# CONNECTIONS

The EERI Oral History Series

# Robert V. Whitman

Robert Reitherman
Interviewer

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Robert Reitherman, Interviewer



Earthquake Engineering Research Institute

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# Table of Contents


| | | |
|---|---|---|
| The EERI Oral History Series | | vii |
| Foreword by Robert Reitherman | | xi |
| Personal Introduction by James K. Mitchell | | xiii |
| Chapter 1 | Growing Up in Pennsylvania | 1 |
| Chapter 2 | College and a Master's Degree | 7 |
| Chapter 3 | A Doctorate in Structural Engineering, Then Moving Into Soil Mechanics | 13 |
| Chapter 4 | Navy Civil Engineer Corps | 27 |
| Chapter 5 | Research and Teaching: A Conversation With Former Students | 33 |
| Chapter 6 | Soil-Structure Interaction | 53 |
| Chapter 7 | Consulting Engineering Projects | 61 |
| Chapter 8 | Seismic Design Decision Analysis Program at MIT | 67 |
| Chapter 9 | Earthquake Loss Estimation | 71 |
| Chapter 10 | Advisory Committees | 77 |
| Chapter 11 | Earthquake Engineering Research Institute | 83 |
| Chapter 12 | Pastimes, Family, and Other Interests | 87 |
| Photographs | | 91 |
| Index | | 99 |

# The EERI Oral History Series

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

*Connections* is a vehicle for transmitting the fascinating accounts of individuals who were present at the beginning of important developments in the field, documenting sometimes little-known facts about this history, and recording their impressions, judgments, and experiences from a personal standpoint. These reminiscences are themselves a vital contribution to our understanding of where our current state of knowledge came from and how the overall goal of reducing earthquake losses has been advanced. The Earthquake Engineering Research Institute, incorporated in 1948 as a nonprofit organization to provide an institutional base for the then-young field of earthquake engineering, is proud to help tell the story of the development of earthquake engineering through the *Connections* series. EERI has grown from a few dozen individuals in a field that lacked any significant research funding to an organization with nearly 3,000 members. It is still devoted to its original goal of investigating the effects of destructive earthquakes and publishing the results through its reconnaissance report series. EERI brings researchers and practitioners together to exchange information at its annual meetings and, via a now-extensive calendar of conferences and workshops, provides a forum through which individuals and organizations of various disciplinary backgrounds can work together for increased seismic safety.

The EERI oral history program was initiated by Stanley Scott (1921-2002). The first nine volumes were published during his lifetime, and manuscripts and interview transcripts he left to EERI are resulting in the publication of other volumes for which he is being posthumously credited. In addition, the Oral

History Committee is including further interviewees within the program's scope, following the Committee's charge to include subjects who: 1) have made an outstanding career-long contribution to earthquake engineering, 2) have valuable first-person accounts to offer concerning the history of earthquake engineering, and 3) whose backgrounds, considering the series as a whole, appropriately span the various disciplines that are included in the field of earthquake engineering. Scott's work, which he began in 1984, summed to hundreds of hours of taped interview sessions and thousands of pages of transcripts. Were it not for him, valuable facts and recollections would already have been lost.

Scott was a research political scientist at the Institute of Governmental Studies at the University of California at Berkeley. He was active in developing seismic safety policy for many years, and was a member of the California Seismic Safety Commission from 1975 to 1993. Partly for that work, he received the Alfred E. Alquist Award from the Earthquake Safety Foundation in 1990.

Scott received assistance in formulating his oral history plans from Willa Baum, Director of the University of California at Berkeley Regional Oral History Office, a division of the Bancroft Library. An unfunded interview project on earthquake engineering and seismic safety was approved, and Scott was encouraged to proceed. Following his retirement from the University in 1989, Scott continued the oral history project. For a time, some expenses were paid from a small grant from the National Science Foundation, but Scott did most of the work pro bono. This work included not only the obvious effort of preparing for and conducting the interviews themselves, but also the more time-consuming tasks of reviewing transcripts and editing the manuscripts to flow smoothly.

The *Connections* oral history series presents a selection of senior individuals in earthquake engineering who were present at the beginning of the modern era of that field. The term "earthquake engineering" as used here has the same meaning as in the name of EERI—the broadly construed set of disciplines, including geosciences and social sciences as well as engineering itself, that together form a related body of knowledge and collection of individuals that revolve around the subject of earthquakes. The events described in these oral histories span many kinds of activities: research, design projects, public policy and broad social aspects, and education, as well as interesting personal aspects of the subjects' lives.

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

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|---|---|
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# Foreword

The interviews for this oral history took place between 2006 and 2008. The session in which Ricardo Dobry and Mishac Yegian participated occurred on October 27, 2006.

Appreciated is the diligence and insight of three members of the Oral History Committee, Thalia Anagnos, Ricardo Dobry, and Loring Wyllie who reviewed the manuscript. Also acknowledged is the work of Gail Hynes Shea, consulting editor to EERI, who carefully reviewed the entire manuscript and prepared the index, and Laura Moger who was responsible for the final page layout work for the publication, as they have on previous *Connections* volumes. Eloise Gilland, the Editorial and Publications Manager of EERI, also assisted in seeing this publication through to completion.

Robert Reitherman
Chair, EERI Oral History Committee
July 2009

# Personal Introduction

To be invited by Bob Whitman to write a personal introduction to his oral history is a high honor for me. My personal association, friendship, and admiration of Bob goes back nearly sixty years—even though we have not worked especially closely together technically, as his career and mine have followed separate paths and focused on rather different areas within geotechnical and earthquake engineering. Interestingly, or perhaps ironically, his career, spent working on very dynamic problems, was founded on the rather quiet ground at MIT, whereas most of mine was spent on the very shaky ground at U.C. Berkeley, working primarily on more static and esoteric problems in geotechnical engineering. That he was so successful in what he did from so far is attested to by the fact that, with the exception of the oral histories of New Zealanders Robert Park and Thomas Paulay, Bob's will be the only one (to date) about one of our distinguished colleagues from outside California to be included in the *Connections* series.

I met Bob Whitman for the first time in the summer of 1951. I had completed the undergraduate program in civil engineering at Rensselaer Polytechnic Institute in June and was hired as a graduate student research assistant at MIT by Bill Lambe, who put me to work on soil stabilization research for military applications. Bob had just completed his ScD program in structures and agreed to move into the soil mechanics program to work with Professor D. W. Taylor developing equipment and procedures for very high speed transient triaxial testing of soils in order to simulate the effects of blast loading. His reputation as a really brilliant student and whiz-bang researcher preceded his arrival into what was, at that time, a rather small group. My recollection is that for that academic year (1951-52), the soil mechanics program consisted of Professors Taylor, Lambe, Harl Aldrich, Bob, a mechanical engineering PhD working on the transient loading for a soil testing project, one teaching assistant (the late Ron Scott), one research assistant (me), and a handful of graduate students. Bob and I, as well as our wives, Betsy and Bunny, became good friends beginning in those early days, and it has always been a pleasure when our paths crossed, often at conferences, in the years since.

Bob was appointed to a regular faculty position at MIT in 1953, and, with the exception of a couple of years on active duty (much of it in Hawaii!) as a Naval officer (August 1954 to December 1956), he served MIT with distinction for many years until his retirement in 1993 and was a key player in the development of the geotechnical engineering graduate program into one of the best in the country. After completing my doctoral work at MIT in 1955, and following a couple of years in the Army, I was recruited by the late Harry Seed for the faculty at U.C. Berkeley. The Berkeley and MIT geotechnical programs maintained strong and collegial interactions, as well as a friendly rivalry, as we grew and developed along separate paths.

The oral history of Bob Whitman that follows is a fascinating document. Not only is it a most interesting and inspiring biography of one of EERI's most accomplished and productive members, but it is also a remarkable history of the development of some of the most important areas within earthquake engineering.

Bob's entry into the soil dynamics and geotechnical earthquake engineering arenas couldn't have been better timed. Soil dynamics was just getting into high gear about the time he finished his ScD, and when the study of liquefaction and earthquake-induced ground failures burst onto the scene following the 1964 Alaska and Niigata earthquakes, he was already primed and poised to hit the ground running.

Oral history interviewer Bob Reitherman asked Bob Whitman just the right questions, and has added some valuable history as well. The contributions of Ricardo Dobry and Mishac Yegian, who were present for one of the interviews, provide meaningful embellishments. The technical discussions are framed in a way that makes them tutorials on the key elements of the topics being addressed. The footnotes and annotated references provide an invaluable resource for accessing additional in-depth information. I think it would be possible to build one or more graduate courses about some of the important subjects just by using these resources.

Bob Whitman's recollections and perceptive appraisals of Donald Taylor, Karl Terzaghi, Arthur Casagrande, Ralph Peck, Bill Lambe, Bill Richart, Laurits Bjerrum, Harry Seed, and other geotechnical giants of the last sixty years are especially interesting to me, as I have known and interacted with each of these individuals in some way also. The 1969 Lambe and Whitman book, *Soil Mechanics*, with its rather different sequencing of topics relative to previous texts, has

become a classic. I was very pleased when Bill and Bob chose to include my own 1976 book, *Fundamentals of Soil Behavior*, in the John Wiley & Sons *Series in Soil Engineering*, for which they served as editors.

Bob's technical and policy contributions lie at the very foundation of much that is now state of knowledge and state of practice in earthquake engineering. The record shows that some of his earliest work with Don Taylor on slope stability under dynamic loading established the basis for the well-known and widely used "Newmark sliding block method" for estimating earthquake-induced ground displacements. He was in the thick of the liquefaction controversies that swirled around—notably involving the Harvard group (Arthur Casagrande, Gonzalo Castro, and Steve Poulos) and the Berkeley group (Harry Seed, Ken Lee, and Ed Idriss). He chaired the National Research Council committee that produced the report, *Liquefaction of Soils During Earthquakes* (1985), a report that has guided practice in this area for more than twenty years. He made major contributions to the development of methods for the study of dynamic soil-structure interactions for analyzing nuclear power plants and earthen retaining structures during earthquakes. He played a key role in the development of the Applied Technology Council's ATC 3-06, *Tentative Provisions for the Development of Seismic Regulations for Buildings*, which (for the first time) provided national maps for earthquake shaking hazard. Bob was a pioneer and leader in the early studies of risk-based geotechnical engineering and in the development of seismic codes. He became highly interested in the problem of earthquake loss estimation and chaired the National Research Council panel that proposed a loss estimation methodology, and then chaired the committee that developed HAZUS (Hazard U.S.). All of these contributions and activities are described in some detail in this oral history. Bob's memory of detail and his clarity of expression about the times and events surrounding this work make for wonderful reading.

With all this experience and expertise, it is not surprising that Bob Whitman has been a highly sought-after consultant for many years on major projects nationally and internationally. What perhaps is surprising is that he has had the time to take them on, especially in light of the many hours he has devoted to teaching, service to MIT, the profession (e.g., president of EERI) and his community of Lexington, Massachusetts. In the concluding chapter of this volume and by the appended photographs, we learn about his wonderful family and other interests.

This oral history will stand as a vibrant testament to a man who has done so much to advance our technical knowledge and to develop methods for earthquake engineering risk assessment and mitigation. I am proud to have Bob Whitman as a colleague and friend.

James K. Mitchell
Cahill Professor of Civil Engineering Emeritus
*University of California, Berkeley*

University Distinguished Professor Emeritus
*Virginia Polytechnic Institute and State University*

March 2009

# CONNECTIONS

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## Robert V. Whitman

Chapter 1

# Growing Up in Pennsylvania

*I went home and asked my father what kind of person builds dams, and he said "a civil engineer." And from that day on, I never really strayed away from the notion that I was going to be a civil engineer.*

**Whitman:** I was born February 2, 1928, and I was brought up in Edgewood, Pennsylvania, a suburb of Pittsburgh. My parents had both been born in the vicinity of Moravia, New York, upstate in the Finger Lakes District. Both grew up on farms.

## Parents and Brother Were Professors

**Whitman:** My father, Edwin A Whitman, was the first member of his family to go to college. (My father never had a full middle name, just the letter A, which he wrote without a period.) He went to Yale. He had the expectation of going on to medical school, but when he graduated the funds were not there, so he accepted a job teaching mathematics at a private school in Pittsburgh.

He remained in the Pittsburgh area thereafter, teaching successively in several public schools and then joined the faculty of what was known as Carnegie Tech, Carnegie Institute of Technology, which is today part of Carnegie Mellon University. He served on the faculty from the late 1920s until he retired in the early 1950s.

He and faculty colleagues wrote some very successful textbooks. One was *College Algebra*,[1] which by the standards of that day was a bestseller. I always thought that book put me through college.

He also co-authored a textbook, *Plane Trigonometry with Tables.* In addition, he developed a series of aids for teaching calculus. He had some students working for him—paid for by some Depression-era funding—making physical models to help get across the concept of integration. Using celluloid or string, they first showed a cube to represent an elemental volume; then a series of cubes to form a column; next a series of columns to form a slice; and finally a series of slices to fill the volume. He also made a film strip and experimented with an animated movie, showing the same sequence of steps. This was when I was about at the junior high school level, so I was getting mathematical concepts fed to me at home as well as at school. I was intrigued by mathematics.

My mother was Elsie Van Duyne, pronounced "van dine." The middle initial V. in my name stands for a full middle name—Van Duyne. She was not the first of her family to attend college. Her older brother had also done so. She graduated from a teacher's college in the state of New York, and just before she married my father she was teaching at Bennington College in Vermont.

I had a much older brother, Philip, nearly twelve years older than I. I did not get to know him well during my early years, because by the time I was beginning school he was off in college. He attended Haverford College and went on to get a doctorate in mathematics at Harvard. He taught at the University of Pennsylvania, then joined the Manhattan Project in World War II. Following the war, he came back to a teaching position at Tufts, then left to work for the Applied Physics Laboratory in Maryland.

**Reitherman:** At Johns Hopkins?

**Whitman:** That's right. There he was involved in operation research studies related to defense. In the mid 1950s, he went to Rhode Island College and headed the mathematics department there. In addition, Phillip co-authored a later updating of *College Algebra*. He also became quite active in faculty leadership. After I completed college, we took several long driving trips to various parts of the country. I was interested in seeking out dams, while he sought out old railroad lines. Phillip died in 1996.

**Reitherman:** Especially for that era, your family tree seems rather heavily loaded with academic fruit—all those higher education credentials—Yale, Bennington, Haverford, Harvard, University of Pennsylvania, Tufts, the Applied Physics Lab at Johns Hopkins. When you grew up, was it just assumed you would go to college?

1. Joseph Bernhardt Rosenbach and Edwin A Whitman, *College Algebra*. Ginn and Company, Boston, various editions.

**Whitman:** There never was any question.

**Reitherman:** Did you know your grandparents?

**Whitman:** Both of my grandfathers were dead by the time I was born. They had both been farmers. One of my grandmothers, my mother's mother, lived with us in Edgewood in her later years, so I got to know her somewhat well. My father's mother continued to live in Moravia, New York. We went up to Moravia each summer until I was about six.

**Reitherman:** Did you live in the same house while you were growing up?

**Whitman:** Yes. My parents had built the house not long before I was born. I attended Edgewood Public School—that was its full name. Edgewood, Pennsylvania was then and still is a small community with a population of about 5,000. My school went from elementary school up through high school. There were about sixty-five students in my class, so it wasn't a large school. It was a good school system. They had a number of very good teachers, and in a small school like that the teachers could have a big influence. The teacher I remember best was Francis Crouse, the man who was my homeroom teacher in high school and also taught mathematics and American history. I admired him, and he had a beneficial effect on me that I would have a hard time quantifying.

## First Acquaintance With Civil Engineering

**Reitherman:** By the time you finished high school, you wouldn't have taken any actual civil engineering classes yet. But do you remember when you first heard that term and knew what civil engineering was?

**Whitman:** One summer, my friends and I spent many days in the creek in the valley below where we lived, building little earth dams. I went home and asked my father what kind of person builds dams, and he said "a civil engineer." And from that day on, I never really strayed away from the notion that I was going to be a civil engineer.

This playful experience in the creek was augmented by a major flood in Pittsburgh in 1936 when I was about eight. The streets were filled with eight to ten feet of water. Subsequent to that, there were dam and flood control projects undertaken in the Allegheny and Monongahela River watersheds. Studies were made at Carnegie Tech, hydraulic models of the spillways, models of the river systems, and so forth. Each year at the open house at Carnegie Tech, I would see what was new. Within the field of civil engineering, I thought my career would go into the area of water resources.

## Trains, Ships, Baseball

**Whitman:** I was in high school in the World War II era. I recall walking home on Sunday, December 7, 1941, and coming upon some friends who broke the news to me about Pearl Harbor. As a student during World War II, I built models of warships, at a scale of about an inch to a hundred feet, carved out of wood. I had a copy of *Jane's Fighting Ships* to provide the information I needed to model the ships, and I built a considerable fleet—aircraft carriers, battleships, cruisers, destroyers, cargo

ships, the submarines, and so forth, and won a science fair prize.

Another thing I got interested in while growing up in Edgewood was model railroading. My brother and I constructed a modest railroad system in the basement of our house. We didn't put in place all the scenery we intended, but we had a working system and from that time forth had an interest in model trains.

**Reitherman:** What size model trains? The big Lionel type?

**Whitman:** We had a Lionel set that ran around a Christmas tree, but the extensive layout I'm describing was done with HO scale trains.

**Reitherman:** Those were such keen trains—very detailed. I never knew what HO stood for, but that was a household name to us kids.

**Whitman:** It abbreviated Half O, meaning half the width of the O gauge. Earlier there was a Standard gauge of about three inches track width, then the smaller O gauge, and then the HO. There were wonderful kits for making your own HO railcars out of wood. Later in life, in the first house my wife Betsy and I had here in Lexington, Massachusetts, I built an HO layout in a back room, then had to rebuild it when the house was expanded. I passed my trains to my two sons-in-law, who are active modelers.

**Reitherman:** How do you remember the Depression?

**Whitman:** My parents had the family background of frugal farmers, and because of their frugality they were relatively well prepared for the Depression. Dad had to take a pay cut at Carnegie Tech, but he had a steady job. People came to the door hoping to get food, and Mom would always have a supply of something on hand, like hot soup, that she could give them.

**Reitherman:** Were you a sports fan?

**Whitman:** I played a lot of sandlot baseball and softball as a youngster, and had a brief stint with American Legion baseball while in high school. I followed the Pittsburgh Pirates avidly. The games were at Forbes Field. On Friday afternoons, children up to about the age of fourteen could get in for a reduced price. I remember the major disappointment when the Pirates lost the 1938 National League championship to the Chicago Cubs right at the end of the season.

**Reitherman:** Did you have a favorite player?

**Whitman:** I had two favorites, Paul Waner and Lloyd Waner.

**Reitherman:** Little Poison and Big Poison. Wow, you got to see them play. Which was which?

**Whitman:** Lloyd, the centerfielder, was Little Poison. Paul, Big Poison, the somewhat better hitter, was the right fielder. Pie Traynor was the manager then. Needless to say, when I got to Boston later on to go to MIT, I shifted my allegiance fairly quickly to the Red Sox. I went to a lot of baseball games when I was a graduate student. Fenway Park was, and still is, small and intimate. Ted Williams was in his prime during my early years in Boston. I never saw any of his finest moments, but did witness one time when he reacted badly to fans booing after a poor play in the field. He clearly was a wonderful athlete. But I still had some loyalty

to the Pirates, and enjoyed the famous home run by Bill Mazeroski that won the World Series in 1960.

**Reitherman:** We have talked about everything except your schoolwork. Were you a good student?

**Whitman:** Until fourth grade, I was considered an indifferent student. That year there was a "geography bee," matching states and their capitals. The previous summer my parents drove from Pittsburgh to Salt Lake City and back, and I had become well versed about capitals. From that point on, teachers treated me as a good student, and I reacted accordingly. There were only sixty-some students in each year at Edgewood, and even in that small group I was only one of several best students. I did win a local prize in an American history contest—and I have always had a love for that subject.

Chapter 2

# College and a Master's Degree

*The subject of earthquakes hadn't appeared at all in my career by the time I got my master's at MIT. Earthquakes, along with geotechnical engineering, were going to be subjects downstream in my life, after my doctorate.*

**Reitherman:** Sometime during your senior year you must have been applying to colleges. You ended up at Swarthmore. What were your thoughts on choices for college?

**Whitman:** I applied to three colleges. My brother had gone to Haverford, and Swarthmore was similar but had engineering. I applied to Cornell because of my parents' origins in that part of New York. And I applied to Yale in honor of my father, though I don't think I was seriously interested in it.

## Going to Swarthmore College

**Reitherman:** Swarthmore, then and today, would be regarded as one of the top liberal arts colleges in the country, devoted to undergraduate education, but it also

has a civil engineering curriculum, which is unusual for a small college.

**Whitman:** When I was there, Swarthmore had accredited programs in civil, electrical, and mechanical engineering. It was a high-quality program: a small place, with teachers who were skilled in motivating you to learn.

**Reitherman:** Did you get a broader undergraduate education there than you might have obtained at a large research university?

**Whitman:** Today, that would be true of a Swarthmore student, but I was there in post-World War II years, when students were returning from the military with all kinds of different backgrounds, some transferring from other schools. It was a time when it was very difficult to enforce curricular regulations across the board. I think I was determined to get through Swarthmore with the least broad education possible. I did take several years of German, because my father thought that would be a good thing for a scientist to do. Indeed when I did my doctoral degree, more than half the references in my thesis were in German, so I made good use of that earlier study. German was certainly the language of engineering in the first four decades of the twentieth century. At MIT, reading proficiency in two foreign languages was at that time required for a doctorate. For me, German was one and the other was French.

When I came up to MIT to do graduate work, the undergraduate requirement to take eight semesters of humanities and social sciences had just been put in place. The graduate program had the policy of requiring all its students to meet all the requirements of the MIT undergraduate program. New graduate students were evaluated in this regard, and I was quite correctly identified as being deficient in the humanities. They set up special seminars to let graduate students take care of these requirements. I took a course in English and one in economics, and both were enormously stimulating to me. I began to get a broader perspective on the world.

**Reitherman:** Up through your undergraduate education, did you have any exposure to earthquake engineering?

**Whitman:** No. The subject of earthquakes hadn't appeared at all in my career by the time I got my master's at MIT. Earthquakes, along with geotechnical engineering, were going to be subjects downstream in my life, after my doctorate.

**Reitherman:** Are there any particular professors at Swarthmore who stand out in your mind?

**Whitman:** Yes, Sam Carpenter, the professor of structures. He was one of these wonderful teachers you sometimes meet in life. Civil engineering students from that time at Swarthmore, when we see each other at alumni events, describe ourselves as "Sam's boys." He very clearly got across the concepts of moment distribution and indeterminate structures. Cutting a section through a structure, isolating a free body, was a powerful idea that he communicated very well. Late in my career at MIT, I volunteered to teach an introductory course on mechanics, and I emphasized the conceptualizing of sections cut through structures. Today that approach may be obsolete, I don't

know—the student might just plug numbers into a computer.

Sam Carpenter had a project to analyze an early version of offshore platforms. I, along with other students, spent one summer working on a physical model of a trussed platform. We applied loads at various places and measured deflections. Sam looked at our work and said, "Let's see. You put the load here and measured the deflection there. If you put the load there, where you first measured the deflection, and measure the deflection here, where you originally put the load, does it come out the way the theorem you were taught would predict?" He made us think. I originally thought I would feature water in my civil engineering, but Sam Carpenter got me also interested in structures. When I went to MIT I spent my master's degree doing about half structures and half hydraulics.

## Getting a Master's at MIT

**Reitherman:** Eventually, of course, we are going to be talking about your prominent earthquake engineering career as a geotechnical engineer, a structural engineer and strong motion expert estimating earthquake losses, and a leader in key roles for organizations such as the Earthquake Engineering Research Institute (EERI) and the National Research Council. When did you begin to study subjects that were indirectly related to your later work in the earthquake field, even if they were not explicitly about earthquake engineering?

**Whitman:** I learned a lot about structural dynamics, modal analysis, and so forth, from Charles Norris at MIT when I went there for graduate school. He was an outstanding teacher. He was one of those people who knew how to get a concept across, going at it with a combination of mathematical rigor and a feeling for the physics of the problem. There was some early work at MIT in the earthquake field, but before my time. John R. Freeman of course was very important in the field, authoring his big book on the subject,[2] and he went to MIT and was active as an alumnus, though he never taught there.

I wish I had known him. Of course, Freeman was way before my time. When I got to MIT, I didn't get acquainted with any specific earthquake engineering right away. The course by Norris dealt with the case of motion applied at the base of a structure as well as forces applied to a structure.

**Reitherman:** When you began your graduate work, were you interested in earthquake engineering?

**Whitman:** No more than the usual casual interest. I had my early interest in the hydrau-

2. John Ripley Freeman, *Earthquake Damage and Earthquake Insurance: Studies of a Rational Basis for Earthquake Insurance*, *Also Studies of Engineering Data for Earthquake-Resisting Construction.* McGraw-Hill, New York, 1932. In his career, Freeman (1855-1932) held the presidency of both the American Society of Mechanical Engineers in 1905-1906 and the American Society of Civil Engineers in 1922. Freeman is the only person to have led both organizations. More than anyone, he was responsible for development of the earthquake accelerograph and operation of those instruments via ongoing funding for the strong motion program of the U.S. Coast and Geodetic Survey.

lics side of civil engineering and my interest in structural engineering that I picked up at Swarthmore. And though I was to later do work in the geotechnical engineering field, that was after my master's. In fact, it was after my doctoral degree, which was in structural engineering. Earthquake engineering came after that, once I had been in the geotechnical engineering field for some time, in part applying soil dynamics to other engineering challenges.

I don't know all the details of early developments at MIT in applying civil engineering to the earthquake problem. There was Arthur Ruge[3] doing some early shake table experiments. After he helped invent the modern strain gauge, the SR-4, I'm not sure what further involvement he had in the earthquake field. I think the R in the historic SR-4 strain gauge was for Ruge.[4]

When I arrived at MIT, I don't think Ruge was still involved in dynamics work or earthquake engineering. In the lab there was some sort of simple mass-spring model for a structure that could be shaken to demonstrate the concept of modal response.

## Early Work on Structural Dynamics at MIT

**Reitherman:** What about other MIT faculty who were involved with the structural dynamics topic? By the time Ruge would have receded from the scene, there were others. There's the book by John Biggs[5] on that topic, for example. And even earlier there was the book that collected chapters on the same general subject, which was authored mostly by MIT professors Charles Norris, John Biggs, and two of Biggs's fellow faculty and consulting engineering firm partners, Bob Hansen and Myle Holley.[6]

**Whitman:** I had a course from both Holley and Biggs, both outstanding teachers. Holley, known at MIT as Chris, was quite a broad thinker.

I took a course on reinforced concrete design from him. He walked into the design session one afternoon and sketched a hip roof building. He said, "Now design this," then walked out, saying, "If you have any questions I'll be

3. Arthur Ruge was the first faculty member in the United States with the title of "Professor of Engineering Seismology." His dynamics laboratory work at MIT contributed to the literature in the *Bulletin of the Seismological Society of America* and *Transactions of the American Society of Civil Engineers* on shake table simulators and strong motion instruments.

4. The S was for Edward Simmons at Caltech. Simmons and Ruge independently invented the same basic electric resistance strain gauge instrument at opposite ends of the continent. The "4" was for four people, because Ruge had one assistant who worked on his project and so did Simmons.

5. John Biggs, *An Introduction to Structural Dynamics*, McGraw Hill, New York, 1964. Biggs joined the MIT civil engineering faculty as an instructor in 1947 and became an assistant professor in 1949. He was the recipient of the ASCE Moisseiff and Wellington awards.

6. C.H. Norris, R.J. Hansen, M.J. Holley, J.M. Biggs, S. Namyet, and John V. Minami, *Structural Design for Dynamic Loads*, McGraw Hill, New York, 1959.

in my office." He was very good at motivating people to think for themselves.

John Biggs, known as Mel, was a very down-to-earth teacher and engineer, with considerable insight into dynamic problems.

Bob Hansen was a pioneer in bringing research grants supported by the Defense Department to the MIT civil engineering department. This work was motivated by the challenges of defense against nuclear attack. It involved basic theoretical and experimental work at MIT and also in designing test structures that were later exposed to nuclear explosions in Nevada and on Eniwetok Atoll in the Pacific. This was the origin of the important growth in structural dynamics work at MIT after World War II.

**Reitherman:** At about the time you were getting your doctoral degree, from 1949 through 1951, there were several other people like yourself who were to become major figures in the earthquake engineering field and who have MIT connections: Clough, Penzien, Bertero.

**Whitman:** When I became Chuck Norris's teaching assistant, I took over from Ray Clough. Clough left MIT to join the faculty of the University of California at Berkeley in 1949. Joe Penzien was another graduate student at MIT at that time, for another year or so to come, Joe being a little younger than Ray. Vitelmo Bertero was there at MIT working on his doctorate as well, starting in 1953. If I remember correctly, Bertero stayed on for a while after his studies and worked with Bob Hansen on his dynamics projects.

**Reitherman:** Vitelmo Bertero has said that the civil engineering work on dynamics that he knew of at MIT at this time, in the 1950s—whether structural dynamics or soil dynamics—was related to the military applications because of the Cold War threat.

**Whitman:** That's right. In my work in soil dynamics, that remained true for quite a while, beyond the early 1960s.

**Reitherman:** How did this early work concerning blast effects transition into earthquake engineering?

**Whitman:** As efforts began to design nuclear power plants against the effects of earthquakes, the structures professors at MIT became involved as consultants. This, plus the rapid growth in the availability of research funds following the Alaska earthquake in 1964, fueled the transition. The change in emphasis from blast to earthquakes can be approximately dated by a book that Bob Hansen edited, dealing with earthquake engineering for nuclear power plants.[7] MIT became a major player in earthquake engineering research at that time.

**Reitherman:** What about John Wilbur? He would have been another professor at MIT when you were a student.

**Whitman:** He was the head of the Civil Engineering Department when I was a graduate student, also when I came on the faculty

7. Robert J. Hansen, editor, *Seismic Design for Nuclear Power Plants.* MIT Press, Cambridge, MA, 1970. Whitman wrote the chapters on "Basic Concepts and Important Problems," "Soil-Structure Interaction," and "Evaluation of Soil Properties for Site Evaluation and Dynamic Analysis." It was also in this book that A. Arias first published his measure of intensity, Arias Intensity, in the chapter on "A Measure of Earthquake Intensity."

and for several years thereafter. He wrote a textbook with Norris on structural analysis that was very popular and went through several editions.[8] He was another very good and thoughtful teacher, and an experienced engineer as well as a researcher.

8. Charles Head Norris and John Benson Wilbur, *Elementary Structural Analysis*. McGraw-Hill, New York, 1948.

Chapter 3

# A Doctorate in Structural Engineering, Then Moving Into Soil Mechanics

*The command decision was made by the department head that, in the interest of the department, I ought to work on soil dynamics instead of structural dynamics.*

**Whitman:** I received my MS degree in 1949. My master's degree from MIT is actually called SM, but it means the same as MS at every other place. As I was finishing work on my master's, which was composed of about equal parts hydraulics and structures, I was contemplating getting a job with the Bureau of Reclamation. That would have fit in nicely with my childhood aspiration to design dams. But as I was procrastinating going out to ask for a job with the Bureau, Charles Norris offered me a job as his teaching assistant. So for the time being, that pushed hydraulics aside and I proceeded to do a doctorate in structural engineering. My thesis

title was "The Flexural Torsional Buckling of Eccentrically Loaded I-Beams."

At that time, when doctoral candidates took the general examination, they had to do it in their major area and two minor areas. My two minors were hydraulics and soil mechanics. I had only had one course in the latter. I did far and away the best on the hydraulics exam, reasonably well on the structures, and terrible on the soils, but they still passed me.

Normally, if you had a minor in soils in the doctorate program you took a year-long, two-course sequence in the subject, taught by Don Taylor, who was head of the soil mechanics program. For some reason, I couldn't fit one of the required courses, the first one, into my course schedule, and I only took the second course. I remember clearly that Taylor, who was a wonderful and insightful researcher but not a remarkable lecturer in the classroom, was lecturing one day on the meaning of cohesion and friction in soils. And I said to myself, there's going to be a question on that topic on the quiz. So I literally set for myself the task of explaining all that he said in twenty-five words or less. Sure enough, there was a question on cohesion and friction in soils on the quiz. And then, to boot, the teaching assistant handing out the test said that this was a very simple test, which also involved doing a slope stability analysis, and he said you ought to be able to do it on both sides of one piece of paper. So I took that challenge. When Taylor handed out the graded quizzes to the students, he pulled mine out and waved it in front of the class and in effect said, "You see—here's the way you can do it."

**Reitherman:** In the proceedings of the symposium held by MIT in 1994 in your honor,[9] you said, "I have been extremely fortunate as regards the two areas of specialization into which I tumbled: geotechnical engineering and earthquake engineering." How did you "tumble" into geotechnical engineering?

**Whitman:** I received my doctorate in 1951. For reasons somewhat peculiar to MIT, my degree was ScD rather than a PhD—they gave me my choice, and I picked one. Literally, the students at MIT in engineering have their choice, whether they wish to get their doctorate as a doctor of philosophy (PhD) or doctor of science (ScD). I didn't know the difference then. Today, you can see the ones who are thinking very broadly about engineering take the PhD. The ones who come from more of the old school, the scientific line, prefer the doctor of science degree, because that degree is held in greater esteem in Europe today than the PhD.

As my doctoral thesis was coming to a close, Bob Hansen asked me to join him on the research projects he was conducting on blast effects on structures. I agreed to join him.

## Donald Taylor

**Whitman:** At this time, Don Taylor, head of the soil mechanics program, got his first large research project on soil dynamics. He must have scoured the landscape to find

9. *The Earth, Engineers, and Education: A Symposium in Honor of Robert V. Whitman*, October 7 and 8, 1994, Department of Civil Engineering, Massachusetts Institute of Technology, Cambridge, MA, p. 205.

somebody to work on that project, and finally, I think out of desperation (or maybe because of that quiz), he turned to me and asked if I would be interested. I replied, "Sure, but I've already agreed to join Bob Hansen." So Taylor went to John Wilbur. The command decision was made by the department head that, in the interest of the department, I ought to work on soil dynamics instead of structural dynamics. Taylor knew little about dynamics, and having launched the new project, turned most of the responsibility for it over to me.

**Reitherman:** And that's the origin of your geotechnical engineering career?

**Whitman:** Yes, it was as simple as that. I began work as a research associate in the fall of 1951. I was appointed Assistant Professor as of July 1953.

**Reitherman:** Say a few words about Donald Taylor and his career.[10]

**Whitman:** Don Taylor assumed the role of leading the MIT soil mechanics program in the late 1930s as Glennon Gilboy developed some health problems. Taylor was a meticulous experimenter, and developed many important insights concerning the strength of soils. Taylor wrote a pioneering textbook in the field,[11] a book that was much more fundamentally written than the contemporary book by Terzaghi and Peck.[12] The Terzaghi and Peck book was written to advise practicing engineers and the construction industry. Terzaghi's *Theoretical Soil Mechanics*, also a 1940s book, was a collection of theoretical work that Terzaghi and others in Europe had done earlier. Taylor in some sense updated Terzaghi, but from a very fundamental teaching standpoint. Taylor died from cancer early in his career, in 1955.

## Historical Aspects of Soil Mechanics

**Reitherman:** Tell me a little bit about the terminology and development of soil mechanics. Eventually, the historical narrative brings us to the field called geotechnical engineering, and the term geoengineering is currently trying to increase its share of the market of discourse in this subject area. But as of the 1950s, is it true that the term "geotechnical engineering" was not used yet?

**Whitman:** Correct, that term was not around. That came along later, well into the 1960s, if not later than that. The day came at MIT when we decided we would change the name of our graduate program from Soil Mechanics to Geotechnical Engineering.

**Reitherman:** How about the term "soil dynamics"?

**Whitman:** That was literally what we started calling the research project when it began in 1951. Perhaps Bill Richart, who was doing work at Harvard on foundation dynamics—a mass sitting on a continuum—was using

10. Donald W. Taylor joined the civil engineering department at MIT beginning in 1933 and was on the faculty until his death in 1955. Taylor was instrumental in the recognition of the strength contributed by interlocking soil particles, as distinct from friction between soil particles.

11. Donald W. Taylor, *Fundamentals of Soil Mechanics*. John Wiley and Sons, New York, 1948.

12. Karl Terzaghi and Ralph B. Peck, *Soil Mechanics in Engineering Practice*. John Wiley and Sons, New York, 1948.

the term "soil dynamics" then. I cannot be sure. I say "Bill" Richart—though his name was Frank, he was known as Bill.[13]

## Karl Terzaghi

**Reitherman:** Geotechnical engineering is a relatively recent term, and "soil dynamics" preceded it. How about the even earlier term "soil mechanics"? What was the origin of that term and the associated concepts?

**Whitman:** That had been around since the days of Karl Terzaghi. It was derived from the title of his influential and pioneering book, *Erdbaumechanik*.[14]

**Reitherman:** In the proceedings of the symposium in your honor in 1994, there is a genealogy of MIT geotechnical faculty in one of the appendices. Karl Terzaghi, according to that list, is the first soil mechanics member of the faculty at MIT, back in the 1925-1926 academic year.

**Whitman:** That's right. Soil mechanics at MIT began with that appointment. Terzaghi did his pioneering work in Istanbul in World War I, assigned there by the Austrian government, and was doing research on the clay soils in the area. There he developed the fundamental understandings that formed the basis for soil mechanics. He did a lecture tour soon after the war in the U.S. and was invited to join the faculty at MIT. He remained only a few years, having disagreements with the president of MIT. Before leaving, he initiated a program related to highway pavement subgrades, which continued after he left and returned to Europe. Just before World War II, he came to Harvard with a faculty appointment there.

We call Terzaghi the father of soil mechanics. In addition to his first book, written in German and translated into English and other languages, there was another book by him on soil mechanics theory[15] and a third co-authored with Ralph Peck.[16]

13. Frank E. "Bill" Richart, Jr. (1918-1994), after getting his PhD under Nathan Newmark at the University of Illinois in structural engineering, was hired as an assistant professor at Harvard in 1948, where he met Karl Terzaghi and became interested in soils and foundations. Moving to the University of Florida, he authored a report on research there, *Foundation Vibrations*, in 1960. After moving to the University of Michigan, he authored a textbook, *Vibrations of Soils and Foundations*, with Richard D. Woods and John Russell Hall, Jr. (Prentice Hall, 1970).

14. Karl von Terzaghi, *Erdbaumechanik*, Franz Deuticke, Vienna, 1925. "Erdbaumechanik" translated into English is "soil mechanics." Terzaghi (1883-1963) grew up in Prague and went to college at the Technische Hochshcule (technical university) in Grasz, Austria, receiving a degree in mechanical engineering. After several years as a practicing engineer he did his doctoral thesis on reinforced concrete design. He developed his soil mechanics book first at the Royal Ottoman College of Engineering in Istanbul around the time of World War I, then at Robert College nearby (which in the 1970s was to become Bogaziçi University). In 1925 he was hired by MIT, then moved back to Austria, then emigrated again to the U.S. just prior to the outbreak of World War II, where he joined the faculty of Harvard University until his retirement, consulting on a number of large dams and other projects.

15. Karl Terzaghi, *Theoretical Soil Mechanics*. John Wiley and Sons, New York, NY, 1943.

16. Karl Terzaghi and Ralph B. Peck, *Soil Mechanics*

**Reitherman:** Did you know him?

**Whitman:** Yes. I knew him late in his career. At that point he was at Harvard in a mostly retired capacity. He was an excellent lecturer on the projects on which he had consulted, and was invited to give lectures to our MIT students and staff. He was alternatingly charming and hard on you. He was infamous for chewing out the poor graduate student who projected one of his slides upside down—even though quite possibly Terzaghi was the one who put it in upside down. On the other hand, he could be absolutely charming in a social setting.

I was fortunate to get to know him and his wife a little socially. Laurits Bjerrum of the Norwegian Technical Institute was invited to come to MIT by Bill Lambe to be a visiting professor for a semester, and Laurits became my office mate. I can mention more about Lambe and Bjerrum in a moment. Talking with Laurits about his project-related work in Norway and co-teaching a course with him was very valuable for me. During a subsequent visit, Laurits was staying in our home, and when Laurits was invited to dinner with the Terzaghis, we were invited too.

**Reitherman:** What were the innovations that Terzaghi introduced to the engineering study of soils?

**Whitman:** What he is most known for is consolidation theory. He developed the theory that when a load is placed on a soil, particularly a soil of low permeability, this would initially cause an excess pore pressure. The water, being less compressible than the matrix of clay particles, is the phase that picks up that load initially. But with time, the excess pore pressure dissipates, and in the process the clay settles.

More broadly than that, there is the concept of effective stress. You can separate the total load being carried by the soil mass into that carried by the pore pressure and that carried by the mineral skeleton. The effective compressive stress in the mineral skeleton produces the resistance to shear.

There is an enormous literature today about the particulate nature of soils, especially the very plastic clay soils, and how forces are actually transmitted between particles. It's essential to geotechnical engineering.

## Other Prominent Soils Engineers

**Reitherman:** Did you also get to know Arthur Casagrande?[17] What was he like?

**Whitman:** Casagrande was hired by MIT to be Terzaghi's assistant. He made very basic contributions to the classification of soils. When Terzaghi returned to Europe, Casagrande was not selected for the vacant professorship. He then left and was hired by Harvard as a faculty member. When Terzaghi wished to leave Europe at the outbreak of World War II, Casagrande arranged to have him appointed to Harvard.

*in Engineering Practice*. John Wiley and Sons, New York, 1948.

17. Arthur Casagrande (1902-1981), like Terzaghi, was educated in Austria—in Casagrande's case in Vienna at the Technische Hochshule—and similarly later immigrated to the United States, joining the MIT faculty in the 1926-1927 academic year, one year after Terzaghi. He later taught at Harvard. Casagrande developed early apparatus for the triaxial test and other laboratory techniques that became standard geotechnical engineering methods.

He was similar to Terzaghi as a person, in that he was a bit crusty as well as socially delightful. He was an excellent experimentalist and developed the concept of a critical void ratio for sands. Arthur was also a good engineer, and he was known for his precise lectures.

**Reitherman:** Ralph Peck[18] was another relatively early soil mechanics pioneer with connections to Terzaghi and Casagrande.

**Whitman:** Peck's father had been a bridge engineer with the Rio Grande Railroad, and Ralph earned his doctorate in structural engineering at Rensselaer Polytechnic Institute (RPI). He then came to Harvard to study soil mechanics with Terzaghi and Casagrande. Peck was picked by Terzaghi to be his man on the site of the Chicago subway excavations. At the end of that project, Ralph went to the University of Illinois to join the faculty. He was an absolutely charming person, but firm as an engineer when necessary. Ralph was much in demand as a consultant, and was active until near his death in 2008. I was privileged to know and work with him.

**Reitherman:** Earlier you mentioned Bill Lambe and Laurits Bjerrum.

**Whitman:** Bill Lambe became the head of the MIT soil mechanics program on the death of Taylor, and steered the group through the heyday of the program. He did not contribute to soil dynamics. Rather, his initial contributions were to the physio-chemical behavior of particulate systems—and then to systematic learning from engineering experiences. I have been fortunate to have Bill as a colleague and friend for many years.

Bjerrum was a Dane who became the founding managing director of the Norwegian Geotechnical Institute (NGI) in Oslo. He led the Institute during the years when it became—in my opinion—the premier consulting/research geotechnical organization in the world. Initially concerned primarily with the challenges presented by the Norwegian quick clay, NGI became a leader in foundation problems concerning offshore oil structures, earthen dams, and other geotechnical problems as well. A close association developed between NGI and the MIT geotechnical programs—much to our benefit. The two subsequent managing directors of NGI did graduate work at MIT. Laurits was a thorough and imaginative researcher, a very good engineer, an excellent lecturer, and a charming gentleman.

## Nuclear Blast Effects on Soils

**Reitherman:** From the soil mechanics standpoint, what is the difference between a blast and an earthquake?

**Whitman:** When I first got into the soil dynamics project, Taylor had already set the research topic of the strength of soils under very rapid blast loading. I think that's because Casagrande had earlier done some work concerning strain rate effect, and it was thought desirable to extend it. My first work was to

18. Ralph Peck (1912-2008) was born in Winnipeg, Canada. He studied with Karl Terzaghi, co-authored several papers and an influential book with him, taught at the University of Illinois, and has consulted on many construction projects in the U.S., Canada, and other countries. The book he co-authored with Terzaghi, *Soil Mechanics in Engineering Practice*, was noted above.

build equipment that could load soil samples in as little as a millisecond—a thousandth of a second.

But as I got into the blast loading problem and began to interact with the military people who were sponsoring the work, I began to realize that the propagation of the blast wave through the soil was of more interest and concern to them than the strength of the soil. So I began to transition my work to the study of waves as they passed through soil. At an early stage of designing missile bases, the loading was conceived as occurring from a blast some distance away rather than a pinpoint hit right on our silos housing intercontinental missiles. Under that scenario, there would be a shock wave moving out through the air, and it would move faster than the type of waves propagating through the ground. The blast waves coming from the air would put a slap on the ground, imparting a blast wave through the ground.

**Reitherman:** Were there two kinds of blast effects considered, dynamic pressure and static overpressure?

**Whitman:** There was the initial very sudden shock followed by the longer duration pressure that followed and decayed.

**Reitherman:** Take a look at this report of yours from 1952,[19] with the precise mechanical engineering drawings that depict a machine that looks as if all its components were built from scratch.

19. Robert V. Whitman, *The Behavior of Soils Under Dynamic Loadings: 1. Hydraulic Machine for Dynamic Compression Tests*. Massachusetts Institute of Technology, Soil Mechanics Laboratory, August 1952.

**Whitman:** It was the very first machine of its type. And it was a beast of a machine. I was handed a mixture of various criteria for designing it, and was allocated the time of an excellent engineer at MIT who was from China and couldn't return to his country in that era. He had done work designing some of Bob Hansen's test equipment. We designed a machine that could do far more than was really required.

We can discuss my time in the military later, but when I was in the Navy and then returned to MIT at the end of 1956, the machine had disappeared. I then designed a much simpler machine, which from the standpoint of studying strain rate effects on the strength of soils, could do the job just as well.

**Reitherman:** Could you explain a little bit about strain rates? How was that used in your experimental work on soils?

**Whitman:** We applied a deformation, forcing the soil to strain, measuring the rate of the application of the deformation divided by the length of the soil. This was done in a span of milliseconds. If we got to 10 percent strain at failure of the soil sample in a millisecond, we had 10,000 percent strain per second. A typical sample would be about four inches long, so 10 percent strain would be 0.4 inch, and we would produce that strain in a thousandth of a second.

**Reitherman:** Later on, did you use a similar experimental method when you began to conduct earthquake engineering research?

**Whitman:** The short answer is no. I'll explain that a little. The effect of a rapidly applied load, to see what the deformation would be, turns out to be of little practical interest in earthquake engineering. I can well

remember going to the Fourth International Conference on Soil Mechanics and Foundation Engineering in London in 1957.[20] I was reporting on the research I had done for Don Taylor, explaining that we found that the strength of soils could be two or three times greater under our dynamic loading for blast as compared to a static test.

When I was in England for that conference, I visited the laboratory of Imperial College in London and talked with Nicholas, or Nick, Ambraseys. He had already been doing research on earthquake topics, and I had not. Ambraseys said to me: "You've got it all wrong. Soil is weaker during rapid loading, not stronger." I didn't understand what he was telling me. If I had understood him, I would have reached a brilliant conclusion then about soils and earthquakes, and done so twenty years prior to Harry Seed. Nick clearly understood that when sandy soils were shaken by an earthquake, they were weaker. Coming from my experience with a blast loading applied over a duration of a millisecond, and observing that soils mobilized more rather than less strength in that case, I had trouble seeing it as Nick did, with his earthquake engineering background. As I've explained, in my career as of that time, I wasn't yet exposed to the topic of earthquakes.

**Reitherman:** Your testing set-up created a single, rapidly applied load, not a cyclic load? And the key difference with earthquakes is that the weakening of the soil occurs because of the cyclic nature of the earthquake loading?

**Whitman:** Yes, because of the cyclic nature of the loading on the skeleton of soil particles—a skeleton that has many contact points among particles—with each contact point carrying shear. Now comes a cyclic loading that causes the particles to rearrange, and if they were rather loosely arranged to start with, they will have a tendency to fall into a somewhat denser state as they are strained back and forth. But if the pore spaces are filled with groundwater, they can't compress that fluid significantly as they arrange more compactly, and part of the load that was previously carried by the soil skeleton gets transferred into the water as increased pressure. If carried far enough, with enough load carried by the fluid, which has no shear resistance, you get liquefaction.

## Soil Dynamics and Blast Engineering

**Reitherman:** Explain a little about your early soil dynamics research on the strain rate effect.

**Whitman:** I got started in soil dynamics studying the effect of rate of loading on shear strength of soils, and even soft rocks, on behalf of the military because of their interest in blast effects. We had this apparatus that could fail typical soil samples, either in unconfined compression or in the usual soil mechanics test, which we call the triaxial test. It had a rubber membrane enclosing the sample so that lateral pressure could be applied. I've tried in a paper to survey the development of soil dynamics

20. The First International Conference on Soil Mechanics and Foundation Engineering was held in 1936 at Harvard University. It was organized by Arthur Casagrande and chaired by Karl Terzaghi.

in the last half of the twentieth century if you want more details.[21]

**Reitherman:** Could you give a simple definition of "triaxial" for the reader?

**Whitman:** First, visualize a cylinder of soil, maybe an inch or two in diameter and three or four times that in length. Soil under the ground would have pressures coming from the sides, from the weight of the surrounding soil, so to simulate that in the laboratory you make a mould for the cylindrical sample, which can hold loose sand in it that would otherwise not hold together to form the cylinder you want to test. Then put that membrane-enclosed sample in a larger cylinder and surround it with fluid under pressure. The pressure is calibrated to represent the pressure at some depth below grade. This is all done to try to represent in situ conditions, when the weight of soil above the sample would put it under pressure. Then the laboratory apparatus applies a force to push down on the sample and test its strength.

These samples could be failed in as little as five milliseconds in our special dynamic testing apparatus. What we found was that with dry sand, there was very little strain rate effect even with our very rapid rates of loading. As soon as you had moisture in the soil, you started getting stronger effects, about a factor of two or three increase in strength at a failure occurring at five milliseconds—as compared to the time to failure of several minutes. I think this research was used in the late 1950s or early 1960s in the design of underground military structures.

21. R.V. Whitman, "*Fifty Years of Soil Dynamics*," Fifteenth Nabor Carrillo Lecture, Mexican Society for Soil Mechanics, 2000.

Then I got interested in wave propagation through soil, because that seemed more germane to the blast effects problem. There were questions of how the stress in the soil from an air shockwave attenuated with depth. We set up a special apparatus at MIT for this experimentation that looked like an iron lung. It contained a sample of sand and clay about a meter long encased in a rubber membrane and subjected to confining pressure, with a device for delivering an impact at one end. We made measurements and took photographs as that wave passed down the length of that sample. The results were interesting, and demonstrated the importance of nonlinearity more than time effects.

A year after I came back from my military service, I became involved in what was called the Dynamic Forces Advisory Board, set up by the Air Force Ballistic Missiles Division. It was to provide guidance on the siting and design of blast-protected missile facilities. It was very interesting for me. Nate Newmark and Ralph Peck were both members and got me interacting with people like Lydik Jacobsen and Mike Agbabian. It was a wonderful experience being on that panel. Ralph Peck and I collaborated on a manual for the Air Force in choosing sites for their hardened missile facilities. Ralph Peck, in addition to being a gentleman in every way when you interacted with him, and in addition to being very sharp and knowledgeable about not only the theory of soil mechanics but the practice of it as well, could say things very clearly and insisted on that.

Another interesting writing project related to that era's research was a seven-volume work on

the effects of nuclear explosions.[22] I prepared Volume 2, which was on the effects on soil, and to some extent, on rock. It was a chance for me to synthesize everything I had learned on that topic up to that point.

This research led to additional research on what we might call the one-dimensional transmission of very intense waves through soil. We set up a special testing apparatus at MIT with samples up to about a foot in diameter. The samples were confined by a ring, so that there was no lateral strain, there was just the straight, one-dimensional compression of the soil. We found what the compressive modulus would be for a sample under blast-type loading. Stanley Wilson, of Shannon and Wilson, consultant to the Air Force, used the results in siting studies.

At this time I also became involved with the people at the Applied Physics Division at the Stanford Research Institute, who had contracts to do field measurements at the Nevada Test Site, where nuclear weapons were tested. They were developing sensors for measuring stress as a function of depth as a blast wave passed over the surface. That in turn led to my spending what in effect was a sabbatical year at Stanford Research Institute, now called SRI. This was from July of 1963 to July of 1964. During that time there, together with a former student of mine, Lynn Seaman, we designed and constructed a ten-foot-long test column, which consisted of a stack of rings about ten inches to a foot in diameter, with a rubber membrane and a neoprene pad between rings so that the stack of rings would have enough flexibility in the axial direction so as to not restrain the soil as it was undergoing axial strains.

**Reitherman:** Is this related to today's testing of large soil boxes on shake tables, or small ones on centrifuges, that have stacked rings along their up-down axis that confine the soil, but allow lateral movement? That seems like a different test set-up, but related to the early testing you describe.

**Whitman:** I think the SRI research may have been the start of that testing idea. The SRI people followed up on the interpretation of those tests to develop analytical methods used in the design of hardened facilities.

**Reitherman:** Would you offer a few numbers that would indicate the magnitude of the blast loads you were studying?

**Whitman:** Numbers anywhere up to several hundred psi were common, with 100 psi translating to over 14,000 psf.

At the time I was doing this research, there was a large targeting error in missiles, so that the concern was not a direct hit but rather a nuclear detonation going off in the vicinity of one of our missile bases. As missiles became more accurate, design pressures for hardening these facilities later went up considerably.

**Reitherman:** Some of the people who were involved with you in soil dynamics in blast effects research are obviously also big names in the history of geotechnical earthquake engineering. Could you comment on the transfer of knowledge from the military topic

22. *Nuclear Geoplosics, A Source Book of Underground Phenomena and Effects of Nuclear Explosions.* Defense Atomic Support Agency, 1964.

of blast effects to the natural disaster subject of earthquakes?

**Whitman:** The repetition of cycles in earthquakes is a key factor distinguishing the earthquake ground motion phenomenon from blast-induced soil effects. But in the 1950s and 1960s, military support was really the only major support for dynamic studies of structures and soils in the U.S. There were many other people, besides myself, who got their start in learning about these dynamic principles in the blast field and later did earthquake research. You recall that Joe Penzien got his doctorate at MIT under Robert Hansen and later became a world leader in the dynamics of structures under earthquake loading. His early work was in blast also.

**Reitherman:** Ray Clough went on sabbatical to Trondheim, Norway in the 1956-1957 academic year to do research at a naval architecture research institute. He said that no one in the U.S. was funding research on earthquake engineering, which to him required doing more research on structural dynamics. But the Norwegians were doing advanced research on structural dynamics, to design ships, so that was an opportunity for him to continue his line of research.

**Whitman:** There was no significant American funding for earthquake engineering research until at least after the 1964 Alaska earthquake. At that point, the National Science Foundation (NSF) began to fund a little bit of research, and then after the 1971 San Fernando earthquake, NSF really began to significantly fund this kind of research.

## Foundation Studies for Radar Facilities

**Whitman:** I also had some experience with soil dynamics studies for another kind of military facility—radar stations. In the 1960s, I was asked to provide consulting services with regard to some of the very large tracking radars, which were being developed to pick up the launch of a hostile missile 6,000 miles away and determine very quickly whether it was a confirmed launch, and if so, where it was headed. This obviously required some very precise measurements to be obtained, and that meant the platform for the radar equipment had to be very stable. When I first became involved, the criteria were to limit any soil deformation to some fraction of a millimeter, whereas geotechnical engineers of the day were accustomed to limiting settlements of foundations to, at best, an inch or two. My primary function was really to be an intermediary between the electronics people giving the performance specifications and the foundation consultants trying to respond to those requirements. The dynamic forces in this case, developed by the movement of the platform when it was in use, were quite small, so the problem was not as difficult as it seemed.

As I got involved in that work, I became familiar with a body of research coming under the heading of elastic half-space theory. That theory dealt with the dynamic motion of a rigid mass resting on the surface of a semi-infinite elastic medium. This was work that Frank (Bill) Richart was building on earlier research by people primarily in Germany. Bill developed testing techniques to study the key parameter in that theory, the shear modulus

of soil. Stanley Wilson, working at Harvard in that same time—late 1940s and early 1950s—had developed a dynamic triaxial test that was vibratory in nature. It shook a sample to measure its natural frequency and one could deduce its shear modulus. Richart, first at the University of Florida and then at the University of Michigan, had gone on to refine that testing technique.

There was a remarkable aspect to that theory. It predicted that if you had a vertical excitation of this rigid mass, even though the mass was sitting on a purely elastic body with no presumed damping in its material, there was a large damping effect. Physically, what was happening was that each time you pushed down the mass, waves were generated and went outward. Since the medium went on infinitely, the energy never came back. You might get significant resonant effects for rocking and torsion, but not for vertical or horizontal translation.

This prompted the Army to undertake tests at the Waterways Experiment Station at Vicksburg, Mississippi. The first tests involved casting large concrete cylindrical blocks. They were usually four or five feet in diameter, but some were up to ten feet in diameter. These were placed on the surface of the soil, which in that area is loess, or windblown soil.

The stiffness of the soil was quite uniform with depth, which was ideal. Eccentric mass shakers were placed on the blocks, and results were plotted in terms of amplitude of response versus frequency of excitation. With those shakers, the force increased with frequency. The theory predicted what the peak response should be—and the theory predicted it would be independent of the stiffness of the soil. You just needed to know Poisson's Ratio, which would be in a fairly narrow range. Richart and I were consultants for the tests, and we published two papers in 1967—one report on the testing and one on design procedures.[23,24] The results were absolutely stunning. The theory's predictions were right on.

There had been a lot of dispute about how to design these radar facility foundations, and some of us had been advocating applying this theory, but these tests showed its validity—at least for a soil like the one in Vicksburg with uniform stiffness.

The work begun by Richart led to advances in earthquake engineering. Because soil is a very nonlinear material, as the dynamic strain increased, the effective shear modulus tended to decrease. That became a crucial finding in earthquake engineering that people like Harry Seed and Ricardo Dobry followed up on.

**Reitherman:** Maybe you could distinguish between inelasticity and nonlinearity in soil response. Could you have a case where the soil behaves nonlinearly, even though the material itself is still elastic?

**Whitman:** Theory has been developed to understand what goes on when you apply a point load to an elastic sphere, and a packing of such spheres. When you have shear action,

23. F.E. Richart, Jr. and R.V. Whitman, "Footing Vibrations: Comparison of Test Results with Theory," *Journal of Soil Mechanics, Proceedings of the ASCE.* Vol. 93, no. SM6, pp. 143-168, 1967.

24. F.E. Richart, Jr. and R.V. Whitman, "Design Procedures for Dynamically Loaded Footings," *Journal of Soil Mechanics, Proceedings of ASCE.* Vol. 93, no. SM6, pp. 169-193, 1967.

you start to get slip over the contact points, and if you apply load cyclically, you have minute slips going on that dissipate energy. You can have elastic behavior of each sphere, but nonlinear behavior of the overall soil. This gets back to the importance of confining pressures. As the geophysicists became involved in measuring in situ properties of soil—using underground explosions or pounding on the soil—they typically measured the velocity of compressional waves, or, actually, dilatational waves. These waves travel faster than the shear waves, and the velocity difference in saturated soils can be enormous. To a seismologist, the compressional wave is the P wave and the shear wave is the S wave. Richart and I tried to convince people they had to get in situ measurements of shear wave velocities. You had to put a shear-type action into the soil.

**Reitherman:** How do you get that kind of shear into the soil in a field test?

**Whitman:** One of the techniques was to put a timber, like a railroad tie, on the ground, drive the back wheels of your Jeep up on it to hold the timber in firm contact with the ground, and have a pendulum that could swing down and hit the end of the timber. This sideways impact was different than the tests that were made previously by hitting the railroad tie vertically.

One small contribution we made in our research at MIT at this time was the importance of distinguishing dynamic and static properties of soil. If you measure P waves in situ of sands, and back-figure the compressive modulus, you get a much larger value than for measurements in the laboratory. Up to then, people had been attributing this mostly to time difference. The field tests were dynamic, the lab tests static. But we managed to carry out static laboratory tests with specially designed apparatus in which we could apply very small compressive stresses and measure the very small vertical strains. We demonstrated that, at those very small levels, the laboratory and field test values were compatible.[25] This emphasized again the importance of the nonlinearity of soil. For dynamic problems such as earthquakes, it is more the stress level than the rate of loading that is important.

25. R.V. Whitman, E.T. Miller, and P.J. Moore, "Yielding and Locking of Confined Sand," *Journal of Soil Mechanics, Proceedings of ASCE.* Vol. 90, no. SM4, pp 57-64, 1964.

Chapter 4

# Navy Civil Engineer Corps

*Our Ford was hoisted onto the LST in Pearl Harbor, and when we arrived at the coast of Kauai at Port Allen, we "stormed the beach" in our station wagon.*

### Marrying Betsy

**Whitman:** I met my wife, Betsy, the year before I finished my doctoral degree. Betsy's maiden name was Elizabeth Cushman. Her family was from Newton, Massachusetts. She was educated as an interior designer at the Rhode Island School of Design, receiving her MFA in 1949. Her first job was in the architecture-engineering firm of Thomas Worcester, doing drafting and also design. At that time she met Harry Seed, or H. Bolton Seed, when he was working there also. So Betsy met him long before I knew him and when I worked on geotechnical projects with him in the 1960s. We'll talk about Harry more later.

When the Worcester firm went out of business, Betsy worked briefly at a much larger engineering firm where she was doing drafting that didn't interest her. Then Saul

Namyet, who had been a colleague during Betsy's first job but had subsequently come to MIT for study, suggested she might be interested in doing drafting for Bob Hansen. One day Namyet told me "This chick is coming in Monday to work here. You ought to check her out." So, I did check her out. It took three years of checking—she had to check on me too of course—before we got married.

After about a year or so at MIT, the work for her dried up and Betsy had to look for a new job. She was very fortunate to walk into The Architects Collaborative just as their interior designer had given notice she was leaving to move to Italy. She had an interview with several of the famous architects there, several of the partners.

**Reitherman:** It wasn't Walter Gropius' firm, TAC, was it? He would have been there in Cambridge about that time.

**Whitman:** Yes, it was Gropius. Betsy was hired on the spot.

**Reitherman:** Wow! Walk into the office of Walter Gropius and just like that, you're hired!

**Whitman:** She worked primarily with Benjamin Thompson, and when Thompson set up Design Research, in 1953, Betsy became one of DR's original team. I don't know if you've ever heard of Design Research, but it was a pioneering and innovative outlet for furnishings of Scandinavian and modern design.

**Reitherman:** The Design Research store in Cambridge was quite a breath of fresh air and unusually innovative when I knew it in the 1970s—fabrics, tables, lamps, and other furnishings that were designed to have a very clean architectural appearance. It's hard for someone who didn't live in that era to realize what a revolution Design Research was.

**Whitman:** That five-story Design Research building that you knew was designed by Thompson and built in 1970. The original location of the store was across Brattle Street from it in a remodeled old building. In addition to furniture and furnishings, Thompson sought out and purchased lines of products like Marimekko fabrics, which no one in this country had seen before. Betsy was there for just over a year until we departed for my first naval assignment, and then worked part-time for several years after our subsequent return.[26]

## In the Navy, Off to Hawaii

**Whitman:** The Korean War was underway during the three years following my graduate study. Because I was involved in defense research, I was deferred from the draft for a time, but then such deferments ceased and my name was near the top of my draft board's list. In addition, I was married in June of 1954. If I were drafted, I knew in advance I would be sent to the Pentagon or to the Army Corps of Engineers research facility in Vicksburg, Mississippi, to continue to do research work. As

26. Later the day of this interview, Betsy recalled wielding a sledgehammer with the other initial employees to knock out partitions in that original Design Research location in the process of getting the first store established. Betsy's taste in furnishings is revealed in the Whitman residence. Much of their furniture is today considered classic and is much sought after, but at that time they were considered avant-garde and had a limited following in America.

Betsy and I discussed this prospect, it seemed like a better idea to be "an officer and a gentleman" during the first years of our life together. Given my prior interest in the navy, it seemed natural to volunteer for the Naval Officer Candidate School program—knowing that it was the Navy's policy that new officers would not receive assignments in research. I was directed to report to Newport, Rhode Island in August 1954.

After four months of training in Newport, in December 1954 I was commissioned ensign in the Navy Civil Engineer Corps. All officer candidates were interviewed as part of the process of making assignments, and we knew we would be asked to name three preferences for places to be stationed, even though we weren't guaranteed getting any of our choices. Betsy and I had talked this over, and decided to name places that would be pleasant to be stationed and that we might otherwise never visit. Our list was: 1. Hawaii; 2. San Francisco; 3. Seattle. That was when we never imagined the freedom to travel that exists today.

The officer interviewed the seventy-two about-to-be-commissioned officers, in alphabetical order, so of course I was near the end of the list. When I said I wanted to go to Hawaii as first choice, he pounded the table and exclaimed, "I've interviewed sixty-nine of you today, and you're the first one who had sense enough to ask for Hawaii!" So we thought there was a pretty good chance of success, and in fact that was the assignment I was given. Perhaps it also helped that I was first in the class of several hundred officer candidates, and had set a new academic record. A year later, however, that record was broken by an enlisted man who had never been to college. The Navy's newspaper for the enlisted ranks made great to-do that the record, which had previously been held by a PhD, had been broken—which I thought was a very appropriate reaction.

We had another two months of training at Port Hueneme in southern California before shipping out to Hawaii. We were to depart San Francisco on a Navy transport ship, in the spring of 1954. There were a few days of on-again, off-again waiting there. At first the sailing was delayed because the ship was still being worked on. The next day it was towed to its departure dock—towed because the engine wouldn't start. We were boarded and did a test cruise around the Bay, and the engine stopped. The next morning we sailed out of the Bay, were told this was just sea trials, and went back under the Golden Gate Bridge into the Bay again. We went out to sea again and finally did a lifeboat drill. Then it was announced we would proceed to Hawaii, and were also told that we had been followed by a tug all this time in case we broke down. But the rest of the trip went well.

There was one interesting bit of geotechnical engineering news as we got off the ship in Hawaii. The newspaper had a headline that said "Peck Sued for $1,000,000." Ralph Peck had been consulting for the city and county in connection with the first tunnel that was being put through the mountains behind Honolulu. Ralph later told the tale of being on an airplane at the Honolulu airport as lawsuit papers were being delivered there to serve him. The plane sat there for a while, as the authorities figured out whether or not it should take off. Eventually Peck was served the papers and

the plane could leave. This was merely a legal tactic by the contractor to put pressure on the city and county for more money, and when that was straightened out, the contractor hired Peck directly to tell him how to finish the job.

**Reitherman:** When you say you were sent to Hawaii, where were you specifically stationed?

**Whitman:** At Pearl Harbor Naval Shipyard. We were there a year and half. My job was in the public works department of the shipyard. That department provided utilities, transportation, and maintenance for buildings. During the later part of our year and a half there, the Public Works Officer was Captain Lloyd Root. Following his retirement from the Navy, Lloyd applied for the position of office manager in the Seattle office of Shannon & Wilson—a preeminent geotechnical firm—and used me as a reference!

I was given a variety of tasks. I became the local expert in termite control. I was liaison between officers occupying quarters within the shipyard and the maintenance shop. Of greater interest, I helped implement a preventive maintenance program. We used the simple computer systems available at that time to set up scheduled maintenance work for all the buildings—with the goal of preventing buildings from decaying to the point where it would cost a lot to fix them up. This involved several trips to mainland shipyards with similar programs. There were also occasional technical problems sent my way.

The junior officers of the public works department were, on a rotating basis, on call to deal with emergencies during hours when the office was unstaffed. There was a manual of instructions for guidance, and I recall reading that if an earthquake occurs in Alaska and a tsunami is threatened, the dry dock facing the ocean should be flooded. I remember thinking how daunting it would be for a mere ensign to be faced with that situation. Fortunately, the situation never happened while I was there, but that was my first introduction to an earthquake threat. The assignment to Hawaii also provided the opportunity to learn about the geological evolution if the islands. All-in-all, while not research, it was a worthwhile professional experience.

Of course, we enjoyed being in Hawaii. We lived in military housing near the shipyard. Honolulu and all of Oahu was there to be explored. We went to several of the other Hawaiian Islands. There was an LST landing craft, whose function was ostensibly to re-provision the garrisons in outlying places on Kauai, Hawaii, and other islands, but in fact their major mission was to transport people for R&R—rest and recuperation—at these nice places. The LST (Landing Ship, Tank) was developed to hold tanks and other vehicles in its hull, and when it beached, the bow opened up and the vehicles drove out. Our Ford was hoisted onto the LST in Pearl Harbor, and when we arrived at the coast of Kauai at Port Allen, we "stormed the beach" in our station wagon. We also had pleasant visits to Maui and the Big Island.

Our first daughter, Jill, was born in Hawaii in 1956. A week after bringing her home, Betsy was sent back to the hospital for several days. I had crash on-the-job-training in single parenting—wishing that there was a grandparent closer than 6,000 miles away.

We left Hawaii at the end of November, 1956, and I was discharged from active duty in December—having reached the rank of Lieutenant, Junior Grade, or Lt, jg. Hawaii was indeed a fine place for our initial married years. Thirteen years later, in 1967, we were back in Hawaii for most of the summer while I was lecturing at the University of Hawaii.

Chapter 5

# Research and Teaching: A Conversation With Former Students

*I guess I do have broad interests. And I haven't had enough sense to stay away from them.*

*[This chapter includes a conversation with Whitman, Mishac Yegian, and Ricardo Dobry. Yegian and Dobry are former doctoral students of Whitman and are now professors in their own right.]*[27]

**Reitherman:** When you returned to MIT after your service in the Navy, what did you teach?

**Whitman:** I rejoined the faculty at MIT at the beginning of February 1957 and resumed research into soil dynamics as applied to large explosion effects.

The first course I taught was at the graduate level and dealt with slope stability. I handled the theory, but the practical aspects of the problems, and especially the

27. Mishac Yegian is a professor at Northeastern University, and Ricardo Dobry is a professor at Rensselaer Polytechnic Institute.

selection of soil strength parameters, was handled by Laurits Bjerrum, then visiting from the Norwegian Technical Institute, or NGI by its Norwegian acronym. That was an education in geotechnical engineering right then and there, just sharing an office with Bjerrum for six months.

**Reitherman:** Was he the connection for your later stay at the Norwegian Technical Institute in 1984?

**Whitman:** The geotechnical group at MIT and the counterparts in Oslo had a long-standing relationship by then, which dated back to this first visit by Bjerrum in 1957.

**Reitherman:** Let me understand what "slope stability" precisely meant then. Was "stability" confined to the gravity-loading case?

**Whitman:** Yes, we weren't dealing with earthquakes yet. Guarding against failure of natural or man-made slopes is a classic problem in geotechnical engineering and predates the development of geotechnical study of slope stability in earthquakes.

For some years thereafter, I continued to teach a graduate advanced-level subject that covered slope stability as well as foundation stability and stability of retaining walls, and soon developed a new subject dealing with soil dynamics. Eventually, my teaching load was typically an undergraduate subject in the fall and soil dynamics in the spring—and the latter evolved to include a heavy dose of earthquake-related material.

## Sliding Block Analysis Method

**Reitherman:** Did Newmark's sliding block analysis method come later?

**Whitman:** Yes, Newmark's sliding block method came later. However, I had an early acquaintance with it. [Laughter from Dobry and Yegian.]

**Yegian:** Bob is being very humble. That is his humble way. But you need to hear the full story. Explain what happened, Bob.

**Whitman:** It was 1953. Don Taylor was on a consulting project with Nathan Newmark, Arthur Casagrande, and likely some others, having to do with what would happen to the soils along the Panama Canal if it were subject to nuclear attack. Taylor asked me about it, and I responded with an analytical scheme to evaluate how far a rigid block of soil would slide down a slope if it were subjected to transient ground motions large enough to cause shear stresses that momentarily exceeded the strength of the soil. Employing numerical integration performed by hand calculations, I evaluated the net relative displacement resulting from six cycles of applied ground motion. I wrote Taylor a memo that he took to the meeting, which he later incorporated into a report, attributing the approach to me.[28]

Taylor came back to MIT and reported to me concerning the meeting. Newmark read my memo on the Panama Canal slope stability issue, in which I described this new sliding block analysis idea. Newmark said he was impressed and that if I did not pursue it, he

28. Letters from Donald W. Taylor to U.S. Army Engineers South Pacific Division, dated 14 April 1953 and 20 May 1953.

would. I was busy with other things. Newmark did go ahead and develop that method and apply it to earthquake problems a decade later.[29] He worked out the significant combinations of parameters, computed results using a number of different earthquake ground motions, and assembled the results in a convenient chart.

**Reitherman:** This sliding block theory seems to still have significant currency, even after more than forty years.

**Whitman:** Conceptually, it is still a very valuable tool, though its use for predicting permanent displacements has been somewhat superseded by the availability of finite element computer analyses.

**Dobry:** May I kindly disagree with you, professor? My impression is that top geotechnical engineers like to use simplified methods that give them easily understandable results, so they can quickly get a handle on what is going on. That is a smart way to approach a problem, rather than first jumping into a complicated computer analysis that requires the input of many parameters. I think a lot of practicing geotechnical engineers still use the sliding block method as one of their basic tools.

**Yegian:** With retaining walls, it is still the same basic method, although some computer programs handle the integration, so it is a modification of the original method. In my practice, even when I use a sophisticated computer model, I compare the results with a simplified sliding block analysis.

**Whitman:** I'm glad to hear that, and now I know I have to direct questions about the latest state of practice to these two young gentlemen.

**Reitherman:** You are also on notice that these two former students, and now colleagues, are not going to let you get off the hook being too humble. That account of the origins of the sliding block analysis method would not have come to light just by waiting for you to bring it up.

**Whitman:** This isn't the first time this question has arisen. Bill Marcuson brought up my early sliding block analysis in his paper in the 1994 symposium in my honor.[30] He included several figures and quotations from my memo. One of the sentences reflects Taylor's keen insight—he suggests that progressive loss of strength as a result of cyclic shaking may be more important than successive slips at constant shear strength.

29. Nathan Newmark, "Effects of earthquakes on dams and embankments," *Geotechnique*. Vol. 15, p. 139-160, 1965.

30. William Marcuson, "An Example of Professional Modesty," *The Earth, Engineers, and Education: A Symposium in Honor of Robert V. Whitman*, October 7 and 8, 1994, Department of Civil Engineering, Massachusetts Institute of Technology, Cambridge, Massachussetts.

## Nathan Newmark

**Reitherman:** What was Nathan Newmark[31] like?

**Whitman:** I always admired and got along extremely well with him. He was very good to me in my career. We talked earlier about Terzaghi and Casagrande being what you might call crusty, but I never found Newmark that way. He was always forthright and clear about his positions, but he was very good at listening to other people's viewpoints.

**Reitherman:** I note a parallel between Newmark and you, Bob. Your research dealing with soil particles, structural elements like a steel frame, and figuring out how to integrate risk analysis into forecasting what will happen when an earthquake strikes a large city seems to be an extremely diverse portfolio, and reminiscent of Newmark's breadth. You explain your broad career as the response to nudges provided by opportunities and outside forces—Norris wanted you to specialize in structural dynamics, Taylor wanted you to help him with geotechnical research, and so on. But you had to have genuinely broad interests and capabilities to be able to pack so much into one career.

**Whitman:** I guess I do have broad interests. And I haven't had enough sense to stay away from them. But I don't compare myself with Newmark. If my career has resembled his even slightly, that would be a big compliment.

## Teaching at MIT

**Reitherman:** What was it about teaching that you liked?

**Whitman:** I always enjoyed the challenge of getting concepts across to students, and the interplay between student and teacher. I suppose it was in my family blood. As we discussed earlier, my mother and father were teachers and enjoyed teaching.

**Reitherman:** Did you use techniques in your teaching that you had observed when you were a student?

**Whitman:** Until very recently, the typical new university professor, an assistant professor, simply started teaching, without any tutoring in how to teach. So I must have been influenced by observations while I was a student. I always strove to emphasize concepts. I hoped students would ask questions, so I tried to allow time for them. I tried to be clear on the blackboard.

31. Nathan Newmark (1910-1981), civil engineering professor at University of Illinois at Urbana-Champaign. Newmark researched, taught, and consulted on a wide array of earthquake engineering and other engineering topics, including inelastic response spectra design methods, analysis of tall buildings (such as the consulting role he played on the tallest seismically designed building in the world at the time, the 44-story 1956 Latino Americana Tower in Mexico City), the mechanics of landslides and how to compute slope stability under earthquake excitation and other geotechnical engineering problems, advanced analysis methods for nuclear power plants, and reinforced concrete seismic design. He also chaired his university's Digital Computer Laboratory for a decade beginning in 1947. The ASCE Newmark Medal is devoted to both structural engineering and geotechnical engineering accomplishments with regard to mechanics, following Newmark's involvement in both fields.

Later I experimented with introducing small design projects in undergraduate subjects. This is tough to do in civil engineering; projects must be artificial to some degree to simplify them. I did this first in a sophomore-level subject combining statics and strength of materials. The projects would involve designing the main load-carrying system for a bridge. Toward the end of my time on the faculty, there was a move to introduce design throughout the curriculum, and I developed a series of design exercises for the geotechnical subject.

**Reitherman:** Ricardo and Mishac, when did you first meet Bob Whitman?

**Yegian:** I went to MIT to work on a doctorate in 1972. Before that I worked on my master's in Austin at the University of Texas. There was a lot of research on piles going on in Texas, related to offshore structures. I was interested in going to MIT to do my PhD. My professors at UT-Austin were not happy about my leaving. But when they finally knew I was going to leave, they told me that there was one person I should work with, and that was Professor Whitman. I had just been accepted, and I wrote a letter to Bob asking if I could work under him. A few weeks later I drove my U-Haul truck to Massachusetts and showed up. But Bob was nowhere to be seen. He was in Europe at the time. Professor Lambe called a big meeting of all the graduate students and faculty to assign students their advisors, and I was assigned to him [Lambe]. At that point, I was, let's say, a concerned kid, since I had my heart set on working with Professor Whitman. Two weeks later, when Bob returned from his trip, he read my letter and came to talk to me. He asked, "Do you still want to work with me?" I enthusiastically said "Yes," he said "Done. You're working with me." By that time, I was assigned to Professor Lambe, also a famous professor, and I was concerned, about the switch, but Bob said not to worry about that, he would talk to Lambe. They moved me from one office to another and that was that.

The most valuable talent that Bob had as an educator was that he did not just try to give you book knowledge. He would talk about the subject matter and excite us. As we were starting out, after he left the classroom I would say, "I don't know much about this subject of liquefaction yet, but it sounds very interesting." Bob got me interested, and we co-authored a paper later that was one of the first to describe how to do a probabilistic evaluation of liquefaction risk. That's how I got into earthquake engineering, from being excited about it by Bob.

When I had Bob as a professor, he was using photographic slides extensively, which was a relatively innovative technique in teaching civil engineering then, and it was exciting to see actual earthquake pictures. I recall once I somehow inserted a slide incorrectly and it jammed and melted. The projector was stuck. Bob had to pull out the slides one by one, hold them up and squint at them, then describe what he would have showed us. Fortunately, I made that mistake with the projector only once.

**Dobry:** Bob is a superb communicator, not only in teaching but in workshops, seminars, presentations, papers. I have never seen anybody who can present so much relevant information in his teaching as Bob Whitman. We were given the theory, the empirical relationships, the data, the case history. He was able to do that because he could explain the kernel of

**Table 1:** *Doctoral Candidates Supervised by Robert Whitman*

| | | |
|---|---|---|
| Al Homoud, Azm | Iglesia, Geraldo | Protonotarios, John |
| Bouckovalas, George | Lambe, Philip | Richardson, Archie |
| Christian, John | Lin, Jeen-Shang | Sarrazin, Mauricio |
| Dobry, Ricardo | Luscher, Ulrich | Solteriades, Michael |
| Goulois, Alain | Moore, Peter | Stamatopoulos, Constantine |
| Healy, Kent | Nadim, Farrokh | Ting, Nai-Hsin |
| Hoeg, Kaare | Paikowsky, Samuel | Yegian, Mishac |

each idea quickly and then relate it to the other elements of the problem. He could quickly use a simplified method and get across enough knowledge to the students so they could see the overall problem. You understood that reality is so rich, that the problems are so complex, that no single theory adequately explains it all.

I met Bob in 1965 at the Sixth International Conference on Soil Mechanics and Foundation Engineering in Montreal, Canada. I came from Chile, where I had done my undergraduate study in civil engineering at the University of Chile in Santiago. When I met Bob, I also had finished my master's degree in geotechnical engineering from the National University of Mexico. My Chilean colleagues and I met with Bob—I don't recall exactly where—though I seem to remember something about a long discussion late at night in a bar. Perhaps Bob was spending time with us because of an MIT program relating to Latin America that was then underway.

**Whitman:** Earlier, in 1963, MIT had started to develop an international program with Latin American nations, the Inter-American Program in Civil Engineering. It was an outgrowth of the relationship Charles Miller had with Puerto Rico, and when Charlie came in as head of the civil engineering department at MIT, he thought that good experience could be extended.

**Dobry:** This conference in Montreal was only five years after the huge 1960 Chile earthquake, which had many soil effects. Eventually, after the conference, Bob came to Chile to see the effects of the earthquake firsthand.

**Whitman:** That was the first earthquake I visited, albeit after the fact, to examine the land where ground failures had occurred. Then I had field experiences after the 1967 Caracas and 1971 San Fernando quakes. Of course, there were many other earthquakes that I studied "from a distance," whether geotechnical or related to loss estimation.

**Dobry:** He invited me to come to MIT to be his doctoral student, and I was flattered when I received his letter. I came at the beginning of 1968. I was a graduate student from 1968 to 1971. At that time, I don't know if I had even heard the word "centrifuge," and I certainly was not aware of centrifuge testing in the geotechnical field. My doctoral work was analytical at MIT, though I also did some experimental research with a shake table. Experimentation

using a centrifuge only came later in my career, as it did in Bob's.

**Whitman:** While Martin Duke had described apparent soil amplification effects after visiting Chile following the 1960 earthquake, it was Harry Seed[32] who became the first champion of this challenge. He took up the cause in the mid 1960s. As you suggest, it was a very controversial matter—for a time pitting engineers from the San Francisco Bay Area against those from southern California.

**Reitherman:** What was Harry Seed like?

**Whitman:** Harry was a very good friend. Our relations with each other were nothing but cordial, supportive, and collaborative. I have seen him in a situation where someone would seriously challenge him, and then Harry could be very forceful in defending his position. He was persistent, thorough in his research, aimed to develop tools useful to the engineer, and persuasive. He had a succession of excellent students, Ed Idriss and others too numerous to mention, many of whom are now themselves very successful researchers and engineers. Seed can certainly be called the father of what today is often called geotechnical earthquake engineering.

One early important contribution was a computer program called SHAKE.[33] It was developed to study the effect of local soil conditions on earthquake ground motions, which made it possible to consider the important influence of the strength of the ground shaking. Around the time of the Caracas earthquake in 1967 and soon after the 1970 Gediz earthquake in Turkey, Harry Seed was striving mightily to find cases to support his theories and contentions about the effects of local soil conditions on earthquake ground motions. We had somewhat different approaches at MIT, and typically we compared notes to reach some improved level of understanding.

**Reitherman:** What got Seed interested in the earthquake problems of soils?

**Whitman:** When he first got to Berkeley in 1950, Harry worked in a large research program underway there concerning road paving. He did repeated-loading soil tests to develop better methods for analyzing the soil under pavement. When the 1964 earthquakes occurred in Alaska and Niigata, it was natural for him to adapt his testing techniques to the earthquake problems of soils, problems that were so dramatically demonstrated in these earthquakes. He probably was involved with seismic studies before then, but 1964 is when I observed the transition in his career. It was

32. Harry Bolton Seed (1922-1989), like Whitman, received his PhD in structural engineering (University of London, 1947) rather than geotechnical engineering. Seed then studied soil mechanics at Harvard. He joined the University of California at Berkeley faculty in 1950 and was active in teaching and research there until his death. From the mid 1960s onward, he specialized in what today is called geotechnical earthquake engineering, receiving many professional and academic honors and authoring approximately 300 reports and papers.

33. Per B. Schnabel, John Lysmer, and H. Bolton Seed, *SHAKE: A Computer Program for Earthquake Response Analysis of Horizontally Layered Sites.* College of Engineering, University of California Berkeley, 1972.

then that he pioneered the first systematic study of earthquake-induced liquefaction.

## Liquefaction

**Dobry:** Mishac gave a seminar at Woodward-Clyde. Ed Idriss, who was my boss, said after the seminar that we should recruit him after he graduated.

**Yegian:** I was a young, talkative graduate student, explaining our approach and pointing out some weaknesses in the methods used by Harry Seed, all the while not knowing that Professor Seed was seated there in the room.

I had never met him and didn't recognize him. Had I known he was in the room, my lecture would have been different. Afterward, Ed Idriss came up to me and said, "You were discussing the work of Harry Seed. That's Seed, sitting there…and he would like to see you." Boy, was I scared. But Neville Donovan, who was also in the room, wrote a note to Bob, which he passed on to me and I have kept it all these years, saying they all enjoyed the seminar. Bob was the one who funded my trip to California, and it was big favor to me. On the same trip I met with Bill Moore, and he told me that he was the first one who did a back-of-envelope analysis of liquefaction in 1964 after Niigata, but I don't know the details about that.

Speaking of liquefaction, there is a very influential plot that Bob developed[34] that relates strength of soil to the strength of the ground shaking, plotting the likelihood of liquefaction. At least twenty people over the last twenty years have taken that concept and worked on it, one of whom was Professor Seed.

**Whitman:** This graph puts on the vertical axis a measure of the strength of the ground shaking. On the horizontal axis is some measure of the resistance of the soil to liquefaction, originally a density inferred from blow counts, or blow counts directly. I'm referring here to the Standard Penetration Test[35] or SPT blow count, the number of standard hammer blows it takes to drive the cylindrical sampler a foot down into the soil. One plotted a point that represented an actual case where liquefaction occurred, or did not occur. A curve was drawn to separate points representing liquefaction from those corresponding to non-liquefaction. It was a framework where you could combine field and laboratory data.

34. Robert V. Whitman, "Resistance of Soil to Liquefaction and Settlement," *Soils and Foundations*. Japanese Society of Soil Mechanics and Foundation Engineering, Vol. 11, no. 4, pp. 59-68, 1971.

35. In 1947, Karl Terzaghi and Harry Mohr developed a relationship between the allowable bearing pressure for foundation design and the blow count driving a particular kind of cylindrical soil sampler (Gow sampler) into the ground. Mohr, who authored *Exploration of Soil Conditions and Sampling Operations* in 1940, a Harvard graduate school of engineering publication (Bulletin 269), had worked for the construction firm of Charles Gow in Boston and refined the design and use of the Gow sampler. Terzaghi's goal was to have soils engineers adopt a single standardized method so that their data would be comparable. The next year, this standard test, the Standard Penetration Test, was presented in the book by Terzaghi and Peck, *Soil Mechanics in Engineering Practice*, and the method then slowly spread from the Boston area to the rest of the United States and the world.

The idea is to evaluate the expected measure of ground shaking and the measure of the site's resistance to liquefaction, and plot that point. If it falls above the separating curve, liquefaction should not occur.

**Dobry:** The shape of that curve has remained amazingly constant over the years, as many more case studies have been accumulated. In Bob's first plot, he was also going in the direction of relying on field data, rather than laboratory tests. Since then, geotechnical engineering has moved more and more to techniques to assess the soil in the field, so that has also remained a surprisingly robust approach and has stood the test of time.

**Whitman:** I don't know who deserves most credit for originating this approach. The end of the 1960s was a time of great turmoil concerning the analysis of liquefaction. Harry Seed proposed theoretical approaches that involved laboratory testing and various correction factors, which probably even Harry was not very sure about.

At the time, I was doing a lot of consulting for Stone and Webster on the design of nuclear power plants and liquefaction issues. William (Bill) Swiger came back from a meeting and said it would be helpful if I could put together some information relating case histories of liquefaction to the blow count at the site, to put in a consulting report. I submitted a paper—which Mishac has just mentioned—with my figure for a conference in Japan, but for one reason or another it was not published until two years later. My plot was a very early version based on very little data. A lot of people were thinking along the same lines as I at the time. I believe Alfred (Skip) Hendron, from the University of Illinois, deserves at least some of the credit for the idea of relying on case studies in place of laboratory tests. Harry was the one to develop the approach most fully and make it the most commonly accepted way to evaluate liquefaction potential. He deserves full credit for the successful development of this approach.

**Reitherman:** When was the word "liquefaction" first used?

**Whitman:** In 1938, there was a huge failure during construction of Fort Peck Dam, an enormous earthen structure. This was in the absence of any earthquake. The slide involved "only" something like 8 percent of the total volume, but by any measure it was a huge ground failure. It appeared that there had been an abrupt loss of strength. That was when the term liquefaction, or spontaneous liquefaction, came into the language of soil mechanics.

**Reitherman:** Has the understanding of how pore water pressure and soil particles interact changed much since the 1940s?

**Whitman:** The basic understanding of interaction between mineral skeleton and pore water has remained the same. The concept at that time was that there was a critical void ratio, below which the mineral skeleton, when sheared, would expand, and if it expanded it would accept more of the total stress and the pore pressure would drop. If the actual void ratio were greater than that critical void ratio, the mineral skeleton would tend to contract when sheared and the pore pressure would increase, which would reduce shear strength.

That was all established theory when I got into the field. There had been considerable static testing at both Harvard and MIT during the

1940s, which had elucidated important new fundamental understanding. The trouble is, there is no such thing as *a* critical void ratio—it depends on many factors. In addition, as the term "liquefaction" came to be applied to loss-of-strength happening during earthquakes, there arose the question of just what we mean by "liquefaction." Casagrande would never admit that a sand boil was liquefaction, because that didn't involve any steady-state shear distortion.

Several controversies developed between Casagrande and his adherents and Seed and his adherents. One was whether simply developing excessive deformations as a result of earthquake shaking was liquefaction, or whether it was only liquefaction if you had some massive displacement with very low stress that developed along some sliding surface. Another disagreement between the two schools of thought was that Casagrande wanted to rely on laboratory tests, while Seed had begun to use in situ blow counts from field studies.

And, by the way, there was another controversy in the field, though a minor one: How do you spell "liquefaction"? With an i or an e? The consensus has gone toward the e, "liquefaction," though the Japanese often spell it with i, "liquifaction"—and after all, it does come from the word liquid.

**Reitherman:** In Charles Richter's *Elementary Seismology*, published in 1958, the word "liquefaction" does not appear. Richter discusses "earthquake fountains," and shows photographs of sand craters and ejected sand—today, obvious signs of what has become known as liquefaction. Richter theorized that the cause might be due to the shaking of underground aquifers, the opening up of passages in aquifers that allowed water to flow to the surface, or compaction of a stratum of soil that caused pressurized water to spurt—much like a squished sponge—which harkened back to John Milne in the 1880s.

**Whitman:** Harry Seed had one of his students do a survey of the literature after the 1964 earthquakes. The point was to document past cases of liquefaction, even though instances of it hadn't been recognized as such at the time. There had actually been quite a lot of documented sand boils, the temporary fluid consistency of the ground, lateral spreading, and so on, but liquefaction had not yet taken off as a field of study, so people didn't recognize it. George Housner published a paper on sand boils before the 1964 earthquakes,[36] which was an isolated research advance that didn't initially take hold and didn't lead others to do research on that topic. After 1964, liquefaction was observed and noted in earthquake reconnaissance studies and became a major geotechnical earthquake engineering subject.

**Reitherman:** In your discussion of liquefaction in your 2007 Seed Lecture, you mentioned some other people besides Seed who were influential in the two decades after 1964 in studying liquefaction, in particular the key role of cyclic strain in development of excess pore pressure, and numerical predictive methods: Ricardo Dobry, Les Youd, and Liam Finn.

**Whitman:** Les Youd worked for many years for the U.S. Geological Survey in Menlo Park, then became a professor at Brigham Young University. He developed some of the early

36. George W. Housner, "The Mechanisms of Sandblows," *Bulletin of the Seismological Society of America*. Vol. 48, April 1958, p. 155-161.

maps of liquefaction susceptibility, which are important for implementing risk reduction efforts and guiding geotechnical investigations for specific projects. And he ran some laboratory tests showing the densification of sand under cyclic shear strain. He introduced important concepts and had some very nice plots of those results.

Liam Finn has been a professor at the University of British Columbia. He has done a lot of work with the people at the Waterways Experiment Station in Vicksburg, Mississippi. He has written finite element codes to account for the nonlinear behavior of sands and pore pressure build-up, which have been used in the analysis of earth dams. He also became involved with centrifuge model testing. Liam has spent some time living and working in Japan, learning about and participating in the research there.

**Dobry:** In the 1970s, Bill Marcuson tried to get some consensus on the precise definition of liquefaction, but I don't think he succeeded.

When you look back at early people in the field, you have to mention Martin Duke.[37] He did work after the 1960 Chile earthquake and wrote several papers about how the soil conditions in Puerto Montt, Valdivia, and Concepción affected the damage. He looked at what today would clearly be called a liquefaction failure at Puerto Montt.

**Reitherman:** What about the work by Gonzalo Castro in his doctoral work on liquefaction under Casagrande? He says in the preface to his thesis that "the lectures on Soil Dynamics by Professor Robert V. Whitman at MIT provided an important background for this investigation."[38]

**Whitman:** He did a lot of good work on the conditions under which one could get steady flow at constant shear stress.

**Reitherman:** And he did all his work in the laboratory?

**Whitman:** Gonzalo became very much one of the leaders of the school of thought that the way to evaluate this critical state condition on a practical job was to do undisturbed sampling and laboratory testing, as opposed to relying on what could be considered the crude in situ technique of penetration resistance. The trouble with the approach of using undisturbed soil sampling is that there is no such thing as a truly undisturbed soil sample. Gonzalo developed a number of procedures to correct for the inevitable disturbance. Steve Poulos, on the faculty at Harvard, was another important contributor to this work.

37. C. Martin Duke (1917-1988), civil engineering professor at the University of California at Los Angeles, was an early researcher, consultant, and educator in the earthquake engineering field, beginning in the 1950s. While he was president of EERI, the 1971 San Fernando earthquake occurred, and Duke was the prime force behind the effort to better document and deal with the special topics of what became known as the field of lifeline earthquake engineering—transportation systems, water systems, and power and other utilities. Along with LeRoy Crandall, Duke was largely responsible for the formation of the still-active ASCE Technical Council on Lifeline Earthquake Engineering (TCLEE).

38. Gonzalo Castro, *Liquefaction of Sands.* PhD thesis, Division of Engineering and Applied Physics, Harvard University, January 1969.

**Reitherman:** You led the National Research Council Committee on Earthquake Engineering panel that wrote the report on liquefaction.[39] Usually, when such a panel is convened, it is to resolve some controversy, or produce a consensus on what the state-of-the-art is in a particular field. Was part of the purpose of that 1985 panel report to resolve some of the disagreements you've discussed? Or was it just time for a state-of-the-art report in that particular field?

**Whitman:** A little bit of both. There certainly was still a lot of controversy in the air. There was the Casagrande-Poulos-Castro school, and Seed and his adherents on the other side. There was the concern over excessive deformation and resulting settlement of foundations, and there was the slope failure problem, with steady state flow taking place.

The method for producing that report was as follows. A very complete draft was prepared before the panel convened. I wrote it with the assistance of my graduate student, Sam Liao. It set forth all the viewpoints and summarized the state of the art from those different viewpoints. When we met, I told the panel, if you don't agree with something in here, you've got to write a better version.

A "summit meeting" was held one day involving Seed, Castro, Andrew Schofield, and me. One of the key things that happened was that Harry Seed came prepared to say that you could correlate blow count data to the critical state sliding resistance. It was a new idea. It identified two parts of the problem. One is where you identify a site where you might be developing excess pore pressures, and the other problem was to decide what the potential consequences are. Prior to that, the blow count approach had been aimed at simply identifying sites that had the potential for having problems, whereas Harry was now also proposing to use the blow count to define the resulting problem—which had been the main emphasis of the Casagrande-Poulos-Castro approach. The report got rewritten to reflect these thoughts. I believe this report marked a turning point in the history of what we call "liquefaction analysis."

Ricardo, as another member of that National Research Council panel, is that how it happened?

**Dobry:** Yes, that's right. And in addition there was another story, one that will link to the topic of centrifuge research that Bob has done. Andrew Schofield of the University of Cambridge was there. And though Schofield was agreeing more with Seed than Castro, he was also saying that they needed to use centrifuge modeling to simulate the actual field conditions.

**Whitman:** Schofield is now retired. He was very much an originator of the centrifuge as a tool in geotechnical engineering. A remarkable person. You walk into his office and ask him a question, and after a while you think he's not saying anything relevant to your question at all, and then you finally realize he has been answering the question beyond what you asked. On the occasion of that panel meeting, he introduced us to another and more ancient use of the word liquefaction, from an old Renais-

39. Committee on Earthquake Engineering, *Liquefaction of Soils During Earthquakes.* National Research Council, 1985.

sance love poem describing the flowing nature of a lady's gown.[40]

## *Soil Mechanics* by Lambe and Whitman

**Reitherman:** How did you and Bill Lambe write your textbook on soil mechanics?[41]

**Whitman:** The textbook gradually grew out of the teaching of the soil mechanics group at MIT. We had it in note form several years prior its final publication. At the end of the 1950s, Taylor's textbook, *Fundamentals of Soil Mechanics*, was our primary text for our graduate subjects—supplemented by other elementary texts at the undergraduate level and Terzaghi's books for graduate students. Taylor's book was a pioneering text, in that he focused on fundamental considerations, but that text was rapidly becoming out of date.

Bill Lambe had written a successful book about testing of soils,[42] and publishers were after him for a more general book and invited me to join him. By this time, I was teaching the undergraduate subject, and was interested in a book that we could use at that level as well as for graduate subjects. Then there was the dean of engineering encouraging the faculty to write textbooks, with Ford Foundation funding used to give the faculty some relief from teaching to do that work.

**Dobry:** I was a graduate student at MIT at that time. As soon as the Lambe and Whitman book appeared in the bookstore, several of us graduate students bought it. Both Bob and Bill Lambe were disappointed, because they were ready to give us autographed copies. So I have two copies of the book: the one I bought, and the one I was given. It's still a useful classic.

**Yegian:** Ricardo Dobry said that he had two copies of the Lambe and Whitman textbook. Well, I have three. One doesn't have the cover and is marked up with notes and was used so much it fell apart. The cover of another wore off. You can tell how much I have used Bob's book; I'm wearing out my third copy now.

**Whitman:** Of all the things I've done, I'm probably proudest of that book. Lambe and I were recently accorded the lofty status of "heroes" of the American Society of Civil Engineers Geo-Institute, with the award primarily citing that book. It's an informal award, in the shape of a plastic desk ornament.

The book is organized into three basic sections. First came dry soil, then wet soil with pore pressures established by external conditions, and then wet soil with transient flow.

About the time the book had been published, I was over in England, when Victor de Mello

40. Robert Herrick (1591-1674) wrote the poem "Upon Julia's Clothes" (*English Verse*, Oxford University Press, 1940):

    When as in silks my Julia goes,
    Then, methinks, how sweetly flows
    The liquefaction of her clothes!
    Next, when I cast mine eyes and see
    That brave vibration each way free,
    O how that glittering taketh me!

41. T. William Lambe and Robert V. Whitman, *Soil Mechanics.* John Wiley, New York, 1969.

42. T. William Lambe, *Soil Testing for Engineers.* John Wiley, New York, 1951.

gave the Rankine Lecture,[43] and I was seated next to my good friend Peter Rowe (Professor at University of Manchester) at a table for dinner. Peter proceeded to tell me he was rather disappointed in the Lambe and Whitman book, because there wasn't anything new in it. I was taken aback by his comment, but then I thought more about the context in which it was made. Some professors intend to present a grand new concept in their books, but we tried to make a book that would be very useful for teaching. There wasn't necessarily anything startlingly new in our book; no radical new theories or innovations. What we really set about to provide was a good, clear, interpretation of what we thought was the best known science as of that time. We didn't just use our own data; we used good data from wherever we could find it.

**Reitherman:** When did you know that the book would be a success, that it would have a lot of readers? Did it take off right away?

**Whitman:** It took off fairly quickly. As of today, over 80,000 copies have been sold, including the original Lambe and Whitman edition, the SI metric units version, and a soft cover edition for foreign sale. There have been translations into other languages: Spanish, Portuguese, Chinese, Russian, German, Polish, maybe some others. In addition there are the usual knockoffs.

43. The Rankine Lecture, annually awarded by the British Geotechnical Association since 1961, is named after William Rankine (1820-1872), Scottish engineer and physicist who investigated many topics, including research in what was to become soil mechanics.

**Dobry:** I am in awe of the book. When I first read the book, I had already gone through a thorough master's program in Mexico and used soil mechanics textbooks. But in this subject area, most of the books are not organized around central concepts. In that respect, the Lambe and Whitman book was revolutionary. This book had a conceptual continuum. Readers think their way through the book.

**Yegian:** Most technical books have a life of no more than four or five years, unless they are significantly updated in a second or third edition. Why would a book, so many years after its publication, still be alive and well? And I am very happy to have just heard that number—80,000. I recall that Ricardo wrote a letter to Bob one time in which he compared the book to classical music that never gets obsolete. Why would this book have such a following unless it was a classic? The fundamentals are written so well, in such an organized way, that it is valid today. There is always a practical twist to the examples. I had my undergraduate and master's soil mechanics at the University of Texas at Austin, but I didn't really learn soil mechanics. I learned a lot there, but I didn't learn soil mechanics. I learned two things at MIT with Bob: soil mechanics and earthquake engineering.

I recently told the chair of my civil engineering department at Northeastern University that I wanted to teach the graduate soil mechanics course and give up teaching my usual soil dynamics course this year. I looked at eleven soil mechanics or geotechnical engineering textbooks. Over this past summer, I spread them out in my home office. Long story short, I am giving my students a set of class notes, which are 75 percent credited to and extracted from

Lambe and Whitman. And I promise, Bob, next time I teach the course I will also assign the whole book so the sales will reach 80,020.

Just this past week my students have been working on an example from your book that you will recall, Bob. There is a concrete dam with three alternative cases: no sheet piling, sheet piling upstream, and sheet piling downstream. Then you compare them and discuss their merits under different conditions to show how the engineer should approach making a wise design decision. It's a fascinating example from a practical and theoretical standpoint. The Lambe and Whitman book will out-survive all the other geotechnical textbooks, and we know that many have come and gone since it was published.

I have a comment to make, though I have no firsthand facts to back this up. Knowing the book and Bob so well, I think Bob wrote more than half of it and should be given most of the credit for it.

**Whitman:** Well, perhaps I wrote somewhat more than fifty percent, and developed the organizational scheme, but the writing contributions by Lambe and me were not greatly imbalanced.

**Yegian:** You see what Ricardo and I mean? Bob Whitman is so humble, you never hear him talk of his accomplishments, and when you pry out of him some discussion about them, he understates them.

**Dobry:** Bob would not lecture to his students about his own consulting experience, project by project, as some instructors do. Instead, he would pull together information from a variety of sources when discussing a topic, mixing his MIT-related research or consulting data and case studies with work by others. He looked at it from the student's point of view and figured out how best to present the material.

One of the most advanced areas of research and computer program development in geotechnical engineering, especially for problems involving liquefaction, is the effective stress approach. You take the soil skeleton as a phase, you take the water as another phase, and you combine them with relationships in between. This has taken off in the last twenty years. For students who have studied from the Lambe and Whitman book, the approach is easy to understand.

**Yegian:** History is also important. A teacher should teach students the history of their field. Most geotechnical books jump right into the technical topics. The Lambe and Whitman book explains the history and even puts photographs of people like Terzaghi in the book, with a biographical sketch of him. Just recently, someone emailed around to geotechnical engineers looking for a photo of Terzaghi. Jim Mitchell immediately emailed back to say, here it is. And he sent the photo and biography from Lambe and Whitman. The book has helped document the history for the profession and the instructors.

## Risk-Based Geotechnical Engineering

**Reitherman:** Later on, Bob, you combined risk engineering with structures in your 1973 paper on damage probability matrices.[44] I was

44. Robert V. Whitman, John W. Reed, and S.-T. Hong, "Earthquake Damage Probability Matrices," *Proceedings of the Fifth World Conference on Earthquake Engineering*. 1973.

the consultant to your panel on loss estimation for the National Research Council in the mid-1980s. I first learned about damage probability matrices in detail from your paper. I learned how to convert a damage probability matrix to a set of fragility curves, and vice versa. Your 1973 paper seems to be the way this kind of probabilistic thinking was introduced to most earthquake engineers. Within a decade it took hold.

**Yegian:** Although the damage probability matrix was not originally developed for geotechnical engineering application, I found it very useful. I applied it to the task of describing the risk of experiencing various states of damage given the liquefaction severity, rather than the shaking severity.

**Dobry:** Both Mishac Yegian's and Sam Liao's doctoral theses used the damage probability matrix as applied to geotechnical engineering.

**Yegian:** Bob's concept of the damage probability matrix allows for the numerical integration of the damage state probability with the probability of the earthquake ground motion level to get the overall probability, which is what we really want to calculate. I found the matrix formulation more useful than the fragility curve for that reason. I was able to take the matrix description of the seismic hazard from Allin Cornell and combine it with the matrix describing the probability of the site falling into various damage states.

**Dobry:** When I did my PhD thesis a few years earlier, your work, Bob, was mostly deterministic. When Mishac was there you were moving toward the probabilistic approach.

**Whitman:** The year I had a sabbatical, 1963-1964, and spent it at the Stanford Research Institute, I went to Stanford University nearby to take a course on stochastic analysis. After the first four lectures, the graduate student teaching the course convinced me I would never understand stochastic analysis—from him. But the next year, Allin Cornell[45] came to MIT as an assistant professor. Allin has just retired this year [2006] from the Stanford faculty, where he was for many years after he left MIT. I began to take opportunities to learn probability from him when he was at MIT. Following the 1971 San Fernando earthquake, Allin suggested that this was a chance to collect damage information and work out a technique to estimate risk to buildings. That started us on the Seismic Design Decision Analysis (SDDA) research program at MIT.

**Reitherman:** Can you tell us a little about how you came to pick your topic for your 1981 ASCE Terzaghi Lecture, on "Evaluating Calculated Risk in Geotechnical Engineering"? That's another influence you have had—moving risk-based concepts into geotechnical engineering.

**Whitman:** By the time I was invited to present this lecture, I had received a good exposure to basic probability, and was interested in potential applications within geotechnical

45. C. Allin Cornell died December 14, 2007. Born in 1938, he received his undergraduate degree (architecture) and master's and PhD degrees (civil engineering) from Stanford University. He was on the MIT faculty from 1966 to 1983. He then joined the Stanford faculty. His career spanned the disciplines of engineering and earth science.

engineering. By no means, however, was I the first geotechnical engineer to do work along these lines. Arthur Casagrande had presented an early Terzaghi Lecture in 1964 with the title "The Role of the 'Calculated Risk' in Earthwork and Foundation Engineering." To oversimplify greatly, Casagrande meant risk that is very carefully thought about, with potential dangers and benefits balanced in the engineer's mind. The analysis was more qualitative than quantitative. I decided it was time to inject quantitative thinking into the discussion.

**Yegian:** In my mind, the damage probability matrix is the origin of the current probabilistic performance-based seismic design idea. It is the same idea. You quantify shaking relative to different performance levels, and you do so in a risk-based formulation.

**Whitman:** The Mercalli Intensity Scale, and even more so some of the other scales like the Medvedev-Sponheuer-Karnik (MSK), long had some of that performance-based thinking built in. The intensity scales make statements like "few" chimneys or "most" chimneys fall down, for example, and that idea of a distribution is key to what currently goes under the heading of performance-based seismic design.

**Dobry:** The way the geotechnical failures were described by the intensity scales did not help lead to our modern field, because those effects were typically all assumed to be indicators of very severe ground motion. Without understanding how susceptible a soil might be to liquefaction and how even moderate shaking might make it liquefy and develop ground deformation, such ground failures biased the intensity rating toward the high end of the scale. If railroad rails were bent because the underlying bed was distorted by liquefaction or settlement or landsliding underneath, it was taken as a sign of tremendous shaking.

**Reitherman:** The intensity scale Fusakichi Omori developed in 1900 was very forward-looking, trying to correlate observed effects with ground accelerations. He conducted shake table tests, the world's first, with John Milne at what was later to be part of the University of Tokyo, to try to find out what acceleration would make objects tip over. That intensity approach didn't take hold in the U.S., nor did the idea of quantifying percentages of instances where an indicator applied, which is built into the MSK Intensity Scale. The MSK scale makes verbal statements about damage to "a few" buildings of some construction type, but in addition has a defined quantitative definition. "Few" is listed as meaning less than 10 percent, for example, with "many" meaning 20 percent to 50 percent and "most" over 60 percent. I've never understood why some of these advances quantifying ground motions weren't adopted in the U.S.

**Whitman:** I don't know why a number of developments from abroad didn't catch on here.

## Workshops That Allowed Seismic Codes to Progress

**Whitman:** There were a few key geotechnical issues in the 1980s that were resolved through some influential workshops in such a way that building code seismic provisions could progress. There were several NCEER (National Center for Earthquake Engineering Research), workshops, with NSF and USGS sponsorship.

The first was held in Buffalo, the second at MIT. Then there was the third one, headed up by Geoffrey Martin, held at the University of Southern California. Do you recall the topics of each, Ricardo? You had a lot to do with them.

**Dobry:** The first question was what reference ground conditions to use for the national map that USGS was going to produce, which would depict the seismic hazard in terms of strong ground motion. There were advocates of different reference points—hard rock, soft rock, firm soil.

**Whitman:** Californians wanted to consider "rock" any material that had a shear wave velocity of at least 2,000 feet per second. In the East, we thought that rock was rock, not like the soft stuff common in California. The second question was the influence of local soil conditions on the ground motion parameters.

**Dobry:** Bob then urged the formation of a committee to develop new recommendations for the codes on the question of the influence of local soils on ground motions. This relates back to our earlier discussion about Harry Seed, amplification factors, and so on. There were nine of us on the committee, and we built on the input of the workshop at USC. That was a key event in formulating what went into the NEHRP Provisions and the Uniform Building Code. That key workshop at USC was held in 1992. Since then, that pattern has been very influential in succeeding codes. The 1994 edition of NEHRP changed to the A-B-C-D-E-F soil classification scheme that is in the code today. For some time now, we have had these soil factors defined in terms of geotechnical properties of the upper 30 meters of soil at the site.

**Reitherman:** Many, if not most, workshops don't seem to really make historic contributions to knowledge. But you all seem to be describing a few workshops that really did have a historic impact.

**Dobry:** The 1985 liquefaction workshop and National Research Council panel report we previously discussed, which Bob led, had a big impact.[46] The 1992 ground motion workshop,[47] also very influential, shared a characteristic with this earlier one in that most of the work and writing was done prior to the workshop.

**Yegian:** These workshops we've been discussing were successful partly—largely, in my opinion—because Bob was centrally involved. He has a talent for listening, objectively evaluating, summarizing, and getting a group to come to some agreement. We don't have a lot of Bob Whitmans. I think that today soil-structure interaction is at a point where the field and the consensus is floating around. We need more Bob Whitmans.

**Dobry:** The first time I heard "Bob Whitman" pronounced, in 1963, was when I was getting my master's in Mexico City. Someone had attended the 1960 workshop in Golden, Colorado on soil mechanics[48] and related that

46. Committee on Earthquake Engineering, *Liquefaction of Soils During Earthquakes.* National Research Council, 1985.

47. Whitman, Robert V., ed., *Proceedings From the Site Effects Workshop.* National Center for Earthquake Engineering Research, Buffalo, New York, 1992.

48. *Proceedings of the ASCE Research Conference on the Shear Strength of Cohesive Soils*, 1960.

the workshop was not reaching any conclusion until Bob Whitman stood up and explained to everybody what they had been saying and what they needed to agree on, and everybody agreed. That made me interested to meet this man named Bob Whitman.

## Providing Leadership

**Yegian:** Leadership is the word. Bob hasn't mentioned it yet, but for many years, Massachusetts was the one "island" east of the Rockies where seismic regulations were included in the building code, beginning back in 1975. How come? There were many arguments for doing nothing, such as the position that we needed more research, which is always true. Bob listened to everybody, and everybody presented their data, but he moved everybody forward. He presided over the component issues—like what acceleration to pick for ground motion in the regulations. He had to work with the practicing engineers, the researchers, the code people. He was essential to the process by which Massachusetts adopted statewide seismic regulations back then.

As a fellow resident of Lexington, Massachusetts, I know how much the leadership of Bob, and his wife Betsy, has been appreciated. He has been a Town Meeting member for thirty-seven years and has chaired the Zoning Board of Appeals and the Permanent Building Committee. I was just recently watching the Town Meeting on our local cable television. There were many emotional statements about the issue. Then I saw Bob go to the microphone. I told my wife, "Okay here it comes. We're going to get a solution." And Bob told the group that if they could just answer one key question among the many being debated, they could move forward. And they did.

**Reitherman:** Bob, the local democratic procedures in New England may be among the oldest in the country, but perhaps they may be the least understood, at least outside New England. Can you please explain what a Town Meeting is?

**Whitman:** The Town Meeting is a meeting of particular people, but also refers to a group of people. Originally, there was direct democracy where all the voters were members of the Town Meeting, the town's legislative body, with an administrative body of three to five selectmen to run things from week to week. As the town grew, it transitioned to representative democracy, with elected Town Meeting members. Lexington today has nine precincts, each of which has twenty-one members. Each year, seven of those members are elected to a three-year Town Meeting term. We convene at the annual Town Meeting, for up to a dozen nightly sessions, as well as special Town Meetings. I have now served in Town Meeting for about forty years, and I recall having succeeded in seventeen elections.

An interesting sidelight: While serving as chair of the Permanent Building Committee, we oversaw the building of the new junior high school. This school was built into a steeply sloped hillside.

The only two things that went wrong with that construction project were soils problems. There was a very tall back wall for an auditorium. The wall would eventually be braced at its top when the slab for the top floor of

the building was constructed. We urged the contractor to provide temporary bracing for this wall, but that didn't happen. A rainstorm occurred that pushed over the top of the wall half a foot. We called in an outside consultant, who suggested putting in anchors to prevent further movement, with everyone knowing that soon the upper level slab would be poured and prevent any movement. This quieted fears concerning a damaged building, and of course today no one can see that the back wall of the auditorium is out of plumb.

The costs of this and another contractor blunder were fully covered by the contractor, who otherwise did a fine job.

Then I was a member of the Board of Appeals, ruling on various zoning issues.

**Reitherman:** There had to have been some passionate civil wars between neighbors.

**Whitman:** I'd like to think I came out of it with some respect for doing my best, and where judgment is called for, being clear about the reasons for reaching that judgment. And then you have to be prepared to be sued—I've probably been named in more lawsuits than any of you, although I was never at risk financially. The Town defended us members of the Board. Sometimes we lost the suit, and had to go back and reconsider the matter, given the outcome of the legal ruling.

**Yegian:** I myself came before the Board of Appeals in Lexington to ask to put a garage closer to my property line than the usual zoning rules permit. When I got the application papers, it listed various requirements, but then I saw that Bob was on the Board. I ended up doing a survey, CAD drawings, and putting other materials in a document an inch thick. I went to the meeting, and I was told that it was the most thorough application they had seen. I wanted to tell them, but didn't, that I was a student of Bob's and that I was afraid of embarrassing him by not having a first-rate package to submit. Bob recused himself and sat in the back, but I knew he was watching.

**Reitherman:** Just imagine if Mishac had been asking for a variance for the house itself, rather than just the garage. He would have felt the need to do a multi-volume report!

**Yegian:** Yes, Bob had a great effect on motivating his students. I know this oral history will include other aspects of his career, such as loss estimation, and leadership roles such as president of EERI. But I want to ask him now, what led you to do so many things?

**Whitman:** They were interesting challenges and I couldn't stay away from them.

**Dobry:** I think there is one word that does not apply to Bob Whitman: "narrow." He sees a whole subject and identifies what he needs to do to have a big impact.

**Yegian:** Besides teacher and researcher, Bob has always been a practitioner. You could see that from his lectures. He was an engineer talking about engineering. He has vast consulting experience, and that came through in his teaching. I don't think it's a luxury for the students, I think it's a must: Instructors should have practical experience in what they are talking about. I have found that essential in my teaching.

Chapter 6

# Soil-Structure Interaction

*The phrase soil-structure interaction has many connotations. Here we are referring to the effect of foundation soil flexibility on the response of structures.*

**Reitherman:** Perhaps you could start the discussion of soil-structure interaction by giving a simple definition of it.

**Whitman:** The phrase soil-structure interaction has many connotations. Here we are referring to the effect of foundation soil flexibility on the response of structures. This became a hot topic in the mid- to late 1960s in connection with seismic response analysis of nuclear power plants. The classic approach was to use a rocking spring, and maybe horizontal and vertical springs, to model the flexibility of the supporting soil. R. G. Merritt[49]

49. R.G. Merritt and George W. Housner, "Effect of Foundation Compliance on Earthquake Stresses in Multistory Buildings," *Bulletin of the Seismological Society of America*. Vol. 44, no. 4, pp. 551-569, 1954. In his 1941 PhD thesis at California Institute of Technology, Housner presented an approximate estimate for the influence of radiation damping.

and George Housner discussed rocking action in 1954. Damping values of 5 percent were typically assigned to these springs, with little justification.

We have already discussed my somewhat earlier involvement with foundations for radar towers and with the elastic half-space theory for foundation vibrations. I began to urge use of that theory as a basis for evaluation of spring constants and the associated radiation damping. Having different damping values for different parts of a model for a structure and its supporting soil presented problems for the typical modal analysis procedures in use at that time. The concept of weighted modal damping was developed as a useful approximation that made it possible to proceed with the required analyses. However, the large damping values implied by that half-space theory became controversial, and the Nuclear Regulatory Commission was reluctant to accept them.

There were big questions—such as how much damping to use. Energy radiated out from the foundation as the structure swayed back and forth, and then that radiated energy wasn't available to participate in any resonant condition. You would have more damping of the motions, and that implied lesser design forces on the structure. This radiation damping through the soil was separate from, and in addition to, material damping within the soil. The Nuclear Regulatory Comission was very skeptical that you should count that soil-structure interaction damping contribution.

By the early 1970s, a new set of finite element analysis tools was being developed at Berkeley, principally through the efforts of John Lysmer, resulting in software such as FLUSH.[50] These new procedures offered the hope of providing rational answers to the various questions and problems associated with the use of foundation springs. The effects of embedment and of limited depth to bedrock could be modeled directly. Using an iterative approach, the effects of strain levels on modulus and internal damping at different points within the soil could be accounted for in a systematic manner.

In this relatively early stage of development of dynamic finite element methods, there were also uncertainties concerning the accuracy and appropriateness of calculated results. One question was: Where do you introduce a ground motion into that system? At the ground surface? At the subsurface rock? At the base of the foundation? You would get very different answers. Other developments were also underway by various contributors, especially the generalization of spring and damping constants into frequency-dependent impedance functions, which were then used with Fourier analysis techniques when computing response to earthquake ground motions.

Controversies vis-à-vis the relative merits of the several methods for analyzing soil-structure interaction were impeding regulatory proceedings for nuclear plants. In particular, there were hold-ups in final approval for plants where earlier models with foundation springs

50. J. Lysmer, T. Udaka, C.-F. Tsai and Harry B. Seed, *FLUSH – A Computer Program for Approximate 3-D Analysis of Soil-Structure Analysis Problems.* Earthquake Engineering Research Center Report No. EERC 75-30, University of California, Berkeley, Nov. 1975.

had been used and approved. At an ASCE Specialty Conference in Chicago in 1973,[51] major disagreements were evident. To some extent, the controversies persisted at a 2nd ASCE Specialty Conference in New Orleans in 1975.[52] At about this time, ASCE appointed a committee to report on the state-of-the-art concerning soil-structure interaction for nuclear plants.

The members of this committee were P. K. Agarwal, Asa Hadjian, Ed Idriss, Bob Kennedy, John Lysmer, Harry Seed, and me, with Harry as chair. Our charge: Assess the advantages and limitations of several methodologies, with special emphasis on proper practice in their use. We exchanged and critiqued drafts. We met to explain and clarify positions, and to agree upon wording. The discussions were tough, but polite. But without much difficulty, we agreed on what was virtually a textbook for soil-structure interaction.[53] The essence of the report appeared in a paper by Seed, Whitman, and Lysmer in the Newmark Memorial Volume[54] in 1977.

51. *Structural Design of Nuclear Plant Facilities*, American Society of Civil Engineers, 3 vols., Dec. 1973.

52. *Second Specialty Conference on Structural Design of Nuclear Plant Facilities*, American Society of Civil Engineers, December, 1975.

53. I.M. Idriss, R.P. Kennedy, P.K. Agarwal, A.H. Hadjian, E. Kausel, J. Lysmer, H.B. Seed, and R. V. Whitman, *Soil-Structure Interaction Effects for Nuclear Power Plants.* American Society of Civil Engineers, Reston, Virginia, 1979.

54. Harry B. Seed, Robert V. Whitman and J. Lysmer, *Soil-Structure Interaction Effects for Nuclear Power Plants.* W.J. Hall, ed. Prentice-Hall, Chapter 13, 1977.

## Earth Retaining Structures

**Reitherman:** In your 2007 Harry Seed lecture,[55] you discuss four topics: effects of local soil conditions on earthquake ground motions, liquefaction, soil-structure interaction, and earth retaining structures. We've discussed the first three topics. What about retaining walls?

**Whitman:** In 1970, Harry Seed invited me to contribute to a paper he'd been asked to prepare for an ASCE state-of-the-art conference on retaining walls.[56] My part of the paper was quite minimal, but it was my first introduction to the topic of the earthquake behavior of rigid retaining walls. The state-of-the-art at that time was what was called the Mononobe-Okabe theory, developed after the 1923 Kanto earthquake in Japan. Mononobe[57] and Okabe[58] developed a simple static formula—in effect, a

55. Robert V. Whitman, "Early Evolution of Earthquake Geotechnics: A View From the Northeast," Seventh H. Bolton Seed Memorial Lecture, U.C. Berkeley GeoEngineering Group and ASCE San Francisco Section Geotechnical Group, Berkeley, California, May 4, 2007.

56. Harry B. Seed and Robert V. Whitman, "Design of Earth Retaining Structures for Dynamic Loads," *Proc. 1970 Specialty Conference on Lateral Stresses and Earth Retaining Structures.* American Society of Civil Engineers, pp. 108-147, 1970.

57. N. Mononobe and H. Matsuo, "On the Determination of Earth Pressure During Earthquake," *Proceedings of the World Engineering Congress*, Tokyo, vol. IX, part 1, pp. 177-185, 1929.

58. N. Okabe, "General Theory on Earth pressure and Seismic Stability of Retaining Wall and Dam," *Journal of Japan Society of Civil Engineers.* Vol. 10, no. 6, pp. 1277-1323, 1924.

type of equivalent lateral force analysis—which assumes active soil pressure on the wall. Harry had surveyed a number of consulting firms to confirm that this was the basic method used as of the 1970s.

Several years later, David Elms at the University of Canterbury in New Zealand published a paper in which he made use of the Newmark sliding block type of analogy to look at the behavior of gravity retaining structures during earthquakes. These are "gravity" structures, like "gravity" dams, in which the retaining structure, a reinforced concrete wall in the case of retaining walls, relies on its mass to be stable and resist overturning or translation. This is different than retaining walls with rock nails or moment-resisting foundations. Elms concluded that if the dynamic lateral forces got too big, the wall would just slide a little bit and that would be the end of it. The question was: How much lateral motion was tolerable?

At MIT about this time we were thinking along the same lines. It's a complex situation. During the earthquake, the retained soil may have larger motions at the surface than those at the base of the wall. This led to one of the first geotechnical finite element modeling efforts I was involved in, with Farrokh Nadim, who later became the director of the earth hazards program at the Norwegian Geotechnical Institute in Oslo.[59]

We did some modeling at the centrifuge at Cambridge University using a tilting retaining wall model about a foot high. It couldn't slide, but it could tilt. The inertial forces on the wall itself tended to make it tilt. Then you had the soil behind the wall deforming in shear. Depending on whether the wall or the soil is stiffer, the soil will either be pushing against the wall, or the wall will want to move more than the soil will, and that will decrease the forces between the soil and the wall. One result of the testing was that the time of the maximum force between soil and wall was not when the Mononobe-Okabe theory predicted it. Subsequently, Andy Veletsos at Rice University did additional theoretical work that demonstrated what was happening much more clearly. The significant conclusion is the importance of evaluating the inertial force on the wall itself, and if that is done, then the force predicted by the Mononobe-Okabe theory is satisfactory for design.[60]

The situation with gravity retaining walls is related to the problem of estimating earthquake-induced loads on foundation walls. In Taiwan, actual earthquake response records were obtained from the SMART-1 array of large concrete models of nuclear structures embedded in the soil, and it was found that the maximum force between the soil and the structure occurred at the time when the structure was rocking back and hence pressing against the retained soil.

59. Robert V. Whitman and F. Nadim, "Seismically-Induced Movement of Retaining Walls," *Journal Geotechnical of Engineering*, Proc. ASCE, v. 109, no, GT7, pp. 915-931, 1983.

60. Robert V. Whitman, "Seismic Design and Behavior of Gravity Walls," *Design and Performance of Earth Retaining Structures*, ASCE Geotechnical Special Publication No. 25, pp. 817-842, 1990.

## Centrifuges in Geotechnical Experimentation

**Reitherman:** We mentioned centrifuge geotechnical testing earlier. Perhaps readers without a geotechnical engineering background may wonder, "Why would anyone put soil in a box and spin it around in a centrifuge?"

**Whitman:** It all goes back to two characteristics of soil. Unlike steel, for example, which behaves very elastically up to half or two-thirds of its failure point, soil behaves very nonlinearly even at low stress levels. If you're going to physically model soil in an experiment, you have to take that into account. Second, the pull of gravity on an earth mass generates significant naturally occurring stresses on soil. The centrifuge can simulate the in situ condition of the soil, based on the equivalence of gravitational mass and inertial mass, a basic principle of physics that has to do with all kinds of scientific disciplines. Put a 50g acceleration on a particle of soil by spinning it around and it can't tell the difference between that and having fifty times as much weight.

I think the Soviets did some early geotechnical research with centrifuges, studying the effects of blasting in the construction of dams. In my career, I first became familiar with this field through researchers at Cambridge University in England—first Ken Roscoe and then Andrew Schofield. Initially, some of these centrifuges were very small, with a diameter of rotation of only a meter or a meter and a half. At the end of the spinning arm was, in effect, a basket or bucket containing your soil model that could swing from its at-rest vertical position, as it hung there before the test was started, through an upward arc to take a horizontal position as the centrifuge sped up. At the end of the opposite arm would be another model or just a counterweight. The longer the arm, the higher the acceleration at a given velocity.

Because the path of the mass is always curving rather than going in a straight line, we have velocity rather than just speed, and thus inertial forces—because a change in velocity is acceleration. Once you achieve the desired velocity, you keep it steady so you have a constant outward acceleration that simulates gravity-induced forces on the soil at some depth. You just have to imagine the frame of reference rotated 90 degrees.

By the time I went for my sabbatical leave from MIT to Cambridge in the summer of 1976, they had in operation a centrifuge with a radius of about fifteen feet. At the end of the arm, the model of the soil might be up to about two or three feet in its horizontal dimension. I went to Cambridge not to do centrifuge research, but to do a year of writing and to learn in general about what was going on there. Cambridge was one of the leading geotechnical research centers in the world.

I got drawn into the centrifuge field because they had me advise a graduate student on dynamic soil testing. This was just before they were adding earthquake simulation to the sample in the "basket." They were about to use what was called the bumpy road method. The tip of the arm had a wheel in contact with the cylindrical wall that contained the apparatus, and on the wall was a path that made the bucket slightly slow down and speed up, which imparted accelerations that in the model simulated horizontal ground shaking. Bruce Kutter,

now at the University of California at Davis, got his PhD at Cambridge and was instrumental in developing the bumpy road technique and making the centrifuge into an apparatus useful for earthquake geotechnical engineering. Onboard shaker devices that were integral to the platform or "basket" were developed later.

Originally, there were wire-connected sensors to measure what was happening in the soil, and later radio-connected sensors were developed.

**Reitherman:** How were the wires run from the sensors at the tip to electronic equipment in the laboratory, when the arm was moving around its hub?

**Whitman:** The shaft had contact brushes to provide the electrical pathway.

**Reitherman:** Like a distributor in an engine, where the rotor sweeps over contacts?

**Whitman:** Yes, but without the intermittent connections the distributor provides as it sends bursts of electricity to the appropriate spark plugs.

After my sabbatical, I received funding from the Nuclear Regulatory Commission for some earthquake testing at the Cambridge centrifuge facility. How do you contain the soil and yet allow the soil to undergo the horizontal shearing deformations you want to study? We adapted the stacked ring approach, which I talked about in connection with blast-related testing at SRI.

We first tried Teflon-coated rings, which could easily slide over each other. We made the rings as light as possible so their inertial forces wouldn't overwhelm the forces within the soil to be tested. We carried out tests with dry sands to verify the technique. Using a membrane, we did tests on saturated sands. Then we used a different setup with saturated sand contained within a box with rigid sidewalls, and simulated a structure, an oil tank model, sitting on top of the sand. That was our first test that included a structural model. Because of the rigid confinement we had to put the structural model in the middle of the sample to minimize the confinement effects at the perimeter. We were measuring the pore pressure at the middle of the tank, at its edges, and in the free-field. The numbers came out about the way we expected, which was not the outcome a number of other researchers had expected.[61]

**Reitherman:** Did it take a while for the geotechnical field to adopt centrifuge testing?

**Whitman:** Yes, definitely, as shown by the history of centrifuge geotechnical testing in the United States. I wasn't the first from the U.S. to go to Cambridge. Before me Professor Jim Cheney and Professor Kandiah, or Arul, Arulanandan at U.C. Davis had spent time there. Arul obtained a small-size centrifuge at Davis. Faculty there got the idea of using the large centrifuge at the NASA Ames facility at Moffett Field in the San Francisco Bay Area, where they did g tests on astronauts. I was on a review committee for a proposal to move the NASA centrifuge up to Davis, which was accomplished with NSF and other funding. I think Bruce Kutter was the key Davis faculty member in pursuing that

61. Robert V. Whitman and P.C. Lambe, "Earthquake-Like Shaking of a Structure Founded on Saturated Sand," *Proceedings of the International Conference on Centrifuge Modelling.* Balkema, Rotterdam, pp. 529-538, 1988.

opportunity successfully. The centrifuge there now has been substantially rebuilt.[62]

About this same time, the Army Corps of Engineers researchers in Vicksburg got interested in centrifuge geotechnical research and developed a centrifuge. Ricardo Dobry developed one at Rensselaer Polytechnic Institute (RPI), and there was another at the University of Colorado. Those are the ones in the U.S. that were developed in the decade or two after I initially became involved, so it was a relatively slow process. We had a small one at MIT that had a diameter of about ten feet. We mainly used it for testing instrument packages that we used in experiments conducted elsewhere, mostly at Cambridge and RPI. Now with the NSF-NEES (Network for Earthquake Engineering Simulation) program, the UC Davis and RPI facilities have been upgraded.

One effort to establish the validity of this line of research was funded by the National Science Foundation and called the VELACS project.[63] It was led by Arul Arulanandan, with Ron Scott from Caltech providing his support for the idea. In a different field, space exploration, Ron had a key role in designing the equipment used to collect geological samples on the moon in the very first manned space mission. In the VELACS study, the experiments were run to show that centrifuge testing could provide data to test various predictive theories, and MIT was involved in that. One of the VELACS tests at MIT was with saturated sand, using a retaining wall model. As the sand was shaken it tended to get denser and increase pore pressure, and hence the force against the wall. But as the wall began to rotate, the sand was sheared and tried to dilate, thus decreasing the pore pressures and momentarily decreasing the force on the wall. This was about when I retired from MIT in 1993, and ended my involvement with centrifuge research.

**Reitherman:** It sounds analogous to the way water gets denser as you cool it, but as it begins to freeze it has the unusual property of expanding, because the molecules suddenly shift into a lattice structure that takes up more space. The molecules themselves, like the grains of sand, don't enlarge, but the matrix does, with the result that ice floats in water rather than sinking.

**Whitman:** Similarly, in the saturated sand behind the wall, the sand particles didn't change size, but the matrix did, as a function of shear stress.

62. The disassembly and move of the apparatus to U.C. Davis in 1987, with funding from NSF, the University of California, Tyndall Air Force Base, and Los Alamos National Laboratory, is described in Daniel Wilson et al., "The NEES Geotechnical Centrifuge at U.C. Davis," *Proceedings of the 13th World Conference on Earthquake Engineering*, Vancouver, Canada, August 1-6, 2004. U.C. Davis had collaborated on the original installation at NASA Ames in the late 1970s.

63. VELACS stood for VErification of Liquefaction Analysis by Centrifuge Studies. The universities in the project, besides those of Arulanandan (U.C. Davis) and Scott (Caltech), were the University of Colorado at Boulder, MIT, Princeton University, Rensselaer Polytechnic Institute, and Cambridge University.

Chapter 7

# Consulting Engineering Projects

*One of the things that intrigued me was that they were planning to build this dam on the same clay that Terzaghi had used to work out his ideas on consolidation and effective stress when he was a professor in Istanbul.*

**Reitherman:** Which of your consulting projects are particularly memorable?

**Whitman:** I had some interesting consulting experiences, though compared to many of my faculty peers at the time, I would say that my consulting experiences were varied rather than vast.

## Caracas High-Rise

**Whitman:** There was the major earthquake in Caracas in 1967. The houses, or mere huts, of the poor people had been erected on the slopes surrounding Caracas and were essentially undamaged. But several modern tall concrete buildings located in one part of the city collapsed.

Together with Harry Seed, I had been involved in investigating why the collapses occurred in particular locales as related to subsurface conditions. This was when Harry was very interested in finding any case where patterns of damage could be traced to differences in soil conditions, and this led to a joint paper.[64]

Subsequently, I was called on as a consultant for the planning and design of several apartment buildings that were over thirty stories tall. Originally, I was involved in the choice of the earthquake ground motions, based on the experience that Seed and I had accumulated from the 1967 earthquake.

I also found myself working with the structural engineers doing the dynamic analyses to take into account the flexibility of the foundations. There was also an interesting conventional geotechnical aspect to the project. The original plan had been to put this new building on piles, to go down through the top 10-meter layer of looser, softer, soil. Together with Bill Lambe, we proposed they should excavate this ten meters of soil instead, and the owners liked the idea of having this underground space for parking. This implied the use of a mat foundation and whether it would have adequate overturning resistance in an earthquake. My analysis indicated it would be necessary to widen that mat, and once the suggestion was made, they liked that concept also. The wider spaces on the lower levels were to be used for office space. In the upper stories, the layout of apartments called for a narrower building to have the floor area closer to windows. So it was a case where many design factors came together rather nicely. There was some follow-up work during construction concerning how much the building would settle, and the predictions worked out very well.

64. Harry B. Seed, H. Dezfulian, Ricardo Dobry, I.M. Idriss, and Robert V. Whitman, "Soil Conditions and Building Damage in the 1967 Caracas Earthquake," *Journal of Soil Mechanics, Proceedings of ASCE.* Vol. 98, No. SM8, pp. 787-806, 1972.

## Alibey Dam

**Whitman:** Another interesting consulting project was for the design and construction of Alibey Dam near Istanbul. Victor de Mello, who was at MIT as a visiting professor from Brazil, Bill Lambe, and I were called on to provide advice. One of the things that intrigued me was that they were planning to build this dam on the same clay that Terzaghi had used to work out his ideas on consolidation and effective stress when he was a professor in Istanbul. There was a narrow valley filled with deep clay, interspersed with sand layers. The clay was so soft that the dam could not be built all at once, so it had to be built slowly as the clay consolidated. We also had to decide whether to cut off the underflow through the layers of sand, by some method such as sheet piling and a long upstream blanket, or to excavate the permeable layers and replace with different fill. We opted for the upstream blanket.

This was also the first application of Allin Cornell's theory of seismic risk analysis to a major engineering project. Allin had just finished developing this method at MIT, along with Robin McGuire, and here came the first opportunity to put it into practice. The effects of the selected design earthquake were analyzed, on our behalf, by Ulrich Luscher, using

a sliding block analysis. Victor de Mello, Bill Lambe, and I left the project during the early stages of construction, but I understand it was completed much as planned.

## Lake Maracaibo Dikes and Liquefaction

**Whitman:** There was another consulting project concerning dikes along the shores of Lake Maracaibo in Venezuela. That is a major oil-producing region, especially at the east end of the lake. Considerable subsidence had occurred as a result of the extraction of the oil. The Venezuelans had been building dikes, up to twenty or thirty feet high, to protect the land-based facilities at the edge of the lake as the water rose—which is to say, as the land went down. In the 1980s, they needed to raise the dikes still higher, and by then became concerned with their seismic safety. A consulting board was formed. Along with me as chair, there was Bill Marcuson of the Waterways Experiment Station; Arnold Verruijt, who was from the Technical University of Delft in the Netherlands; Kenji Ishihara from Japan, who by that time had become well known for his work on liquefaction and other earthquake problems; and Killian De Fries, a Venezuelan consulting engineer.

At the edge of the lake, there had been over time various streams emptying into it, creating quite variable soil deposits. Fortunately, the earthquake threat at this location was not major. The biggest earthquakes would be in the Cordillera Mountains fifty to eighty miles to the south, with smaller possible earthquakes nearby. The most obvious way to make the dikes resistant to liquefaction was to flatten their slopes. However, the slopes on the lake side were hard to access and could interfere with the drilling going on there, and just landward from the dikes were all the industrial facilities and housing. So we developed techniques for analysis to use flatter slopes where they could be used, and to densify the soil by vibratory techniques. A denser soil would be more resistant to liquefaction. There hasn't been a major earthquake since then, so we don't really know how well we did. We couldn't precisely quantify the risk along those thirty or so miles of dikes with all their variable soil characteristics.

**Reitherman:** Perhaps even today such risks are not yet quantified? For example, it has only been in the last few years that the topic of earthquake-caused dike failures in the Sacramento Delta, east of San Francisco Bay, has been raised.

**Whitman:** And after Hurricane Katrina in 2005, of course, there has been more attention and money spent on quantifying dike vulnerability in that region for the hurricane hazard.

Of course, our densification scheme meant tinkering with the downstream slope of a dam, and you had to be sure you didn't damage it in the process of improving it. Field tests were carried out to measure vibrations and the amount of densification. This was good experience for Bill Marcuson, who was just starting then in his work for the Corps of Engineers in dealing with dam safety in the U.S. It was a great opportunity for me to learn more about the work of Kenji Ishihara and the work being done in Japan.

## British Columbia Hydro

**Whitman:** For British Columbia Hydro (BC Hydro), a number of engineers were involved to assess the safety of a large dam. The consequences of ground shaking on the dam were quantified in terms of the probabilities of being in various damage states, which ranged from a sudden, catastrophic failure, to a slowly developing total failure, on down to lesser levels of damage to the dam that involved no water release. Those states of the dam were coupled with an analysis of downstream consequences, in addition to estimating the property loss or disruption effects on the dam itself. The leadership of BC Hydro was very interested in using risk analysis to make decisions.

**Reitherman:** Was a different inundation map coupled with each damage state?

**Whitman:** The focus was just on the flooding caused by the severe damage to the dam. For those damage levels, the maps depicted both the depth of flooding and the time it would take for the water to get there.

There were two separate consulting groups. One I could describe as a traditional consulting group of consulting engineers, who evaluated whether they thought the dam would be safe under a particular level of shaking associated with a deterministic scenario. They ended up concluding that they initially thought it was going to be safe, but weren't really very sure. The other group, which I was in, did a risk-based analysis, including looking at the scenario the other group had been given. We concluded that for that event there was a 40 percent to 60 percent chance of a sudden serious failure such as major overtopping. Desmond Hartford was the BC Hydro engineer most heavily involved, and he was a great believer in formalizing the risk assessment process. He has subsequently had a key role in the Canadian group that is producing guidelines on the evaluation of risks associated with hydroelectric facilities.[65]

As our project was concluding, BC Hydro management probably made a reasonable decision that the dam was a risk and mitigation measures would be studied, but that its risk was not as great as that for other projects, so in this case the remediation wouldn't be done on a crash basis.

## Post Office Square, Boston

**Whitman:** There was a plan to take an existing plaza or square in downtown Boston where there was an old parking garage, demolish the old garage, replace it with a very deep multi-level parking garage underground, with the surface turned into a nice park. This would make it similar to Union Square in San Francisco. The plan was to go down seven stories, the deepest excavation in Boston, and that hole was to be immediately adjacent to the foundations of tall buildings. Obviously, that was a safety concern. There were also political concerns, over whether the resulting park-plus-garage was the best use of the city's land. An important community advocate for parks was at first in opposition, saying that parks should only be built where people live, but he

65. D.N.D. Hartford and G. Baecher, *Risk and Uncertainty in Dam Safety.* Thomas Telford, London U.K., 2004.

came around when he saw the benefit of a park for downtown workers.

The solution was to use what is called the top-down excavation procedure. First of all a slurry wall was poured around the perimeter, all the way down—seven stories—and also the interior columns were inserted into drilled holes. A layer of soil was excavated to permit construction of forms for the roof, which was at the grade of the surrounding streets. The roof, which now braced the top of the slurry wall, was subsequently used as the staging area for the construction equipment and soil removal work. Next, another layer of soil was excavated and the forms used for the roof were dropped to provide forms for the first of the underground floors. That floor was poured, and provided further bracing for the slurry wall. This procedure was repeated until the base of the garage was reached.

All of this occurred while very careful measurements were made of movements of the slurry wall and adjacent buildings. I had the task of heading the committee that met approximately weekly in a building overlooking the site to review all the plotted data, and made decisions about altering the pace of construction so as to avoid damage to the surrounding structures. Things were somewhat tense during early stages, but the procedure worked just fine.[66] It wasn't an entirely new method. I know it had been used in Europe.

## Oil Storage Tanks in Kawasaki, Japan

**Whitman:** I also had a consulting project for an oil company in Japan. We carried out a seismic hazard analysis to estimate what the shaking threat was to oil storage tanks. Kawasaki is on the coast, near Tokyo. We tried to quantify the probability of failure in those systems, with various remedial measures, and provide guidance. These were crude studies compared to what one might do today. But for the client, it accomplished two purposes. First, it emphasized they did need to do something, with the major remedial measure being to lower the groundwater by continuous pumping. It also helped them define the earthquake risk with the regulatory authorities.

66. Robert V. Whitman, E.G. Johnson, E.L. Abbott and J.M. Becker, "Field Instrumentation Program Vital to Deep Excavation Project," *Proceedings 1991 Geotechnical Engineering Conference.* ASCE, pp.173-184, 1991.

Chapter 8

# Seismic Design Decision Analysis Program at MIT

*A central concept in the SDDA studies was optimizing earthquake performance as compared to the cost of that protection.*

**Reitherman:** The Seismic Design Decision Analysis (SDDA) program at MIT produced thirty-three influential and far-ranging earthquake engineering studies. While it included some geotechnical engineering topics, the scope of the SDDA program was much broader than that and marks a shift in your career into the fields of risk analysis and building codes. How did it all start?

**Whitman:** It was Allin Cornell's suggestion, made after the 1971 San Fernando earthquake occurred. SDDA was a coordinated research program for a variety of MIT faculty and graduate students to produce a systematic framework for seismic design, based largely on 1971 San Fernando earthquake data. A central concept in the SDDA studies was optimizing earthquake performance as compared to the cost of that protection.

By the way, in addition to the published reports, we had internal study reports that were valuable—not developed to the point of being published, but they were included in our widespread dissemination program. There were eighty-nine of these internal reports.

## Specifying the Ground Motion to be Used in Design

**Reitherman:** In the title of the program, does the word "decision" refer primarily to the decision as to what level of ground motion to use in design?

**Whitman:** Yes, that was the key idea. This was related to the experience in Massachusetts around the same time in adopting a statewide seismic code. The key question in Massachusetts, in the 1970s era of the Uniform Building Code seismic provisions, was what zone to pick—Zone 1, 2, or 3. A little later, when I was involved in the ATC 3 project, the definition, selection, and mapping of design ground motions were key issues also. Of course, we had to consider the performance that resulted if you used a certain seismic ground motion level.

Using strong ground motion and building motion records from the 1971 San Fernando earthquake, we tried to compare in SDDA studies the performance you would get when you combined a particular level of seismic design with a level of earthquake ground motion. Our data was relatively sparse then, of course.

**Reitherman:** But before 1971, the database was much more sparse. Without the San Fernando earthquake, your SDDA series of publications would have been very brief rather than voluminous, wouldn't it?

**Whitman:** You're dead right on that. Before the San Fernando earthquake, the information on building damage and especially ground motions and motions in structures was very limited.

**Reitherman:** At this time, the basis for the UBC seismic provisions was the SEAOC Blue Book,[67] which was a non-probabilistic basis for specifying the ground shaking hazard, based on the strongest recorded intensity. The SDDA program, by contrast, seems like a clean intellectual slate on which was sketched a more complex picture of the risk—in a way that continues to develop to the present day.

**Whitman:** Our task was not to manage the incremental updating of an existing code, and we looked at earthquakes as a national, not just a California, problem. Today, performance-based design is founded on the basic notion that the benefits of a particular type and level of seismic design, as well as the costs, must be presented to decisionmakers so that they can choose wisely. In California, performance-based design usually means: How far beyond the life safety level of the building code does a client want to go? Whereas in many regions of the country, the first decision that needed to be made was whether to have any seismic requirements at all.

We weren't just looking at the mean response of structures, and then adding safety factors to produce an overall result practicing engineers could use to at least get results as reliable as the code provided in the past. We were looking at the uncertainty in the range of performance.

67. *Recommended Lateral Force Requirements and Commentary.* Structural Engineers Association of California, Sacramento, California.

Today, you can easily do multiple analyses of a building on the computer and get a better feel for uncertainty that way.

**Reitherman:** Can you comment on a few of the graduate students who produced SDDA studies who later went on to have careers in the earthquake engineering field?

**Whitman:** Most of them were master's students. John Reed ended up doing a lot of seismic consulting work with Jack Benjamin. Mishac Yegian is a geotechnical engineering professor at Northeastern University with a specialty in earthquakes. Stavros Anagnostopoulos is a professor back home in Greece and has been quite influential in earthquake engineering there. Martin Czarnecki joined John Blume's firm and has worked on many earthquake engineering projects. Tappan Monroe was with Pacific Gas and Electric, coming to MIT for a year or so, and did economic studies of earthquake loss. Incidentally, the co-author on the SDDA report he wrote, Cynthia Blair, was my secretary. She was a Bryn Mawr senior who had completed her graduation requirements mid-year, was going to start graduate school at MIT in the fall, and was looking for a job to fill in the time. I realized she was going to be more than a secretary when I gave her a report to type and she came back soon saying, "Professor Whitman. Would you mind looking at equation 14? I think it's wrong." And she was right. So when she finished her "day job" work and had time left over I had her work with Monroe. Daniele Veneziano ended up a professor at MIT and has done probabilistic work in the earthquake field.

Richard de Neufville became a civil engineering professor at MIT and a well-recognized leader concerning technology and public policy. Fred Krimgold was the architect in the mix who went on to work at NSF and is now a professor at Virginia Polytechnic Institute. John Christian was on the MIT faculty during the SDDA years and has done research on earthquakes as well as other areas. He was later an executive with Stone and Webster, and is now in private practice as a geotechnical engineer.

**Reitherman:** The SDDA program was quite a training ground, producing so many prominent earthquake engineers.

## Scope of the SDDA Program

**Whitman:** The SDDA program branched out beyond buildings to look at utility systems. The study of the water system of Boston when subjected to earthquakes was innovative in its system analysis, though it was crude by today's standards. Also around this time I was on the founding executive committee for TCLEE, the Technical Council on Lifeline Earthquake Engineering, when it was established in 1974, and I later chaired it. I became good friends with Martin Duke, the founder of TCLEE. It was Martin who nominated me for EERI membership, when it was still by invitation only. I became a member in 1972.

We also took on the subject of how to deal with existing buildings in the SDDA studies, not just design provisions for new buildings. We had engineers look at Boston buildings and estimate their resistance to various shaking levels. Ground failures were also studied.

**Reitherman:** The SDDA research program was certainly comprehensive. I'm not aware of any comparable coordinated research effort

at the time that spanned so many topics. Of course we have more data, more methods, more researchers in the earthquake engineering field today. But in an intellectual sense, have there been completely new ideas that weren't explored in the SDDA program?

**Whitman:** It would seem too big a claim to say that nothing new has been produced since that 1970s program. A big change is the increase in the level of confidence we can have today in predicting performance. That idea was there in the SDDA work, but we couldn't realize it the way you can today. Analyses of building response and performance today have advanced enormously.

One of the things we realized, as we got into the SDDA program, was that it was very hard to justify spending money on the seismic resistance of construction if all you wanted to do was optimize the dollar cost of damage. Until you start putting a cost on casualties and disruption, it takes rather narrowly defined conditions to justify a lot of preventive cost. That led us to broaden our scope.

One of those conditions would be a very high probability of strong ground motion, along with rather high vulnerability of the construction. Howard Kunreuther at the Wharton School at the University of Pennsylvania, for example, in studying single-family dwellings, found the economics of property protection—without consideration of danger and disruption—a difficult sell for the ordinary homeowner.

**Reitherman:** In 1975 you were elected to the National Academy of Engineering, I would imagine mostly on the basis of your long career full of accomplishments in the geotechnical engineering field. When the SDDA program ended in 1977, at least in terms of the publication of the last report, where did your career go then? SDDA seems to have been a link to your work in earthquake loss estimation, because to compare the cost of protection with the benefit of protection you need to estimate losses for both cases.

**Whitman:** Yes, while still of course interested in geotechnical topics, I ended up spending a good deal of my time in subsequent years on the earthquake loss estimation problem.

Chapter 9

# Earthquake Loss Estimation

*A turning point in the HAZUS project was when we concluded that computers had a big role to play, and that our goal was not to oversee the development of just a manual.*

### Damage Probability Matrix

**Whitman:** As background to the work on estimating losses from future earthquakes that went on in the 1970s and later, we should mention the important previous work done by Karl Steinbrugge, Ted Algermissen, and others. Karl and others in the insurance industry really got the whole field started, at least in terms of systematic loss estimation and on the scale of large numbers of buildings. In the 1960s, the federal government was already funding an effort led by Karl and Ted to produce scenario loss forecasts for major urban regions. The first one published was on the San Francisco area in 1972.[68]

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

**Reitherman:** When we talk about the damage ratio concept, we should mention again the damage probability matrix that you and your co-workers came up with in the SDDA program that you headed at MIT. Simply reading down a column in a damage probability matrix is an instructive thought process. You can visualize either the probability that one building will end up in a given damage level—from none to complete—or envision the percentage of a large number of buildings that will end up in each damage state. Reading across a row, you can visualize the damageability or fragility of the building—or collection of buildings—as the ground motion gradually increases.

**Whitman:** That was the probabilistic thought process that Allin Cornell got me thinking about. There is also an element of this approach in the Medvedev-Sponheuer-Karnik, or MSK, Intensity Scale developed in Europe. The MSK scale estimates what percentage of a general type of building will have that degree of damage, perhaps just qualitatively in terms of "most" or "few."

In the 1980s, the ATC 13 report was produced.[69] It used the damage probability framework for compiling loss estimates of experts. That study was focused on California. Later, there was an effort to apply the ATC 13 approach to a loss study of Boston. The question arose as to how to modify the ATC 13 damage probability matrices. It was not clear to me why they needed to be modified. I think there was an assumption that an unreinforced masonry building in Boston had a different vulnerability than an unreinforced masonry building in California.

**Reitherman:** The damage probability matrix also forces you to put your best numbers in the table, to predict the range of behavior you will get. That made it so useful for clarifying uncertainty, as well as making it ideal for calculations.

**Whitman:** One can also convert a damage probability matrix into a fragility curve, and vice versa. Fragility curves became commonly used in earthquake engineering a little bit later.

## National Research Council Panel on Earthquake Loss Estimation

**Reitherman:** Let's talk about your role as chair of the National Research Council Panel on Earthquake Loss Estimation Methodology.[70]

**Whitman:** FEMA commissioned that effort because, in their role of providing earthquake loss forecasts to the emergency management field, they were getting conflicting advice on how to conduct such studies. At a minimum, there were two main methods by then, the original approach of Algermissen, Steinbrugge, and others—as in the NOAA studies—and the later ATC 13 study.

**Reitherman:** As that project started, did it proceed as you initially expected? You had previous experience with such panels, being

69. Applied Technology Council, *ATC 13: Earthquake Damage Evaluation Data for California*. Applied Technology Council, Redwood City, California, 1985.

70. Panel on Earthquake Loss Estimation Methodology, *Estimating Losses From Future Earthquakes*, National Academy Press, Washington, D.C., 1989; also published by the Federal Emergency Management Agency as FEMA-177.

on the Soil Dynamics Panel of the Committee on Earthquake Engineering Research at the National Academy of Sciences in the 1960s and chairing the National Research Panel on Liquefaction that we talked about earlier.

**Whitman:** Each panel effort is different. You, Bob, were there as the consultant to the earthquake loss estimation panel, so you'll recall that basically you and I wrote most of the report, except for the appendices, which each had a separate author.

There were some passions involved with advocating or defending either the NOAA-type or ATC-type approaches. In retrospect, the report did a good, fair job of summarizing both as well as bringing in other information. I recall Art Zeizel of FEMA later saying that two-thirds of the way through the Panel's work that he wasn't sure if the project would end successfully, but that he was very glad it all came together.

## HAZUS

**Whitman:** The next major development in earthquake loss estimation, HAZUS, which is a contraction of Hazard U.S., was not based on a choice between those two approaches or even a simple combination, but was an evolutionary advance that went further.

**Reitherman:** To start out, let's note that you were the chair of the committee that oversaw the development of HAZUS. You'll recall that when FEMA funded NIBS, the National Institute of Building Sciences, to do the project that ended up producing HAZUS, FEMA said that the justification for funding that effort was the NRC panel report. So *Estimating Losses From Future Earthquakes* ended up being a bridge spanning from the earlier NOAA and ATC efforts into the future to what became HAZUS. It's obvious, but let me note that you personally were the key individual connecting those two efforts, chairing both of them.

**Whitman:** In going forth with HAZUS, we certainly tended to pick more ambitious alternatives for producing a more advanced method whenever we faced choices. We didn't guide it down the easiest path. In selecting the team to develop the method, we opted for some new, rather than tried-and-true, talent. The team was headed by Hemant Shah of RMS, and included Charlie Kircher, who I would call the "chief architect" of the building damage portion of the method, Weimin Dong, Thalia Anagnos, Chris Arnold, Scott Lawson, Jawhar Bouabid, and others. Then Dames & Moore was selected to do the first pilot testing, in Portland, Oregon. EQE was picked to conduct the second pilot testing, in Boston. Hank Lagorio was head of the committee of users, mostly drawn from the emergency management field, who advised on how they would use the results.

**Reitherman:** I remember the first meeting of the committee, in 1994, in the old NIBS building across from the beautiful Episcopalian church on G Street in Washington. You had a letter from Gary Johnson of FEMA that was our charge. It said we should guide the development of a standardized nationwide method for estimating earthquake losses. And then at some point one of us asked, "Is this going to be a paper document describing the steps to use to produce a loss study? Or will it involve software?" I certainly had a blank look on my

face, and I don't think any of us knew then how computer-intensive the HAZUS method would be. HAZUS still lives on as updatable software, and it rode the wave of the greatly increasing power of small computers that are now so accessible to not only researchers but local governments.

**Whitman:** A turning point in the HAZUS project was when we concluded that computers had a big role to play, and that our goal was not to oversee the development of just a manual. The HAZUS effort—and of course it didn't have that name until several years later when that was what the software was called—began in 1993, when personal computers were widespread but rather expensive and slow. Computer technology developed rapidly year by year and enabled HAZUS to become more powerful as well. I'm glad it went the way it did.

**Reitherman:** We had no idea that this earthquake loss estimation method would later be adapted to have modules for other hazards, like floods and hurricanes. That was made possible because within a few years the small, inexpensive computer every building, planning, or emergency services department had could hold and process vast quantities of census, geographic, and other data. In many respects, the inventory data is a common platform across hazards, and it's useful for non-hazard purposes.

**Whitman:** There was another benefit to the choice to proceed with a computerized method. One of the disciplines imposed by a computer is that every step has to be clearly defined. It sharpens all sorts of details and clarifies your thinking. I recall Charlie Miller at MIT, who was one of the pioneers in setting up program-oriented languages, emphasizing that one of the values of a computer program is not just that it does calculation work for you but that it forces you to make some very clear decisions.

I never imagined HAZUS would be so widely used, not just by FEMA, but by consultants and local jurisdictions all over the country. It was hard to keep track of who was actually using HAZUS as it went through various versions, but it became clear over the years that it was very widely used by state and local governments as well as by FEMA.

I've been impressed recently with the EERI Seattle study[71] and the loss analysis by Charlie Kircher of a possible recurrence of the 1906 San Francisco earthquake.[72] The Seattle study deals with lifelines in a relatively qualitative way, outside the HAZUS method, whereas HAZUS is used for the building stock in that study. I think that reflects continuing challenges in obtaining inventory data on lifelines and analyzing those complex systems. To do lifeline loss estimation, you have to spend more, and it's still not clear how to present all the results in a meaningful way.

**Reitherman:** Do you think HAZUS should get credit for at least confronting the lifeline

71. Earthquake Engineering Research Institute, *Scenario for a Magnitude 6.7 Earthquake on the Seattle Fault.* Earthquake Engineering Research Institute, Oakland, California and Washington Military Department, Camp Murray, Washington, 2005.

72. C. Kircher, H. Seligson, J. Bouabid, and G. Morrow, "When the Big One Strikes Again—Estimated Losses Due to a Repeat of the 1906 San Francisco Earthquake," *Earthquake Spectra.* Special Issue II, Volume 22, April 2006.

topic and providing methods for estimating their losses, even if those methods have been little used as compared to studies of building losses?

**Whitman:** Yes, that was an advance. I have to say that HAZUS had to stop short of a complete network analysis of lifelines, though we went a little way down that path with water systems.

**Reitherman:** I recall the committee meetings as some of the most interesting I've ever attended. Each was a combination of a seminar and a business meeting.

**Whitman:** NIBS had to proceed with allocating a limited budget, to be spent on one consulting team or another for a particular scope, so in prioritizing the budget and selecting consultants we had that business responsibility, as you say. The seminar aspect you refer to was needed so that we could hear directly from experts and discuss the underlying science and engineering among ourselves.

**Reitherman:** You remember the HAZUS meeting we had in Boston, when you hosted a dinner for not only those of us on the committee, but consultants and agency people? A bus was chartered to drive us out to your home in Lexington on a snowy evening.

**Whitman:** Was that the occasion when you all showed up wearing bow ties? I assume you were the ringleader of that plot?

**Reitherman:** Yes, the ringleader. Or you could just say we thought we should conform to the Robert V. Whitman dress code.

Chapter 10

# Advisory Committees

*We gave our recommendation for a new Massachusetts seismic code to the state commissioner, he said okay, and that was that.*

**Reitherman:** Sometimes, being on an advisory committee is an insignificant role, but in your case, you have had some major influences in that capacity. One committee you headed had a big effect on building codes. Massachusetts was the first state outside the relatively high-seismic West to adopt seismic regulations. How did that come about?

## Massachusetts State Seismic Code

**Whitman:** The story starts in the early 1970s with the MIT Seismic Design Decision Analysis program we've discussed and the idea of rationally analyzing the appropriate level of seismic protection for a region. It turned out that there were already seismic provisions in the Boston city code by reference to a model code. Not only that, but it put Boston in Zone 3, the highest zone, the same as coastal California. This city regulation was somewhat obscure and had not been enforced. After the 1971 San Fernando earthquake, the building official of

the city of Boston told the local engineering community that it was obviously foolish to put the same seismic requirements on a building in Boston as in California, but if the engineers didn't come up with better provisions he was going to start enforcing that. Within a year or so there was a decision to have a statewide Massachusetts code, so that meant seismic provisions would be statewide also. I was asked to do something about the situation, so I wrote letters to the Boston Society of Civil Engineers and the American Society of Civil Engineers. At that time, the Boston Society was about to become the local section of ASCE.

**Reitherman:** You mean the Boston Society of Civil Engineers existed apart from ASCE?

**Whitman:** That's right. You have to understand that the Boston Society existed before ASCE was even formed.[73] I suggested that a committee be appointed to study the issue and produce recommendations. I ended up the chair. We plunged in, met, and considered what we thought was an appropriate basis for seismic design. At that time, the new state building code was almost in existence and there was a newly appointed state commissioner to oversee that code. We gave our recommendation for a new Massachusetts seismic code to the state commissioner, he said okay, and that was that.

**Reitherman:** I'm surprised. The social science literature generally says seismic code adoption is a difficult process, one that many stakeholders need to be involved in from the beginning and over a long period of time, and that engineers by themselves don't have the full range of capabilities needed to achieve implementation.

**Whitman:** We had only a few complaints in Massachusetts, and the work our engineering committee produced was implemented rather smoothly. The concrete people said we were being too tough on them and not hard enough on steel construction. The steel people complained we were too hard on them and too easy on concrete. Probably the major change in building construction required by the new code was that unreinforced masonry was not allowed. There was resistance to that from the masonry industry, but it was accepted. When we laid out all our thinking and background studies, people went along with it.

**Reitherman:** Considering your contemporaneous ground motion risk contour mapping for the ATC 3 project, I'm surprised that you ended up with the whole state in one zone, especially because Massachusetts is stretched out east-west and the coastal area to the east is more seismic.

**Whitman:** The committee produced a contour map, but the state building commission wanted the whole state in one zone for simplicity of administration. So, erring on the side of conservatism, some areas of the state were lumped in with the slightly higher zone.

**Reitherman:** Since 1975, how has the seismic code in Massachusetts evolved?

**Whitman:** There was an advisory committee to the state building commission, and I was on that group. We next tackled the question

73. The Boston Society of Civil Engineers was established in 1848; the American Society of Civil Engineers was established in 1852. The Boston Society merged into the American Society of Civil Engineers in 1974.

of existing buildings. The code was essentially an all or nothing proposition. If you did less renovation work than a particular amount, you didn't have to do any seismic upgrading. If you crossed that threshold, you were supposed to bring the building all the way up to current code. We worked out a set of graduated requirements aimed at encouraging some upgrading, balanced against costs. In essence, if the owner was going to spend a lot to renovate a building, making it more valuable, then significant seismic retrofitting would be required. I ask around occasionally to see how the enforcement of the code is going, and on the whole it seems to be well carried out.

## National Earthquake Hazards Reduction Program (NEHRP)

**Whitman:** In 1977 and 1978 I was part of the advisory group for the earthquake hazards reduction implementation plan done for the Office of Science and Technology Policy.[74] The main function of it was to frame the National Earthquake Hazards Reduction Program. Frank Press was still Science Advisor at that time. Karl Steinbrugge was the chair of that effort. Chuck Thiel was energetic in advancing that work too. Those were fascinating times.

**Reitherman:** Did these planning efforts pan out? Did NEHRP end up developing roughly as you had envisioned?

**Whitman:** Basically, yes. There were difficult issues to resolve as to which federal agency would do what. This was at the same time that FEMA was being dreamed up and getting established, and that brand new branch of the federal government ended up as the lead NEHRP agency.

**Reitherman:** You've said that the1964 Alaska and 1971 San Fernando earthquakes were milestones in developing support and funding for earthquake engineering research. You've mentioned federal planning studies in the 1970s that were important. Was the next significant milestone the passage of the NEHRP act in 1977?

**Whitman:** Yes.

**Reitherman:** After 1977, are there other equally significant milestones? Or is this whole period since 1977 one NEHRP era?

**Whitman:** Every earthquake that occurs is some sort of a milestone, because we keep learning new things. For example, it took the Northridge, California earthquake to bring out some new knowledge about steel frame construction that would not have automatically been developed by NEHRP research. The growth of computational capabilities and of strong-motion recording systems has been a milestone of a sort. There have been changes in the landscape of earthquake engineering. The large university-based research centers have come into being and have flourished, and now there is the NSF-funded NEES (Network for Earthquake Engineering Simulation) program with large-scale testing facilities. New entities focusing primarily on lifelines have come into existence. It has been a constantly evolving scene.

74. Working Group on Earthquake Hazards Reduction, *Earthquake Hazards Reduction: Issues for an Implementation Plan.* Office of Science and Technology Policy, Washington, D.C., 1978.

Engineers sometimes complain that too much NEHRP money is being spent on earth sciences and not enough on engineering, but that's not new.

## ATC 3

**Reitherman:** In your 2007 Seed Lecture, you have a fascinating bit of information about how the ATC 3-06[75] national maps depicting the hazard of earthquake shaking were produced. Those maps were a historic step forward in seismic provisions in the U.S. In essence, with periodic revisions and the convening of one working group after another to apply the most up-to-date information, the current seismic shaking maps in the NEHRP Provisions have a provenance that goes directly back to ATC 3-06.

**Whitman:** One of the goals of ATC 3 was to come up with a national zoning map based on clearly defined rational principles. They were going to be risk-based and nationally consistent. Originally, the plan was for Ted Algermissen of USGS to work with his group to come up with the contours of expected accelerations. I was on the committee of ATC 3 in charge of that function, along with Bruce Bolt and several other people. Our committee was related to another committee that dealt with local soil conditions, which was chaired by Neville Donovan. Ted produced a probabilistic map for acceleration values, which was an advancement over similar maps he had done earlier. It had extremely detailed contours that squiggled around all over the map, and the general reaction of the structural engineers was that it had far too much detail. It conveyed too much confidence in how much we really knew. So one of the ATC 3 tasks I was given was to take Ted's map and smooth out the contours so they were broader and more sweeping rather than so detailed.

The other thing that came out of ATC 3 was to also plot peak velocities, which would be associated with peak response of structures with periods out at one second or more—rather than the acceleration values that had more to do with a half-second or quarter-second on the period axis. Ted declined to produce that velocity map, and he was right to say that at that time he had no fully scientific basis for doing so. To move ahead with that task, I was given the task of transitioning the acceleration map into a velocity map.

That velocity mapping task required that I study how damage attenuated with distance. I developed some rules, described in the Commentary of ATC 3, which I used to shift peak acceleration contours out farther in some areas of the country. I presented my work to the ATC 3 team and was told to go back and draw it up.

When I later became president of EERI, one of my goals was to get a better national map drawn up than the one I had done, but USGS still felt there wasn't sufficient scientific basis for doing that, and it took another ten years.

**Reitherman:** Those maps in ATC 3-06 are so historic, I have to ask: Exactly how were they drawn? Back then I imagine they weren't numerical values that were then computer plotted.

75. *Tentative Provisions for the Development of Seismic Regulations for Buildings*, ATC 3-06. Applied Technology Council, Redwood City, California, 1978.

**Whitman:** No, a computer was not used. I sat down at a light table, took Ted Algermissen's original acceleration map, which was the size of a typical blueprint, and just drew by hand on an overlay.

In addition, to respond to the requests of building officials in the ATC 3 project, maps were prepared that took the highest value in a given county and made that jurisdiction all one color. That was thought to make the provisions easier for a building department to administer.

**Reitherman:** Did people involved in ATC 3 realize at the time how momentous their work was?

**Whitman:** I certainly was excited to be part of it. It was a chance to take advantage of the scientific developments in the field.

**Reitherman:** The commentary to ATC 3 lays out the risk-based concept of the document in a very clear way. It says that if buildings are designed to its provisions, the combination of the ground motion criteria and structural design provisions should produce an equal probability of failure across the country. It also sticks its neck out and tabulates the probability of collapse for a group of buildings designed to its provisions.

**Whitman:** As I say, the latest thinking about risk was one of the most important ingredients in ATC 3, and that certainly extended to the ground motion portion of it that I was involved in.

**Reitherman:** What do you recall of some of the key participants in that project?

**Whitman:** There were so many experts involved, especially among the structural engineers—too many to try to mention. I would single out the key role of Roland Sharpe in organizing the various individuals and committees. I remember his dedication to the job, his humor when things were not going well, his ability to finally get people to agree on things and move ahead.

## NAS, USGS, ANSI, ASCE, and Other Advisory Boards

**Whitman:** I'm trying to recollect some of my other entanglements. Back in 1966, I was a member of what was called the Soil Dynamics Panel of the Committee on Earthquake Engineering Research at the National Academy of Sciences. As early as 1972, I was asked by the United States Geological Survey (USGS) to be part of the advisory panel for their National Center for Earthquake Research in Menlo Park, California. So I was well-invested in earthquake engineering as the Seismic Design Decision Analysis (SDDA) program started up at MIT. The Massachusetts state code was my first foray into the building code arena. Then there was ATC 3, as we've discussed. Then I got involved chairing a committee with ANSI, the American National Standards Institute, to revise its seismic code provisions, mostly related to structural provisions. Then there was an ASCE committee dealing with an ASCE seismic standard, ASCE 7.

In 1976 there was a National Science Foundation Advisory Group on Earthquake Prediction

and Hazard Mitigation.[76] At that time, Frank Press was the President's Science Advisor. He was primarily interested in pushing earthquake prediction, but Nate Newmark and others convinced him that if he was to get any support from the engineering community, there had to be a hazard mitigation component, which requires earthquake engineering research. This was just before the National Earthquake Hazards Reduction Program (NEHRP) legislation passed.

76. NSF and USGS, *Earthquake Prediction and Hazard Mitigation: Options for USGS and NSF Programs.* Washington, D.C.: U.S. Government Printing Office, 1976. This report is often called the Newmark-Stever report, the "Newmark" part of the document's nickname was for Nathan Newmark (1910-1981) of the University of Illinois. "Stever" was for H. Guyford Stever, who was director of the National Science Foundation 1973-1977 and was the President's science advisor.

Chapter 11

# Earthquake Engineering Research Institute

*I was extraordinarily thrilled when I was voted into EERI in 1972. At that time, there were probably fewer than one hundred members in the organization.*

**Reitherman:** You mentioned earlier that your involvement with C. Martin Duke led to your membership in EERI. Say a little more about that.

**Whitman:** Back then it was by nomination and election. I was extraordinarily thrilled when I was voted into EERI in 1972. At that time, there were probably fewer than one hundred members in the organization. Soon thereafter, the nomination-election procedure in the bylaws was eliminated, or at least you just had to list some references on an application form and didn't have to be nominated by a member, and the membership grew very fast. When I joined, an EERI meeting was basically a plenary session, because the number of members was so small. That made the meetings very enjoyable, with everybody getting together.

I was on the EERI Board of Directors from 1978 to 1981. One of my tasks was putting a strategic plan together, when Susan Newman shared executive duties between EERI and the Seismological Society of America in the same small office. I was president of EERI from 1985 to 1987. There were two large and important earthquakes during those years, as you'll recall—in Mexico City and Chile—with a great deal of attention paid to learning from them. By this time, EERI was getting too dependent on federal grants, and we needed to identify the services provided to members and align them with the dues being charged to members.

## President of EERI

**Reitherman:** Were you the first EERI president from outside California?

**Whitman:** Yes. I think there were disadvantages because I probably spent less time on the West Coast to stay in touch with people there, while it maybe also gave me more of a national perspective on where the organization should be headed. This was when we were deciding to set up a dedicated office or headquarters for the organization, and Susan Tubbesing was hired as executive director in 1988—that was just after my tenure as president, when Frank McClure was president. In the time since Susan was hired, the headquarters role has been strengthened and a big push made to enlarge the membership and put the finances on a secure footing. That effort has been successful.

## EERI Distinguished Lecture

**Reitherman:** You also delivered the EERI Distinguished Lecture in 1994, didn't you?[77] I remember that you gave it at the annual meeting held in Pasadena, California that year. I recall you saying there was a distinction between the federal program, the National Earthquake Hazards Reduction Program (NEHRP), which is primarily a program to develop information, and a de facto nationwide earthquake risk reduction effort.

**Whitman:** That nationwide risk reduction effort is carried on by practicing engineers in their daily work, various federal agencies other than the four NEHRP ones, state and local agencies, owners of buildings and lifelines, and so on. I argued that we should first set the risk reduction goals, and then use them as a basis for a strategic plan for NEHRP. NEHRP is called an earthquake hazard reduction program—actually we should use the term earthquake risk reduction and reserve "hazard" just for the earthquake phenomena themselves—but it has really been an earthquake research or information program.

**Reitherman:** In a country such as the United States, where the number of people and the amount of property exposed to a hazard, such as earthquakes, is much greater today than a decade or two ago. We would expect total risk to grow even if the quality of the construction remains the same. Is there merit to the idea

77. Robert V. Whitman, "Toward A Strategic Plan for the National Earthquake Risk Reduction Program," *Earthquake Spectra.* Vol. 11, no. 2, 1994, p. 297-317.

of trying to measure risk per square meter of floor area or per capita?

**Whitman:** That's essentially what FEMA did in using HAZUS: to do runs with default inventories for all areas of the country, presenting dollar losses as related to dollar value at risk. If you see losses going down as related to exposure, we are on the right track.

Chapter 12

# Pastimes, Family, and Other Interests

*Chebeague Island is about four miles long and up to a little over a mile wide. The year-round population is about 350.*

## Family

**Reitherman:** You've said a little about your wife Betsy earlier. Tell me more about your family.

**Whitman:** We have two daughters, Jill born in 1954 and Gwen in 1963. We had a third daughter, Martha, born between those two children, who had a serious birth defect and died at age three. Jill, after attending Middlebury College, got a master's degree in marine science at the Rosenstiel School at the University of Miami. Then she went to the Scripps Oceanography Institute for a doctorate. She is now on the faculty of Pacific Lutheran University near Tacoma, Washington. She is currently chair of the earth sciences department, and a few years ago was instrumental in setting up an environmental studies program. Gwen, after going to Swarthmore College, began to specialize in university administration and continuing education. She did that in Washington D.C. for an association of universities, then in Minnesota, then

at Purchase College of the State University of New York. A few years ago, she decided she wanted to be a teacher, and is now a fourth grade teacher in White Plains, New York.

**Reitherman:** We discussed at the very beginning of this oral history the strong higher education background of the family you were born into, and now we see that trend continuing in your children. Do you have an annual custom of everyone getting together at holidays, like at Thanksgiving or Christmas?

**Whitman:** We have two grandsons on the West Coast and two granddaughters in New York. Betsy and I alternate between spending Christmas with the two families, and in the year we're not spending Christmas with one we go to the other for Thanksgiving. It's easier for us to go visit them than for them to pack up their kids and visit us.

We talked earlier about the Lexington Town Meeting. Betsy and I have been active in the Episcopal Church, and Betsy has played many roles in the town, with the League of Women Voters, the Lexington Garden Club, as a member of the town's Design Advisory Committee. We're both active in supporting candidates for public office.

**Reitherman:** Tell me a little bit about the house you used to live in here in Lexington, the one where you hosted that party for the HAZUS group.

**Whitman:** Actually, we lived in a Techbuilt house prior to that. They are now recognized as historic. It was partially prefabricated and was designed by architect Carl Koch. After thirty-one years there, we moved to the house you recall, which was on a wonderful short street with lots of children. That house was of Civil War vintage.

## Chebeague Island

**Reitherman:** I believe you also have a summer house up in Maine?

**Whitman:** Chebeague Island is located in Casco Bay, near Portland. You get there from the mainland by a ferry boat ride about ten minutes long. It just carries passengers, no cars. It runs about every two hours. There is a larger ferry from Portland that comes less often, carrying passengers, and it occasionally carries a car if the tides are right and the vessel has sufficient draft. There's also a local barge service to transport vehicles. Chebeague Island is about four miles long and up to a little over a mile wide. The year-round population is about 350. In the summer that figure swells up by about four times. There's a nine-hole golf course, tennis courts, a library, recreation center, and an assisted living facility. About a third of the year-round residents are involved in fishing and lobstering, a third commute to the mainland for jobs there, and a third live primarily off the summer economy. From Chebeague Island, which is the largest island in Casco Bay, you can see several other islands. Visualize glaciers coming down to the sea and scooping out troughs, with the ridges in between now covered with water in some places, and creating linear islands.

While we're there, Betsy and I play golf, though we've finally given up tennis. I used to do a great deal of sailing. We had a Bullseye sloop, a famous Nat Herreshoff design from

the early twentieth century, originally made of wood, but which after World War II began to be made out of fiberglass. Herreshoff is famous for designing winning America's Cup yachts for several decades. Our sailboat has a twelve-and-a-half-foot water line with a full keel, and it's a very stable boat. We have spent many a fine day sailing on Casco Bay.

I maintain a summer vegetable garden, and Betsy maintains the yard landscaping and a cutting garden. There's a general store on the island where you can get some staples like milk, though every two weeks or so you go to the mainland to do more shopping for perishables.

**Reitherman:** When did you first go to the island?

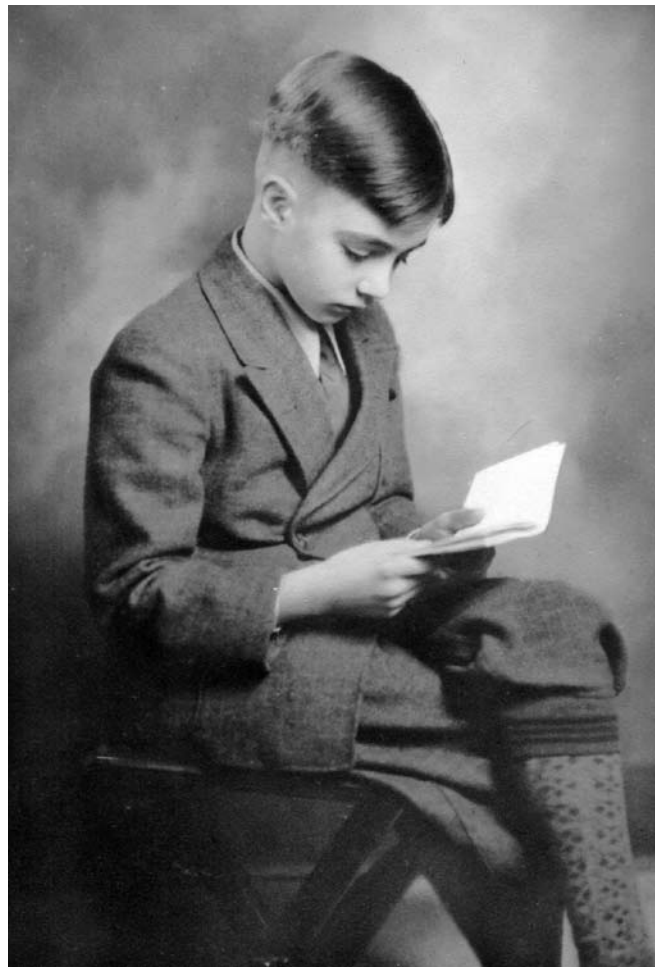
**Whitman:** In 1965. We rented a summer house for seven years, until it became clear that the owners were going to use it more. In 1972 we purchased a cottage that had been built in 1907. It isn't so small, having four bedrooms, but when you sit in the living room, you see the studs and the backside of the exterior sheathing. So in that part of the world, it's definitely just for summertime living.

**Reitherman:** How did you find Chebeague Island in the first place?

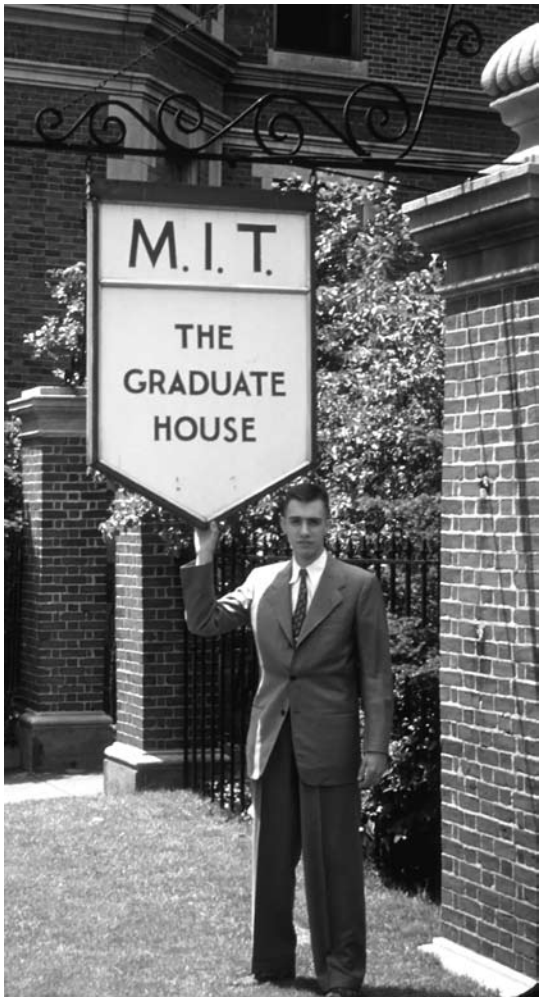
**Whitman:** It was my doctoral supervisor at MIT and later my mentor when I was on the faculty, Charles Norris, who had a summer house there and used to talk about it. One summer Betsy and I decided we wanted to spend our summer vacation on the Maine coast, and lo and behold there was an ad in the Sunday *Boston Globe* for a rental house on the island. By chance it was owned by someone from Lexington. We got the keys, went up and looked at it, decided we didn't like that particular place, but then the local taxi lady on the island drove us around telling us about what was available. She showed us a seven-bedroom house on a point of land. It was a wonderful place—one bedroom for drying the bathing suits, another devoted to the girls' dollhouses. That's the one we rented for seven years before taking ownership of our present cottage.

We have a number of good friends who live on the island year-round, some of whom help look after our place after we leave at the end of the summer. Betsy has been on the local land trust and has done archiving for the library and local historical society. While we can't vote on the island, we are avid followers of the local politics. In 1907, the island became a separate town, seceding from the mainland town of which it had been part for many years. That was an interesting process. Chebeague Island is a very pleasant part of our lives.

# Photographs



*The young scholar, circa 1938, age ten.*




*Bob Whitman as a graduate student at MIT.*



*Father, mother, and brother Phil.*



*Bob Whitman at his wedding with Elizabeth (Betsy) Cushman in 1954.*



*Bob and Betsy on their honeymoon on Nantucket Island.*



*Bob Whitman as an ensign in the U.S. Navy.*




*Betsy and Bob in Hawaii, when Bob was an officer in the U.S. Navy.*



*The Techbuilt home of Bob and Betsy Whitman in Lexington, Massachusetts.*



*A panel of geotechnical engineers at the 1960 ASCE Research Conference on the Shear Strength of Cohesive Soils; standing left to right: Stanley Wilson, Arthur Casagrande; seated left to right: A. A. Warlam, Robert Whitman, Alan Bishop, Wes Holtz, Woody Shockley.*



*Consultants at Lake Maracaibo, Venezuela. Left to right: Kenji Ishihara, Bill Marcuson, Robert Whitman, Arnold Verruijt, Killian De Fries.*



*Members of the committee, which was chaired by Bob Whitman, that oversaw the development of HAZUS. Left to right: Michael O'Rourke, Bill Holmes, Roger Borcherdt, Bob Whitman, Bob Reitherman, David Brookshire, Bob Olson, Richard Eisner, 1997.*



*Bob and Betsy Whitman dressed for a Scottish Country Dance ball.*



*The Whitman cottage on Chebeague Island, Maine.*



*The Whitman family at the Chebeague Island cottage. Left to right front row: daughter Gwen Kaebnick, Gwen's daughter Rebecca Kaebnick, David Marsee (son of daughter Jill), Gwen's other daughter Hannah, daughter Jill Whitman, and (on slightly higher step) Betsy Whitman. Back row: Gwen's husband Greg Kaebnick, Bob Whitman, Don Marsee (husband of Jill), and Bobby Marsee (Jill's other son).*



*Robert V. Whitman, EERI president 1985-1987.*

## A

Acceleration. *See* Ground motion
Advisory boards
American National Standards Institute (ANSI), 81
American Society of Civil Engineers (ASCE), ASCE 7, 81
Applied Technology Council (ATC), ATC 3, 80–81
HAZUS, 73–75, 85
Massachusetts state seismic code, 77–79
National Academy of Sciences (NAS), Committee on Earthquake Engineering Research, Soil Dynamics Panel, 73, 81
National Research Council (NRC): Committee on Earthquake Engineering Panel on Liquefaction, 44–45, 50, 73
National Research Council (NRC):Panel on Earthquake Loss Estimation Methodology, 48, 50, 72–73
National Science Foundation (NSF), Advisory Group on Earthquake Prediction and Hazard Mitigation, 81
U.S. Air Force Ballistic Missiles Division, Dynamic Forces Advisory Board, 21
U.S. Geological Survey (USGS), National Center for Earthquake Research, 81
Agarwal, P.K., 55
Agbabian, Mihran (Mike), 21
Aldrich, Harl, xiii
Algermissen, S.T. (Ted), 71, 72, 80, 81
Al Homoud, Azm, 38
Ambraseys, Nicholas N., 20
American National Standards Institute (ANSI), Whitman chair of advisory board for, 81
American Society of Civil Engineers
ASCE 7, 81
American Society of Civil Engineers (ASCE), 78
H. Bolton Seed Memorial Lecture, 59
Newmark Medal, 52
Research Conference on the Shear Strength of Cohesive Soils, 94
Technical Council on Lifeline Earthquake Engineering (TCLEE), 52
Terzaghi Lecture (1981), 48
Anagnos, Thalia, 73
Anagnostopoulos, Stavros, 69
Anchorage, Alaska earthquake (1964), 11, 23, 39, 79
Applied Technology Council (ATC)
ATC 3, 80–81
Arnold, Christopher, 73
Arulanandan, Kandiah (Arul), 58, 59

## B

Bertero, Vitelmo V., ix, 11
Biggs, John, 10
Bishop, Alan, 94
Bjerrum, Laurits, xiv, 17, 18, 34
Blair, Cynthia, 69
Blast engineering, 11, 14, 18–23, 33–34, 53–54
Blume, John A., 69
Bolt, Bruce A., 80
Borcherdt, Roger D., 95
Bouabid, Jawhar, 73
Bouckovalas, George, 38
British Geotechnical Association, Rankin Lecture, 52
Brookshire, David, 95
Building codes. *See* Seismic codes

## C

Caracas, Venezuela earthquake (1967), 38, 39, 61
Carpenter, Sam, 8
Casagrande, Arthur, xiv, 18–19, 34, 42–44, 49–51, 94
Castro, Gonzalo, xv, 43–44
Christian, John, 38, 69
Clough, R.W. (Ray), 11
Consulting projects, 61–65
  Alibey Dam, Turkey, 62–63
  Boston excavation for parking garage, 65–66
  British Columbia Hydro, 64–65
  Caracas, Venezuela high-rise, 62–63
  Lake Maricaibo dikes, Venezuela, 63–64
  oil storage tanks, 65
  radar facility, 23–24
Cornell, Allin, 7, 48, 62, 67, 72
Cushman, Elizabeth (Betsy), 27–31, 51, 92, 93, 95, 96
Czarnecki, Martin, 69

## D

Damage probability matrix, 48–49, 72–73
Damping, 24–25, 53–54
Dams
  Alibey Dam, Turkey, 62–63
  British Columbia Hydro, 64–65
De Fries, Killian, 63, 95
de Mello, Victor, 46, 63
de Neufville, Richard, 69
Dikes, 63–64
Dobry, Ricardo, xiv, 33–52
Dong, Weimin, 73
Donovan, Neville, 40, 80
Duke, C. Martin, 39, 43, 52
Dynamic finite element method, 54

## E

Earthquake Engineering Research Institute (EERI), vii, 9
  Whitman involvement in, 83–85
Earthquake loss estimation. *See* Loss Estimation
Earthquake risk management. *See* Risk management
Earthquakes
  Anchorage, Alaska (1964), 11, 23, 39, 79
  Caracas, Venezuela (1967), 38, 39, 61
  Gediz, Turkey (1970), 39
  Kanto, Japan (1923), 55
  Niigata, Japan (1964), 39
  San Fernando, California (1971), 23, 38, 48, 67, 68, 77, 79
Eisner, Richard, 95
Elms, David, 56
Excavation, deep, for Boston parking garage, 65–66

## F

Finn, Liam, 42, 43
Flooding, 64–65
Foundations. *See* also Geotechnical engineering and Retaining structures
  damping, 53–54
  elastic half-space theory, 23–25, 54
  mat, 62–63
  radar facilities, 23–25
  settlement, 23-25
  stability, 34
Freeman, John R., 9

## G

Gediz, Turkey earthquake (1970), 39
Geotechnical engineering
centrifuges, 57–59
damping, 24–25, 53-54
elastic half-space theory, 23–25, 54
evolution of, 14–25
excavations, 65–66
flooding, 64–65
nuclear explosions, 11, 22, 33–34
seismic codes, 77–81
sliding block analysis, 34–35, 56, 63
soil-structure interaction. *See* Soil-structure interaction
terminology, origin of, 15
wave propagation, 18–19, 21–25, 49, 53
Gilboy, Glennon, 15
Goulois, Alain, 38
Gow, Charles, 52
Gropius, Walter, 28
Ground motion, 23, 34–35, 48–50, 62
acceleration, 49, 51, 57–59
centrifuge testing, 57–59
contour mapping, 78, 80–81
damage levels, 72
damage probability matrix, 48–50
design, level used in, 68–70
liquefaction. *See* Liquefaction
location at which applied, 54
soil conditions, effect on, 39, 50
wave propagation, 18–25, 49, 53–55

## H

Hadjian, Asa H., 55
Hansen, Robert J., 10, 11, 14, 19, 23, 28
Hartford, Desmond, 64
HAZUS, 73–75, 85
Healy, Kent, 38
Hendron, Alfred (Skip), 41
Herreshoff, Nat, 89
Hoeg, Kaare, 38
Holley, Myle Jr., 10
Holmes, William T., 95
Holtz, Wes, 94
Hong, S.-T., 52
Housner, George W., 42, 52, 54

## I

Idriss, I.M. (Ed), xv, 39, 40, 55
Iglesia, Geraldo, 38
Ishihara, Kenji, 63, 95

## J

Jacobsen, Lydik S., 21
Johnson, Gary, 73

## K

Kaebnick, Greg, 96
Kaebnick, Gwen Whitman, 96
Kaebnick, Hannah, 96
Kaebnick, Rebecca, 96
Kanto, Japan earthquake (1923), 55
Kausel, E., 59
Kennedy, Robert, 55
Kircher, Charles A., 73, 74

Koch, Carl, 88
Krimgold, Frederick, 69
Kunreuther, Howard, 70
Kutter, Bruce, 57, 59

## L

Lagorio, Henry J. (Hank), 73
Lambe, Philip, 38
Lambe, T. William, xiii, 17, 18, 37, 45–47, 52, 59, 62
Lawson, Scott, 73
Lee, Ken, xv
Liao, Sam, 48
Lifelines, 52, 74, 79
Lin, Jeen-Shang, 38
Liquefaction, 20, 37, 40, 50, 63
  damage probability matrix, 48–49, 72–73
  effective stress approach, 47
  Lake Maricaibo dikes, Venezuela, 63–64
Loss estimation, 71–75. *See* also Risk managment
  damage probability matrix, 48–49, 72–73
  HAZUS, 73–75
  lifelines, 74
  SDDA. *See* Seismic Design Decision Analysis (SDDA) program at MIT
Luscher, Ulrich, 38, 62
Lysmer, John, 52, 54, 55, 59

## M

Marcuson, William, 35, 43, 52, 63–64, 95
Marsee, David, 96
Marsee, Donald, 96
Marsee, Jill Whitman, 96
Marsee, Robert, 96
Martin, Geoffrey, 50
Massachusetts Institute of Technology (MIT)
  Seismic Design Decision Analysis (SDDA) program, 48–49, 67–70, 72–73, 81
  soil mechanics program, 15
  teaching at, 33–52
  Whitman graduate education at, 7–18, 67
Massachusetts state seismic code, 77–79
Mazeroski, William (Bill), 5
McClure, Frank E., 84
McGuire, Robin, 62
Medvedev-Sponheuer-Karnik (MSK) Intensity Scale, 49, 72
Mello, Victor de, 46, 62
Merritt, R.G., 53
Miller, Charles, 38
Milne, John, 49
Mitchell, James, 47
Mohr, Harry, 52
Mononobe, M., 55–56
Mononobe-Okabe theory, 55–56
Monroe, Tappan, 69
Moore, Peter, 38
Moore, William W., 40

## N

Nadim, Farrokh, 38, 56
Namyet, Saul, 28
National Academy of Engineering (NAE), Whitman elected to, 70
National Academy of Sciences (NAS), Committee on Earthquake Engineering Research, Soil Dynamics Panel, 73, 81
National Center for Earthquake Engineering Research (NCEER), 49

National Earthquake Hazards Reduction Program (NEHRP), 79–80, 82, 84–85
National Research Council (NRC), 9
Committee on Earthquake Engineering Panel on Liquefaction, Whitman chair of, 44–45, 50, 73
Panel on Earthquake Loss Estimation Methodology, Whitman chair of, 48, 50, 72–73
National Science Foundation (NSF), 23, 50
Advisory Group on Earthquake Prediction and Hazard Mitigation, Whitman chair of, 81
Network for Earthquake Engineering Simulation (NEES), 59, 79
Network for Earthquake Engineering Simulation (NEES), 59
Neufville, Richard de, 69
Newman, Susan, 84
Newmark, Nathan, xv, 21, 34–35, 55, 56, 82
Niigata, Japan earthquake (1964), 39
Norris, Charles H., 9–12, 13, 36, 89
Nuclear explosions, 11, 22, 33–34. *See also* Blast engineering
Nuclear power plants
earthquake effects on, 11, 53
liquefaction effects on, 41
Whitman's geotechnical work for, 55–56
Nuclear Regulatory Commission (NRC), 54, 58

## O

Okabe, N., 55–56
Olson, Robert A., 95
Omori, Fusakichi, 49
O'Rourke, Michael, 95

## P

Paikowsky, Samuel, 38
Panama Canal, 34
Park, Robert, xiii
Paulay, Thomas, xiii
Peck, Ralph, xiv, 15, 16, 18, 21, 29, 52
Penzien, Joseph, 11, 23
Poulos, Steve, xv, 43–44
Press, Frank, 82
Protonotarios, John, 38

## R

Radar facilities, geotechnical studies for, 23–25
Rankine, William, 52
Rankine Lecture, British Geotechnical Association, 52
Reed, John W., 52, 69
Reitherman, Robert, 95
Research
centrifuges, 57–59
elastic half-space theory, 23–25, 54
nuclear explosions, 11, 22, 33–34
radar facilities, 23–25
strain rates, 20–25, 54
uncertainty in results, 54
wave propagation, 18–19, 21–25, 49, 53
Retaining structures, 34, 35
Boston excavation for parking garage, 65–66
earthen, 55–56
Richardson, Archie, 38
Richart, Frank (Bill), 15
Richart, William, xiv, 23, 24, 25

Risk management, 36, 63–64, 64, 65, 68. *See also* Loss estimation
contour mapping, 78, 80–81
damage probability matrix, 48–49, 72–73
HAZUS, 73–75, 85
SDDA. *See* Seismic Design Decision Analysis (SDDA) program at MIT
Root, Lloyd, 30
Roscoe, Ken, 57
Rowe, Peter, 46
Ruge, Arthur C., 10–11

## S

San Fernando, California earthquake (1971), 23, 38, 48, 67, 68, 77, 79
Sarrazin, Mauricio, 38
Schofield, Andrew, 44, 57
Scott, Ronald F., xiii, 59
SDDA. *See* Seismic Design Decision Analysis (SDDA) program at MIT
Seaman, Lynn, 22
Seed, H. Bolton (Harry), xiv, 20, 24, 27, 39–44, 50, 55–56, 59, 62
H. Bolton Seed Memorial Lecture, 59
Seismic codes
ATC 3, 80–82
Massachusetts state code, 77–78
SEAOC Blue Book, 68
Seismic Design Decision Analysis (SDDA) program at MIT, 48–49, 67–70, 72–73, 81
Seismic risk management. *See* Risk management
Shockley, Woody, 94
Sliding block analysis, 34–35, 56, 63
Slope stability, 14, 33–34
Panama Canal, 34
Soil-structure interaction
centrifuge research, 57–59
damping, 53–54
Soil dynamics, 15–16, 33–37. *See also* Geotechnical Engineering
amplification factors, 39–42, 50
blast loading, 18–23
cyclic loading, 20, 23, 25, 35, 42
damping, 24–25, 53–55
dynamic and static properties, 23–25
early days of, 14–26
failure point, 57
nuclear explosions, 11, 22, 33–34
pore pressure, 17, 20, 41–44, 58, 59
slope stability, 33–34
strain rates, 20–24, 54
strength of soils, 15, 18–25, 53
wave propagation, 18–25, 53
Solteriades, Michael, 38
Stamatopoulos, Constantine, 38
Stanford Research Institute (SRI), Whitman sabbatical year at, 22, 48
Steinbrugge, Karl, 71, 72, 79
Strain rates, 20–24, 54
Structural dynamics, early work on at MIT, 10–11, 15
Structural Engineers Association of California (SEAOC) Blue Book, 68
Swiger, William, 41

## T

Taylor, Donald W., xiii, 15, 18, 20, 34–35
Terzaghi, Karl von, xiv, 15, 16–20, 36, 45, 47, 52, 62

Thiel, Charles C. Jr., 79
Thompson, Benjamin, 28
Ting, Nai-Hsin, 38
Traynor, Pie, 4
Tubbesing, Susan K., 84

## U

United State Geological Survey (USGS), National Center for Earthquake Research, 81

## V

Van Duyne, Elsie (mother), 2, 92
Veletsos, Andrew, 56
Veneziano, Daniele, 69
Verruijt, Arnold, 63, 95

## W

Waner, Lloyd, 4
Waner, Paul, 4
Warlam, A.A., 94
Whitman, Edwin A (father), 1–2, 92
Whitman, Elizabeth (Betsy) Cushman, 27–31, 51, 92, 93, 95, 96
Whitman, Elsie Van Duyne (mother), 2, 92
Whitman, Gwen (daughter), 87, 96
Whitman, Jill (daughter), 87, 96
Whitman, Martha (daughter), 87
Whitman, Philip (brother), 2, 92
Whitman, Robert V.
  advisory boards. *See* Advisory boards
  Chebeague Island, Maine, 88–89, 96
  consulting projects. *See* Consulting projects
  education
    early, 3
    MIT, master's degree, 9–11
    MIT, ScD degree, 14–26
    Swarthmore College, 9
  family, 2, 27, 95
  military service, 21, 27–31, 93
  MIT. *See* Massachusetts Institute of Technology (MIT)
  National Academy of Engineering, election to, 70
  photographs, 91–96
  Symposium in Honor of, 14, 16, 35
  teaching, 33–39
  Terzaghi Lecture (1981), 48
  Town Meeting member, Lexington, Massachusetts, 51–52
Wilbur, John B., 11, 12, 15
Wilson, Stanley, 22, 24, 94
Worcester, Thomas, 27

## Y

Yegian, Mishac K., xiv, 33–52, 69
Youd, T. Leslie, 42

## Z

Zeizel, Art, 73