say a word or two about why you included it as an important item.

Housner: After the war, it became clear that electric power would be generated by nuclear reactors, and that this posed a special problem in the seismic regions of the U.S. Nuclear reactors would require a high degree of safety, beyond the level of safety required for ordinary buildings.

In the early 1950s, the Atomic Energy Commission engaged Holmes and Narver, a Los Angeles engineering firm, to prepare a handbook on nuclear reactors and earthquakes. I was a consultant on earthquake engineering, and Charles Richter was a consultant on seismology. The manuscript was completed in 1956, and the handbook was published by the

AEC. I think that handbook helped to begin the design of nuclear power plants on the basis of dynamics and design spectra.

Scott: So in short, that began the design of nuclear power plants for seismic regions on a rational basis—a very important development.

Housner: I cannot say that all the nuclear power plants in the United States were designed properly for earthquakes. For one thing, in the early days there was no code or standard procedure to guide the designs, and in some parts of the country. There was not a good assessment of seismic hazard.

Alaska Earthquake Report, 1964

Scott: I see you listed the engineering report on the 1964 Alaska earthquake. That earthquake was a very important event in U.S. earthquake studies, including earthquake engineering.⁹⁹

Housner: It was a very important earthquake, and the publication of the engineering volume of the big seven-volume National Academy of Sciences report on the earthquake was very important to me personally. I was chairman of the committee that prepared the engineering volume, and Paul Jennings and I contributed a number of papers. Paul was the assistant chairman, and he and I invested a lot of time and effort on the project. Because not many copies of the report were printed, I do not know how extensive its influence was on the engineering community. But the project did have a big impact on my thinking.

98. Housner, G.W., *Nuclear Reactors and Earthquakes*. AEC Handbook, earthquake engineering portion, 1963.

99. *The Great Alaska Earthquake of 1964*. Engineering volume, National Academy of Sciences, 1972.

At the time I was a consultant to the Pacific Gas and Electric Company for the design of a nuclear power plant they were planning for Bodega Bay, about 50 miles north of San Francisco. PG&E formed a six-member team, including Hugo Benioff and me, to investigate the Alaska earthquake, and a little later the National Research Council undertook a project to prepare a comprehensive report on the earthquake.

For that we visited Alaska to study aspects of the earthquake. This was also my first experience in looking at the effects of a great earthquake (M8.4). The earthquake was remarkable for its geological, seismological, geotechnical, tsunamic, and structural effects. This opened my eyes to the possible disaster that a great earthquake could inflict on a large city. I think all of us who were involved in studying the Alaska earthquake underwent a big change in our thinking about the effects of earthquakes.

Earthquake Engineering Research

Scott: You also listed the 1969 earthquake engineering report, which essentially went well beyond what you had done earlier in the Alaska earthquake report.¹⁰⁰

Housner: Yes, I talked about this publication before, and checked it for inclusion here because I believe that our work on its preparation was in itself a big educational effort for those of us who were involved. I think we all came out of it with a much better understanding of earthquake problems. Also, this publication really identified earthquake engineering as

an engineering discipline in its own right. I always felt that this was an important publication, and when the printing by the Academy of Sciences was exhausted, I arranged to reprint it here at Caltech so that it could be distributed to a wider audience.

Earthquake Engineering Design Criteria

Scott: You said a little about the monograph on earthquake engineering design criteria when you discussed EERI. I believe EERI considered it one of their important publications.

Housner: Yes, I do feel that monograph I co-authored with Paul Jennings was very influential for earthquake engineering.¹⁰¹ It did not treat the technical details of design, but rather discussed how engineers should look at the earthquake problem, and what were its important elements.

The book got a rather wide distribution, and I appreciated Frank McClure making a point of telling me that it was a good book. While at the time I believe the information in the book was new to most engineers, I think it is now a standard part of most engineers' basic knowledge.

Confronting Natural Disasters

Housner: I chaired the committee that prepared the report on natural disasters for the National Research Council.¹⁰² It gave the initial impetus to the International Decade for Natural Disaster Reduction. The report was aimed at a rather wide audience and presented

100. Housner, G.W., ed., *Earthquake Engineering Research*. National Academy of Sciences, 1969.

101. Housner, G.W. and P.C. Jennings, *Earthquake Engineering Design Criteria*. Earthquake Engineering Research Institute, 1982.

102. Housner, G.W. et al., *Confronting Natural Disasters*. National Research Council, 1987.

information on the effects of natural hazards and what needed to be done to reduce future disasters. This report had a world-wide influence on government agencies, as well as scientists and engineers. The committee that prepared the report had eighteen members, all well-known names, and had acquired a number of governmental representatives, so it was a team effort.

I learned that there are many natural hazards, ranging from the Seventeen-Year Locust to snow avalanches, but we had to limit the report to a small number of what we called "rapid onset" hazards. These included earthquakes, hurricanes, tornadoes, floods, landslides, tsunamis, volcanoes, and wildfires. I was surprised to learn that these hazards had caused about 2.8 million deaths worldwide in the twenty-year period from 1965 to 1985. The economic losses and human misery were beyond calculation.

The report got a wide distribution, and I believe it had a greater impact in developing countries than in the United States or Japan. By that I mean a practical effect, for it is clear that many people read the report and thought about it, so that it also did have an intellectual effect, which led to a variety of meetings and conferences on the problems raised.

Loma Prieta Inquiry

Scott: The next publication you selected was a major landmark report on California's earthquake problem. You discussed it in your chapter on the work of the Board of Inquiry on the Loma Prieta earthquake, but say a few more words about it here.

Housner: I chaired the Board of Inquiry appointed by Governor George Deukmejian to report on the 1989 Loma Prieta earthquake. It was a very successful report,¹⁰³ which conveyed the appropriate information to both the engineering community and the state's governmental agencies, as well as to some members of the public. About 5,000 copies were distributed, mostly in California, although some copies also reached foreign countries. So I know that many people read the report and learned from it. I consider this one of the more successful reports that I have been involved with.

Observations on Several Research Publications

Housner: The previous items selected for inclusion here were not research publications per se—they were textbooks and educational reports, and I consider my contribution to getting them out to have been very worthwhile, worth the considerable amount of time it required. Now, however, I would like to turn to a selection of twenty-two of my research publications. I do not think it necessary to discuss each of the papers individually, so I will limit myself to making a few comments on several of them.

Estimation of Linear Trends

Housner: My first publication that did not involve earthquake engineering was a paper on the estimation of linear trends that appeared in 1948 in the *Annals of Mathematical Statistics*.¹⁰⁴

103. *Competing Against Time: Report to Governor George Deukmejian from the Governor's Board of Inquiry on the 1989 Loma Prieta Earthquake*. Governor's Office of Planning and Research, State of California, 1990.

The motivation for doing the paper came from my co-author, Joseph Brennan, who was a staff engineer at the Pacific Gas and Electric Co. in San Francisco. We had been roommates when we were students at the University of Michigan. One of the statistical problems he was involved with was estimating the lifetime of various items, such as power poles, transformers, etc., when only sparse and unreliable data were available.

The special feature of this paper was the use of a dimensionless method, known in statistical circles as a parameter-free method. I think the method has some advantages, and apparently some others thought so too, but I do not know what use has been made of it. Incidentally, we originally submitted the paper to an economics journal, where we thought it would be most useful, but it was turned down. The negative review of the paper listed a number of reasons, which I interpreted as a way of saying, "These authors are not economists and we should not publish their paper." I think it is not unusual for a paper to be turned down because the author is not a member of the community.

Two Papers on Strong Motion Earthquake Analyses

Housner: The two papers on analysis of strong motion earthquakes report on the results of spectrum analyses made with the electric-analog computer.^{105, 106} Gilbert McCann, Professor of Electrical Engineering at Caltech, developed a large electric analog computer that

was well suited to calculate the response spectra of recorded earthquake accelerations.

The principle of the electric analog computer is that the same differential equations show up in many different fields of study, including electrical engineering, and it is easier to do experiments on the electrical circuits than it is to build a model of a vibrating building and measure the response. At that time we did not have available the digital computers that now make the problem much easier. Later Don Hudson developed a small analog computer that was faster to use.

Scott: You have worked on at least four generations of approaches to strong motion studies and response analysis. First, the era of the hand calculator. Then there was the mechanical approach with the torsion pendulum in the 1930s and early 1940s. Next came the electric analog computer in the 1950s. Finally, digital analysis was feasible when the higher powered computers became available and reasonably accessible.

Housner: Yes, and each generation reduced the time required.

Paper on Limit Design

Housner: The 1956 paper on "limit design" of structures to resist earthquakes was an early attempt to connect the spectrum intensity with

104. Housner, G.W. and J.F. Brennan, "The Estimation of Linear Trends," *The Annals of Mathematical Statistics*. XIX, 3, Inst. of Mathematical Statistics, September 1948.

105. Housner, G.W. and G.D. McCann, "The Analysis of Strong Motion Earthquake Records with the Electric Analog Computer," *Bulletin of the Seismological Society of America*. Vol.39, no.1, 1949.

106. Alford, J.L., G.W. Housner and R.R. Martel, "Spectrum Analyses of Strong motion Earthquakes," *Bulletin of the Seismological Society of America*. Vol.43, no.2, April, 1953.

the energy dissipation during an earthquake.¹⁰⁷ This was clearly an appropriate direction in which to go, because the energy dissipated by inelastic deformation is the key item in preventing a structure from failing. People have tried to follow-up on energy design, but it has not yet gotten into the code design process because of the unknown properties that many structures have. Designers now recognize the significance of inelastic deformation and energy loss, but we have not yet reached the point where energy dissipation is explicitly incorporated in the code.

Behavior of Structures During Earthquakes

Housner: The first presentation of the design spectra, as opposed to the response spectra, was in the 1959 paper on structural behavior in earthquakes.¹⁰⁸ The big advantage of the design spectrum is that it is a means of providing the same degree of earthquake resistance to different types of structures having different periods of vibration. The design spectrum is now commonly used for the design of special structures.

Scott: Would you say a word or two more about how the response spectrum and design spectrum differ, and how the latter is more useful to designers?

Housner: The response spectrum is calculated from an accelerogram, whereas the design

spectrum is a smooth curve that specifies the design forces. The response spectrum is dealing with past earthquakes, whereas the design spectrum is aimed at future structures and earthquakes.

Generation of Artificial Earthquakes

Housner: In the early days there were so few recorded accelerograms that they were only a sampling of possible earthquake ground motions, far too few for what was really needed. Engineers would like to know what kind of ground acceleration can be expected at a certain distance from an earthquake of a certain magnitude. While early-day accelerograms provided helpful guidance in design, they really did not give engineers the range of information needed. Paul Jennings and I co-authored a 1965 paper showing how you could provide an answer to such a question.¹⁰⁹ It is now very common to use the digital computer to generate such artificial ground motions. It is much easier to do this now than it was in the pre-digital days.

Scott: Say a bit more about the kinds of information used to generate the artificial earthquakes wanted. Remember that there may be quite a few non-engineers among the readers of this oral history.

Housner: A recorded accelerogram has a certain wiggly appearance, the amplitude and duration of the wiggles depending on the duration of shaking and magnitude and distance of

107. Housner, G.W., "Limit Design of Structures to Resist Earthquakes," *Proceedings of the World Conference on Earthquake Engineering*. Berkeley, CA, 1956.

108. Housner, G.W., "Behavior of Structures During Earthquakes," *Journal of Applied Mechanics*. ASCE, Proceedings Paper 2220, Vol.85, EM4, 1959.

109. Housner, G.W. and P.C. Jennings, "Generation of Artificial Earthquakes," *Journal of the Engineering Mechanics Division*. ASCE Vol.90, EM3, June 1965.

the earthquake. An artificial accelerogram is constructed with appropriate wiggles, duration and amplitude, and its response spectrum coincides with the previously specified design spectrum. It is now a commonly used proceeding to specify a design spectrum, and then to construct a corresponding artificial accelerogram to use in analyzing structural response of multi-mode structures.

Analyzing Earthquake-Induced Water Pressures

Housner: I also want to mention what I still think was an ingenious analysis of the fluid pressures on a dam in an earthquake.¹¹⁰ The title refers to the fact that the analysis was based on looking at the momentum of the fluid.

Scott: You are referring to the effects of earthquake motion on the water behind a dam, as well as on the dam itself, and particularly the resulting changes in water pressure on the dam during and immediately after the earthquake. I can see how that kind of analysis would help the engineer anticipate the kinds of forces the dam might undergo.

Housner: While that was known before, this paper contains a subtle analysis of the problem which pleased me. It was a simple analysis that explained how and why good results were obtained by it.

Scott: You have commented on seven of the 22 selected research papers. What about the other 16? Would you like to make any general

observations about them, or about groupings of them?

Housner: No, I think this is enough on the research papers.

Two Additional Publications

Housner: I will wind up here by commenting on two additional publications, neither of which were included in my initial selection. In fact, I had not even anticipated the publication of the first one, and learned about it a couple of years ago when Don Hudson came to my office saying, "Here is a book for you."

A Compilation of My Papers

Scott: What was Don Hudson's unexpected gift book?

Housner: He handed me a rather heavy volume whose title I was surprised to read: *Selected Earthquake Engineering Papers of George W. Housner*.¹¹¹ Don then explained that it was Anil Chopra's idea to have ASCE publish this volume. I presume that Don put the thing together.

Scott: I borrowed a copy from the EERC library a year or so ago and looked it over. It is a pretty substantial, hefty volume.

Housner: Yes, it contains a large number of papers that I suppose were selected by Don and Anil. I never undertook to read through the volume, as I do not want to re-live the past, although I do appreciate the recognition it confers.

110. Housner, G.W., "The Momentum Balance Method in Earthquake Engineering," *Mechanics Today*, 5, Ch. VIII, Pergamon Press, 1980.

111. Housner, G.W., *Selected Earthquake Engineering Papers of George W. Housner*. American Society of Civil Engineers, 1990.

My World War II Experience

Housner: The second publication that I did not include in my initial selection dates from the World War II era. When my colleague John Hall expressed an interest, I showed him the 52-year-old and almost illegible mimeographed report, "History of the Operations Analysis Section/15th Air Force," which I had written in Washington, D.C., in 1945, when I returned from Europe. John decided to put it in a more permanent form, so he asked our secretaries, Sharon Beckenbach and Denise Okamoto, to redo it on the word processor. They printed out 20 copies, which John had bound in hard covers and gave to me and my colleagues.¹¹² I think this publication may have set two records, first, being printed more than 50 years after it was written and still in the author's lifetime, and second, being issued in a first edition of only 20 copies.

That finishes up what I would like to say about my publications.

Scott: We can end this chapter with the two lists of your selected publications. Readers wishing to see a more complete list of your publications can refer to the ASCE volume of your selected engineering papers mentioned above, which seems to be quite widely available in engineering libraries, and has a rather extensive bibliography of your writings.

Selection of Nine Books

Housner, G.W. and D.E. Hudson, *Applied Mechanics-Statics*, Van Nostrand, 1950.

Housner, G.W. and D.E. Hudson, *Applied Mechanics-Dynamics*, Van Nostrand, 1951.

Housner, G.W. and T. Vreeland, *Analysis of Stress and Deformation*. Macmillan, 1966.

Housner, G.W., *Nuclear Reactors and Earthquakes*, AEC Handbook (earthquake engineering portion), 1963.

The Great Alaska Earthquake of 1964. Engineering volume. G.W. Housner, Technical Editor and contributor. National Academy of Sciences, 1190 pp., 1972.

Earthquake Engineering Research, G.W. Housner, Ed. National Academy of Sciences, 314 pp., 1969.

Housner, G.W. and P.C. Jennings *Earthquake Engineering Design Criteria*, Earthquake Engineering Research Institute, 140 pp., 1982.

Housner, G.W. et al., *Confronting Natural Disasters*, National Research Council report, 60 pp. 1987.

Competing Against Time, Report of the Governor's Board of Inquiry on the 1989 Loma Prieta Earthquake, G.W. Housner, Chairman. Governor's Office of Planning and Research, State of California, 264 pp., 1990.

Selection of Twenty-Two Technical Papers

Housner, G.W. and J.F. Brennan, "The Estimation of Linear Trends," *The Annals of Mathe-*

112. George W. Housner, *Operations Analysis Section, Fifteenth Air Force: History—1943-1945*. Limited edition reprint. California Institute of Technology, March 1996.

mathematical Statistics, XIX, 3, pp. 380-389, September 1948.

Housner, G.W. and G.D. McCann, "The Analysis of Strong motion Earthquake Records with the Electric Analog Computer," *Bulletin of the Seismological Society of America*. Vol.39, no.1, pp. 47-57, January 1949.

Alford, J.L., G.W. Housner and R.R. Martel, "Spectrum Analyses of Strong motion Earthquakes," ONR Report, August 1951, *Bulletin of the Seismological Society of America*. Vol.43, no.2, April 1953, pp. 97-121; Earthquake Research Laboratory Report, California Institute of Technology (revised August 1964).

Housner, G.W., "Bending Vibrations of a Pipeline Containing Flowing Fluid," *Journal of Applied Mechanics*. Vol.19, pp. 205-209, 1952.

Housner, G.W., "Spectrum Intensity of Strong motion Earthquakes," *Proceedings of the Symposium on Earthquakes and Blast Effects on Structures*. UCLA, pp. 20-36, June 1952.

Housner, G.W. and R.G. Merritt, "Effect of Foundation Compliance on Earthquake Stresses in Multistory Buildings," *Bulletin of the Seismological Society of America*. Vol.44, no.4, pp. 551-571, Oct. 1954.

Housner, G.W., "Earthquake-Resistant Design Based on Dynamic Properties of Earthquakes," *Journal of the American Concrete Institute*. Vol.28, no.1, Proceedings V. 53, pp. 85-98, July 1956.

Housner, G.W., "Limit Design of Structures to Resist Earthquakes," *Proceedings of the World Conference on Earthquake Engineering*. Berkeley, California, pp. 5:1-5:11, June 1956.

Housner, G.W., "Dynamic Pressures on Accelerated Fluid Containers," *Bulletin of the Seismological Society of America*. Vol.47, no.1, January 1957, pp. 15-37.

Housner, G.W., "The Mechanism of Sandblows," *Bulletin of the Seismological Society of America*. Vol.48, no.2, pp. 155-162, April 1958.

Housner, G.W., "Behavior of Structures During Earthquakes," *Journal of Applied Mechanics*. ASCE, Proceedings Paper 2220, 85, EM4, pp. 109-129, October, 1959.

Housner, G.W., "Design of Nuclear Power Reactors Against Earthquakes," *Proceedings of the Second World Conference on Earthquake Engineering*. Tokyo, Japan, 1960.

Berg, G.V. and G.W. Housner, "Integrated Velocity and Displacement of Strong Earthquake Ground Motion," *Bulletin of the Seismological Society of America*. Vol.51, no.2, pp. 175-189, April 1961.

Housner, G.W., "The Behavior of Inverted Pendulum Structures During Earthquakes," *Bulletin of the Seismological Society of America*. Vol.53, no.2, February 1963.

Housner, G.W. and P.C. Jennings, "Generation of Artificial Earthquakes," *Journal of the Engineering Mechanics Division*, ASCE 90, EM3, June 1965, pp. 251-253.

Housner, G.W. and C.R. Allen, "Earthquake and Geological Hazards in California," Contributors to: *Report of the Geologic Hazards Advisory Committee*, California Resources Agency, April 1967.

Housner, G.W., "Seismic Events at Koyna Dam, India," *Eleventh Symposium on Rock Mechanics*. University of California, Berkeley, June 1969.

Tsai, N.S. and G.W. Housner, "Calculation of Surface Motions of a Layered Half-Space," *Bulletin of the Seismological Society of America*. Vol.60, no.5, pp. 1625-1651, October 1970.

Housner, G.W., D.E. Hudson, et al., and P.C. Jennings (Ed.), *Engineering Features of the San Fernando Earthquake of February 9, 1971*, Earthquake Engineering Laboratory Report EERL 71-02, California Institute of Technology, 1971.

Housner, G.W., "The Momentum Balance Method in Earthquake Engineering," *Mechanics Today*, Vol.5, ch. VIII, pp. 113-127, 1980.

Housner, G.W. and M.A. Haroun, "Earthquake Response of Deformable Liquid Storage Tanks," *Proceedings of the Pressure Vessels and Piping Technology Conference*. ASME, San Francisco, California, August 1980.

Housner, G.W. and C. C. Thiel, "The Continuing Challenge: Report on the Performance of State Bridges in the Northridge Earthquake," *Earthquake Spectra*. Vol.11, no.4, EERI, November 1995.

Nonprofessional Interests

"I have always been interested in books and have collected a sizable library."

Scott: I hope you will say something about your "nonprofessional" interests—things you like to do that are not directly related to earthquake engineering.

Housner: Yes, I do have those other interests that have always meant a great deal to me.

Old Books

Housner: My main interests centered around my home are books, art and music. I have always been interested in books and have collected a sizable library. I have some books at home and technical books at the office. In addition to a lot of technical books, I also have a collection of historical items at my office. For example, I have copies of Isaac Newton's *Principia*¹¹³ and his *Optics*¹¹⁴, both of which were printed in his lifetime and may actually have been in his hands at one time.

I also have a copy of Leonard Euler's book, *Methodus Inveniendi Lineas Curvas*, published in Lausanne and Geneva, 1744.

113. Isaaco Newtono, *Philosophiae Naturalis Principia Mathematica*, London, 1726.

114. Isaaco Newton, *Optice: Sive de Reflexionibus, Refractionibus, Inflexionibus and Cororibus*, London, 1706.

That book has his first-time analysis of the deformations of a beam in bending, and contains his derivation of the well-known Euler buckling load of columns. I also have Jacob Bernoulli's *Ars Conjectandi*, the first substantial book on probability calculations.¹¹⁵ This is the same Bernoulli whose name we see in references to the "Bernoulli-Euler beam," the standard plane-sections-remain-plane beam in structural engineering.

Scott: You have some very old books—what is the oldest one in your possession?

Housner: The oldest book I have was published in 1543, and was written by Nicholas Tartaglia, a well-known name in the history of mathematics. The title is *Opera Archimedes*, and it presents the mechanics and hydrostatics of Archimedes. Tartaglia comments that in preparing the book he eliminated errors, expurgated unnecessary parts, and clearly explained the text. (Nicolatum Tartaleam, *Opera Archimedis, Syracusanai Philosophi et Mathematici Ingeniosissimi...* Venice, 1543.)

I also have a copy of Euclid's *Geometry* that Tartaglia edited, and that was published in Italian (not Latin) in 1565.¹¹⁶ The book's title page has the signatures of seven early owners, starting with a Dr. Acardi who signed in 1565. Six subsequent owners also signed, after crossing out the previous owner's name. So the book passed through the hands of seven known owners. Afterward, of course, when it became an historical item, I have no idea how many own-

ers it had. My library has a couple of hundred books related to the history of science and engineering.

I also have more recent books of considerable interest. For example, I have the four-volume Report of the Investigative Commission on the Quebec Bridge Collapse. I have also have Gustave Eiffel's three-volume 1907 work, *The Resistance of Air*.¹¹⁷ Eiffel is famous, of course, for designing the landmark tower in Paris, but lost his reputation through involvement with Ferdinand Delessup's scheme for digging the Panama Canal. Eiffel was sentenced to jail, but did not actually serve time. After that he concentrated on studies related to aeronautics.

Interest in Chinese Poetry: 1978 Trip

Scott: Joe Penzien suggested that you say something about your interest in Chinese literature and poetry, and how that turned out to be very useful in the course of the 1978 trip to China.

Housner: I had been told that as leader of the team I would be expected to speak at meetings and banquets, and also to reply to toasts made by our Chinese hosts. So I collected a supply of appropriate quotations from Chinese literature, which proved very helpful. I got one good quotation from the *Analects of Confucius* in which the very first paragraph says:

Learning with diligence and
perseverance—how pleasing.

Old friends from faraway—how
delightful.

115. Bernoulli, Jacobi, *Ars Conjectandi, Opus Posthumum...*, Basle, 1713.

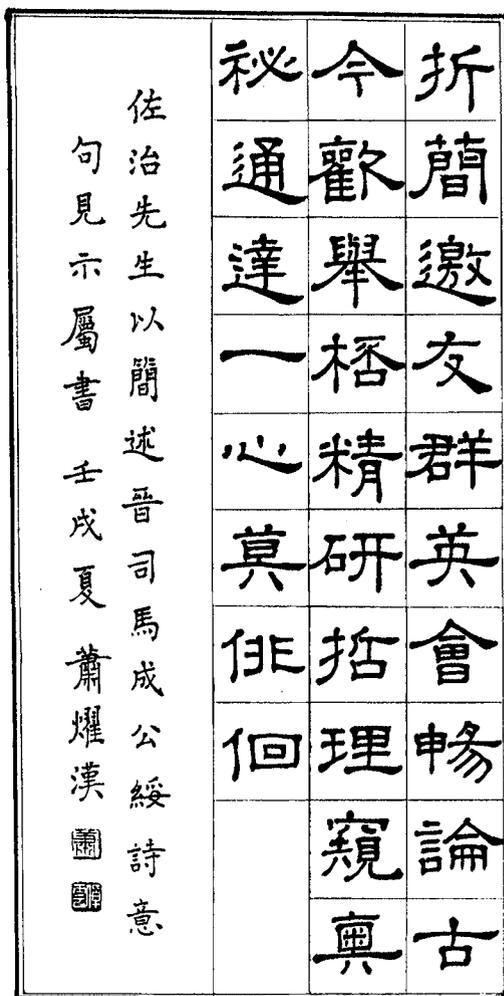
116. Tartalea, Nicolo. *Euclides Megarense Philosopho...*, Venice, Italy, 1565.

117. Eiffel, Gustave, *Recherches Experimentales sur la Resistance De L'Air Executees a la Tour Eiffel*, Paris, France, 1907.

This quotation was especially appropriate, both because before 1978 the Chinese government had denounced Confucius and he was not to be quoted, and because during that period old friends of people in China were not allowed to visit that country. While we were there in 1978 we met quite a few old friends who had studied in the U.S. before the Communist government took over in 1949.

I also found quite a few good quotations in the book of translations made by Arthur Waley, a British scholar and poet. He was well-known for his translations from Chinese and Japanese, and was particularly known for his translation from the Japanese of the book *The Tale of Genji*.¹¹⁸

I especially liked a Chinese poem written by Ch'eng-Kung Sui around the years 250 A.D. In fact I liked it so much that by mail I got an expert calligrapher in Taiwan write it out for me in Chinese. Here is the English translation, which came from Arthur Waley's book *Hundred and Seventy Chinese Poems*, published in 1935:



I sent out invitations
to summon guests.
I collected together
all my friends.
Good talk
and simple feasting;
Discussion of philosophy,
investigation of subtleties.
Tongues loosened
and minds at one.
Hearts refreshed
by discharge of emotion.

118. Shikibu, Murasaki (Lady Murasaki), *The Tale of Genji*. Translated from the Japanese by Arthur Waley. Houghton Mifflin Company, 1925-1933.

At the Harbin City Hall we attended a meeting that had been gathered to welcome our team. I read the Ch'eng poem in English, and a member of our delegation, Dr. Liu Shih-Chi, read it in Chinese. When I said that the poem had been written in the third century by Ch'eng-Kung Sui (221-293), many in the audience shook their heads "No" to indicate that they thought my identification of the poet was incorrect. Noting the disagreement, at that point Dr. Hui-Xian Liu said, "Professor Housner challenges you to identify this poet."

That night about 11:00 a sheath of papers was shoved under the door of my hotel room. When I picked it up I saw that it was a bunch of xerox sheets from a book of Chinese poetry, identifying the poet as Ch'eng. I think the Chinese were much impressed that Americans would know Chinese literature and poetry.

Scott: According to Joe Penzien, he and the other members of the U.S. delegation were also impressed, as well as surprised, since they had no previous inkling of the extent of your interest in Chinese literature.

Art and Music

Housner: In addition to books on art, I have a set of 25 Japanese wood-block color prints dealing with the 1855 Ansei earthquake that caused heavy damage in Tokyo. These Namazu-E (catfish pictures) were issued after the earthquake, and depicted the catfish in various activities, some of them subversive. The prints are based on ancient folklore that an earthquake is caused by the twitching of a giant underground catfish.

Scott: When you say some of the activities of the catfish were "subversive," do you mean they could be interpreted as critical of those in power?

Housner: Yes, they show people who profited from the earthquake consorting with the catfish. I have many books on art, Oriental art in particular. I have a collection of Japanese paintings, and a collection of Japanese color prints. The original prints date to 1690-1850. I recently gave a collection of 80 special Japanese prints to the Pacific Asia Art Museum in Pasadena. These were by artists active in the period 1890-1930, a time of transition between older-style and modern prints. I also have a few European works of art, my favorite being "The Book of Job," a set of engravings by the British artist William Blake. I also have books on European art.

Scott: Your interests in Asian art and in earthquakes fit together well, did they not? You made many trips to Japan and China on earthquake-related matters.

Housner: Sometimes I have been able to pursue both interests in the same trip. For example in 1993 the Japan Academy, of which I am a member, invited me to spend two weeks in Japan, not necessarily on technical matters. I told them I was interested in Japanese art and would like to visit some of the museums. So they worked out a varied itinerary. They arranged for my friend, Professor Emeritus Shunzo Okamoto, to take me around to four museums in Tokyo. Then Professor Kenzo Toki's wife and Professor Hiro Iemura's wife guided me to four museums in Kyoto.

Then, Professor Emeritus Keizaburo Kubo guided me around some interesting engineer-

ing projects, including the world's longest suspension bridge, then under construction. This bridge goes from Kobe to the adjacent island, and the fault that generated the 1995 earthquake runs diagonally under it. One effect of

the earthquake movement was a one-meter increase in the distance between the two bridge towers. Since the deck of the bridge had not yet been installed, the change in dimensions was not a serious problem.



From Housner's collection of Japanese color prints made after the 1855 Tokyo earthquake. Folklore has it that earthquakes are caused by the wiggling of an underground catfish. The prints are called *namazu-e* (catfish pictures) and illustrate the populace's reaction to the earthquake. In this print, the catfish is being feasted at the expense of those who profited from the reconstruction of the city.



A demonized catfish about to cause an earthquake.

I also have a deep interest in music, mainly classical European music. I have a collection of compact discs and long-playing 33 1/3 rpm records. I even have my original collection of 78 rpm records. Recently, I was fortunate enough to be able to purchase a modern record player for long-playing records that is based on the tracking principle developed for the compact disk player. Use of this player improves

the acoustical performance of the records remarkably, so as far as I am concerned they are not obsolete.

I am a member and supporter of the Pasadena Symphony Association, and the Coleman Chamber Music Association, in Pasadena. The Coleman is the oldest chamber music association in the country, having been founded by Alice Coleman in 1904. I also support the Los Angeles County Art Museum, and the Huntington Library and Art Museum.

In addition to these interests in art and music, I have always had a great interest in ancient civilizations, such as Sumeria and Babylon, Egypt, Greece, Rome, the pre-Columbian New World, and China.

Scott: Would you say something about how and when these remarkably varied interests developed. You did say that in your youth you were an avid reader of just about everything, and a great patron of the Carnegie Public Library in Saginaw. So the groundwork for these interests was presumably laid quite early. But would you say a little more about this?

Housner: My interest in books began when I was in high school and I purchased them by mail order. But of course in those days I did not have enough money to buy anything significant. In the 1930s Los Angeles and Pasadena had art stores and second-hand bookstores that had catered to the inhabitants before the Great

Depression, and these gave me an opportunity to develop my interests. Also bookstores and art stores in San Francisco were very inviting. Now, however, these have mostly disappeared in both cities. There was a time from 1945 to about

1960 when the prices of books and art objects were very low, and this encouraged me to purchase things. Now the prices are so high that I probably could not afford such acquisitions.

Closing Comments

"I could not wish for a better career."

Observations on the Interviews

Scott: We have spent a good deal of time over several years on these oral histories and their editing. Having come to the end of our task, do you have any final observations about the interviews or the process?

Housner: Yes I do. I have found the experience of undergoing the oral history interviews to be very much like undergoing a psychoanalysis—a technical psychoanalysis. It has left me with somewhat mixed feeling about the process and its end result. For one thing, there is something of an imbalance between the interview picture and reality as I lived it. In terms of time spent, my career was devoted 90 percent to teaching and research, and 10 only percent to extracurricular activities. But the oral history interviews have the percentages reversed, with 90 percent of the attention focused on the extracurricular 10 percent.

Scott: That is a valid observation. I think you will see a similar disproportion in virtually all oral history interviews. Interviews focus on activities through which interviewees have made special contributions that are in some way remarkable. Those are the things that will be most interesting and enlightening to readers.

Housner: Yes, while my career was mainly concerned with academic activities, a detailed rendition of those would make very dull material for oral interviews.

Scott: True, and I think readers knowledgeable in earthquake engineering will find a great deal that is fascinating in these interviews and their selective account of your career. I certainly did when conducting the interviews.

Housner: I have a final reservation. Around 1970 I read with great interest the book called *La Vida* written by the sociologist Oscar Lewis. It was a look at the culture of poverty in San Juan, Puerto Rico, and was based on a series of oral interviews with the members of a family, mainly the daughters. That book was very enlightening to me, and I don't think my interviews are as interesting as those in *La Vida*.

Scott: I guess you should have the last word on this, but you are sort of comparing apples and oranges.

Final Notes on My Career

Housner: I feel that my career has spanned the golden age of academia in science and engineering. This golden age began slowly in 1945 and reached its climax in the 1980s. Since then, however, some difficulties have developed that seem to me to forebode problems in the future. For example, the appointment and subsequent promotion of a young faculty member is now a highly formalized process, involving letters of recommendation, maintenance and scrutiny of lists of publications, information on numbers of research grants received, a sense of urgency regarding the acquisition of tenure, etc. These things were relatively unimportant when I started my academic career, but now I feel that they exert too great a pressure on the young faculty member.

Scott: Many of your academic colleagues in other fields share these sentiments. Ironically, some fine faculties were developed in those earlier, less formalized and seemingly more relaxed times. The quest for excellence was handled by means other than formal procedures, paper work, counting of grants and so forth. You are also right about publication pressures. In the social sciences, at least, the "publish or perish" doctrine has increasingly resulted in publication of research papers that are not very good, and that do not contribute very much. The reader must learn how to cope with this flood in finding what is valuable.

Housner: Yes. I also think the research grant system has had an unfortunate influence on academia. The amount of grant money that a faculty member receives seems now to be more important than the quality of the research done. We are no longer in the golden age, but are in the silver age, which will probably be followed by an iron age, as was foretold by the ancient Greek poet Hesiod in his poem "Works and Days."

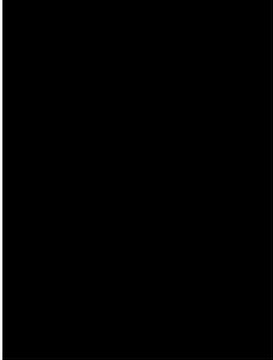
Scott: So our golden age in academic earthquake engineering lasted about forty years and then began to lose some of its luster. But a lot of progress has been and is still being made.

Housner: Yes, I feel that earthquake engineering has made great advances since the 1933 Long Beach earthquake. There is no question but that these advances have reduced the numbers of lives lost during earthquakes, and have also reduced economic losses. In this sense, earthquake engineering has been a most satisfying field of activity.

Scott: There has been great progress in technical knowledge about earthquakes and earthquake engineering, there is now much more public awareness of seismic problems, and closer attention is being given to public policies for seismic safety. It must be truly rewarding to have seen all these things happening in your lifetime.

Housner: Yes, it has, and I would like to close by acknowledging a special benefit from my career in earthquake engineering, which

made it possible to develop widespread friendships over a lifetime. I have friends and acquaintances in all the approximately forty countries that are members of the International Association for Earthquake Engineering. I have visited fifteen of those countries, and at the World Conferences on Earthquake Engineering have had discussions with earthquake engineers from all the major seismic regions of the world. To sum up, I could not wish for a better career.



Photographs



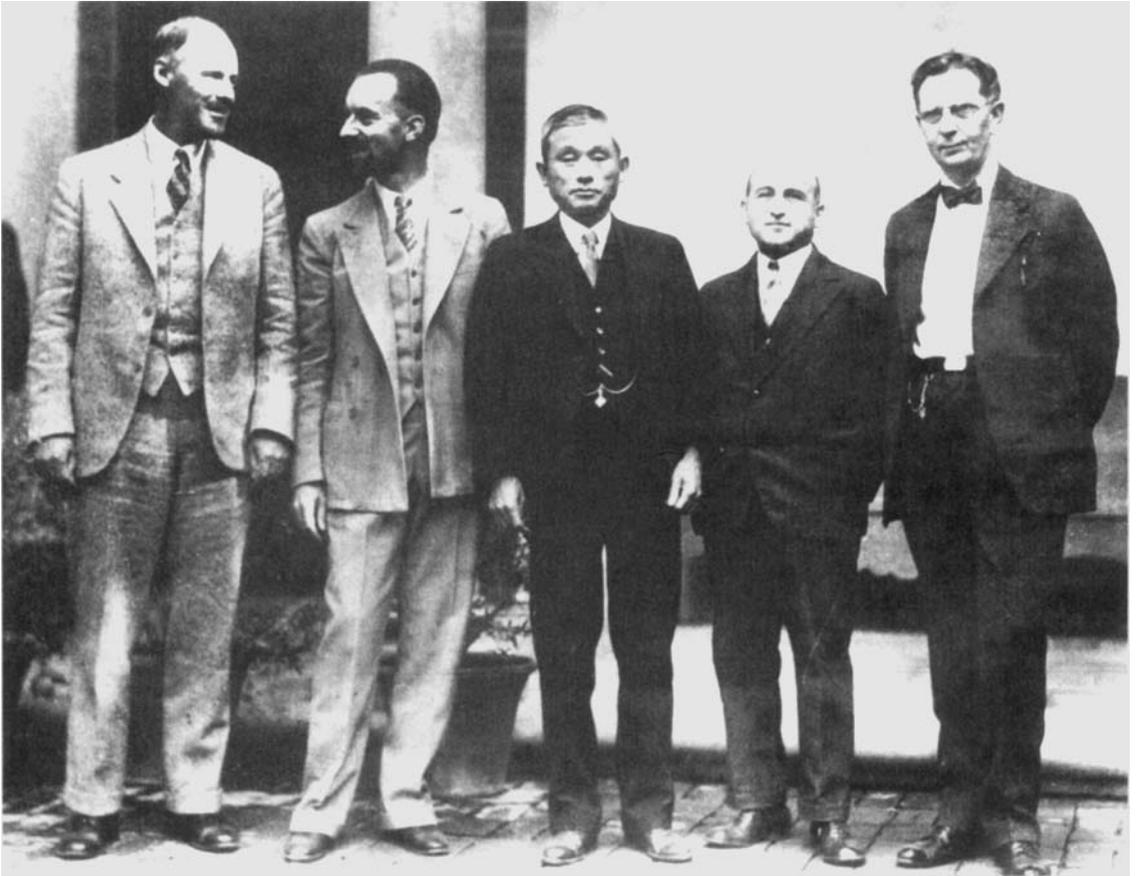
George William Housner in his office, 1985. (photo: Floyd Clark, Caltech)



High school graduation, June 1928.



A 1929 meeting at the Caltech Seismological Laboratory in Pasadena brought together many of the world's leading authorities on earthquake engineering. It was before Housner's time, but he learned from many of them and mentions them in this oral history. Front row, left to right: Archie King, L. Adams, Hugo Benioff, Beno Gutenberg, Harold Jeffreys, Charles Richter, Arthur L. Day, Harry Wood, Ralph Arnold, and John Buwalda. At the back are Alden C. White, Perry Byerly, Harry Reid, John Anderson, and Father J.P. MacElwane. This meeting led to an invitation to Beno Gutenberg to join the faculty at Caltech.



This photo was taken on the Caltech campus during Suyehiro's visit in 1931. Left to right: John Buwalda, R.R. Martel, Kyoji Suyehiro, Beno Gutenberg, John Anderson.



Housner (left) and a tentmate in Libya during World War II service in the Operations Analysis Section of the Air Force, a division of the National Research Council, August 1943.



Assistant Professor Housner in front of the demonstration shaking table on the Caltech campus, 1950.



Housner and camera in Japan, 1955. He spent a month of his summer vacation traveling, meeting with leading earthquake engineers, and visiting university engineering schools in Japan.



Housner at a National Science Foundation function with John Ide, head of engineering at NSF.



Above: A conference in Messina, Italy, in 1958 commemorating the 50th anniversary of the 1908 Messina earthquake. In Housner's words: "A warm day after lunch." Lydik Jacobsen had the next seat over, but it was then occupied only by his briefcase. (photo: Gaetano Maricchiolo)



Left: Housner (left) and Don Hudson in New Delhi, India, 1959. Housner and Hudson were invited to University of Roorkee, India, to help start the School of Engineering. This photo was taken during an independence day celebration in New Delhi. (photo: Liberty News Pictures)



Something on the other side of the fence attracted Housner's interest after the 1971 San Fernando, California earthquake.

Visiting the memorial ruins of the 1976 Tangshan earthquake, which destroyed the city of Tangshan. From left: Jai Krishna of India, Paul Jennings and George Housner of Caltech, and Hui-Hsien Liu, Director of the Institute of Engineering Mechanics in Harbin, China, 1982.





Housner and Frank Press, president of the Academy of Sciences, dine in Tokyo, 1987.



Housner receives a handshake from California Governor George Deukmejian after being appointed Chairman of the Board of Inquiry on the Loma Prieta Earthquake, 1989.



Housner presents the Mayor of Tangshan, China with the first two volumes of the English translation of the Chinese report on the Tangshan earthquake, 1996.

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