CONNECTIONS

The EERI Oral History Series

John A. Blume

Stanley Scott
Interviewer

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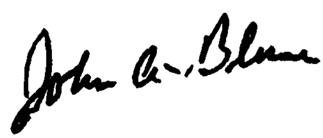

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Earthquake Engineering Research Institute

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

The opinions expressed in this publication are those of the oral history subject and do not necessarily reflect the opinions or policies of the Earthquake Engineering Research Institute or the University of California.

Scott, Stanley, 1921-
John A. Blume/Stanley Scott, interviewer.
p. cm. -- (Connections: the EERI oral history series)
Includes index.
ISBN 0-943198-44-5
1. Blume, John A. 1909- --Interviews. 2. Civil engineers--California--Interviews. 3. Earthquake engineering--California--History. 4. Earthquake resistant design--History. I. Blume, John A., 1909- . II. Title. III. Series.
TA140.B64S36 1994
624'.092--dc20 94-39697
[B] CIP

Published by the Earthquake Engineering Research Institute

499 14th Street, Suite 320
Oakland, CA 94612-1934
Tel: (510) 451-0905
Fax: (510) 451-5411

EERI Publication No. OHS-2
Printed in the United States of America

1 2 3 4 5 6 7 8 00 99 98 97 96 95 94

Acknowledgments

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

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

The EERI Oral History Series

This is the second volume in *Connections: The EERI Oral History Series.* The Earthquake Engineering Research Institute has initiated this series to preserve some of the rich history of those who have pioneered in the field of earthquake engineering and seismic design. The field of earthquake engineering has undergone significant, even revolutionary, changes since individuals first began thinking about how to design structures that would survive earthquakes.

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

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

Recognizing the historical importance of the work that earthquake engineers and others have been doing, Scott began recording interviews in 1984. The wealth of information obtained from these interviews led him to consider initiating an oral history project on earthquake engineering and seismic safety policy. Oral history interviews involve an interviewee and interviewer in recorded conversational discussions of agreed-upon topics. After transcription, revision, and editing, the interviews and the tapes are placed in the Bancroft Library at the University of California at Berkeley for research purposes and scholarly use. Occasionally, interested professional organizations sponsor publication and wider distribution of interviews, as the Earthquake Engineering Research Institute is doing with *Connections.*

In due course, the Regional Oral History Office of the Bancroft Library approved such an oral history project on a continuing, but unfunded, basis. First undertaken while Scott was employed by the Institute of Governmental Studies, University of California at Berkeley, the effort has been continued on his own, following his retirement in 1989. Modest funding for some expenses has been provided by the National Science Foundation. The John A. Blume Foundation also made a contribution.

Scott's initial effort has grown into a more extensive program of interviews with earthquake engineers who have been particularly active in seismic safety policy and practice. Key members of the Earthquake Engineering Research Institute became interested in the project when asked to read and advise on the oral history transcripts. The suggestion that EERI publish interviews with Henry J. Degenkolb, and perhaps others, led to a formal decision that EERI initiate an oral history series, which continues with this volume.

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

EERI Oral History Series

Henry J. Degenkolb	1994
John A. Blume	1994

Interviews completed or nearing completion include:

John E. Rinne	Michael V. Pregnoff
George W. Housner	William T. Wheeler
William W. Moore	

Interviews with several others are in progress.

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Foreword

The interviews with John A. Blume were all conducted in his comfortable home in Hillsborough from March 1987 through January 1988. The sessions typically ran one-and-a-half to two hours. John talked extemporaneously, using only a brief topical outline that he had sketched out, often on the back of an envelope. His conversation was extremely well organized, which typifies the way he has always worked, organized his thoughts, and handled discussions. For the most part the transcribed text required only very minor subsequent editing. The initial transcriptions of his remarks were clear and lucid enough to appear in print virtually unchanged.

In between interview sessions, John did a good deal of homework, checking old files, and referring to publications and lists. In reviewing the transcripts, he read successive drafts punctually and carefully, correcting where necessary, and sometimes adding new material when he wanted to elaborate a point, or shed additional light on a topic.

Because of John's other commitments, the interviews were spread out over more than a full year. When the protracted interview sessions began drawing to a close, I felt real twinges of regret, as I had come to look forward to those meetings with considerable anticipation. They were thoroughly interesting, stimulating, and challenging. For me, they were really enjoyable—fun rather than work. Interviewing John was something like sitting with a good old friend or well-traveled uncle and prompting him to recount his adventures over the years. His penchant for being well organized made the interviews, in many respects, among the easiest to conduct of all the oral history sessions I have been involved in. Moreover, in the process I learned a lot more about earthquake engineering and the conditions that influence the practice of earthquake engineers.

This oral history series chronicles the thinking of such prime figures as John Blume—and other remarkable practitioners like Henry Degenkolb, Mike Pregnoff, and William Moore, to name only a few. Perhaps these men and their history may help many students along on the road to becoming better engineers. In the process, they will also learn more about some wonderful human beings—and fine engineers—and see powerfully reaffirmed the truth that there is much more to the practice of structural engineering than following the codes or learning textbook techniques of design.

The philosophical, humane, ethical, socially responsible side of good engineering practice shines through in the recollections and reflections contained in this oral history series. This is, indeed, one of the great strengths and values of the whole oral history enterprise: its ability to open new windows on the lives of distinguished predecessors and contemporaries.

Rubbing elbows with a discipline's elder statesmen in this way helps us see how they conducted highly successful practices and professional lives. Oral history helps shed light on aspects of a discipline that may be discussed informally, but are usually not written down or addressed in books and journal articles. Personal memoirs like these of John Blume capture and record such evidence, saving otherwise transient information for the permanent record, where it has great potential for use in helping inform and educate new generations of professionals.

Stanley Scott
Research Political Scientist, Retired
Institute of Governmental Studies
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Introduction

The extraordinary career of John A. Blume, which spans more than fifty years, is characterized by his contributions to dynamic theory, soil-structure interaction, and the inelastic behavior of structures. His seemingly limitless energy and determination enabled him to be both a researcher and practitioner, and to excel in both.

Blume entered Stanford University as a freshman in January 1929. He graduated with a Bachelor of Arts degree in Engineering in 1933. While studying for his Engineer's Degree, Blume worked with Lydik Jacobsen, a mechanical engineer, mathematician, and early proponent of dynamic theory. Blume came to see that far-reaching advances in structural performance could be made by combining dynamic theory and structural engineering.

Blume's thesis, "The Reconciliation of the Computed and Observed Periods of Vibration of a Fifteen-Story Building," was a pioneering effort in the dynamic analysis of a highrise building. Blume was convinced that in order for buildings to withstand severe earthquake loading, both elastic and inelastic ranges of motion had to be understood and considered in design. This was a revolutionary theory that Blume would continue to refine and push for inclusion in building codes and engineering design practice for the next fifty years.

Thirty years after leaving Stanford, Blume returned for his Ph.D., taking a half-time course load while running his business full time. His dissertation was entitled "The Dynamic Behavior of Multistory Buildings with Various Stiffness Characteristics."

Among numerous honors and awards for his work, Blume received the Leon S. Moisseiff Medal (three times) from the American Society of Civil Engineers, the Medal of the Seismological Society of America, and the Housner Medal of the Earthquake Engineering Research Institute. The John A. Blume Earthquake Engineering Center at Stanford University is devoted to the advancement of research and practice in earthquake engineering.

Blume was a founding member of the Earthquake Engineering Research Institute, and later served as President. He helped organize the First World Conference on

Earthquake Engineering, held in Berkeley, California, in 1956, and presented papers at every World Conference for the next twenty years.

Over the years, Blume authored over 190 papers and books and gave over 300 talks and lectures. His contributions to advances in ductile concrete, energy dissipation, dynamic response of structures, soil-structure interaction, unreinforced masonry, and lifelong efforts to include inelasticity and dynamics in design codes have led to innovative and very real improvements in seismic design and practice. In his own words, Blume "simply lived and breathed earthquake matters for decades."

A Personal Introduction

I first met John Blume shortly after the end of World War II in late 1945. He had just opened his own office, and I was still in uniform with a few days' leave between duty assignments. I was looking up one of my college classmates, Don Teixeira, who was John's first employee. John, Don, and one other engineer occupied a small narrow office at 68 Post Street in San Francisco. The office resembled a hallway more than anything else, with three drafting tables in a row and a small window at one end. Even in this brief meeting, I was impressed by John's exuberant energy and friendly interest in my naval career.

On leaving the service in mid-1946, I went to work for a general contractor as a field engineer. When the project was completed in early 1947, I learned that my next assignment would be in Hurlong, Nevada, and I decided to explore other options. Don told me that John Blume was very busy and looking for another engineer, and I was hired as his fifth employee. By then, John had moved into a two-room office, with four engineers crowded into the back office. I was given a drafting table behind John's desk in the outer office.

The dress code for engineers in those days was a business suit with tie, and almost everyone wore a hat. Small green aprons, furnished gratis by the blueprint and drafting supply shops, were worn to protect our white shirts at the drafting table. The drafting tables were generally supported on saw horses and covered with heavy tan detail paper to provide a smooth surface. Tied to each table was a sandpaper spatula to sharpen pencils and a rag to wipe the excess graphite and pencil dust.

We all did our own drafting. John was a terrific draftsman, but he was soon too busy to do any drafting. For many years he kept a drafting table in his office, but it was always covered with copies of technical papers and reports. He was always preoccupied with the several jobs we had going concurrently, all of which demanded his immediate attention. He would sometimes stride furtively through the drafting room, hoping no one would intercept him before he got to his office. He managed to provide detailed guidance and criticism to the staff by the memos we all referred to as "Blume-o-grams."

As the office prospered over the years, we moved to larger and more comfortable quarters, and our staff became larger. One unforgettable character was a young Mexican-American draftsman named Ray Ferniza. Ray was not only a good draftsman, but an excellent mimic and a natural clown. He would invent nicknames for everyone—staff, associates, clients—and mimic them with a few physical characteristics that were instantly recognizable. His nickname for John was "El Gavilan," the hawk. He would mimic John by floating around the drafting room with a dark piercing gaze highlighted by a small black mustache. I don't know if John was aware of Ray's antics, but I know that he was fond of Ray and would probably have been more amused than annoyed.

John was a gifted engineer, with good business sense and the ability to attract important clients. As our projects became larger and more numerous, the demands on John's time increased exponentially. He was soon forced to delegate responsibility and usually had one of us get involved in most projects. He would often thrust us on an unsuspecting client as his stand-in. It was baptism by fire. The authority and responsibility John gave us in dealing with important clients provided many Blume employees with the confidence and experience to later strike out on their own.

The first ten years or so, our projects were conventional civil and structural engineering assignments (port facilities, military installations, schools, telephone buildings, etc.). Although special attention was paid to good detailing (probably as a result of John's prior association with H.J. Brunnier and Standard Oil), earthquake engineering was pretty much in accordance with code provisions, which were fairly minimal. John's underlying interest and pioneering efforts in structural dynamics enabled him to obtain commissions for a few special studies for knowledgeable clients like the telephone company, but it was difficult to convince clients that the code provisions were inadequate. The work by Blume and others in the development of the early quasi-dynamic seismic code provisions, as well as the damage studies from the El Centro [1949], Taft [1952, also called the Kern County earthquake] and Olympia [1940, also called the Puget Sound earthquake] earthquakes helped to raise the level of public awareness of seismic hazards. John's papers on the reserve energy technique in 1960 helped to explain how some structures were able to resist earthquake forces far in excess of their design capacity.

Largely through John's growing reputation for innovative approaches to earthquake engineering, the firm began to attract clients who wanted a high level of confidence in expected seismic performance—as in the case of structures housing critical facilities such as nuclear power plants or other important structures like vibration-sensitive research centers or highrise buildings with large numbers of occupants. Much of this work has been in a consultative capacity, with the firm advising and assisting other engineers.

I believe that it was the opportunity to participate with John in avant garde earthquake engineering that attracted the "best and brightest" of the young engineers to come work with him. Many of them have since gone on to make valuable contributions to the practice of earthquake engineering.

I have been privileged to have been associated with John Blume since he hired me in 1947, through the merger with the URS Corporation in 1971, and as President of URS/John A. Blume & Associates. I retired in 1987, but still work as a consultant for the company. About once or twice a year, several of us early Blume employees get together with John for a nostalgic lunch.

Joseph P. Nicoletti
URS/John A. Blume & Associates

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John A. Blume

Chapter 1

Before College

"As a teenager and youngster in San Francisco... I felt many minor earthquakes, so I was quite interested in the subject."

Scott: Maybe we could begin with a little about your family background.

Blume: My father's father was a Swedish sea captain, who came to this country at the start of the Civil War, and got a job right away with the U.S. Navy because of his sea experience. He served throughout the Civil War, mostly under Farragut. He has written his history, which I have to work over. Grandpa Blume [Nils August Blume] was quite active until he died of influenza at the age of 99. He had a very active and interesting life, not only in the Civil War, but for many decades. He dictated an autobiography for family use when he was in his 70s or 80s. I hope to dig this out and do something with it. He would not allow Swedish to be spoken in his house and yet he never lost his Swedish accent!

Grandpa Blume was living in San Francisco in 1906, the whole family was, and barely survived that earthquake. I wasn't born yet, of course, but in my early childhood I heard many stories—all true, albeit exaggerated now and then—about that earthquake and what it did. The wood house they were living in collapsed in the lower story, and some people were killed. The family walked out of the windows of the second and third stories to get out onto the street. Subsequently the house was burned to the ground in the fire that ensued.

Scott: The sea captain was your father's father?

Blume: Yes. My father and my mother were not married yet in 1906, though they both lived in San Francisco. My father's father, Nils August Blume, married a French girl in 1869 who was about to enter a convent in Honolulu. Her name was Pauline Challamel. They had three daughters and two sons, one of whom, Charles August Blume, was my father. My father and mother, Vashti Rankin, met (I presume in San Francisco) sometime after the 1906 earthquake but well before I was born, April 8, 1909. Her father, John Edward Rankin, was born in Ireland and apparently came to the USA as a boy. He worked on a farm in Michigan until the Civil War. He served in the cavalry from Michigan for the duration, and was in most of the major engagements, except while he was hospitalized. When he became ambulatory he helped take care of the wounded and acquired an interest in medicine. Near the end of the war, he was in the party that "captured" Jefferson Davis.

After the war, he studied medicine and became a practicing doctor, first in the east and then in Gonzales, California. He married Phoebe Lane of Buffalo, who was of English descent but (I believe) born in the USA. They had one daughter, Vashti, who was my mother, and an adopted daughter, Cora. Anyway, my mother's father, Dr. John Edward Rankin, was a country doctor living in Gonzales, California [south of San Francisco in the Salinas Valley]. My mother-to-be was working in San Francisco as a designer of women's dresses. She had her own business. She was very artistic—painting, poetry, drama, design. After the earthquake she went back to Gonzales to live with her family for a while, until the city got straightened out. Somewhere along the line, my mother and father met and got married. I was born in Gonzales in 1909. I mention all these things because throughout my childhood I heard all these stories about the earthquake and the fire. Also as a teenager and youngster in San Francisco—I was raised in San Francisco mainly—I felt many minor earthquakes, so I was quite interested in the subject.

My father was a handsome, active, strong macho type, and my mother was a gentle, talented, artistic, beautiful woman—not at all the same type. I have mixed blood as well—Irish, French, English, Swedish and Danish (Nils Blume had Danish blood). It is little wonder that I am so complicated. All of my male ancestors in the Scandinavian countries were professional men—civil engineers, doctors, ministers, and a sea captain—each one his own boss; it seems to run in the family. My mother died when I was three years old—a tragedy. My father remarried a couple of years later, to Alice Holland, who did a fine job of raising me. I have a half sister, Beverly Mae Dalton.

Three Years in Hawaii

Blume: I dropped out of school when my father obtained contracts in the Hawaiian Islands. He was a steel-erecting contractor. To digress a moment, he built most of early San Francisco, including the City Hall that's now there, and the civic auditorium, and many of the theaters and big buildings of the 1900 to 1924 era. He had little education, but he was a self-taught, brilliant man and very hardworking. He obtained contracts in the Hawaiian Islands to erect buildings and towers and gas holders. He took me and my stepmother with him. So we went to Honolulu where I had an exciting three years.

Scott: When was that?

Blume: That was about 1922, '23, and '24. I was about 13 when we moved. I was a beachcomber, and became quite a proficient swimmer and surfer. When I needed spending money, I took tourists out surfing and got what little I needed that way. I engaged in every form of water sport for the Outrigger Canoe Club in the Islands—water polo, canoe racing, swimming, surfing, sailing, volleyball, all the beach sports—until my father obtained a contract in Hilo, Hawaii to build some radio towers for the Navy. As I recall they were 150 feet high. I went to work on that job as an apprentice ironworker. Even though I was just a teenager, I was very large for my age, and strong. To make a long story somewhat shorter, I spent about a year, or year and a half, as an ironworker on buildings, radio towers, gas holders, various structures. For some reason I had no fear of height, except I was always afraid my father would fall. I never worried about myself. He in turn worried about me. It was quite a team.

My original intentions were to follow an athletic career, mainly in long-distance swimming, although in those days there was no money in it. But lo and behold, I developed a problem with my heart. They called it "athletic heart," and I've been told since by many doctors there's no such thing, so it must have been something else. They put me in bed for many months, and fed me digitalis by the bottle. I finally got out of that mess, and came back to the mainland with my family. We again lived in San Francisco. I was medically barred from athletic competition all the way through the rest of high school. I went to Lowell High School in San Francisco. They wanted me to convalesce further without getting into competition. This turned me against swimming and water sports.

I forgot to mention that in the islands for two to three years I did not go to school at all. I was either beachcombing or working as an ironworker. I had a lot of fun. I have been a very active person all of my life. The islands were just perfect for that activity. Coming back to school, I was able to skip a year or more. I don't know how I did it, but I took some examinations and skipped some years of schooling, including the eighth grade of grammar school.

Scott: So you caught up on some of what you'd skipped or missed?

Blume: Yes, I caught up on about half of the time I had missed. In high school I was active in everything except competitive sports—glee club, quartet, drama, chemistry club, music, all those things. I also tried to help out financially because my father got into trouble with some of his contracts and lost a lot of money. I took summer jobs, and I took Saturday work.

Furniture Hauling and the Santa Barbara Earthquake

Blume: One of the most interesting summer jobs [summer of 1925, at age 16] I had was working on a long-distance moving van, hauling furniture between the San Francisco Bay Area and the Los Angeles and San Diego areas. The truck I worked on was a solid-tire Mack truck with a chain drive, and it hauled a four-wheel trailer behind it. It wasn't a semi-unit, they didn't have them then. Here I was barred from athletics for medical reasons, and yet I was carrying pianos upstairs! And worse than a piano is a round barrel of books—hard to hold, and very, very heavy—just a staggering load. I did all this and it didn't seem to bother me at all.

We came into Santa Barbara one night in June 1925, after driving half the night. We always slept in the truck, and never went to a motel or hotel. Early the next morning the Santa Barbara earthquake occurred, while we were parked there. So here I was—after hearing about earthquakes, now I was in one. It was a big quake, even though it was not great on the Richter scale. It was a very sharp and hard-shaking. Many buildings were damaged, and I believe about fifteen people were killed. We helped in search and rescue work in the ruins.

Even though my experience was mostly as an ironworker, I'd also worked part time as a carpenter and a laborer, and I was astounded at the way those buildings were put together—or, rather, not put together—poor workmanship, and poor detailing of the connections. So I made myself a vow, then and there, that someday I would do something about it. I had the double background of this 1906 history, and being in the Santa Barbara earthquake of '25, and I think I've done a little about it. I've worked very hard at it for a long time.

Scott: Would you say a little about the Mack truck with the solid tires and chain drive? That must have been quite an experience driving those trucks, riding in one of them for long trips to L.A. and back. I remember those trucks from my early youth in the Texas panhandle. I'd seen a few, but mostly they just went around within the little agricultural trading town that we came to for provisions now and then. I didn't know that they did cross-country runs.

Blume: I'm not sure the truck was designed for what we used it for, but we jammed the truck full of furniture, hooked a trailer on behind and jammed that full of furniture, and started out. We'd often be gone 6-8-9 days before we'd get home. We would never get to a bed. We'd just sleep on the truck while it was going. I thought it was high adventure, and enjoyed it very much. To keep our strength up, and also because we were young kids, always very hungry, we'd stop for a rib steak and chili beans about every four hours, around the clock. We knew all the best places to go.

The truck was not designed for high-speed work, but we probably did about 50 miles an hour at top speed on the level. When we'd come to an uphill grade we'd have to shift down to low gear, and if not driving at the time I'd often get out and walk alongside the truck on a steep mountain road; I could walk faster than the truck was going. I recall going over the old Grapevine Road into Los Angeles. I also recall going down the coast, and coming into Santa Barbara from what is now the Vandenberg area. The old highway used to be like a roller coaster, going up and down. We'd throw the truck out of gear going down and just go like a bat. When it would hit the bottom of the valley the wheels would follow the road surface and start the climb up, but the mass of the sprung weight, namely the truck body and the furniture, in full compliance with the laws of momentum and Newton, would want to keep going down grade on a straight line. The net result would be much groaning and scraping, not to mention impact, as the truck and the tires were jammed together. It's a wonder it didn't all fall apart. It was strictly against the law, I'm sure, to throw a truck out of gear, but we knew we had a way to stop it on the hill on the other side of each valley or dip.

It was very noisy, trying to sleep on the truck. While one fellow was driving, the other would lie down on the floor boards. The other would be behind the engine, and you would have the heat of the engine and the noise of the transmission, the gears shifting. You didn't get much sleep, but somehow nature took care of you.

Wide Variety of Jobs: Invaluable Experience

Blume: I did the moving van work during most of one summer vacation. A few years later [during college] I drove dump trucks on highway work. In this case I was assigned a big dump truck, with a double transmission, which, as I recall, gave me 12 forward speeds and three in reverse, and I had to use all of them. We worked on the new highway cutoff in Marin County, hauling crushed rock and its dust out of the Hutchinson Quarry near San Rafael to make a base for a segment of the new highway, the one that's now the official 101. It was fill—fill onto and into mud. On that job, the owner of the truck—my boss—got paid by the ton of material hauled. We'd load up in a bunker at the quarry, and the instructions were to put as much material in the truck as we could, and drive as fast as we could. This resulted in spilling on the highway occasionally, and an average of four or five traffic tickets per day, which we methodically mailed in to the boss every night.

Scott: And he was willing to pay, because that meant more tonnage?

Blume: That's right. I guess he made a net profit out of the whole thing, but the police threatened to shut us down altogether, and we had to correct our ways.

Actually, all the way through high school and college I worked at something or other during every vacation. I've driven about every kind of a truck there is. I also worked building houses—as a carpenter, laborer, concrete worker, cement finisher, mason. The only two trades I haven't done much of are plumbing and wiring, although I've done a little of those, too. All of this experience with my hands came in very good for me later on in life. I think the experience of making things go together, and making them work, was invaluable to me as an engineer later on. Also, I enjoyed construction work as a hobby. I designed the house I live in now, and did all the outside work, I also built two separate buildings in the back, all by myself. One building is a nice studio building, which matches the house in finish and architecture. I built that in 1979, when I was 70 years old, and I deliberately did not use any power tools. I wanted to see if I could do it by hand, and I did it. Two years prior to that, in 1977, I put a new roof on the main building—a new shake roof, all by myself. That was a very dirty, hot job, but it was fun to be up there at tree height with the squirrels and bluejays. Many people came by, and some said, "That's a nice-looking job, when you're done will you come up to my place and give me a bid?" So I've enjoyed working in construction all my life.

Chapter 2

Years at Stanford

"As a young upstart student I could see quite an opportunity here to bridge the gap between the two...combining structural engineering and dynamics in the earthquake field."

Blume: I graduated from Lowell High School in December 1928. I had previously taken the college entrance examinations, and applied at Stanford. I was allowed to enter in January 1929. It was a class of only 50. As I recall, they let 50 enter in the winter quarter in those days, probably to make up for drop-outs in the fall quarter. I found entering in the winter a little disconcerting, because you're not exactly among your peers in class and elsewhere. If I had to do it over, I'd wait. I'd go truck driving again for a few months, or steel erecting, and enter in the fall quarter with everyone else.

I was not quite sure when I first entered Stanford whether I was going into engineering or medicine. I had the engineering desire from the earthquakes I'd been in and heard about. I had a little medical desire from the fact that my grandfather, my mother's father, was a country doctor. For the first year or two I took a lot of chemistry and hard science, and math, and I finally decided to go for engineering—earthquake engineering—even though there was no such thing in the curriculum. One reason I chose Stanford was that they had worked on the shaking table down there, and also had a background of earthquake damage in 1906—very severe damage, by the way. And Bailey Willis had attracted my eye. He was the ebullient geology professor who literally bounced when he walked. He and Andy Lawson, the UC Berkeley geologist, were not only friends, but competitors. I've heard them argue over various things such as the foundation stability of the Golden Gate Bridge.

There was no program in earthquake engineering at Stanford, or anywhere else for that matter, so I more or less had to write my own program. I majored mainly in structures, but I took courses in every other department that I could that might have a bearing on earthquake engineering. For example, I took every course given by Professor Lydik Jacobsen, who was a mechanical engineer and a physicist, and whose specialty was mechanical vibrations and dynamics. I took courses in the aeronautical department, to get advanced structural analysis. I took all the geology I could fit in, which was quite a bit. I took all the math I could get.

Worked Way Through College

Blume: I worked my way through Stanford. My father went broke in the 1929 crash, not only because of the property he lost to mortgage foreclosures, but also because some major contracts of his with the Navy at Pearl Harbor had gone bad. It involved concrete work, and as I understand it, the Navy inspectors insisted on dry concrete with very little water, because they'd just heard of the water-cement ratio law put out by Duff Abrams. If concrete is over-wet it loses its strength—that is true—but on the other hand it has to have enough plasticity to be placed in the forms properly and to fill the voids. They made him use such dry concrete that he couldn't place the material economically or properly. This was a case of overkill with the new Duff Abrams law. My father sued the Navy and it was in the courts for years. The case was thrown out on some technicality. I mention this here only because he fully intended to help me get through Stanford, but he just couldn't do it. I offered to quit Stanford and come home to help out, but he said, "You'd be another mouth to feed. You'd be better off to go to school."

You'll recall that it was October 1929 when the stock market crash came, and I entered Stanford in January 1929, so I was a freshman. I'll never forget the professor of economics, who was late for his October day class. He came into class waving a newspaper with big headlines about the crash. He said, "The bears have the bulls by the balls, class dismissed."

So my father was unable to help me through Stanford, but I managed to work my way through with various jobs. The principal job I had, starting with my second year and lasting for five more years, was as a waiter at a French restaurant in Palo Alto. The restaurant was called L'Aiglon. I believe it means "the eagle." It was run by a French chef and his wife. He did all the cooking, and she took care of the dining room area. The waiters were all students from Stanford. I became head waiter a couple of years later. They served a wonderful dinner with choices on each course, four or five courses, for fifty cents. This included a nice-sized filet mignon steak, if you preferred. On Sundays the price went up to seventy-five cents, and they put on two extra courses. The food was excellent.

I actually waited tables there six to seven days a week, for five years. Being in downtown Palo Alto, it was quite a chore to get to class on time after a luncheon. As an engineering student I had laboratories every afternoon from 1 to 4, while the economics majors and others were taking my girl friends out canoeing on Lake Lagunita.

In addition to waiting on tables at the French restaurant, which by the way was at 173 Litton Avenue, Palo Alto—I remember the address—I worked as a tutor, as an assistant coach in swimming and cross-country running. By the way, shortly after I entered Stanford, I had thrown away the medical limitations about exercise. I also obtained many odd jobs—I could write a book about the odd jobs I've had over the years. I worked in a cannery, a butcher shop, as second cook in a hotel restaurant—and I knew nothing at all about cooking. I've had some very interesting odd jobs.

Scott: Was this also during your Stanford period?

Blume: Yes, all this is during Stanford. I had a problem with transportation, with getting back and forth between the university and Palo Alto. I obtained an old Studebaker roadster, which was not running, but which I thought I could make run. I bought it for $25, after borrowing the $25 with no collateral. I found the main problem with the car was the water pump shaft, which had been practically worn through and leaked like a sieve.

The car was a large Studebaker, and in order to fix the water pump I had to take off the radiator, pull everything out with wheel pullers and put in a new water pump. I got the parts by going to junk yards and cannibalizing other Studebakers. I worked on this car for a week during spring vacation, and got it running. But I didn't have the know-how to time the thing, so it sounded like a concrete mixer. I went to a mechanic shop and garage, and told a mechanic about my problem. He said he could fix it, but did I have any money? I told him no, I'd like to get time on the money. He said, "What have you done to this car yourself?" I told him, and he said, "Anybody who'd do that, I'll fix it for nothing." So he did. He timed the car and it ran beautifully. I used it for five or six years, even driving up and down the highway to San Francisco. It had no top, and when it rained I simply drove faster and leaned down close to the windshield and let most of the rain go over the top of my head. I had a lot of fun with that car, and got a lot of use out of it.

The starter wouldn't work and I had to crank it. On a cold, wintry morning I had a standard procedure. I'd pull the choke out all the way, leave the ignition off and take two half-turns on the crank. Then I'd put the choke in half-way, turn the ignition on and give it a good crank. If it didn't start, I'd start walking. But it started about two times out of three that way. That car and I were inseparable, except for the starting problems.

Graduation and Work in Cannery

Blume: I graduated cum laude [course work completed December 1932, diploma received June 1933], and found that the world did not then have any engineering jobs to offer, so I went to work in the summer of 1933 at a cannery in San Jose, helping to pack fruit in cans. I started out at 25 cents an hour, but soon got promoted to 37.5 cents an hour, working as what they called a checker. A checker is one who takes the canned fruit from the girls who are doing the canning, and gives them credit for what they've done. They get paid piece work. That was quite a job. I worked about 8-10 hours on canning, and about two or three more hours washing down the place afterwards, and eating all the fruit I could until I couldn't eat any more. After going through this for almost two months—apricots, pears, peaches, tomatoes—I said "This is not the life. I can make more money when I'm going to school than I am here."

Graduate Studies

Blume: So I went back to Stanford [that fall, 1933] and completed my two-year graduate course for the degree of engineer. I made more while I was going to school than I could full-time on the outside. But I was busy. I had five or six jobs. I had one for my room, one for my meals at the restaurant, another for a little cash. I was taking heavy courses. I was very, very busy.

Scott: How did you find the time to study?

Blume: That's a very good question. Fortunately, as a rule, throughout my six years at Stanford I got by with very little sleep. Six hours was about my average. And I also had time for athletics. I played handball on the team. I threw the javelin, but I stayed away from swimming. I was also in the Stanford Glee Club for awhile, until I didn't have the time for that any more. That was a lot of fun too. My engineering courses were very time-demanding, especially the laboratory work in electrical and mechanical engineering. Each week I'd spend three hours in the lab, and then I'd have to spend five or six hours writing a report on what we did, all for one unit of credit.

Scott: Those kinds of courses do demand a lot of time.

Blume: And I had many of those. So I was always busy, rushing from one place to another, though it seemed to agree with me, and I got by all right. But if I had it to do over, I'd like more time to think about things. I went to Stanford for a total of six years, the first time, getting two degrees [Bachelor of Arts, June 1933; Engineer's Degree, June 1935].

Scott: Still, your academic work didn't seem to suffer. You evidently did well.

Blume: I did well, although I could have done better if I'd had the time to study more. I think my average was probably about an A- or a B+, something of that sort. I was elected to Tau Beta Pi and Sigma Xi honorary societies. But I don't recommend the procedure I had for everybody. It's too hard on the system, and I think you'd get more out of the school if you had more leisure time. I had practically no leisure time that I didn't steal from something else.

Thesis Project: The Alexander Building

Blume: In my first year of graduate work I began doing work in earthquake engineering, in addition to studying. First of all I got involved in a thesis with my thesis partner, Harry Hesselmeyer. This thesis was "The Reconciliation of the Computed and Observed

Periods of Vibration of a Fifteen-Story Building."[1]

The building we selected was the Alexander Building at the corner of Bush and Montgomery Streets in San Francisco, fifteen stories above grade. We considered about 40 other buildings before selecting this one. We chose it because of its symmetry and being on a corner, and being rectangular in plan view, and above all, we were able to get permission to use it, which was not easy. We were also able to get all the plans—the architectural plans and the structural plans. Our thesis advisor was Lydik Jacobsen. He was my advisor then, but also we became close friends later on.

We had no digital computers or electronic devices, but we did have an electric calculator. It was a Marchand as I recall. We literally wore that machine out on this thesis study.

Scott: The Marchand was a regular electric calculator?

Blume: Yes, a great big unit about a foot-and-a-half square. Our problem involved the solution of fifteen simultaneous equations, one for each story of the building, over and over and over again with different constants. By trial and error, which today we'd call iteration, we were closing in on the true parameters of the structure. In other words we were developing its characteristics—stiffness, vibration modes, and how the various materials—the steel frame, the concrete fireproofing, the brick masonry walls—how these things all work together to form a dynamic unit. This type of work had never been done before in all the history of earthquake engineering. It was a real pioneering effort. I've got the thesis in that red book there.

Scott: I've heard about it and seen it referred to.

Blume: Harry and I got along very well working together. He was a very quiet type, and I was probably the opposite, but we meshed well. What one didn't think of, the other did.

Scott: Was this done as a joint thesis?

Blume: Yes, a joint thesis for the Engineer's Degree at Stanford. We were asked to give a talk on the results at the annual meeting of the Seismological Society of America.[2]

Scott: When would that have been?

Blume: That must have been 1934. The one who invited us to give the talk was Perry Byerly, who had been talking to Lydik Jacobsen about our work. I was selected to give the talk. In order to give it, I had to buy a suit of clothes, which I didn't have. So I scraped up enough extra money to buy a dark gray suit, of which I was very proud. I gave the talk at Bacon Hall at the University of California, Berkeley campus.

1. Blume, John A. and Harry L. Hesselmeyer, "The Reconciliation of the Computed and Observed Periods of Vibration of a Fifteen-Story Building," Engineer's Degree thesis. Stanford University, CA, 1934.

2. "The Reconciliation of the Computed and Observed Periods of Vibration of a Fifteen-Story Building." SSA Annual Meeting, Berkeley, CA, April 1934.

Stanford Vibration Lab and Lydik Jacobsen

Blume: Getting back to the early days and the vibration laboratory at Stanford, the laboratory was under the direction of Lydik Jacobsen, who was a genius in dynamics and mathematics, and a very dynamic person himself in the way he moved and spoke. Lydik was enthusiastic about dynamics and the earthquake problem, but he had the disadvantage of not being a structural engineer. In fact, he had very little patience with most structural engineers, because they made what he called very crude assumptions, which you have to do in design. But in research, that's taboo.

I could see quite a gap between the structural engineers and Lydik Jacobsen and his work. He was way ahead of them in dynamics. In fact, they weren't even thinking dynamically, most were thinking statically. One or two had expressed the thought that there might be an advantage in a flexible first story. Among them was L. H. Nishkian, who actually designed a couple of buildings with that theory in mind.

The soft first story idea has been around for a long time. It was, and is, one of a great many aspects of dynamic thinking. Most tall office buildings tend to have a first story that is more flexible than their upper stories. This is the result of a higher first story, more penetrated by doors and windows. The soft story is compared to a spring that absorbs energy and tends to reduce the distortion of the upper stories. Theory and early shaking table tests supported this concept. The main problem is that unless one is absolutely certain of the greatest demands from ground shaking (and one is not), the spring could be overtaxed and fail, thus causing collapse of the whole structure. In other words, it is a potentially dangerous concept, and far from a panacea for a very complex problem.

Static vs. Dynamic

Scott: A moment ago you referred to thinking statically as opposed to thinking dynamically. Would you say more on that?

Blume: So-called static design neglects time as a very important element in the way things really are in nature. This elimination of time greatly simplifies the design effort, and properly so in many cases. Peak load or force, assumed to be constantly applied, is used to determine member sizes and other items. For example, a train or a truck crossing a bridge creates a complex, time-varying loading. Standardized constant loads are used to simulate the loading in a practical manner. Wind exerts pressures and forces on structures, and these forces are taken at or near their peak values and assumed to be fixed or static. For most structures, this treatment of floor or deck loading and of wind is satisfactory. But there are exceptions.

In the earthquake field, static design, unless very carefully modeled to simulate dynamic conditions, can be very misleading. There are various reasons for this. One is that structures per se are time-related—they have natural periods of vibration, some of which may tune in to the ground motion, which also tends to have important if not dominant periods, or time bands of energy. Another reason is that earthquake shaking intensity has a much greater possible range of values than train, truck or wind. Resonant or quasi-resonant response is part and parcel of dynamic thinking, whereas in the static approach it is generally ignored.

In short, the element of time is part of nature and cannot be ignored if progress is to made. There are now several ways of treating the problem as one in dynamics; they vary from rigorous to approximate. But, in general, many if not most engineers could not understand Lydik Jacobsen when he spoke, because he was too far out. He, in turn, could not understand them in the way they approached problems and dealt with design matters.

Combining Structural Engineering and Dynamics

Blume: As a young upstart student I could see quite an opportunity here to bridge the gap between the two. In fact, I took my early Santa Barbara intentions or vows, and made a subset of them, which consisted of combining structural engineering and dynamics in the earthquake field. So I worked in both fields, got to understand both sides, and was able to translate or communicate between the two. Probably this is the reason, among others, that I've been called by some the father of structural dynamics in the earthquake field.

Lydik Jacobsen was a brilliant man, very outspoken, very dynamic. I greatly enjoyed working with him, but he was a hard taskmaster, which was good for me. I needed that. I wrote a memorial upon his death, which was published in the August 1977 *Bulletin* of the Seismological Society of America.[3] I would recommend that reading for anyone who wants to learn more about Lydik Jacobsen and his work.

Three-Dimensional Model With Five Degrees of Freedom

Blume: Between the time of finishing the thesis and going to work for the Coast and Geodetic Survey, I had a job at the vibration lab at Stanford. This job was to design and build a dynamic model of the Alexander Building, working for and with Professor Lydik Jacobsen. The model was to have five degrees of freedom per story.

Scott: Explain what you mean by five degrees of freedom.

Blume: In three-dimensional space there are six degrees of freedom—translation along three axes, often called X, Y, and Z, and rotation about those three axes. In the model there were the three rotations and the two horizontal translations for a total of five.

This kind of work had never been done before. Having five degrees of freedom per story made it very complex to design and to build. We finally made the model using aluminum plates for the floors, and steel springs for the wall stiffness. For the rotational stiffness about the horizontal axes we provided thin-gauge steel plates resting on aluminum tubing. On top of these steel plates was a steel ball bearing, which allowed the floor to roll. The bending of the steel plate provided the flexibility for overall flexure of the structure.

3. Blume, John A., "Memorial: Lydik S. Jacobsen (1897-1976)," *Bulletin of the Seismological Society of America*, Vol. 67, No. 4. SSA, El Cerrito, CA, August 1977.

This model had fifteen stories, plus an extra one at the base that was used to model soil characteristics, and rotation and translation of the building in the ground. Thus, way back in 1934 we were experimenting with soil-structure interaction, decades ahead of its time. The model was used at the Stanford vibration laboratory shaking table.

Again, we had no electronic strain gauges or instruments. We had to resort to various devices to record the motion. This was an indestructible model. In other words it could withstand heavy shaking, with exaggerated motion, all to scale, without being broken. It could be used over and over again for repeated testing.

Scott: How was the motion registered and recorded?

Blume: To record the motion, mechanical gauges were mounted between each pair of floors. These gauges would exaggerate the motion so it could be recorded by a moving picture camera. Going back and looking into the frames of the film, one could reconstruct the distortion of the building under various ground motions. It sounds rather cumbersome, but remember there were no digital computers or electronic gauges in those days.

I think that if the world had not subsequently become computer conscious, this type of modeling would have progressed to a stage where we could have learned a great deal—more than we did—about structures and response to earthquakes. Unfortunately, in those days the money for research was scarce and soon ran out. By the time it dribbled back and was available again, the computer age was upon us. With high-speed digital computers to do the work, there's no need for models of this type. So the model and the machine are museum pieces. In fact, they reside now at the John A. Blume Earthquake Engineering Center at Stanford, on display.

Scott: That is the one shown in this picture here—in the *Engineering News Record*?[4]

Blume: Yes. The cover story of the September 18, 1980, *Engineering News Record* showed a picture of myself with the model.

4. "Pioneer Paces Seismic Field: John Blume Builds on 50 Years of Discoveries," *Engineering News Record*, September 18, 1980, Vol. 205, No. 12. New York, NY, 1980.

Chapter 3

Starting Out as an Engineer

"I recall my starting pay for five and one-half days a week, risking my life every night I went to work, was $170 a month. I didn't complain. I thought that was pretty good."

U.S. Coast and Geodetic Survey (1934-1935)

Blume: The thesis work was completed in mid-1934, in a hardcover volume. At about the time I gave my talk at Berkeley, the Long Beach earthquake of March 1933 had shaken loose a little money for research in the earthquake field. It didn't last long, but it was nice while it did last to be able to do certain things.

Scott: It shook loose money from what sources?

Blume: Money from the federal government. The United States Coast and Geodetic Survey was commissioned to conduct what was called the California Seismological Program of 1934-35. This consisted of many parts. I was engaged while I was still at Stanford finishing up work for my Engineer's Degree. For the last six months of 1934, I worked half-time for the U.S. Coast and Geodetic Survey designing, building, and testing the world's first multistory building vibrator.

Multi-Story Building Vibration Machine

Some prior work had been done in Germany using a bicycle wheel with an eccentric mass, and shaking small wooden, residential buildings with this device. They may have shaken small masonry buildings, also. They had larger machines using an unbalanced mass to vibrate and compact soil fill, but no work had been done on a uni-directional shaking machine that would put out reciprocating forces in one direction, either horizontally or vertically, to shake large structures such as dams, bridges, tall buildings, even the ground, for earthquake research.

Lydik Jacobsen was advisor on this U.S. Coast and Geodetic Survey vibrator work, and contributed a great deal to the early concept. As his background was in mechanical engineering, and at one time he worked for Westinghouse, he was extremely valuable in this effort. I designed a machine with three wheels upon which we would bolt lead plates for an unbalanced mass. This has been written up in the Seismological Society *Bulletin*,[5] as well as in many other articles. The principle involved was centrifugal force. We would place the machine high up in a building, or on top of a dam—you didn't have to go to the top, but we usually did—and jack it into place, securely wedge it into place very solidly. Then I'd start the machine and get it up to as high as 600 revolutions per minute, and I'd take the drive belt off then, and let the machine sweep through all the periods of vibration of the structure as the machine decelerated. This took about 5, 6, or 7 minutes. We'd be taking records on delicate instruments that magnified the structure's motion about 200 times; as we passed through each natural period of the structure, we could see the periods very plainly.

Scott: You mean where there was resonance?

Blume: Yes. Quasi-resonance would amplify the motion at the natural periods of vibration—very interesting work.

Shaking All Kinds of Structures

Blume: When I left Stanford I went to work full-time for the Coast and Geodetic Survey using this shaking machine. We shook all kinds of structures. The Alexander Building was on the list. The following structures were tested and shaken by the 300-pound machine: Palo Alto Transfer and Storage Building, November 1934; Searsville Dam, November 1934; Colorado Street Bridge in Pasadena, January 1935; Morris Dam at San Gabriel Canyon, and believe it or not, the Los Angeles City Hall, both January 1935. In February 1935, we shook the Bank of America Building in San Jose. The series of tests went on for seven days. That building is supposed to have a flexible first story. We shook the site of the new San Francisco Mint in March 1935. We also shook the ground at Mare Island Naval Shipyard, April 1935 and later the causeway to Vallejo; the Hills Brothers warehouse building in San Francisco, June 1935; and did another series of five days of testing on the Alexander Building in San Francisco, July 1935, which was the guinea pig building for my thesis study. The last structure that I shook with that machine was in

5. Blume, J.A., "A Machine for Setting Structures and Ground Into Forced Vibration," *SSA Bulletin*, Vol. 25. SSA, El Cerrito, CA, 1935.

August 1935, the Baker River Dam in northern Washington, near the Canadian border.

In all of these tests the 300-pound machine was able to get definite measurable response in these structures. We were able to learn the natural periods of vibration, the mode shapes, and something about the damping.

Scott: Who designed and built the machine?

Blume: I did, with the aid of Lydik Jacobsen as advisor. I actually built it in the Stanford shops. Fortunately I had taken machine work, as well as welding and forge. In those days structural engineers had to take courses like that. Today you'd call it a trade school class, but it was very much worthwhile. So I knew how to use lathes, drill presses, band saws, and all those things. I actually made the machine—all its parts, and assembled it. And that's been written up in the early literature.

I'm sorry to say that years later when Caltech made and started operating another machine for EERI, they wrote up their results without even a reference to the first machine. That was a strange oversight, and I made an issue of it, which made me unpopular in certain circles. The results we obtained with this machine back in the early '30s and mid-'30s were entirely valid and satisfactory in all respects. We not only showed that the machine could do what it was supposed to, but we obtained very valuable information about the properties of buildings and structures.

Special Publication No. 201

Blume: The work done in this intensive program was reported in Special Publication No. 201 of the U.S. Coast and Geodetic Survey.[6] I'm the author of the chapter in this book on forced vibration, and there are other chapters on other subjects. Dean Carder of UC Berkeley origin was in charge of instruments. Bill Moore of Dames & Moore worked in Los Angeles on this program. Ralph McLean was another engineer in Los Angeles. Frank Ulrich was the man in charge of the program. He was not an engineer by training, but he was an old-time Coast and Geodetic Survey man. I refer those interested to Special Publication No. 201, which has been greatly overlooked in the literature for a long time. The publication also contains a chapter by Professor Martel—who was doing statistical work on the Long Beach earthquake—on brick building damage.

Scott: That's Martel [Raul Romeo Martel] at Caltech?

Blume: Yes. George Housner wasn't yet around Caltech in those days.

Strong Motion Program

Blume: The Coast and Geodetic Survey work on forced vibration and many other special projects was brought to a halt when the money ran out in 1935. But the Survey was charged with the strong motion program of the United States, which they carried out until this was taken over by the U. S. Geological Survey, I believe in 1975—approximately then. The special work was greatly curtailed during the intervening years, and, in fact, it took many of

6. *Earthquake Investigations in California, 1934-1935*, U.S. Dept. of Commerce, Coast and Geodetic Survey. Special Publication No. 201. Washington, D.C., 1935.

us great effort, exerted year by year, to make sure that adequate funds were provided by Congress each budget year just to carry on the minimal work that was done, and to get earthquake records. The first strong motion record of an earthquake was obtained in 1933 at Long Beach. We would have been much farther ahead years ago if we'd had the money to put out more instruments and record more earthquakes.

Special Publication No. 201 Updated 30 Years Later

Blume: In addition to Publication No. 201, which came out in 1935, a special report was published about 30 years later, with Dean Carder as editor and various contributors updating the work of the program.

Scott: You are referring to a special report on the strong motion work of the Coast and Geodetic Survey?

Blume: The title of the book is *Earthquake Investigations in the Western United States 1931-1964.*[7] It's Publication No. 41-2 of the U.S. Coast and Geodetic Survey. It's a special publication that sort of updates the earlier work, and reviews what was done in the early days. I happen to know of it because I was asked to write a chapter on forced vibration, which I did. Also, they asked me to write a foreword for the book.

7. Carder, Dean S., ed., *Earthquake Investigations in the Western United States 1931-1964*, U.S. Dept. of Commerce, Coast and Geodetic Survey. Special Publication No. 41-2. Washington, D.C., 1965.

I sent in a draft copy of the foreword, expecting to get some heavy blue pencil marking, and also to go over it myself after I had a chance to think about it more. I didn't hear anything for a long time. In fact I got so busy with other things that it slipped my mind entirely. The next I knew, I received the book itself, with the foreword all printed out from my rough draft. Fortunately it's not too bad, but it could be a lot better. The foreword is dated June 1, 1964.

Forced Vibration: Media Response

Blume: A peculiar thing happened with regard to the forced vibration program using the building shaker or vibrating machine. When we shook tall buildings and structures in the San Francisco Bay Area, nobody paid any attention to us at all. However, when we got to the Los Angeles area and shook the City Hall and Morris Dam and the Colorado Street Bridge, and other structures, the press were all over us. In fact Pathé News was on hand as well, to make footage for newsreels. I don't know whether the difference in interest between the two areas was due to the nature of the people residing there, or due to the fact that the 1933 earthquake had made the southern California portion of the state more earthquake conscious.

A science fiction writer picked up an article on this shaking work from the magazine *Popular Science*,[8] which I've got in my records as well. *Popular Science* wrote quite an article about the forced vibration of Morris Dam, and other

8. "Tiny Machine Shakes Huge Building in Novel Earthquake Test," Popular Science. Vol. 126, No. 5, May 1935.

work that we were doing, and I got my picture in *Popular Science.* But then a science fiction writer read the *Popular Science* article and wrote a small booklet about some crooked people who threatened to shake down all of Los Angeles unless they were paid ransom. It seems strange that such things could happen, but they did. I have copy of that "book" in my files as well.

Construction Engineer on Bay Bridge (1935-1936)

Blume: I think I mentioned that the funds for the special program [California Seismological Program of 1934-35] of 1934 ran out in 1935. The work was severely curtailed, leaving nothing much but the strong motion instruments, and not enough of them. I had a wife to support, and a lot of bills to pay from expenses of going to school, and other creditors. I had to keep working, so in spite of the Depression I found an opening as a field engineer on the construction of the San Francisco-Oakland Bay Bridge.

You will recall that I had previously worked as an ironworker in the Hawaiian Islands and elsewhere, so I was able to climb around on the high steel. In spite of many young engineers being unemployed then, very few of them could work on the high steel. This gave me a great opportunity to go to work, and I did. At the time I started on the suspension side of the Bay Bridge, the towers had been erected, and they were just starting the cable spinning.

Cable Spinning and Night Work

Scott: About when would that have been?

Blume: In 1935 and '36. They assigned me, as my first major job, to determine the correct final position for each of the strands in the main cables. This meant that I had to work nights—the so-called graveyard shift. I worked nights for about a year and a half. The reason for the night work was to avoid the effects of the sun on the steel, the temperature effects. I wore a light on my hard hat, just like a miner. I found myself walking the catwalks and beams at night with this lamp on my hat, and climbing around the cables.

Fortunately, the height didn't bother me. In fact, I had to remind myself I was high up in the air. It was very cold and damp out there. Even in spring and summer it was cold over San Francisco Bay. We took temperature readings in each of the strands of the cable, made some calculations, and told the steelworkers how much to pull the cables over the tower tops in order for the strands to have the right length and correct sag in each of the many spans.

The catwalks that I walked were from San Francisco to Yerba Buena Island and back, round trip, sometimes making two round trips a night. Walking on the mesh of the catwalk was just like walking uphill in dry sand. I was in pretty good condition in those days.

Day Work and Using Dynamic Theory

Blume: After the cable spinning was all done, I was assigned daytime work—special jobs like measuring stress in the wire ropes and in the steel members. I employed my dynamic theory several times in special assignments. One case in particular that I recall was a rumor that one of the suspender ropes in one of the spans had been fabricated too short, and thus was overloaded, compared to the other suspender ropes in the area. My job, on a low budget, was to find out whether this was true or not. I borrowed an instrument from the Coast and Geodetic Survey, and measured the natural frequency of all the ropes in the span in question. Knowing the approximate length and the area of each of the ropes, I was able to calculate their tension, just like the strings on a guitar. I proved that there was no undue stress, that everything was okay. I simply mention this as an example of the use of dynamics in practical problems.

Scott: With respect to measuring the frequency, what evidence indicated that things were okay?

Blume: By determining the frequency and knowing the approximate length of each rope, we were able to compute its tension. The tensions for the different ropes came out to within a few percent of each other.

Later on I worked on other aspects of the bridge, until the day it was opened. In my files somewhere, I have pictures of the bridge and my work on it. The day the bridge opened was quite a time for San Francisco. The contractors threw parties in the hotels that lasted two or three days. But I already had another job lined up—to go to work for Standard Oil Company of California.

Civil Service Status

Scott: When you worked on the bridge, were you working as a full-fledged structural engineer?

Blume: I'm glad you brought that up. I worked for the State of California Division of Highways. All their jobs then, and I think still today, are civil service examination jobs. There wasn't time for me to take an examination before I went to work, so they put me to work on probation, assuming I would pass the examination later, which I did, among the top people in the state. I worked my way through various titles. I think the first was junior bridge construction engineer, then associate bridge construction engineer. I recall my starting pay for five and one-half days a week, risking my life every night I went to work, was $170 a month. I didn't complain. I thought that was pretty good. So I worked as a professional engineer, but not as a structural or civil necessarily—just as a bridge construction engineer.

The examinations that we had to take included some theory, and a great deal on practical aspects, such as how to erect steel, drive rivets, test rivets, how to paint steel, how to pour concrete decking. In other words, I was a construction engineer for that period of time.

Work at Standard Oil (1936-1940)

Blume: I went to work for Standard Oil Company of California, at the head office—225 Bush Street—the day after the bridge opened [November 1936]. I was assigned to the engineering department. Standard Oil's policy at the time seemed to be that it didn't matter what you had studied, or what your experience was, they threw any kind of a job at you to see how you'd perform under stress. The first job they gave me was to detail the pipes, support hangers, and furnace breaching and stack development for a power plant in Bahrain. This was really mechanical engineering, which was not my thing at all. But I struggled through it, and it was built, and it seemed to work.

Refinery Design Work

Blume: Shortly after that they got wise to the fact that I had structural training and had done a lot of work in earthquake dynamics, so I was assigned to design all the structural components of refinery plants, including the foundations and the anchor bolts for huge towers. Then, when the plants were built at Richmond Refinery, I was sent over there as field engineer throughout the entire construction, starting with the excavation and the pile driving, and winding up with the last coat of paint. This was all very rushed, high-pressure work. It was great experience, but I put in many very long, hard days.

One of the plants I worked on as field engineer was the Hydrogenation Plant at Richmond. Another was the Duosol Plant, and another the Dewaxing Plant. In the Hydrogenation Plant the contractor was a joint venture consisting of Bechtel, McCone and Parsons. I believe this was Bechtel's first refinery project.

Earthquake Design Standards

Blume: When I wasn't working on the design and construction of refinery plants for Standard Oil, I worked on the company earthquake design standards for such things as stacks, vessels, towers, even buildings. So I was back into my first labor of love—the earthquake problem. In fact, many of the standards are probably still in use, no doubt with modifications. These were developed by myself and John Rinne, who also worked for Standard Oil at that time.

Scott: Was Standard Oil just beginning to introduce earthquake standards into the design of their structures, and was this principally for things built in northern California?

Blume: Standard Oil had always been earthquake conscious to a degree. But I think the 1933 earthquake at Long Beach accelerated their thinking along those lines. The building codes then, and even today, do not really cover refinery-type construction. Even though Standard had always been earthquake conscious, I would say they really started getting into it in a bigger way at the time I worked there, perhaps with the prodding of people like John Rinne and myself. One of the great concerns was cost. They wanted earthquake resistance, but naturally they didn't want it to cost very much because of the economic problems in the oil business.

Design Problems of Refineries

Blume: In answer to your question about where they did it, anything in California was given special treatment—also in other parts of the world where they ran into earthquake problems, such as in Colombia. It's quite an art to design those high, vertical towers, which are very slender but heavy, so as to withstand motion, and with nothing holding them up but the anchor bolts in the foundations. We did literally hundreds of these types of installations.

The vertical vessels and stacks at a refinery plant tend to have long periods of vibration due to their height and slenderness. The only structural connection, outside of stairways that might connect one tower to another with sliding joints, is the anchor bolt connection to the foundation. So the earthquake development that we employed in those days was semi-static, semi-dynamic. We allowed for the long periods of vibration, but everything needed to provide resistance of the tower to earthquake motion had to be developed in the anchor bolts and foundation. In other words, we had to develop not only a shear connection, but a moment connection.

It would have been tremendously expensive to do this according to conventional rules about having no tendency for uplift in the foundation. Since this was private property we were able to take a few shortcuts, design under what's called uplift conditions, and still have a stable foundation.

The anchor bolts themselves had to be carefully connected to the walls of the vessel at the skirt or the bottom. This was done to develop the necessary tension and not have them pull out of the connection proper, such as has happened in even minor earthquakes in California and elsewhere.

I enjoyed my work with Standard Oil very much, although at times it was quite high pressure. While I worked for them I passed my two-day civil engineering license examination, and also my two-day structural title examination. I was told I passed both of these very high in the statewide examination ranking on the first try.

A Major Career Decision

Blume: Two things happened after I had been with Standard for a few years, I think after about three or four. First, they started to talk to me about generalizing into the oil business and being earmarked for possible promotion. Second, I began to get offers from structural engineers in consulting practice, probably because they heard about me passing high on the structural examination. I finally accepted an offer with H. J. Brunnier, a structural engineer in San Francisco. I had a long talk with the Standard Oil officials about leaving.

Scott: At the time did you view that as a major, fork-in-the-road career decision?

Blume: Yes. I knew it was a major decision in my life, and it was also a dangerous one because I still needed money very badly to pay my bills. I was always broke while working my way at Stanford, and I had incurred some debt in the form of tuition notes.

I had married Margaret (Peggy) Johnson during my last year at Stanford and, for awhile, she worked part-time as a waitress in Palo Alto. Upon leaving Stanford in 1934, we moved to San Francisco, after which I was the sole generator of income, and not much at that. My USC&GS salary, for example, was only $130 per month for a 5.5 day week. Peggy's health went bad, first leading to a major operation, and then turning into alcoholism, of the periodic variety. The latter required frequent hospitalization and a battery of very costly practitioners. After several years of problems, emergencies, expense and debt, the experts said a divorce might help her because she was leaning on me to bail her out. So we had a friendly divorce and she went home to Canada where she did improve. By then I was deeply in debt. It took me about eight years to get out of the red. Yet despite all that I had thought about it [starting my own business] for many years, and you'll recall that my intention was always to do something about the earthquake problem. My reasoning was along those lines. For example, if I went into the oil business instead of staying in engineering, I would do less and less about the earthquake problem. On the other hand I probably would have advanced very rapidly because I got along well with Standard Oil, in spite of the fact that whenever one rode an elevator in the building the Phi Beta Kappa keys dangling on vests were quite apparent.

I had always had a feeling that I wanted to get into consulting engineering myself. I'm not really a true corporation man, in the sense that I'm quite independent, outspoken. I like to try new things—not that they don't do that in large corporations—but I felt I could be freer on my own, or at least in a smaller firm. So all these things were considered. My departure from Standard was quite sad. In one respect I hated to go, and they made it clear they didn't want me to leave. Nevertheless, in 1940, about a year before the war broke out, I left Standard and went to work for "Bru" Brunnier.

With the Henry Brunnier Firm (1940-1945)

Blume: I had known of Henry Brunnier and his firm for a long time. In fact, I always looked up to that firm as one of the leaders in the field of structural engineering. They had designed most of the then highrise buildings in San Francisco, like the Russ Building, the Shell Building, the Standard Oil Building. Nearly all the early highrise buildings in San Francisco were structurally designed in that office.

Brunnier himself was a very well-known person who had done a great deal for the profession of structural engineering in making it known to the public, to non-engineers. He did this by virtue of his activities in outside things. He was always busy with meetings and with organizations. For example, he was international president of Rotary, and president of the California Auto Club, and also quite a golfer. He was the "outside man."

The "inside man" in the office was Henry Powers, who came to be a great friend of mine. He had very little formal education, but he was a real structural engineer in the sense that he had the feel of structures. He was in charge of the office when Brunnier was away, which was fairly often, on his long trips for Rotary and on other activities.

Defense Work

Blume: I started out in that office doing some Navy ammunition docks and wharves, which was a long way from what I had contemplated. I had already done some wharf and dock work for Standard Oil, so I was able to handle it all okay. It wasn't long before we got busy on rush military work. Even long before Pearl Harbor, we were working night and day, and around the clock sometimes. It was nothing to put in a 60, 70, 80-hour week, week in and week out.

To show how defensive things were in those early days of the war, we designed 6" and 16" gun batteries along the coast of San Francisco and the Peninsula. I also designed mine case-ments, which were the control points for underwater mines placed just outside the Golden Gate and elsewhere. All these would seem archaic today, but they were considered vital in those days.

Building Docks in the Panama Canal During Pearl Harbor

Blume: In about September or October of 1941, Brunnier and Henry Powers asked me to go to the Panama Canal, and take charge of a major design contract. They were working on tremendous docks for the Army and Navy in the Panama Canal Zone. I agreed to do this on the basis that as soon as that job was done I would come back to San Francisco. I went down, and got there in time for the wet season in Panama, where it rains so hard you can hardly breathe if you're out in it. I was put up in a bachelor quarters building, with screening for side walls, because of the heat. I was in a large room with five other fellows, all from Brunnier's office.

We worked night and day, and drank a little beer when we had an hour or so off, and got into friendly "fights" between the occupants of the rooms. I had great success in holding up my end, and got to be known as "the bull." The person who named me that was Al Collin, today of the steel company, whose name was "the beast." They were a great bunch of fellows, all of them.

We had the bulk of our work done on the Corazol Docks, which was a big ammunition depot for the Army—work was about 90 percent done—when the Japanese hit Pearl Harbor on December 7, 1941. Books had been written years prior to this suggesting that if the Japanese ever attacked us, they would attack the Panama Canal Zone and Pearl Harbor, so we were waiting. And there was a little activity that was hushed up, but nothing landed that I know about.

We had one air-raid alert after another, however, and each time the air-raid siren went off we were obliged to take all of the tracings and drawings that we were working on, roll them up, wrap them with waterproof paper, and run about three blocks through the mud and rain to a bomb shelter, where we would sit holding these rolls of drawings until the all-clear was sounded. Then we'd reverse the process, get the drawings back on the table and start work again. There were four to six air raids a day, so you couldn't get much work done, but you had to try.

The excitement in the Panama Canal Zone following Pearl Harbor was tremendous, because frankly we weren't prepared for anything at all. The few old aircraft they had at France Field were obsolete, and they were not armed. They had taken all the machine-gun racks and other armaments off to prevent rust. So after Pearl Harbor each aircraft that I saw had about 50 people working on it feverishly, trying to get the craft into fighting shape. I suspect the Japanese lost the war in the first couple of weeks, because if they had closed the canal, our Atlantic fleet would have had to go around the Horn instead of coming through the canal as they did. The Japanese made a serious mistake right then, although I believe they tried.

I finished up the work I had been sent down to do, and then I had the problem of how to get out of there and get back home. I was told at first that I was there for the duration, but I kept trying. Then one day I finally got notice, and a half hour later I got on a plane. It was at night-time; it was dark and blacked out.

Scott: When you were told you were there for the duration, was it simply a matter of restricted transportation?

Blume: No, it was some sort of a policy. They figured they would put us to work in the zone for the duration, in whatever our specialty was.

Scott: But you were still a civilian, still a Brunnier employee?

Blume: I was a Brunnier employee, but they were trying to commandeer everyone. I think they got over that. At least they let me out. So I got out at night on a plane with black curtains on all the windows so we couldn't see out, and there would be no lights. I got back to San Francisco just before Christmas, 1941. I was very glad to be back.

Wartime Work: "Temporary" Buildings and Minimal Designs

Blume: If I thought I had been busy on the war effort before the war started, you should have seen me after it started, and I was back in San Francisco. Tremendous depots had to be designed for the military. I was the engineer in charge of design of the Lathrop Holding and Reconsignment Depot, Tracy Quartermaster Depot, parts of Hamilton Field, parts of McClellan Field, and parts of Castle Field, not to mention countless wharves and docks for the Navy and the Army.

All during this time I was not working on earthquake problems per se. I did occasionally employ dynamics. For example, the docking force from a large ship hitting a dock or wharf had to be taken care of somehow. It was a matter of energy absorption, which has been one of my guiding principles in earthquake design. We also did some design to prevent missile penetration. The war effort came first, beyond everything. Some of these projects were very big—such as the Lathrop Holding and Reconsignment Depot.

As I recall, the buildings reached a length of 1200 feet, and there were rows of them. Someone computed the amount of curvature of the earth in the building roof lines. All of these structures were designed for temporary use. In fact, one general told us that if they stood up beyond five years we weren't doing our job right. "On the other hand," he said, "I don't want them falling down in four and a half years." Because of the war effort we had to design things with a minimum use of steel, and a minimum use of all metals.

Scott: Were most of these facilities located in the Bay Area, or all over?

Blume: Most of them were in the valley, like Lathrop, which is on the way towards Stockton. Tracy is on the other side of the Altamont Pass. These had previously been tremendous open fields of agriculture, and they were turned into vast military depots. Unfortunately, the buildings did not fall down after five years. In fact, some of them are still in use today. But we designed them fast and economically, and we used a minimal amount of steel and other metal. There wasn't time to prepare nice, neat calculations. Often I would tell a draftsman what to draw, and make a few figures on the back of an envelope. There were even occasions when we had drawings signed in blank by the military officials who were coming in to see how we were doing. They signed in blank because we hadn't yet had time to get the stuff on paper, but they trusted us, and it all came out okay.

After about four years of this, in 1945, I started developing a strep throat, and I had to keep working. Every afternoon I had to visit a doctor, who would inject my throat with silver nitrate, which tasted horrible. Then I went back to the office. Because I was tasting this awful stuff, and because I was tired, I'd stop at Breen's on Third Street and get a brandy eggnog, which was my dinner. I'd get back to the office and work until 11 or 12 at night, with a fever. Somehow I got over this without really getting sick, but a month or so later I came down with pneumonia. Then I was very sick. They gave me sulfa, and the sulfa worked on me. It apparently killed the bugs, but it also ruined my bloodstream. I was cold and shivering much of the time for about six months after that.

Chapter 4

Hangs Out Own Shingle

"During all this design work for things in Saudi Arabia, we were also doing design for things in this country. One thing led to another."

Blume: When I was able to get up and around again, I made another big decision in my life—that I was not going to go back to work with Brunnier, I was going to hang out my own shingle. The war was over, and in June 1945 I rented a small office at 68 Post Street, the Foxcroft Building. The rent was $20 a month for a tiny little room on the back alley.

Scott: Why did you decide to leave Brunnier's office?

Blume: I had a great job with Brunnier, and I liked working there very much. I liked the people, and Powers and Bru, but I also have that independent streak that I can't get rid of. I wanted to be on my own and be able to do research in earthquakes, as well as other things that I wouldn't be able to do in that position. I forgot to mention that somewhere along the line I was given the title of "Engineer in Charge of Design" for the whole office, and had that title for two or three years before I left Brunnier. I was No. 3 in the office. When I finally left I was very careful not to try to entice any of the Brunnier employees to come with me. And I was doubly careful not to get involved with any of the Brunnier clients. I wish that code of ethics had prevailed throughout the profession, but unfortunately a few of my former employees have not seen fit to use the same code.

Getting Started

Blume: Starting out as John A. Blume, Structural Engineer, I had at first a lot of spare time on my hands. I would spend the weekdays making contacts and trying to get work. Whenever I got a little job I'd do it on the weekend. I also became very active in the Structural Engineers Association of Northern California (SEAONC), probably due to the efforts of my friend Franklin Ulrich, who was secretary/ treasurer, and had been for years, and for whom I had worked in the Coast and Geodetic Survey 10 years before.

I needed to become better known in San Francisco. This was quite a problem, because at the time no structural engineer had started out on his own and made a go of it. Most of the firms had been handed down from prior successful firms. Some of the local structural engineers of the day, besides Brunnier, were Henry Dewell, L. H. Nishkian, Harold Hammill, Walter Huber, R. S. Chew, Harold Engle, Fred Hall, Mike Pregnoff, and many others, most of whom had either been in business for a long time in their own names, or had inherited their practice from the prior owners.

I should also mention John Gould, who became known for his work on the San Francisco Fair buildings. In fact, one of my first clients—believe it or not—was John Gould. He had some problems with a couple of jobs, and needed an outside opinion and help. He engaged me to work with him as a consultant.

Six Years of Saudi Arabian Work

Blume: Along about this time [1946], when I was struggling to make a living, one day I had a phone call from a friend of mine who used to work in Standard Oil when I worked there—Wendell Spackman, an architect. Wendell had formed a partnership with Clarence Peterson, called Peterson & Spackman, Architects. It seems they were involved in a small job for the Arabian American Oil Company (Aramco), and needed some structural help, which I was very glad to provide. This had to do with some building work in Saudi Arabia, including portable buildings that would be transported across the dune sand from one place to another on tremendous low-pressure tires.

We developed the mechanics and structural part of this desert operation, and apparently did a nice job because one morning later on I was in my office, and the telephone rang again. It happened to be Jim Stirton, who was then chief engineer and vice president of Arabian American Oil Company. He said, "Jack"—that was the name I went by in those days—"I see you've got your shingle out and I see you've done some work with Peterson & Spackman, which was very good work. We've got another little problem here, could you possibly come over and talk to me about it?" I said, "I'll be there in fifteen minutes," and I was. The problem that Jim Stirton had in mind at that time was to be able to dock, load and unload deep-water vessels (ships) in the Persian Gulf, over seven miles from shore, in order to get to water deep enough to handle these ships' draft. The water was very shallow for miles out.

As I recall, he wanted about 40 feet of draft alongside the docking facility. He said, "We have to do this in structural steel, because we can't punch any other piling through the hardpan or limestone layer. But next week we have to order the steel by tonnage, shape and length."

This reminded me of the old days when I worked for Standard Oil of California, and Jim Stirton was then assistant chief engineer of Standard Oil. He was considered a genius by his peers. He wanted everything done overnight, because he could do it if nobody else could. And he really could. Rumor was that Jim Stirton not only had his engineering down cold, in every branch of engineering—structural, civil, electrical, mechanical, refinery, etc.—but also he was a CPA and had passed the bar. He was really a brilliant man and a workaholic.

To make a long story a little shorter, I worked night and day on that wharf layout and tentative design, so we could order the steel on schedule, and we did. That effort led to about six years of intensive design work for the entire Saudi Arabian oil development, in which from time to time we designed buildings of every type and occupancy—including transmission towers, wharves, docks, and the first offshore platform in the Persian Gulf.

Scott: About when would that have been?

Blume: The design work went on for 5-6 years, as I recall, starting in '46 or '47.

Portable Offshore Platform

Blume: The offshore platform that I mentioned was designed to be portable. In those days there were no offshore companies with patents to worry about, so we designed our own devices. The reason they wanted a portable facility was that they were wildcatting in the Persian Gulf, in the water. If at first they didn't strike anything, they wanted to be able to shift around and try again. So the bulk of our design effort was to make this platform portable. But the portability was never tried, for the simple reason that the first well drilled brought in the famous Safaniya Oil Field, which I understand even to this day is the largest proven underwater reservoir of oil in the world.

Scott: So they started out wildcatting in the middle of the very best place?

Blume: The right place. Now if we had been able to get just a tiny fraction of a percent as a royalty we would have been rich, but all the work we did was on a cost-plus, hourly basis. We were very disappointed that the portability factor was never tested. The platform stayed right where it was, and they built many others. The oil came out artesian style. The volume would depend on the hydraulics and the size of the pipe—no pumping was required.

Designing Many Other Things in Saudi Arabia

Blume: In addition to all the facilities and structures that were constructed in Saudi Arabia, we also worked on many other things—studies, and trial designs that were not all built, such as a ferry to transport railroad cars, and a self-docking vessel to act as a self-docking dry-dock. It was extremely interesting work. At times we'd have as many as 15 or 20 different projects on the boards at the same time. We wrote our own codes. But time went on and Aramco moved its headquarters from San Francisco to New York. We kept on working. Then they moved to The Hague in Holland, and finally they moved to Rome, and then they moved to Dhahran in Saudi Arabia. With all these moves and increasing distance, it became impossible to continue to work for them, but by then, the bulk of the work was done. We literally designed the cities of Dhahran, Ras Tanura, Abqaiq, and many others in Saudi Arabia, plus the Dammam Port and much of Ras Tanura Port.

Scott: You did this work as John A. Blume, Structural Engineer? You must have had substantial help for these major design projects.

Blume: Actually, there were three firms involved in this design work—the architectural work was all done by Peterson & Spackman, the mechanical/electrical was done by James Gayner, Consulting Engineer, and the civil/structural work was done by John A. Blume, Structural Engineer. Which one had the prime contract (it would either be our firm or Peterson & Spackman) would depend on the type of work. Work on an office building, theater building, bowling alley, hospital, for example would be headed up by Peterson & Spackman. But the work on wharves, docks and towers, everything structural, was headed up by us. We worked directly with the Arabian American Oil Company, which used to have its headquarters at 200 Bush Street, San Francisco, and was partly owned by Standard Oil of California.

Designing for Economy

Blume: During all this design work for things in Saudi Arabia, we were also doing design for things in this country. One thing led to another. The Saudi Arabian development stands out in my mind because of a couple of factors. One is we had no building code to go by or worry about. We wrote our own code. Also, especially in a foreign country like that—and there were problems even then in the Middle East—the oil company wanted everything to pay out in less than three years. So we couldn't waste a dime in design. We used materials for all they were worth.

The transmission towers, for example, were only two-legged, and the stability in the other direction was obtained by the stress being carried in the wire overhead, all the way between anchor towers, which were widely spaced apart. One time Aramco took competitive bids for forty miles of transmission towers where the steel bidders were allowed to (a) base their estimates on our design, or (b) design their own towers as light as they could get away with and still guarantee the work. Ours came in as low bid, and it was built that way. It was really light.

In a later trip over there I was met and told that, "Too bad, one of your towers came down." I said, "What happened? They said, "We'll drive you out and show it to you." We got in a jeep, drove a good many miles, and here the towers were in perfect condition. I said "That's not down." "Well, we rebuilt it." I said, "But what happened to it?" They were kidding me, I could see it. He said, "Your design was too light. An Arab drove a truck into it, the tower came down and killed him." I said, "We didn't design for trucks hitting the tower." He said, "You weren't supposed to."

Dammam Wharf and Port

Blume: Dynamics came into play from time to time, even in Saudi Arabia. For example the Dammam Wharf—the one we built seven miles out in the Persian Gulf—has vertical steel piling and five railroad tracks on top of the wharf—a great big structure. It has no batter piling or brace piles. The reasoning was, if a ship got out of control and hit the wharf too hard, we didn't want to sink the ship in the Persian Gulf. We'd rather repair the wharf. But the main point was that the energy of the oncoming ship, which was calculated, would be transmitted into the vertical steel piles, which would absorb the shock like a giant spring. In other words the whole wharf was a spring. It was a case of energy absorption—a principle I've tried to build into the earthquake field for many decades.

The Arabian oil development was a great thing not only for the oil companies, but also for Saudi Arabia and for our country, although things have changed over the years. I was sent to Arabia in 1957 by the U.S. International Cooperation Administration (ICA) via Tudor Engineering Company, on a special mission. I went to see Jim Stirton, who was then living in Dhahran, and we visited for a while. I said, "I'd like to see the Dammam Wharf to see how it's holding up." He said, "I think we can arrange that." This sounded strange for him because he used to bark out orders. He had to call two or three Arab chiefs or executives to get permission to go out and look at the wharf that used to be in his control. He finally got permission, and we drove to Dammam. We couldn't get on the wharf because the Arab who was supposed to let us on hadn't showed up yet. We waited about a half hour; he finally arrived. So, under guard, we were allowed to go out on the wharf we had built and look it over. That was in '57. You can imagine how things are today [April 1987]. The Arabs really control everything over there now.

1957 Trip to Saudi Arabia

Blume: On the '57 trip—I'm getting ahead of myself a little here, but would like to cover this while I'm talking about Saudi Arabia—our mission then was to work out an improvement in the railroad system, the Dammam docking facilities system had to be expanded, and also the airfield.

Scott: This would have been the railroad, docking facilities and airfield for the Arabian American Oil Company?

Blume: No, built by Aramco, but now under the Saudi Arabian government, for the whole country. The ICA sent three of us over there. One was a railroad expert, one was a docking and wharf expert—that was I—and the other was an airfield expert. It happened to be the holy month of Ramadan, when the Arabs stay awake all night, sleep a little during the day, I guess, and they fast. We had to work during the day, then we had to go to long meetings at night so we got a little tired. Finally, they [Saudi Arabian government officials] said, come up to Riyadh, which is the capital of the country.

We first went to Riyadh on the train, on the same train that had the king's car. We got into a series of meetings, which were extremely strenuous, to say the least. They were held at night. We dealt with the princes. The king then was Saud Al Saud, and we dealt with some of his sons. He had many sons, over 30 of them.

Wanted Report Changed

Blume: They wanted us to change our report and build our wharves and docks in a completely different manner so they could work with the Egyptian government and use their method of building ports. We knew it wouldn't work very well, so we held tight to our trial designs, which by the way had been approved in a meeting between Ambassador Richards and the king. We were left, to "mop up the details." But as soon as the official U.S. party had left, the Arabs wanted to change everything. They were being prodded by the Egyptians. They wanted to eliminate all steel. We returned to Dhahran, only to be called back to Riyadh. This time the U.S. Air Force flew us up there in a C-47 cargo plane. But we were "detained."

Under House Arrest for Two Days

Blume: We were essentially taken prisoner, and put in the Yamama Hotel, which was functioning like a jail by having eunuch guards at each end of the corridor, so if we tried to get out of the building, they would simply grab us by the arm and escort us back to our room. We had expected to fly back with the Air Force plane, and were not prepared for a longer stay. There were two days of this, with no toothbrush, no air conditioning, tremendous heat, flies—they opened the windows to make sure the flies came in to bother us. They didn't draw blood, but they did everything they could to shake us down. And we knew how wild the Arabs could be in those days. They were deliberately putting us under duress. We were finally rescued by the military police.

Scott: Whose military police?

Blume: Our Army. They came in a transport plane with a belly; they took a jeep out of the belly.

Scott: This is fascinating. Who instigated this captivity?

Blume: The Saudi princes—they figured they'd get us to change our report, that they could sell it that way. There was about $20 million involved. After we got back we were advised that we were not allowed to speak of this for ten years. This is quite contrary to today, when if you're taken hostage, everybody knows about it. Thirty years have elapsed. I still don't think they like it publicized too much, but I can't help that.

Scott: How long did it last?

Blume: About three days and two nights, but it was a long time.

Scott: What were your feelings at the time? Did you feel like a full-fledged hostage, or did you not quite know what the situation was?

Blume: We didn't know what it was. We were a little afraid they might start drawing blood, which they could have, but they didn't. This was a country that cuts off hands for petty thefts. They didn't touch us. But they turned off the water in the hotel rooms, so you couldn't wash or go to the bathroom very well—miserable little things.

Scott: Then the military police came and got you out of there?

Blume: What they did was to come to the site—four men in a jeep. They found out the building we were in, and they just parked out there in that jeep—at rigid attention with their collars buttoned in 120-130 degree heat, with rifles. There were four of them, two in front, two in back. They just sat in that rigid position. If they had passed out, everybody would have lost face, but they didn't. They just got soaking wet with sweat—it turned to salt. Those poor guys. We could see them out the window. Finally, it got to be late in the afternoon and the Arabs said: "We can't deal with you. We'll take it up with your President Eisenhower. The meeting is over."

Scott: It was kind of a facing-down operation, to see who would blink first?

Blume: That's it. So I have quite a feel for what goes on in the Middle East, and I'd just as soon not go back there.

Chapter 5

Committee Work and Seismic Codes

"...That led to the publication of Separate 66, *...[which] years later, led on a statewide basis to the Blue Book....So out of the chaos and controversy of the late '40s, many things developed...."*

Active in SEAONC

Blume: In 1945-47 the Structural Engineers Association of Northern California (SEAONC) was a rather small organization, interested mainly in the earthquake provisions for the forthcoming San Francisco building code. Also there was a great interest in fees and ethics. I was just starting out in my own practice and was very active in SEAONC. I was appointed "assistant secretary/treasurer," and also became active on the fee committee and the earthquake committee [1946]. The title of assistant secretary/treasurer was rather misleading, because the officers above me were either absent or ill for a large part of the time. I actually found myself sort of running the organization from my little office on Post Street.

[During this period] I was spending a lot of time, both for SEAONC and on the code matters; and I was working way beyond my title. However, the latter changed in 1947 and 1948, when I became vice-president and president respectively. I also was the first statewide president [of SEAOC] under its new constitution in 1949. I was also struggling to get going as a young practicing engineer, as well as spending a tremendous amount of time on the code problem, and also considerable time taking care of much of the administrative work of the association.

As I recall, the administrative responsibilities and pressure came to me by default. For example, Bill Adrian was president but was not very active in these seismic battles, and Bill Moore became president but was traveling around the world setting up Dames & Moore. And neither one was deeply involved in the earthquake problem the way I was.

So it just sort of fell into my hands, which again wasn't bad, because I got to be known a little bit. And I learned a great deal about how everybody thought, at least in those days, about the earthquake problem. It also gave me ideas about what was needed to help solve the problem. I found the experience to be very beneficial in many respects.

The 2-Percenters vs. the 10-Percenters

Blume: Harry Vensano was director of public works for the city and county of San Francisco. He had it in his mind that he wanted an earthquake code for San Francisco, and one was badly needed.

Scott: This would have been about 1947?

Blume: Late '45, '46, '47. The code was finally adopted in '48. That was a tremendous struggle in itself, but years before the San Francisco code was adopted, Harry Vensano had proposed certain things informally to [SEAONC] association members. This started a violent reaction and led to a great many meetings, both public and private. It also led to hotly contested debates between engineers holding different viewpoints.

I've noticed one thing in the earthquake field over the years, not only among engineers, but also among geologists and seismologists: they often have vastly different viewpoints, and they defend their positions very strongly. I'd often wondered why that was, and one day the solution came to me. They were all dealing with very important problems, but with insufficient data to work from, and therefore they were forming widely different opinions.

As a young fellow I'd been at some of the meetings with some of the old timers, and I thought they would come to blows, but they didn't. The camps sort of divided into two groups: the 2-percenters, and the 10-percenters, as I called them. The 2-percenters were people like Brunnier and Nishkian, who had designed tall buildings and knew darn well that they couldn't design for 8 or 10 percent on the base shear and still have any building left to design. Moreover, some felt that over-design would do more harm than good.

Scott: When you say "not have any building left to design," do you mean that the design would have been too expensive to build and/or that there would be no clients?

Blume: Both aspects would apply, although the nonbuilding one was basically what I had in mind. Owners—and architects as well, to please their own clients—want to generate investment income at the least cost up-front. The bigger the columns and the thicker the walls (if indeed there are walls of a structural nature), the greater the initial cost and the lesser the net rental area. If the 10-percenters prevailed on tall buildings, there would not be such buildings. The columns and bearing walls would be prohibitive in size.

Scott: You are referring to 10 percent of gravity as a lateral force requirement?

Blume: Right. The 10-percenters, or 8-percenters, were mainly those who really were thinking of low buildings, and more rigid buildings. It's a fairly simple matter to design them for as much as 8 to 12 percent of gravity. But in the early days, the code draft did not distinguish between the 2-percenters and the 10-percenters, so they proceeded to battle each other very vehemently.

Good Design Not Costly

Blume: As a parenthetical aside I would like to say at this point that even today I feel that, if the architects are reasonable, good earthquake design can be done at very little, if any, extra cost over slipshod methods. For best results, an architect should work with his engineer from the start of early planning of a building. Few do this. The disposition of columns and walls, especially of the important first story, is vital to the effectiveness and the cost of earthquake resistance. I deplore what I call "vagrant architecture"—no visible means of support. Even though the engineer tries to meet the code, or in fact meets the code, by cramming the resistance into central core walls, the net result is not what it should be. Symmetry and smooth transitions are desirable as well as peripheral strength.

Vensano Code (1948)

Blume: It has never ceased to amaze many people, including myself, that San Francisco, with its history of earthquake damage in 1906 and before, had no real earthquake code until 1948. After 1906, for a time San Francisco designed for wind forces of 30 pounds per square foot laterally, but after a few years it was lowered to 20 pounds. Then after a few more years it was reduced to 15 pounds per square foot of wind force, without any seismic requirement per se. It always struck me as strange that a city with San Francisco's background was so far behind in adopting a real earthquake code.

During these years [of discussing the San Francisco building code] I had tremendous exposure and experience as a young practicing engineer, because I not only attended all the meetings, but also as the assistant secretary, I had to write them up. I became the one who drafted the official letters between the association [SEAONC] and the city, and even beat a path to city hall. When the code was finally up before the San Francisco Board of Supervisors for adoption, there was a series of meetings on that subject, and the fighting started all over again.

Finally, it was resolved among the engineers. They looked silly, and it looked bad for them to be squabbling in public. They decided to appoint me as a spokesman. I appeared before the Board [San Francisco Board of Supervisors] and had to answer such questions as, "Won't this code run the cost of buildings up so much that we won't be able to build anymore?" And, "What will happen to the labor unions if you do this?" By the way, the plasterer's union, and other labor unions, were very strongly opposed to a seismic code in those days because they thought it would put them out of work.

When the supervisors finally adopted the code it was not exactly like any other code in existence, including the Uniform Building Code, because of Harry Vensano wanting to change a few things here and there, as he saw fit and felt was desirable. As I recall, the initial code as passed had a minimum base shear coefficient of at least 0.037 and a maximum of 0.08, depending on the type of building and the height.

Scott: What do 0.037 and 0.08 refer to?

Blume: In the case of the '48 San Francisco code, it referred to a factor to be applied to all the dead load and live load above the point under consideration. For example, if you're at the base of the building you'd have to take 3.7% of the entire weight above plus the entire live load, and apply that as a shear force at the base story. In short, you'd have to provide, under code stresses, the resistance to that as a lateral force. This became law for several years, and it made many of the 2-percenters very unhappy. The result was that a joint committee was appointed, representing both the American Society of Civil Engineers (ASCE) of San Francisco and the Structural Engineers Association of Northern California (SEAONC). I won't go into that committee's activities at this time, except to note that it led to the publication of *Separate 66*,[9] which became a steppingstone in all subsequent code considerations. I'll cover that later on.

The 0.037 base shear requirement was something that Harry Vensano wanted to get in his code, and he got it in there. The 2-percenters thought it was too much, too much of a demand on a building. Another thing that bothered some people was the fact that Harry Vensano changed some of the unit values in structural steel, for example.

Scott: You mean changed them from the manufacturer's recommended standards?

Blume: Yes, instead of using the AISC (American Institute of Steel Construction) standards, which are national standards, as printed, Harry changed a few factors slightly here and there.

Scott: Why would he have wanted to change the factors?

Blume: We often wondered, but I think the reason was that he himself had worked in the steel industry as a designer, and he just had personal feelings about what the factors should be. So unfortunately the base shear value, the use of a full live load, and some of these factors in allowable stresses, caused Harry Vensano to be a rather controversial figure for a while.

It was unfortunate in one way, because frankly he was just trying to do the right thing as he saw it. But the controversy that developed from time to time led to other things later on that I think were very beneficial. For example, as I noted just now, a few years later because of the ongoing unrest in San Francisco about the code, a joint committee was formed, representing the American Society of Civil Engineers,

9. Anderson, Arthur W., John A. Blume, et al., "Lateral Forces of Earthquake and Wind," *Separate 66, Journal of the Structural Division*, Proceedings of the American Society of Civil Engineers,. ASCE, New York, NY, 1951. (Also "Lateral Forces of Earthquake and Wind," *Transactions of the American Society of Civil Engineers*, Vol. 117. ASCE, New York, NY, 1952.)

San Francisco Section, and the Structural Engineers Association of Northern California (SEAONC). That was the committee that led to the publication of *Separate 66* in the ASCE *Journal*. That in turn, years later, led on a statewide basis to the Blue Book code of the Structural Engineers Association of California (SEAOC). So out of the chaos and controversy of the late '40s, many things developed over the next ten years or so.

Separate 66 Committee

Blume: After the San Francisco earthquake code went into effect in 1948, many engineers—especially the 2-percenters, as I have called them—were rather unhappy about the code. They especially did not like the minimum base shear of 3.7%, and the use of all live load in figuring seismic weights. The result of all this controversy was that a new committee was formed consisting of members of both ASCE and the Structural Engineers Association. I was originally asked to be chairman of this committee. I declined because I was then [in 1948] president of the Structural Engineers Association (SEAOC), but I became an active member [of the committee]. John Rinne was named chairman. The other committee members were Arthur Anderson, Henry Degenkolb, Harold Hammill, Ed Knapik, Henry Marchand, Henry Powers, Art Sedgwick and Harold Sjoberg. The committee tried to suggest a code for use not only in San Francisco, but everywhere. They tried to involve not only some of the principles of dynamics (albeit in a crude way), but also involve what the committee thought to be rational base shear coefficients, as well as a better application of the lateral forces to the structure than existed in prior codes.

This committee met weekly for well over a year, or perhaps approaching two years. Each meeting consisted of a dinner session in a restaurant, in a private room, after which the tables would be cleared and we would meet for hours, sometimes until 11:00 or 12:00. Naturally, not all of the members of the committee had the same background, and only a few of the committee were very much informed about dynamic matters, principally John Rinne and myself. However, assignments were made by the chairman, and practically everything imaginable pertaining to an earthquake code was researched, and reports were given to the committee. After months and months of sessions, with full attendance by most members, we began to draft a suggested seismic code, which involved some of the principles of dynamics in a rather simplified method.

The code as finally proposed involved the periods of vibration of buildings, something brand new in building codes. It also applied the lateral forces as an inverted triangle on a building or structure, which was also new. In this regard the committee was leaning on work I had done on my thesis at Stanford years before. The suggested lateral forces were based upon the fundamental period, the type of structure, the type of framing, and the shape of a proposed response spectrum, a new subject that was just coming around.

Finally, a report was made to the member associations, and a paper was drafted for the American Society of Civil Engineers *Proceedings*, Structural Division. The paper was published in the Structural Division *Journal* of ASCE, in April 1951. The title was "Lateral Forces of Earthquake and Wind,"[10] and all the committee members were shown as joint authors.

Scott: Is that the report referred to as *Separate 66*?

Blume: That is correct. In those days the Society numbered their papers according to Separates, and this was number 66. I should explain that the separates, if accepted, were later published in the annual *Transactions* as a whole document. In fact, this paper was so published in the *Transactions*, Volume 117, 1952.

Scott: So that was typical—they'd publish the article in a separate version earlier, then in the *Transactions* later.

Blume: Yes, later in the *Transactions*, if the discussion justified the printing of the whole paper, which was true in this case. In fact, this paper was given the Leon S. Moisseiff Award of the ASCE. The publication of this paper drew a great deal of response. In fact, many Japanese responded, as did people from India, and Caltech and southern California in particular. Not all of the response was in agreement, which is typical of the subject matter.

Scott: In other words it started quite a debate?

Blume: Yes. But in general the main objections came from Caltech, although I believe we put most of their objections to bed in our closing discussion.

Scott: Could you summarize in simple terms the nature of their objections?

10. *Ibid.*

Blume: Well, it's complicated, but I think in general the feeling was that the theory and the principles involved were not treated in sufficient scientific detail. Our response to that was twofold. First, we acknowledged we didn't know all the answers yet, might never know them all. And second, we pointed out that to have practical application in the design world, it would be impossible to treat the whole thing in a theoretical, rigorous manner. In other words, building codes cannot involve a great deal of theory—they have to deal solely with useful requirements.

Blue Book Committees

Blume: While we're on the subject of building codes, I should add that the *Separate 66* paper was studied for years by various groups and people, especially in southern California and in Japan. Finally, years later, the Structural Engineers Association of California (the statewide group) decided to attempt another, more comprehensive building code. So another committee was formed in 1957.

The committee was under the chairmanship of Bill Wheeler. Other committee members were Steve Barnes, R.W. Binder, John Blume, Henry Degenkolb, Murray Erick, Herman Finch, Norman Green, H. B. Hammill, Roy Johnston, Pete Kellam, Jack Meehan, Harold Omsted, Bob Preece, Henry Powers, John Rinne, Ernest Maag, John Steinbrugge, and Art Sedgwick. That's a large group, but meetings were held periodically in either northern or southern California.

Most of the work was done by subcommittees, and the study work went on for a couple of years. The subcommittee chairmen were as follows: Steve Barnes, base shear and shear distribution; John Blume, structural frames; John Steinbrugge, diaphragms; Henry Powers, torsion; Roy Johnston, overturning; Blume, setbacks; Powers, drift; Ernest Maag and Murray Erick, foundations; John Rinne, supplementary report.

Without going into the details of this, I should note that the result was published in 1959 as the first so-called Blue Book of the Structural Engineers Association of California. Subsequently, each of the structural engineer associations in California appointed its own earthquake committee every year. Sometimes these groups issued reports, mostly of a local nature. Sometimes these activities were mostly educational. These committees are not to be confused with either the joint *Separate 66* group or the statewide Blue Book group, both of which were special in scope and in nature.

The debate over the 1948 San Francisco code continued for years with reference not only to the joint committee report [*Separate 66*] but also to subsequent local reports. The city code was revised in 1956 to incorporate most of the principles from the joint report and paper, but not the design forces. The latter, for most buildings, were changed to vary from coefficients of 0.075 to 0.035. However, the live load participation was reduced from 100% to 25%. It was not until 1969 that the San Francisco code conformed at least generally with the 1967 Uniform Building Code, which in turn had adopted the 1966, 1967 Blue Book documents as issued by the statewide association (SEAOC).

The Blue Books were revised every few years, and have constituted the backbone of most seismic codes in use. I believe the first adoption [of the code promulgated in the Blue Book] was in the Uniform Building Code [1967]. Even though the Blue Book was a statewide effort, and went into things in great depth, it leaned rather strongly on *Separate 66* for many of its principles. [The SEAOC Blue Book was issued in 1959.]

Ductility and the PCA

Blume: One of the things I have always espoused is the matter of ductility and toughness in building materials and framing for earthquake resistance. I have continued along these lines in committee meetings, especially of the original Blue Book statewide group. In fact, that "code" for the first time required ductility for certain height structures, and it used structural steel as the accepted basis for ductile performance. Following is the infamous paragraph (j) as first issued in 1959:

> (j) *Structural Frame.* Buildings more than 13 stories or one hundred and sixty feet (160') in height shall have a complete moment-resisting space frame capable of resisting not less than 25 percent of the required seismic load for the structure as a whole. The frame shall be made of a ductile material or a ductile combination of materials. The necessary ductility shall be considered to be provided by a steel frame with moment resistant connections or by other systems proven by tests and

studies to provide equivalent energy absorption.[11]

The door was left open and the Portland Cement Association (PCA) accepted the challenge. After the document was published and distributed, PCA, which handles the work for cement manufacturers in the country, became concerned that this was a case where structural steel was being qualified, and concrete was not. So they engaged Professor Nathan Newmark at the University of Illinois, and his colleagues, to do some laboratory test work on concrete to try to develop and demonstrate its ductility when properly designed. I found out later that PCA also investigated all the structural engineers in California and finally came to me as their selection to work with Newmark in developing a procedure and manual on how to make concrete ductile.

Blume-Newmark-Corning Book

Blume: I thought about this quite a while before accepting—I finally did accept. Not that I was interested in the use of concrete vs. steel, but I thought it to be an excellent opportunity to improve knowledge about energy absorption and ductility in any material, and to add greatly to the effort I had undertaken years before to try to do something about the earthquake problem. I had no idea when I got into it how controversial it would turn out to be years later.

About 1958, or possibly early 1959, I started work with Nathan Newmark on how to design concrete to develop ductility and energy absorption characteristics. We have to keep in mind that concrete by itself is brittle when it fails in tension or shear. Also, when it fails in compression it tends to disintegrate into many pieces. This is what we should avoid in building design, and it can be done. After many meetings, much analysis, and much laboratory work, we developed a procedure whereby concrete framing could be designed to be ductile and not fail in shear or diagonal tension. Even columns in compression could be so designed as to have toughness and ductility, to a certain degree. Of course it's desirable to avoid failure in columns in general, if it can be done.

PCA published a book under hard cover, copyright in 1961 as I recall, with the title *Design of Multistory Reinforced Concrete Buildings for Earthquake Motions*.[12] The joint authors are listed as Blume, Newmark, and Corning. Leo Corning's participation in this effort was purely editorial, since he worked as part of the PCA organization. He did a marvelous job, but all decisions as to the contents and procedures to be followed were made by Blume and Newmark.

Scott: So he was, in effect, a staff person of the Portland Cement Association?

Blume: That is correct—a highly respected man throughout the profession. In fact, we had such respect for him we made him a joint author, even though technically he was not such.

11. *Recommended Lateral Force Requirements*, Seismology Committee, Structural Engineers Association of California. SEAOC, San Francisco, CA, 1959.

12. Blume, John A., Nathan Newmark, and Leo H. Corning, *Design of Multistory Reinforced Concrete Buildings for Earthquake Motions.* Portland Cement Association, 1961.

This book, issued in 1961, was preceded by oral presentations by Newmark and Blume before each of the then three structural engineer associations—central, southern, and northern California. The book was distributed free of cost to all structural engineers in the state, and perhaps elsewhere, at the time. I have understood since that it has been translated into four other languages, and is in extensive use as a textbook and reference throughout the world. The authors were not compensated on a royalty basis.

Degenkolb-Johnston Critique

Blume: What I didn't expect was the controversy that developed over the book. Apparently the steel people, namely the American Iron and Steel Institute, took this as an attack on or a threat to the use of steel, rather than what it really was—a needed improvement of a building material that was going to be used anyway, and has always been used throughout the world in modern times.

Scott: In other words, it was not intended as an attack on steel, but rather to find better ways to use concrete.

Blume: Exactly. But the steel industry made it a competitive battle, and they engaged Henry Degenkolb and Roy Johnston as engineers to write a critique[13] about the book and its recommendations.

Scott: Roy Johnston of southern California?

Blume: Yes.

Scott: That would have been in the early '60s, fairly soon after the book came out?

Blume: Yes. It started in 1962, but as a result of what happened, it delayed the use of the principles involved, in getting them into codes, for seven or eight years. This was most unfortunate, because during that time period buildings were being designed without being ductile. I think Olive View Hospital is an excellent example—a brand-new building destroyed by the 1971 San Fernando earthquake, not to mention the thousands of concrete buildings not only here but in foreign countries where they design differently than we do in California.

The critique prepared by Degenkolb and Johnston was done quietly. In fact, I didn't even know about it until it was issued. When it came out it was distributed in supposedly a private manner, at least by the engineers involved. But apparently the steel industry, in promoting steel, spread it over the country. Without publishing, it was distributed by hand somehow. In fact, I heard that at the Third World Conference on Earthquake Engineering in New Zealand it was passed out to the delegates present.

The critique challenged the concept and criticized lack of detail about joint reinforcing, and other matters. When I got a copy of it I talked to Nate Newmark about what he thought we should do. He thought we should just let it "go away." But it persisted, so I requested a meeting of the San Francisco area engineers involved,

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13. Degenkolb, Henry J. and Roy G. Johnston, *Critique of the Portland Cement Association's "Design of Multistory Reinforced Concrete Buildings for Earthquake Motions."* American Iron and Steel Institute, 1963. Unpublished manuscript on file in the Earthquake Engineering Research Center Library, Richmond, CA.

including Henry Degenkolb, so I would have a chance to rebut the critique.

This meeting was held in San Francisco at the Engineer's Club in a private room, on September 18, 1963, and I rebutted the critique for a period of two to three hours, point-by-point, item-by-item. When I was done the chairman called upon Henry and said, "Henry, it's your turn now." Henry said, "I agree with 97% of what John said." So I said, "Let's talk about the 3%," and he said, "It's not very important." So I said, "What are you going to do about this?" He said, "Nothing, our work for the steel industry is completed." To make a long story short, the code adoption of the basic principles of ductility for concrete was delayed for several years after the book was released.

The main concern was the joint details, and admittedly they have been improved by more testing and research that has been done since. But I wish to point out that no use of any material is perfect at the first use. The best example is structural steel, where after almost 100 years of use it's still being improved. In other words, the improvements are evolutionary in nature. So anybody looking for absolute perfection in the first attempt to make concrete ductile was being, I think, a little too critical, and it was most unfortunate what happened.

Scott: Was the controversy partly a matter of the steel people misinterpreting or misusing the Degenkolb-Johnston critique, since Henry said he agreed with 97% of what you said? Did the steel people just kind of use it for overkill?

Blume: I think they must have, although the critique itself was very strong in some of its language. I think if Henry had it to write over he would write it differently. I say Henry—Roy Johnston was a co-author, but I suspect from what I know that Henry did most of the work on it.

I think the motive of the engineers was good—if they were merely trying to make sure that nothing was done that would be a public hazard. This matter was apparently picked up by the steel industry and used in an attempt to get more steel used, as compared to concrete. I think if they had simply talked over the situation and worked it out jointly, without wide distribution of this so-called private critique, we engineers and the public all would have been better off.

While not too many of these buildings have been tested in severe earthquakes, whatever results we have show two things: (1) nonductile concrete can be a hazard in a frame building, and (2) ductile concrete is infinitely better and safer.

I would not like to leave the impression, nor reinforce it if there is one already existing, that I am opposed to structural steel. Such is not the case; in fact, I have designed in all materials, and my office over the years has used as much, if not more, steel than most firms in the country. I have often said that any material of predictable and consistent properties can be made earthquake-resistant. Moreover, my father was a steel-erecting contractor, I worked as an ironworker on erection of steel towers, buildings, gas holders, etc., and I was a construction engineer on the San Francisco-Oakland Bay Bridge. In addition, I appeared in the Bethlehem Steel Company movie, "Men, Steel, and Earthquakes," and co-designed the Bethlehem

Steel San Francisco office building. So, in spite of rumors to the contrary, I am not prejudiced against steel. But it, too, must be properly designed.

The original Blue Book paragraph (j) about steel really stirred up a controversy, as some of us were afraid it would. But it got action that was in the public welfare, even though it turned out to be a lengthy, painful process.

Afterwards: Kept Off Committees

Blume: I was in a somewhat peculiar situation in the years after the Degenkolb-Johnston critique came out [in 1963] and the time of approval of the ductile concrete concept in 1966. I was no longer engaged by PCA, but apparently everyone thought I was, and kept me off of seismic committees to avoid a conflict of interest. I knew things were going on, but I did not know what. If anyone asked me a question, I responded with dispatch and tried to be helpful without prejudice. As I recall, Pete Kellam and Bob Dalton were the only ones to send me any information and ask my advice.

For example, Bob sent me data with a letter of February 11, 1966, from which I learned of the testing that had been going on by PCA, as requested by the contemporary Blue Book committees. Much of it was, as I replied to Bob, "Not conforming to BNC recommendations [recommendations put forth in the Bume-Newmark-Corning book[14]], ACI specifications, and/or realistic building geometry or loading." This was indeed a shock to me—time and money had been spent on testing specimens for which the results could have been anticipated—negative results. I listed 10 specific reasons why the tests were meaningless in large part, and I noted that the one specimen that was not too bad in similitude had a ductility value of 23![15]

Uniform Building Code Provides for Concrete Ductility (1967)

Blume: I do not know what went on thereafter, but the Blue Book issued later in 1966 contained some provisions for ductile concrete. Finally, in 1967, the Uniform Building Code included provisions for concrete ductility for highrise and certain other buildings, and this leads to another important point. Even though the original Blue Book code provisions required ductility only above a certain height, I think anyone who reads the book by Blume, Newmark and Corning will get the message very clearly that the principles apply to any frame building. A 2-3-4-story building can collapse just as well as a taller multistory building. So in 1967 the Uniform Building Code was amended to require ductility for all heights of buildings of certain characteristics. I heartily endorse this for all frame buildings. All in all, the use of concrete in buildings has improved dramatically in the last 10 to 15 years.

14. Blume, John A., Nathan Newmark, and Leo H. Corning, *Design of Multistory Reinforced Concrete Buildings for Earthquake Motions.* Portland Cement Association, 1961.

15. Ductility is the ratio of the ultimate strength divided by the elastic limit. Most ductility values, as applied in design, are in the range of 4 to 6.

Hazards of Nonductile Design

Blume: The most hazardous structures are certainly the unreinforced brick buildings, but also along with them I'm afraid of many nonductile-design concrete frame buildings of even a few stories. I think recent earthquakes in this and other countries have borne out this fact. On the other hand, if the principles are applied—the principles that came out in the book by Blume, Newmark, and Corning—this hazard can be avoided. It's a lot more work to design that way, and it's more difficult in construction, but the end justifies the extra effort and cost.

The concept of ductility came up in the original Blue Book committee meetings in 1957 and 1958. In fact, I repeatedly brought it up, as well as a few other people. At that time, it was felt that the only way to get this ductility was [through] the use of structural steel. After the 1959 Blue Book came out and PCA went to work on it, we found a way to make concrete ductile. The Blume-Newmark-Corning book was issued in 1961.

Scott: Would you explain in simple terms what the new design consisted of? Was it the use of reinforcement, or the design of the joints?

Blume: Well, reinforcement had always been used in reinforced concrete. It's not that as such. It's the proper amount and distribution and use of the reinforcement steel—the *way* it's used. If we take a beam in bending, or a girder in bending, the object of ductile design is to make the reinforcing steel reach its elastic limit *before* the concrete starts to crush. So to ensure this action, the principle is not to over-reinforce, because that makes the steel too strong. We want the steel to stretch like taffy, so ductile concrete involves the use of less steel in certain cases than otherwise might be used.

There's also the principle of confinement. In case the concrete does crack or crush in a severe earthquake, in a local manner, there has to be enough confining steel so that the hoops and ties and spirals keep the concrete from going anywhere. So these were new principles, consisting basically of (1) making sure the member—when and if it should have to fail locally—does so by the stretching of the steel bars, and in no other way, and especially avoiding at all times shear failures or tension failures; and (2) confining the concrete particles from any local crushing, almost like in a basket. Shear and tension failures are abrupt, lack ductility, and lead to complete failure of the system. They should and can be avoided.

Other Issues Had Roots in *Separate 66*

Blume: There were many issues in the early code days, other than ductility. Many of these had their roots in the 1951 ASCE *Separate 66*, and survived being worked over by many committees and eager committee members over the years. One of these, the triangular distribution of the lateral forces, which had its roots in my 1934 thesis effort, I believe still survives in all the codes. The portion of the total force assigned to the top of slender buildings has varied somewhat over the years; I clearly recall proposing that concept in a San Francisco meeting of the Blue Book committee, circa 1958.

In spite of much study and tinkering, for many years the [Separate] 66 forms of the "C" and the "T" computation have also held up well in the Blue Books, the Uniform Code, and the local codes.[16] The 1956 San Francisco code followed essentially the "66" format. So the products of much labor have been used extensively. The fact that the early efforts survived decades of reexamination by hundreds of active committee members indicates that the early work results were not all that bad.

Organizational Activity

Blume: Over the years I've been involved in a great deal of committee activity in various associations and agencies. I'd like to mention a couple more here, even though I'm jumping ahead a bit.

San Francisco Seismic Hazards Committee

Blume: The city and county of San Francisco set up a special public service committee in 1971 under the name of the San Francisco Seismic Investigation and Hazards Survey Advisory Committee, which is generally abbreviated to SIHSAC.

In spite of the long name, this was an official committee of the city. The members who were appointed had to be sworn in, and were paid a few dollars per meeting, but as I recall, not enough to cover dinner expenses. I was a member and chairman from the start in 1972 to the time of my resignation in 1978. During that time 30 meetings were held, each in an evening session of two to three hours, with excellent attendance by all members and also officials of the city, such as the building inspector, the director of public works, fire chief, and other officials from time to time.

Interest in this committee was triggered by the 1971 San Fernando earthquake. One of the key issues that we faced early on was enforcement of the so-called parapet ordinance, which was a real hot potato. The parapet ordinance requires that downtown buildings have their parapets examined, and if found faulty—as most of them were—they had to be either removed or strengthened. The object, of course, was to prevent the parapets from being thrown into the streets and killing people. Past earthquakes have shown that parapets and ornamentation on buildings create real hazards to those in the streets. Of course it's not a good policy to be in the street next to a tall building anyway, but people are, nevertheless.

This committee was unique in the sense that architects, structural engineers, mechanical engineers, electrical engineers, soil mechanics people and seismologists were all on one committee. It is a good example of something I've been preaching for a long time, and that is the interaction of the professions. I've often said that the earthquake doesn't give a damn what you have on your diploma. These various professions have to work together to make headway.

I had to resign from this group in 1978, because I was getting ready to assume the presidency of EERI, which was by then a very active organization.

16. C = applied lateral force coefficient; T = period of vibration.

Scott: Would you say a little about the selection of the committee? It sounds like a pretty able, distinguished group.

Blume: Yes, it was. The selection was generally made by each organization in the city, for example, the structural engineers would have appointed someone, the architects would appoint someone.

Scott: So the city asked each of the organizations to recommend or appoint someone.

Blume: I think the only exception was myself. I think I was appointed by the city to start with. But even though the association would appoint a person, he would have to be blessed by the city fathers and sworn in.

Scott: Would you say a little more about what it has done? Is it still in existence?

Blume: Yes, I believe it's been reactivated recently. The function of the group was manyfold. It was partly educational, to help the fire department and the building department and the others to get up to speed on the latest earthquake findings. It was also advisory to the mayor and to the supervisors in the sense that with any controversial or pending issue, they could come to us and ask our advice if it had anything to do with earthquakes. In general we were a referral group, where the city could come to us for any questions they had on earthquake problems, and they had plenty.

National Academy Hazards Panel

Blume: Another committee that I was chairman of, about 15 years ago [1975], was a national committee sponsored by the National Academy of Engineering. It was actually a panel entitled the Natural Hazards and Disasters Panel of the Committee on Public Engineering Policy of the National Academy of Engineering. This committee was charged not only with earthquakes, but also with all natural disasters, including hurricane, tornado, flood, firestorm, windstorm, and anything of a natural basis. The best example of fire hazard being natural is the 1987 California fires.

This committee was represented by very fine members from all over the country. We met in San Francisco for several days straight. We made ourselves useful in national research budgeting procedures and our recommendations were well received.

Chapter 6

History of EERI

"EERI turned out to be a viable organization. It has done a great deal to help research and other efforts in earthquake engineering and structural dynamics."

Advisory Committee on Engineering Seismology (ACES)

Blume: Going back a few years and talking about the early organizations' concern with earthquake engineering and engineering seismology. In April 1947,[17] eight people met in San Francisco with officials from the U.S. Coast and Geodetic Survey. They met by invitation. The eight people were: John Bolles (architect), Harold Engle, Harmer Davis, John Little, Lydik Jacobsen, Henry Powers, D.C. Willett, and John Blume. The purpose of the meeting was to discuss the possible formation of a committee to advise the United States government on earthquake matters.

In May 1947, Professor R.R. Martel, Col. William Fox (Superintendent of the Los Angeles County Building Department) and George Housner, from southern California, were added to the group. In September 5, 1947, these eleven peo-

17. In 1947, Blume was vice-president of SEAONC, working on code matters for the San Francisco building code (Vensano code, eventually issued in 1949), working for the Arabian American Oil Company, and developing his business as a sole practitioner.

ple, plus Professor [Alfred L.] Miller of the University of Washington, and Samuel Morris of the Los Angeles Department of Water and Power, met for the first time in a very busy all-day session in San Francisco. These people represented five universities, three governmental agencies, and seven professional organizations. The name of the group was Advisory Committee on Engineering Seismology (ACES). Lydik Jacobsen was elected chairman, Col. Fox, vice chairman, and John Blume, permanent secretary. These three also constituted the executive committee.

Founding and First Meeting

Blume: The agenda was enormous. It included about every subject possible on earthquake engineering. Among the 30 items considered on the ACES agenda that day was "to establish an earthquake engineering research institute." ACES met at least once per year, and there was a lot of activity in between the meetings on the part of the officers. Always uppermost in the discussions was the need for an earthquake institute. Of course the institute they had in mind was not just a society, but an actual testing facility with a director—the whole works.

Out of this ACES group was conceived the idea of the Earthquake Engineering Research Institute (EERI). I'm happy to say I had an opportunity to play a prominent part in the formation of that institute, which today has over 2,400 members, and is known throughout the world. Committee activity on the part of the ACES members led to reports on how the institute might be organized and formed. It was finally decided to incorporate as a nonprofit organization.

The first year, our meeting was in San Francisco, on April 2, 1949. Because of legal mechanics, the first members were only four—Lydik Jacobsen, George Housner, John Bolles and Blume. However, minutes later the other members of ACES were brought in as members of the institute. John Bolles was an architect who was active in the early days of the institute, but dropped out of EERI activity a few years later. Frank Ulrich was also admitted as a member.

For many years—I believe until 1973—membership was open only by invitation. It was considered an honor. After 1973, however, membership was open by application, and now members are from all over the world.

The original bylaws provided for seven board members, no more than three of whom could be from any one of the following fields of effort: professional practice, teaching and research, and governmental regulation. The object of all this was to create a balance, for a better exchange of philosophy between the different groups.

At the first meeting, Jacobsen was elected president, Housner vice-president, Blume secretary, and Ulrich treasurer. I served as secretary until 1952, when that office was taken over by Ray Clough, who was then a young professor at Berkeley.

A Clearinghouse Role

Blume: In spite of all the high ideals about forming an institution with a building, laboratory and director, that still hasn't come to pass and may never come to pass. What happened instead is that the institute worked as sort of a clearinghouse for research information and for policymaking. Funds for research were channeled into the universities, namely Berkeley, Caltech, Stanford, and a few others.

Scott: Instead of going to EERI, the research funds went to the university facilities?

Blume: Yes, often with the endorsement of EERI. In fact, EERI would have ad hoc committees who would report on what should be done, on what might be done, and some university—often Caltech—would pick up the idea and go ahead with it.

Scott: So EERI helped generate or promote ideas for research?

Blume: Yes. It also gave the endorsement by a wide body of recognized people that the research was worthwhile. That is something the government is always looking for. In fact, they're still looking for this kind of endorsement.

In 1955, a group of the San Francisco Bay Area members of EERI came up with the concept of a world conference on earthquake engineering —something that had never been attempted before.

First World Conference on Earthquake Engineering

Blume: John Rinne, who was then a director and vice president of EERI, became the active chairman of an organizing committee to explore the possibilities of a world conference, and later to execute the project. The committee members were R.W. Binder, John Blume, William Cloud, Ray Clough, Henry Degenkolb, Martin Duke, Alfred Miller, Henry Powers, and John Rinne. Arrangements were made with the University of California, Berkeley, extension division, to provide the meeting hall and to help with the housing arrangements.

A great deal of work was done, writing to different countries and exploring the idea of whether they could attend, whether they would attend. The job I was assigned was to promote foreign attendance, and to organize panel discussions with people from different countries. I'm happy to say that the meeting came off well. As I recall, we had attendance from over 20 other countries. The 5-day conference (June 12-16, inclusive, 1956) was completely successful.

There were about 40 papers presented at the conference, with the authors from 13 countries. In addition, there were two lively panel sessions with panelists from five and six countries, respectively. The Japanese were very active in attendance and in participation. My paper was a complete update of the work on the 15-story guinea pig, the Alexander Building, from my 1934 thesis. I spent many days on new analyses for this paper. I also moderated one of the panel sessions. Harold Engle was moderator of the other panel. English was the main language, but there was considerable interpreting. All in all, things went very well indeed.

Three Periods of EERI Activity

Blume: Even without the world conference success, EERI turned out to be a viable organization. It has done a great deal to help research and other efforts in earthquake engineering and structural dynamics. My organizational activities were bunched into three time periods. First was the original organization, in which I played a very active role in working with the lawyers, drumming up cash funds to pay for initial expenses, and as the secretary, locally situated in San Francisco, pretty much carrying the ball on the details. Then after several years as a director and officer I dropped out of active participation, came in again about 10 years later, and dropped out again. Finally, in 1976 I was made an honorary member, and following that I was elected president, and started serving the organization all over again. I was president for three years, 1978, '79, and '80, and found it to be an entirely different organization than we originally had. It was large, diversified, and very active. All in all, it's been a very satisfactory experience and the organization is completely successful.

Relations With the Seismological Society

Blume: Going back in history once again, I might add that in the early days it was thought that the Seismological Society of America might get involved in the organization [EERI] or take it under its wing. That was kicked around at one time. Digging into my old files, I hoped to find correspondence that would be very interesting, back in 1948-49, about these things. There was a nice letter from Perry Byerly on this subject, but I have not been able to locate it. SSA was not at all opposed to the concept of EERI, but simply felt it should be an independent organization.

One of the main things that EERI has done is to provide [a forum for] a cross-section of the various types of people and agencies interested in earthquakes, [it has become] the one common denominator. We have architects, engineers of all types, seismologists, building officials, social scientists, educators, officials, insurance people—everyone who should be concerned with earthquakes is represented in the membership. I believe EERI is the only organization that does this. It is very important, because the solution of the earthquake problem, in my opinion, is not just technical—it involves society. People's reactions to earthquakes and their welfare in earthquakes are more important than the buildings themselves. The great loss of life that we have heard about in other countries could happen here, though not as bad, we think, because the buildings here are slightly better. But EERI has fostered and undertaken not only the study of earthquake damage, but research, theory of dynamics, seismology, and is now engaged in putting out formal publications on these subjects. It has become the focal point of earthquake engineering in this country and, to considerable extent, in other countries as well.

International Association of Earthquake Engineering (IAEE)

Blume: After the First World Conference in Berkeley in 1956, the next one was held in Japan in 1960. As a result of that meeting, and some more effort on the part of John Rinne and others—especially Dr. Kiyoshi Muto of Japan—the International Association of Earthquake Engineering was organized. It is still active and consists of representatives from a great many countries. The world conferences have been held under its sponsorship roughly every four years ever since—New Zealand, Chile, Italy, India, Turkey and San Francisco. Volumes of papers and proceedings are published from each conference. So from a very small beginning, things have developed very nicely in this regard.

Chapter 7

Seismic Work for Nuclear Testing

"...I had to lean strongly on what I knew about natural earthquakes and how buildings responded to natural earthquakes."

Blume: Back in September 1963, the Atomic Energy Commission (AEC) set off an underground nuclear test at the Nevada Test Site. The test was called BILBY. This caused reaction in downtown Las Vegas and elsewhere, because buildings 80 to 90 miles away responded to the ground motion resulting from the event. This caused quite a flap.

As a result of this, the Atomic Energy Commission undertook a search for a firm to handle the structural response problems, to ensure safety of the public and structures at all times, and to be of advice regarding limits of ground shaking that might be undertaken. We were asked to put in a proposal for this work, which we did, in 1964. I was very happy to learn later that we were selected to be the contractor, as they called it, on structural response from all nuclear testing, not only at the Nevada test site, but wherever it might be done for the U.S.

Meager Data

Blume: In the early stages of this effort we had very, very little—in fact meager—empirical information to go by in the seismic field of nuclear shots, so I had to lean strongly on what I knew about natural earthquakes and how buildings responded to natural earthquakes. There was a rather ambitious nuclear testing program planned, including some shots that would be considerably greater than BILBY.

Although we preferred that they escalate gradually into what they call yield, or the effective energy of the explosion, so we could gain empirical data as things progressed, this was not practical from the testing point of view. So we had to analyze structures and outline instrument arrays that would record the motion of the ground, and motion in the buildings, and constantly advise the AEC officials as to any hazards that might be involved.

I found this to be quite a challenge and a great responsibility. I personally was involved, and led all of the effort for a great many years. It was also a great opportunity to gather information that would be useful in the natural earthquake program. The main reason for this is that we knew in advance when the "earthquake" was going to happen—which is not true of natural earthquakes, at least not yet. We could set up instruments, station observers, and generally act on a scientific basis to collect data that would build up a body of knowledge, which we did.

Safety Concerns: Possible Structural Damage

Blume: In the meantime, however, we had to be constantly aware of public safety, and advise the officials as to when a shot might be considered feasible from their point of view, but may not be from our point of view.

Scott: Do you mean from the viewpoint of possibly causing structural damage to some existing buildings?

Blume: Yes. We constantly had to keep that in mind.

Scott: In other words if you thought they were working up to the point where the next shot might cause damage nearby, or especially in Las Vegas, some 100 miles away, you would advise them of that?

Blume: That's correct. At times it would make me very unpopular with the testing laboratories, notably Lawrence, Sandia, and Los Alamos, whose main function was to test the weapons and devices.

Scott: When that happened, how did you resolve the matter?

Blume: Well, we had to demonstrate our analyses before these scientists, and believe me they had some brilliant people involved from all the laboratories. I gave dozens, perhaps hundreds, of presentations explaining not only what we thought but how we arrived at the conclusions. Our efforts might have reduced some of the shot levels involved, but not drastically. We were willing to go along or extrapolate to a certain extent, as long as we could instrument and monitor, and the public was not in danger.

One of the things that made it very difficult was the nature of the buildings in Las Vegas. Las Vegas is from 70 to 100 miles away from the heart of the test site where the shots originate, and yet the long period surface waves would come rolling through and last for a full minute or more. The nature of these waves was such that it made the tall buildings really respond. These tall buildings were designed under the old Uniform Building Code, zone 1, which is so light, earthquake-wise, that the wind-design criteria would be much more of a lateral force factor than earthquake design requirements.

Scott: Zone 1 is the lowest, or next to the lowest?

Blume: It's next to the lowest in the old Uniform Code, so that was a bad omen to start with. The second bad feature in Las Vegas was that most of the engineers who designed these buildings were from the east [coast], without any real knowledge of earthquake matters. As I have always said, to do a good job you not only need to follow the code, but you also have to have a feel for earthquakes. So here we were with many buildings that we really didn't like, which I would say was the choke point on the whole testing program.

In fact we found one building to be so bad that we had to report it as an individual case to AEC management, and believe it or not, after months of study and consideration, the United States government fixed this building up under our direction, so it would not affect their test program.

Scott: You mean, even though it was a privately-owned building, they fixed it up so they could get on with their testing program?

Blume: That's right. I don't think it had ever been done before, or [has been] since. Fortunately, it was fairly simple to fix up. There were one or two weak stories, which we corrected with more walls. But in the process of breaking into the old structure to connect the new walls, we found that the old structure had not been built according to its own drawings. It was worse than we thought it was. But we fixed it up, and it's been through a great many shots since with no problems.

Reporting on the Shots

Blume: All of our prognostications as to what might happen had to be submitted in writing, and also orally before scientific committees. Immediately after each shot we had to write another report explaining how things compared with our predictions, and explain any variations. Fortunately, the variations turned out to be minor. All the way through this program, what we learned—and I've published many papers on what we learned—has been a great help to the earthquake field and vice versa. Also, the knowledge in the natural earthquake field helped to carry us through those early years in the test program. In other words, the two types of earthquakes—natural and test-shot—were synergistic.

Nuclear and Natural Earthquakes

Scott: Would you compare and contrast the earthquakes caused by the underground nuclear explosions with natural earthquakes?

Blume: A seismologist can tell immediately by looking at a time-history record of the motion whether it's nuclear or natural [ground motion]. There are perhaps two things involved here. You said "earthquakes" caused by underground explosions. That is one possibility, an explosion triggering a natural earthquake, which would eventually have happened anyway. No doubt some small earthquakes have been so triggered. But our main problem in this testing program was the earth shaking—the "artificial" earthquake caused by the explosion—which is slightly different. An earthquake caused by explosion in Nevada is surprisingly long-lasting. I mentioned a full minute of strong motion. It's also surprisingly periodic. By that I mean the natural periods of the waves tend to repeat over and over again, which is tough on a structure that has a period in the same range.

The Las Vegas Valley is deep, of alluvial-type material, like a bowl of stiff jelly actually, and the waves bounce off the mountains and the sides of the valley and keep pouring in from all directions. But, as with natural earthquakes, there is first the compression wave, then the shear wave, then the surface waves. The waves are quite similar in the two kinds of earthquakes. As far as magnitude is concerned, it's hard to compute the definite magnitude, but roughly the biggest shots in Nevada would have been 6 or 6+ on the Richter magnitude scale, which is a big earthquake.

When you're in a tall building during one of these shaking events you can definitely feel the motion. The persons who were not concerned were the gamblers, who wouldn't even look up from the table. They'd just carry on with their gambling, regardless of what happened. I've seen chandeliers swing back and forth, with the gamblers playing games right below them without even looking up.

Reassuring the Public

Blume: In the early stages of the program, in fact for many years, during each major shot the Nevada operations office of AEC wanted me personally to be at the top of one of the tallest buildings in town. Of course it would be one tall building for one event, and for the next event I'd go to another building, etc. They would personally assure the public by radio and TV, I guess, that everything was all right—"Dr. Blume is going to be on top of one of the tall buildings." It was rather strange to hear this coming over the radio early in the morning while you're waiting for a shot to go off.

Scott: Your presence there was to reassure the population.

Blume: Right. We also had a scale program set up to record human observation of the motion. Each event had trained observers at all the buildings at various points, and they would rate the motion according to a scale that we developed. Of course I would also do my own rating. I was at the top of the Mint Building in downtown Las Vegas for one shot, and I happened to be in the penthouse right opposite the swimming pool, which was full of water.

The shot finally went off, and after the compression and shear waves went by, the surface waves started pouring in, and the water in the swimming pool started to respond. The water was sloshing over both ends alternately, onto the paved tile.

One of the newsmen who was up there didn't get enough action shots, so afterward he stacked about 30 silver dollars on top of each other to make a pile. He then proceeded to hammer the table with his hand (out of camera range) and took pictures of the silver coins falling over.

Scott: He manufactured his own miniature earthquake!

Blume: That's right. This reminds me of when I was shaking the Los Angeles City Hall with a shaking machine years before. The Pathé News camera people were there, and they too did not get enough action from the first go-around, so they proceeded to take a glass of water and put it on the machine. Then out of range of the camera, they would hit the machine with a sledge hammer so the water jiggled. I guess you call these things "local color" in the news media.

We had many interesting events occur during these shots. During one shot we were stationed in the Dunes Hotel in a stairwell with instruments up high in the building. There had been public announcements in advance that there would be a shot, and to be aware of ground motion and building response. But this one woman apparently didn't hear the forecast, because she ran down the stairs completely nude yelling, "There's an earthquake, there's an earthquake!"

The Howard Hughes Protests

Blume: Howard Hughes did not like nuclear explosions, and he did all he could to block two of the largest of the underground shots in Nevada — BOXCAR and BENHAM. Each of these was to have roughly 4 times the energy of BILBY. The AEC [Atomic Energy Commission] made advance public announcements of these events. In fact, public briefings were held, in which I, among others, participated. On April 24, 1968, two days before BOXCAR went off, a large public meeting was held in Las Vegas. About the same time Hughes had people in Washington, D.C. trying to block the test, and giving press conferences about the hazards involved, especially to Hoover Dam, of all things! I pointed out at the public meeting the fact that Hoover Dam had already survived, without incident, natural earthquake motion 154 times stronger than from event GREELEY, which had already taken place, and that GREELEY had almost the same yield as was proposed for BOXCAR

The then Hughes dam expert, according to the press, was a botany professor at an eastern university. I should have mentioned that Howard Hughes was then resident in Las Vegas. The story was that when he arrived he could not get the rooms he wanted so he bought the hotel and occupied the top story. There was no doubt about his feeling the motion from the shots. BOXCAR went off on schedule in spite of all the protesting, and everything went according to plan. The yield was 1,300 kilotons.

Another big event, BENHAM was scheduled for December of the same year, 1968. Howard Hughes had a whole battery of experts of all types. They asked for, and got, a special AEC briefing session on December 17, two days before the shot was to go. Hughes had 15 or so experts, good ones for the most part—on groundwater hydrology, structures, dynamics, radiation, and the like.

The AEC consultants, including myself, each briefed this group about this phase of the operation. The Hughes people would ask questions and offer any comments they wanted to. The meeting went on all day, with no stop for lunch or anything else. The AEC chairman of the meeting, Jim Reeves, smoked a corncob pipe. Whenever the meeting looked like it was dragging on too long, and people were getting nervous, he would pull out his corncob pipe, slowly fill it with tobacco, tamp it and light it, to show that he had all the time in the world. The experts had very few questions actually, because we'd done such a good job briefing them.

We learned later that Howard Hughes was also working through his people in Washington, D. C. to get the shot stopped. That failed, and the meeting with the experts in Las Vegas also failed, because there was nothing they could bring out that could counteract what we had already planned for.

Scott: In other words, Howard Hughes was getting a rather elaborate independent review done of the AEC and your activities, which stood up well.

Blume: That's right. The result was it cost Howard Hughes a lot of money, which I guess didn't matter to him. The BENHAM shot went off on schedule, and everything went according to plan. It was rated at 1,150 kilotons.

Damage Claims Increased With Publicity

Blume: It was interesting to note that the more a shot was pre-publicized, the more damage claims would come in afterwards. We investigated all structural damage claims, and even leaning over backwards for the property owners, we found only a tiny percent of the claims had any reality to them. They'd complain about cracks in walls, foundations, swimming pools, and we'd look with magnifying glasses and we'd find there would be coats of paint, spiders, dust—in other words the crack was pre-existing. On one wall we found the wall had been painted four times since it originally cracked. The property owners were not, in general, trying to cheat the government. They just didn't realize the situation until they were advised that the ground was going to shake. On one occasion, AEC received a telephone call from a property owner who said, "I have damage to report from the shot that went off this morning." He was informed that the shot had not gone off—it had been postponed on account of weather conditions until the next morning—and he said, "I'll call back later."

Modest Code Improvements

Blume: I mentioned before about the Las Vegas building code then being zone 1. In many of our publications we pointed out that it should be a higher zone—2 or 3 for taller buildings—which would "feel" the motion from distant earthquakes, if not from nuclear shots.

Scott: You mean even without the nuclear shot consideration, Las Vegas really should have been in a higher zone?

Blume: Right. In fact, one of the things I have long pointed out in my work in general is that tall buildings that have long periods of vibration are subject to distant, large earthquakes, which produce large surface waves with long periods of vibration. In other words, if the tall buildings in Las Vegas should be subjected to motion from an earthquake somewhere in the Owens Valley in California [such as happened] in 1872, they would respond strongly to that motion. So I believe that tall buildings should be designed on a different basis than low buildings.

I'm happy to report that Las Vegas, after listening to this kind of talk for a few years, did graduate to zone 2, and took a little more interest in the earthquake design of its buildings. But even zone 2 is not very much of a safeguard from very large, distant earthquakes.

Scott: An Owens Valley earthquake could have something a bit like the Mexico City effect, couldn't it?

Blume: That's right. The Las Vegas valley is not nearly as soft as the Mexican valley, but the same principle applies. Waves can be periodic, and have many repetitions and quite substantial amplitude. Mexico City is like a bowl of jelly. Las Vegas has a lot of stiff gravel mixed in with the clay and sand. It's the same principle.

Chapter 8

Return to Stanford for Doctorate

"In 1964 I decided that if I were going to keep that up, and even extend the work, I should go back to school and get caught up to date on everything new that had emerged since I had previously studied."

Scott: You said we might start today with a discussion of your returning to Stanford for your doctorate.

Blume: I think it might be worthwhile to cover this subject briefly, as many people have peculiar ideas about why I went back to Stanford after being out for 30 years. In fact, I was called a 30-year dropout. I had been working very hard in the early 1960s and prior years on some exotic analyses of buildings and other structures for dynamic treatment of the earthquake problem. In 1964 I decided that if I were going to keep that up, and even extend the work, I should go back to school and get caught up to date on everything new that had emerged since I had previously studied. So I went back to Stanford, my old alma mater, in 1964. I embarked on a two-year program toward getting my doctorate in engineering.

Before starting this program I had no idea of all I was getting into. If I had it to do over again, I'd think twice. Nothing I have said should reflect on the Stanford program, but rather on the uncertain workload I was under with my business. I did not and could not quit my office at all. In fact, I continued to run my office and manage several projects, while I went to school half-time. What upset my early plans was the great demand on my time, especially for travel out-of-state for clients [PG&E, the Nuclear Regulatory Commission, the Atomic Energy Commission, General Electric]. When I started out, I found myself doing homework on airplanes, cutting classes, and in general not being a good student.

I enrolled in regular courses for credit for about half of the normal workload at school. I found myself in competition with top students from all over the world. It was very interesting. After one quarter during which I missed classes and had to scramble to get caught up on my work, I had to knuckle down and really face the problems of being a graduate student. I want the record to be clear that this was an earned degree in all respects. I say that because some people have implied that I had some special treatment because of being an old-time alumnus of Stanford. Such was not the case. The school does not work that way at all.

I took courses in matrix algebra, computer analysis of complex structures, computer software technology, decision theory, probabilistics, statistics, and in general, everything that was fairly new to me. In addition to my courses for credit, I audited all the other graduate courses that I thought might be of value to me. My dissertation was done under the advisorship of Professor Don Young, who was a very fine gentleman, and a wonderful scholar. He had co-authored several books with Timoshenko, one of the world's greatest engineering educators [Stephen Timoshenko, 1878-1972].

The title of my dissertation was "The Dynamic Behavior of Multistory Buildings with Various Stiffness Characteristics."[18] I completed my work and was awarded my Ph.D. degree in January of 1967. I have since been very glad that I went through this program, not for the title, but for what I got out of it, which I have used many times since.

Managing Conflicts of Work and Study

Scott: How did you manage to get through all this and still run your own business?

Blume: It was quite difficult, not only because of the complexity of the school work, but also because trying to plan my time was virtually impossible. For example, I'd set aside a day to work on my dissertation or some other subject, say a Tuesday. My office would get hold of me or track me down on Monday and say, "Client X wants you to go to Washington, D.C. tonight for two days of hearings," which would mean I not only failed to get the Tuesday time free to work with, but also the week was over half gone.

I often had to give up a lot of sleep. In my first quarter back at school I made at least six trips out of the state, and possibly seven or eight,

18. Blume, John A., "The Dynamic Behavior of Multi-Story Buildings With Various Stiffness Characteristics." Ph.D. dissertation. Stanford University, CA, 1967.

within a 2.5 month period. So you can see what that did to my school work.

But I toughed it out. I'm pretty stubborn, I guess. Things improved later on. After I reached the point where I had finished my courses for credit, I was then on my own for the dissertation work.

It was a very complex dissertation, involving a great deal of computer work. In fact, I found that during the day at the computer center [at Stanford], if one had a mistake in his first run, he'd have to wait an hour or so to get a second run. I soon learned that if I arrived at the computer center at 11:00 or 12:00 at night, I could get almost instant turn-around between the hours of midnight and six in the morning. Toward the end of my dissertation, for about the last six or seven months, I virtually worked every night at the computer center, from midnight until dawn. By doing that they even let me run the machine at times, as well as do my own programming and card punching. I got so I could program directly on the card punch machine. I got a tremendous amount of work done that way, but it was hard on my sleep routine.

Chapter 9

Nuclear Power Plant Design

"The design analysis of nuclear power plants forced technology ahead by many years—even decades ahead of its time."

Scott: Did your work on nuclear power plant design come at approximately the same time as the doctoral work at Stanford?

Blume: Actually, some of it came at the same time, but some of it came many years prior to my Stanford work, as well as years after. It so happens that, in my opinion, one of the great boosts to the earthquake engineering field was the advent of nuclear power plants. The design analysis of nuclear power plants forced technology ahead by many years—even decades ahead of its time. Approximations that are inherent in the design of tall buildings, for example, are too crude to be accepted for a nuclear power plant analysis. The result was that as these power plants came into being, a great deal of research and hard thinking had to go into the earthquake criteria by the pioneers in the field. This included Nathan Newmark of the University of Illinois, and our office.

Early Work for General Electric

Blume: Before doing work for the Nuclear Regulatory Commission, NRC, we had done prior work for several years for General Electric Company. As I recall, the first nuclear plant I worked on—I believe the first plant anyone worked on in dynamics for the earthquake features—was a small plant in Japan, a boiling-water reactor being built by General Electric Company of Japan.

We were called upon to do earthquake analyses of a type that had never been done before, and I personally conducted most of this work. I don't recall the exact date, but it was in the late '50s or early '60s. After this plant for GE, we worked on several other plants for General Electric in various parts of the United States, Europe, and the Middle East.

Scott: These analyses were on nuclear plants?

Blume: Yes, nuclear power plants. The procedure soon turned out to be that instead of designing by any assumed lateral forces, we would determine the earthquake ground motion for the site in question, develop response spectra suitable for this site, then design the plant under elastic stress conditions to withstand this earthquake demand. We would also expose dynamic models to ground motion records. This was a tremendous advance over building codes in general. In fact, no building code would apply to a nuclear power plant. Such plants are intended to be much too conservative to be designed according to normal building codes.

Work for the Nuclear Regulatory Commission

Blume: After working for GE for several years, I was approached by the Nuclear Regulatory Commission (NRC) to act as a consultant to help them in the licensing of nuclear power plants being designed by others. They had Dr. Nathan Newmark doing the same thing. It was a little awkward at first to figure out how to avoid conflicts of interest, because we were still on plants for private industry.

Scott: You had been designing nuclear plants for private industry, and now you'd been asked to help advise the Nuclear Regulatory Commission on seismic criteria for judging new nuclear facilities?

Blume: For judging whether or not they should issue a license to a new plant, and on what seismic criteria. A system was worked out whereby if we had any connection whatsoever with a plant, we would not also act as an NRC advisor. Also, if we had not had any connection, we would guarantee not to become involved in the future with a plant on which we were advisor. Then we'd go ahead and help NRC analyze a plant and the criteria, and decide whether it should be licensed. Our office did this for several years, and so did Dr. Newmark and his associates. All this involved considerable travel across the country, and the constant new application of theory. Many advances were made.

Scott: Would you say a little more, in layman's terms, about the ways in which a nuclear power plant has to be designed to much more conservative criteria and standards? Also a little more about the new theory or approaches that you used, both in your work in design and your work in advising the NRC?

Blume: Yes. Normal building codes, even for normal highrise buildings, have seismic design lateral force coefficients ranging from 2% to

10% or 12%. If, however, we're to build a nuclear power plant at the same kind of site as such conventional structures, those code coefficients would not be used at all. Instead, very extensive geologic and seismological research and investigation would be undertaken to determine, as well as could be done, what the probable maximum shaking of that site would be in the future, for several hundred years. Then the plant would be designed on an elastic basis, without going into the great range of ductile response, to withstand that shaking.

This might mean from 5 to 10 or even 15 times greater lateral forces to be resisted, than in an office building at the same site. Bear in mind, however, that in the office building it is considered all right for the building to go into the inelastic range in responding to earthquake forces, and absorb energy while stretching or yielding. Such stretching or yielding is not allowed under the nuclear plant design criteria. The net result of all this is that nuclear plants, such as Diablo Canyon, on which we worked for years, are tremendously strong as compared to any modern building. The dynamic theory used in their design is much more advanced. It would be economically impossible to design an office building to the same standards as a nuclear power plant, nor is it necessary.

PG&E and Diablo Canyon

Blume: In 1967 the Pacific Gas and Electric Company (PG&E) came to us to talk about the plant they were proposing to build at Diablo Canyon, which is on the coast just a few miles north of San Luis Obispo [California]. We agreed to participate in this effort. Our scope on this job was structural response and dynamics. During 1967, and probably 1968, we developed the criteria for the design of the plant to be in accordance with the seismic exposure or possible ground shaking, which was determined by others—namely geologists and seismologists. PG&E had a whole battery of consultants on this project.

I made frequent trips to Washington and Bethesda to attend hearings and meetings regarding the criteria, and soon found myself at the opposite end of the table from my former book coauthor, Nathan Newmark. For this particular plant I was representing the owner, PG&E, and he was representing NRC. We had many a lively session before we hammered out what turned out to be the design criteria for the plant.

Hosgri Fault and Conservative Design

Blume: The criteria [in 1967] generally consisted of designing for four different earthquake possibilities, including one earthquake right underneath the plant, where there was no known fault. A few years later, probably around 1970 or '71, it was discovered by others that there was a fault in the ocean bed about three or four miles offshore from the Diablo Canyon plant. It was given the name Hosgri. This fault caused a flap that would continue for at least 8 or 10 years, in fact it's probably still going on in the minds of many people. As soon as it was discovered, PG&E asked us to consider what effect the discovery might have on previous design criteria, which we did.

We found that because of having designed the plant for a sharp earthquake right below it, though there was no known fault there, basically the plant could withstand any earthquake reasonably assigned to the Hosgri fault. But it was not that simple.

Thus the debates went on for years as to how big the offshore fault was, and how big an earthquake it might produce. We had designed for an earthquake with its epicenter anywhere within a radius of so many kilometers, including straight down. The focal point of most earthquakes is not at the surface at all, it's down in the earth, maybe five to ten miles. The criterion for this—which was something new, by the way—is called a non-associated earthquake, meaning an earthquake not associated with any known fault.

Scott: Then this was a very conservative design philosophy.

Blume: Yes, it was quite conservative. Something had happened in South America in a previous earthquake that led a few scientists to believe that there can be such "non-fault" conditions. So due to the philosophy in the nuclear plant business, earthquake-wise, if there was any doubt, the conservative assumption was made.

Hearings and Protesters

Blume: After the Hosgri fault was discovered, there were all sorts of hearings and meetings by various committees. Most of them were open to the public, and I recall testifying many times. The rooms were usually filled with nuclear plant objectors, some of whom were dressed in skeleton costumes with the word "plutonium" across their chests, and ladies with small babies sitting in the front row. These were all great distractions intended to influence the commissioners to vote the plant down. Various organizations opposed to nuclear power plants appeared, such as the Abalone Alliance. Apparently, they started out with the idea that a nuclear plant would kill all the abalones.

Our part in all of this was neutral. We took no political position one way or the other, but merely testified to the facts. But it was very disconcerting to spend weeks and months on a report, and testify as to the findings of that study, only to have none of the objectors listen at all. The commissioners listened, of course, but the public objectors rarely, if ever, listened to what was said. They were, however, quite aware of the TV cameras.

The hearings went on for years. In fact, I once made a list of meetings I had attended outside my home base area on this plant alone, and it amounted to about 40 trips—each one of which took two or three days, and on a few occasions the hearings went on for weeks. Most of the hearings were in the Washington, D.C. area so I was a frequent commuter on the airlines going back and forth to Washington. Through all of this, PG&E demanded my personal attention, which I was glad to give because I was interested in the problem, but it was extremely time-consuming.

Reanalyzing Design Criteria

Blume: Returning to the subject of the Hosgri fault, it first took a while to confirm that there was indeed a fault out there in the ocean. When this had been accomplished, the problem was to determine whether it was active, or could be active, and if so, how big an earthquake it could support. The prevailing philosophy was, and is, that there is a relationship between the length of a fault and the size of the potential earthquake on that fault. The objectors wanted to hook the Hosgri to a hypothetical chain of faults involving the San Andreas. The PG&E consultants considered this as ridiculous. And so it went, for a long time. I shall not attempt to cover the details here—the problem was too complex, and the record voluminous. Suffice it to say that a design earthquake on the Hosgri was finally established, for which the plant had to be reanalyzed.

The maximum magnitude was [postulated at] 7.5, a very large event. I undertook several studies of a probabilistic nature to obtain the site peak accelerations and the probability of same. The main alternative procedures developed and/or used were regression analysis of past earthquake data, integration of fault dislocation data over long time periods, and consideration of plate boundary dislocation rates. All faults in the region were considered, including the San Andreas. The techniques employed and the results obtained were published in the November 1979 issue of the ASCE *Journal of the Structural Division*[19] as a joint paper with Professor Anne Kiremidjian of Stanford. Our original analysis was found to be sufficiently conservative.

19. Blume, John A. and Anne S. Kiremidjian, "Probalistic Procedures for Peak Ground Motions," *Journal of the Structural Division*, Proceedings of the ASCE, Vol. 105, No. ST11. ASCE, New York, NY, November 1979.

"Mirror Image" Problem

Scott: While we're talking about Diablo Canyon, could you talk about the so-called "mirror image" design problem that came up?

Blume: As a result of the Hosgri and other developments over the years, PG&E was required to reanalyze the plant in detail. We helped them out on some parts of it. The client requested us to perform various dynamic analyses, and provided us with their plans, drawings, weights, etc., in order to develop dynamic models and determine earthquake effects.

While we were engaged in work on Unit One of the plant, the client gave us the drawing for Unit Two, without any word or clue that it was not for Unit One, as needed and requested. They inadvertently supplied us with a Unit Two drawing, and our people were under the impression that it was for Unit One.

This sketch or drawing went into one of our reports, all of which at the time were intended for Unit One. PG&E accepted our report and used it for Unit One, apparently unaware of the fact that they had given us the drawing for Unit Two. Unit Two was a mirror image of Unit One. Later on, when this was discovered, it became the "mirror image" problem.

I'll never forget attending a meeting of about 40 PG&E people in their office building in San Francisco right after this problem had first been discovered. They reported what the problem was, and then everybody looked at me. I informed them, and showed them, much to their shock, that what they were talking about in our report was actually supplied by them, and was intended to have been something else.

I also pointed out to them that only specific assignments were given us from time to time, and data and documents were provided us to do the work, but that we had no way of reviewing their data and documents as to whether they were for one unit or another. We had to take them as presented. My position was, and still is, that we were not responsible for the mirror image problem, nor for that matter for anything else that caused things to be done over. Nevertheless, I suspect there are some who felt that since it was in one of our reports, it was our error. Our only "error" was accepting the client's drawing as the right one, as we requested it; but we had no alternative, no warning.

"Laundry List" Reports

Blume: I personally played no part in the mirror image affair until after it was discovered. However, I was of course responsible for everything in our office, as head of the firm. My personal efforts on Diablo Canyon were mainly devoted to establishment of seismic design criteria, seismic safety and policy, countless meetings and hearings, research, and testimony. The assigned analysis of structures and systems and the preparation of reports showing the results were done by others in the firm. As an example of the work I did on the project, I cite the "laundry list."

As the Hosgri matter unfolded and it seemed as though there would be no end of questions and obstacles, not to mention hearings and meetings, I proposed that a laundry list be maintained of all the issues, actual and probable, and that these be tackled head on, one at a time, with written reports or papers. This procedure was followed and it proved to be very useful and effective.

I did most of the research and wrote most of the "LL" series of reports, about 50 LL documents, in all. Most of them got into the official records. Accordingly, most became available to the public, as well as to those involved with the project.[20]

Setting the Record Straight

Blume: After the Hosgri matter got straightened out internally, PG&E did make an attempt to correct the record. I remember being at a public meeting in Washington, D.C. where one of their top people clarified the record for the sake of that meeting's minutes. In other words, PG&E tried to correct the record for our name and reputation, but as in all such things, press and TV retractions are few and far between. If they appeared anywhere, they'd be put on the back page in small print. All in all, I consider the whole matter most unfortunate for all concerned, and a bad end to our long labor of love.

The mirror image problem turned out to be only a trivial part of the overall problems, not only for PG&E but for some of its subcontractors. There were other matters that also led to reanalysis and design. The final upshot was that PG&E turned it over to Bechtel Corporation

20. A typical reference: Blume, J. A., "Probabilities of Peak Site Accelerations Based on the Geologic Record of Fault Dislocation," Section LL-41 of Final Safety Analysis Report, Units 1 and 2, Diablo Canyon Site, Amendment No. 50, "Seismic Evaluation for Postulated 7.5 M Hosgri Earthquake," Pacific Gas and Electric Company, San Francisco, CA, 1977.

to practically do the whole thing over. We continued on, to do work with Bechtel for quite a while in this process, even long after the mirror image had been discovered. One thing led to another, and it was found that the project's original bookkeeping, if you want to call it that, was not as good as it could have been. None of the problems that I knew about would have caused a nuclear or any other disaster. The plant was so damn strong to start with, earthquake-wise, that even with these minor problems cropping up here and there, the overall seismic adequacy of the plant was still there. Nevertheless, they spent years in reanalysis and reconstruction, and a big beefing up, such as welding in the field, and other things.

PG&E was acting as its own architect and engineer, which they had done successfully on countless hydro and other power plants, and other major projects. But this nuclear game is more complicated and involved with hearings, meetings, criteria, changing public attitudes and regulations. I don't think they'd want to try it again on their own. We put everything we had into our part of that project, because we believed in it.

Outside of this mirror image matter, there was absolutely nothing even questionable about our work. There really wasn't an error per se with the mirror image either. The analysis was correct. The problem was the drawing that went with the work was backwards. As I said before, we had no way of knowing this, because they doled out the drawings and the work, item by item. We were never given overall responsibility for an end product, the way we like to work. Instead we were given specific assignments of things to do—portions of the work. Do A, B, C, but don't touch D, and do E and F, but don't touch G, etc.

Scott: So you just had to presume that someone was overseeing the whole thing.

Blume: That's right. It was the only way we could operate and help them at all.

Scott: I think it's good to get this on the historical record.

Blume: Yes, I think it's time it came out in better shape. To show you how such matters go, Herb Caen, in one of his columns in 1986, made some crack about the city not going to URS/Blume to site the U.S.S. Missouri in San Francisco Bay because the battleship might wind up in the wrong state. This is an example of how far such things go, and how unfair they can be. The press and the media blew the mirror image item all out of proportion, and let it hang there.

Chapter 10

Tall Buildings, Irregular Structures, Excessive Energy, Challenging Design Problems

"The best way I've resolved them [difficult design problems] is to figure out what's best for the public interest, as well as I could."

Tall Buildings

Scott: I'd like you to talk a little about the theory and practice of building tall buildings, especially in relationship to their seismic resistance.

Blume: Tall buildings have always been of great interest to the public and to most engineers. In fact, when I was still a student at Stanford, when I went there the first time, I was greatly impressed with San Francisco's Russ Building and Shell Building as examples of then so-called skyscrapers.

I admired the office of H. J. Brunnier for having designed these buildings structurally. That's the office I went to work for in 1940. Later on, as I got into my own practice, I found that designing tall buildings was not all it was cracked up to be, in the sense that the structural engineering fees one could obtain from many of these projects were really not adequate to do the job properly, at least as I thought it should be done.

We have worked with many architects over the years—some successfully, and some less so, financially that is. I found that the glamour wasn't always as great as it appeared to be. In spite of this reluctance to take on jobs—because the responsibility was great, and the possibility of lawsuits was also great, and it was essential to do the job right even at a financial loss—we did get involved with several pioneering highrise structures in San Francisco, Portland, Seattle, and Los Angeles—even in New Zealand and Detroit.

Pioneering Work in Dynamic Analysis

Blume: I recall being consulted on one [highrise] in Puerto Rico. Our scope on these projects was not always that of complete structural design, but often only the earthquake dynamic analyses, something in which we had pioneered.

Scott: In other words, you would analyze another firm's design?

Blume: Partly that, but it was more a situation in which we would determine the response spectra for the site, rather than rely on the codes as the only approach, and perhaps go through some dynamic analyses in order to determine how the structure might react to an expected earthquake. This was all pioneering stuff.

As early as 1956, working with a New Zealand laboratory, we ran dynamic analyses of a proposed 20-story administration building for the City of Auckland, New Zealand. Much of the results of this work have been published in the ASCE *Journal.*[21] In 1957 we did dynamic analyses and some structural design of the 42-story Wells Fargo Building at Montgomery and Bush in San Francisco. The architect on this building was John Graham & Associates of Seattle. In 1964 we did dynamic analyses of the Union Bank Building in Los Angeles—42 stories. A.C. Martin of Los Angeles was the architect. I think all of these were pioneering, first-time efforts, wherein time histories of ground motion were employed, and the responses calculated by computer analysis. And, of course, we analyzed a great many existing tall buildings in Las Vegas and elsewhere.

Scott: Could you say a word or two about the state-of-the-art design methods you pioneered? Did you mean for tall buildings?

Blume: Originally, it was basically for nuclear power plants, but the same principles applied to tall buildings, with one big difference. That is, with the buildings we had to allow for the inelastic range of deformation, which we're not allowed to use in a nuclear power plant design.

Using dynamic analysis would reveal to us, and for most of the clients, where the tentative design might be improved to better resist earthquake motion. In other words, we would reveal any weak spots, and hope to get them corrected.

21. Blume, John A., "Structural Dynamics in Earthquake-Resistant Design," *Transactions of the American Society of Civil Engineers,* Vol. 125. ASCE, New York, NY, 1960.

First, the building had to be designed to meet and pass any local building codes that might apply. Many years later, dynamic analysis was required by Los Angeles, for example. But at the time we first did the analyses, it was not a required situation. What was required was to pass the static code, or the code as printed, whether it was static or pseudo-dynamic. The analysis we performed was something extra that was put into these buildings. Unfortunately, life is complicated and often we would not get everything done to a building that we would hope to get, for various reasons, mainly economic.

In those cases we would have to decide either to disassociate from the project, which we have done on some jobs, or to convince people that something should be done. But life is not black or white—it often has gray areas. Sometimes compromises would have to be reached in the interest of practicality. But many people feel the analysis is the end product; in fact we've had owners and clients seek a dynamic analysis when we were pioneering in this field, and having obtained it, they would say, "That's all boys, thank you very much." In other words, they would hope to get by without doing anything about the dynamic analysis, which doesn't do the structure any good whatsoever. So as in all pioneering efforts, we faced several ticklish ethical problems, and I hope we got them all resolved to everyone's best interest, especially the public's best interest.

Using Time Histories

Scott: What do you mean when you refer to time histories of ground motion?

Blume: I'm talking about the actual ground motion record itself. If you take a record of ground motion as it occurs, plotted against time, the terminology is that it's the "time history," it's a roll of tape like a movie film. As the tape rolls out you imprint a ground motion on it and call that a time history. It's a record, in other words, of motion plotted against time.

Scott: A record of the shaking?

Blume: The actual shaking of whatever you're situated on, whether it's the ground or a building, or whatever.

Scott: For these buildings, what time histories would you use? What record?

Blume: That's a very good question. That was half the battle—choosing the time history that best represented the conditions under consideration at the time. Not having a time history of the actual site under consideration, we would characterize the site as related to the geologic environment, whether there was a fault nearby or not, what type of soil conditions are there. Then we would select one or more—usually several—records of actual earthquakes that best represented the site conditions, best modeled the site conditions. Naturally, the recorded earthquake would not be at the true scale that we wanted—by scale, I mean the intensity of shaking—so we would take the earthquake records we had selected and scale them up or down to represent the intensity of earthquake that we were designing for.

Scott: Scale them up or down by simple percentages?

Blume: Right. Of course we let the computer do the scaling. This procedure is something like what we did for nuclear power plants, but not exactly.

Working on Tall Buildings With Portman & Associates

Blume: Later on we made an association with a firm from Atlanta—John Portman & Associates. They are architects and engineers. We worked with them on many projects, including the Embarcadero Center in San Francisco, which is a series of multistory buildings, all of over 40 stories. Embarcadero One, followed by Two and Three, and also the Hyatt Regency Hotel in downtown San Francisco—a very complex structure with an open atrium. Subsequently, we worked with the same firm on the design of the Bonaventure Hotel in Los Angeles, and the Renaissance Center in Detroit, Michigan.

Portman has, and had, its own engineering department. They would do most of the structural design, with an occasional boost from us as requested. Our main work was the dynamic analyses of these systems and structures to best reflect the state-of-the-art design methods, work in which we had pioneered.

Irregular Structures

Scott: In your listing of the various buildings, I've noticed that several of them are unusual structures, very interesting structures. Some people think they're beautiful structures, but many of them would also qualify—in old-fashioned structural engineering terms—as *irregular* structures. I'm presuming that in the case of irregular structures—the Bonaventure, for example, with five separate towers linked together—the dynamic analysis must be absolutely crucial in attempting to ensure their seismic resistance.

Blume: Yes. That's a very unusual building. The first thought with that structure was to let the five towers be independent of each other. This got into some horrendous problems architecturally, especially with the elevators. There was also a requirement in the Los Angeles code that separations had to get wider as you went up in a building—separations between adjacent buildings—and you can imagine the complications with a very large, irregular opening between a tower and the core. The owner and the clients finally decided to tie the whole system together through the elevator shafts, which are actually important structural components. So this system will have many modes of vibration—one of which will be all five towers going together. And there are all sorts of combinations, with the towers wagging like the tail on a dog. From a pure dynamic point of view, it's not the best solution, but from the overall point of view of the structure as a whole, and its economic existence, it seemed to be suitable. Even though it was very complex to design, we expect it to perform well in an earthquake.

We didn't always get such irregular buildings to work with, although we've had several of them. The classic theory is that buildings should be symmetrical in all directions—some buildings are like this, and it's very desirable. But when you're dealing with vast expenditures of money, and owners, architects, planners, economists, the structural engineer is not the only man in the arena, as important as his task is. These are difficult situations. The best way

I've resolved them is to figure out what's best for the public interest, as well as I could. As I mentioned, we've walked away from some jobs because we couldn't get what we considered a decent resolution.

Work for Pacific Telephone & Telegraph

Blume: We did some unusual-type buildings for the telephone company. Around about the same time that we were working on projects in Saudi Arabia [1946–1953], we also got involved in another series of designs, mostly in California, although some in Nevada, where again we worked with Peterson & Spackman on telephone company buildings. Pacific Tel & Tel was expanding tremendously after the war, and they had to put up equipment buildings, office buildings, and relay buildings of all types all over California and parts of Nevada. I would estimate that we did the structural design on 250 or 300 buildings, including the buildings that first connected microwave communications across the United States. These were located on mountain peaks in California and Nevada, all the way to Utah. I understand that television was first transmitted across the country on that system.

Designing for Growth and Rigidity

Blume: Every one of the telephone buildings was designed with earthquake forces in mind, and these designs became rather complex because of the fact that each building, almost without exception, was planned to have future growth, perhaps upward, perhaps sideways in one or two directions. We couldn't count on walls remaining to provide earthquake resistance. They might have to come down to allow for future growth. The result was that we designed a building for the way it was to be initially, then postulated how it might be in the future, and provided for that option in the initial construction. It was very interesting, and costly.

Scott: You had to have support systems that were independent of much of the wall systems?

Blume: That's right. We developed a reinforced concrete framework to do much of the work. A telephone building with equipment has not only to be strong but also to be quite rigid, so as not to move too much and upset the equipment. The microwave buildings also had to be extremely rigid under gale force winds. So between the Saudi Arabian work, which was multiple [types of] work, and the telephone buildings, which was also multiple work, I really was a busy engineer for a good many years during that era—late '40s to early '50s.

Microwave Relay Structures

Scott: You also wanted to talk about some smaller buildings that you worked on.

Blume: Yes, we've had some very interesting smaller buildings, as well as big ones. For example, the relay stations on mountain tops between San Francisco and Utah, for the first transcontinental radio relay system, which was opened on August 17, 1951.

Scott: This was for line-of-sight microwave transmission?

Blume: Yes. The signal would be picked up by two tremendous horns mounted on the roofs of the buildings, sent down into the internal works where it would be boosted in energy many times, then sent on to the next station. Stations were roughly 25 or 30 miles apart. These buildings were small concrete buildings on top of Mount Rose and other mountain peaks going across California, Nevada, and Utah.

I was asked to check the accessibility of the sites during winter months, so in January of 1950, accompanied by a telephone company engineer, Jim Reilly, I traveled throughout the route and hiked to the top of each mountain site, under winter conditions. The scenery was wonderful, and the cold temperature was exhilarating, to say the least. It gets *very* cold in eastern Nevada! These structures and the framing on the roof to support the horns had to be extremely rigid to withstand gale intensity winds without excessive vibration. Even though they were small buildings, they were very expensive to build, because of the remote locations. We also worked on a relay system going between San Francisco and Los Angeles.

Excessive Energy: Sonic Booms

Blume: Throughout my career I have specialized in earthquake problems, but not exclusively in earthquakes. For example, our firm came to be known not only for earthquake problems, but also for any other outbursts of nature with excessive energy, such as ocean waves, sonic booms, windstorms, explosions—anything to do with excessive energy and its effect on structures.

White Sands Experiments

Blume: In 1963 and '64, this country was quite concerned over the advent of supersonic commercial aircraft. Tests were undertaken—I believe the first was in Oklahoma City—to check the effects of sonic booms on people and animals. I understand that the turkey farms and mink farms had the most commotion among the animals from the sonic boom. We were engaged first by the Federal Aviation Agency (FAA) to conduct a series of tests on the effects of sonic booms on structures. All this was leading up to the question of whether or not they would allow supersonic flight across the country, and if so, how they would handle the damage complaints and lawsuits which would ensue from the effects of sonic booms on people, animals, and structures. Our part of it was the structural concept.

The first series of experiments were on some old structures and a few new structures that we built near White Sands, New Mexico, far away from any occupied region.

Scott: These were structures especially built for the testing?

Blume: Some were specially built, and some were existing old structures. The procedure in this test series was to fly military aircraft at supersonic speeds at various elevations above the structures and above the surface of the ground, so we could measure the results in instruments located all over the place, and also by visual inspection of actual damage, if any.

The sonic boom has a signature, in the time history, like an N—first a positive triangle, then a negative triangle which makes the spectrum look like an N. The positive triangle pushes in on a window or wall, and the negative pulls out. Quite often the failure would be of glass coming out instead of going in, due to the shape of the signal.

Scott: Why was the White Sands area chosen for the testing?

Blume: White Sands was chosen because of two basic reasons: (1) it was government property, and (2) it was far from any significant civilization that might be affected by our sonic boom testing. We were selected for the same reason that I suppose that we were often selected for oddball or unusual jobs. We were known as dynamic nuts, interested in unusual problems and in dynamic situations. We had many subcontractors, in order to assemble enough instrumentation to monitor these structures all over the test site. There were 16 test buildings—9 old ones and 7 new ones. They were designed to expose glass windows of various sizes, different types of plaster, and furniture, and in general to act as a guinea pig installation for an actual community. We had hundreds, if not thousands, of instrumental test points as required by our contract. In fact we had too many, and this made it too difficult to analyze. We got so much data it took us years to analyze it. The government said to get lots of data.

The most valuable information was on what was damaged and why. The procedure would be to get the instruments working in good order, and call for a flight of an F-104 or similar plane. We'd tell them what elevation to come in at and sock a boom to us—like a bomb going off.

Scott: That's another reason the site was chosen, I'll bet. They had an Air Force base right nearby.

Blume: You're right. That's the third reason. So things were going along pretty well at White Sands in 1964. They decided to bring in the press and the news media. They flew in a planeload of press representatives to witness the testing. Gordon Bain, who was then the deputy administrator for the FAA program, the man by whom we were engaged, was busy one morning explaining to the press and media representatives about the test setup and the fact that the sonic booms were really not all that bad.

One of the press photographers, I believe he was with *Life* magazine, asked if it would be possible for a plane to fly in low, so they could get some good pictures. He wasn't to go very fast so they wouldn't get boomed. Well, the pilot came in at 200 feet elevation and he accidentally went supersonic. It was like an explosion. A glass ashtray flew off a table to crash on the floor. Glass panes broke, plaster broke, and it was bad. Luckily, no one was injured. Later on Gordon Bain apologized to the group and got things calmed down. We then ran some regular tests without the showmanship, which went off in better shape. But it was quite a shock to have such a big boom. The over-pressure of that boom was estimated at between 20 and 40 pounds per square foot, whereas our normal range was never over 5.

Scott: The pilot hadn't quite understood what he was supposed to do?

Blume: That's partly it, or else he got nervous and pushed a little too hard on the fuel. When you're only 200 feet above the ground, at those speeds it's hard to keep your mind on everything going on.

Commercial Supersonic Flights

Blume: That program provided a great deal of valuable information for the National Sonic Boom Program. It was probably a large factor in commercial supersonic flights across country not being permitted. As you know, the Concorde now comes into Washington and New York, but most of its supersonic flight is over the Atlantic Ocean.

Scott: They have to go subsonic across the continental United States.

Blume: Right. That was not the end our work on sonic boom. We were given another contract about a year later at Edwards Air Force Base, where we instrumented actual occupied homes with plate glass windows and doors and ceilings—the whole works. In this case it was not only F-104s and larger craft, but big bombers. I believe the B-52 was used, if I remember correctly. Again, we went through the same procedure, but we didn't have a super boom for the press.

My project engineer on both these test setups was John Wiggins, and he greatly enjoyed the activity, as did I. We had many subcontractors, such as Lockheed, Aerojet General, Boeing, and other large corporations, in order to provide enough instrumentation to meet the government requirements. It was rather unusual to have a relatively small firm like ours with up to four giant corporations as subcontractors, but it all worked out somehow.

Excessive Energy: Ocean Waves and Swell

Blume: Another part of our efforts over the years, also with dynamic phenomena in mind, has been ocean work—not oceanography per se, but design of structures in the oceans to resist wave forces and swell. I believe I mentioned that we did the first offshore platforms in the Persian Gulf, which incidentally are not far from where all the excitement is going on today in the Persian Gulf. We did the Dammam Port and Ras Tanura piers, which are deepwater installations. The Persian Gulf is normally not very rough. We've also designed deepwater harbors in Guam, and many other installations.

A Man-Made Island

Blume: Probably the most interesting project of an ocean nature is Rincon Island, halfway between Ventura and Santa Barbara in the ocean off the coast [of California]. Our client, Richfield Oil Company, at the time wanted to build an island offshore from the coast in about 50 feet of water at low tide. This was in 1954.

Scott: This was a man-made island?

Blume: Yes. They [Richfield Oil Company] could do this, providing that the island be constructed of "natural materials." In other words, it was not to be a platform. This was quite a challenge, because even though it was in the Santa Barbara channel, we found out that the wave exposure could be enormous, up to 35' to 40' waves under extreme conditions. The size of the waves would be limited by the depth of the water at the site.

So we designed an island using sand fill, rock rubble embankments, and concrete tetrapod embankments on the ocean side. Tetrapods are large concrete shapes, shaped like a jack used in a child's game. In order to test this island we did experiments in our own wave laboratory, which we had in San Francisco at the time, and to get even larger model tests we were allowed to use the Vicksburg testing hydraulic laboratory [which is] run by the Army. To make a long story fairly short, I hope, the design was finally effected, and one of the most critical things was the size of the concrete tetrapods and the size of the armor rock on the sides, and the gradation of the armor rock so it would act as a reverse filter so the sand could not get through the rock pores.

Scott: To keep the sand from being washed away?

Blume: That's right.

Scott: How big were the tetrapods?

Blume: They were 31 tons each, about the same size as this room, which is about 15' cube. We had a slight patent problem with the tetrapods. The French who owned the patent wanted a great deal of money at first, and they had never used tetrapods in this country before, so it was a test case. The client was thinking of going to court over it, to avoid the patent if necessary. We did some work on this subject, showing prior use of this shape, not only in the game of jacks, but also in medieval Europe where they had a weapon shaped with five points that they threw into river crossings. Horses and men would impale themselves when trying to cross the river, because no matter how you threw these things in they'd land with a point up. As a result of all this the Frenchmen greatly reduced their royalty requirement and the job went ahead and was very successful. There were 1130 31-ton tetrapods used on the outer face of the island.

The island, completed in 1958, contained 68 conductor pipes for oil pumping, and even though it was not a great oil field, it has been a steady producer for a long time.

Scott: So that island is the base for an oil field or a series of wells?

Blume: It's possible that 68 wells could be drilled from that island. If you envision a bouquet of flowers held upside down, grab the stems and that's the island, and the flowers branch off in all directions—it is called whipstocking. But the surprising thing is that the natural habitat life has increased. The fish and all sorts of marine life and vegetation have flourished around the island.

It's in a location called Rincon Point, a point about halfway between Ventura and Santa Barbara. You can see it as you drive down highway 101. In fact, at first they had many car accidents because people would gawk at the island and not watch the traffic.

Scott: So it's some distance out from shore?

Blume: It's quite a way from shore. It's out in 50 feet of water at low tide. We also designed and built a causeway to go out there—a single lane causeway. This is a nice looking structure too, designed to be high and let the waves come underneath. That structure alternates by bents between two piles and one pile, two piles and one pile. The piles are big pipe piles. The island was considered a great risk by many people afraid of the ocean storms, but I think our testing showed that it would be all right, and it has been.

Ocean-Related Work

Scott: Well in nearly 40 years it's endured a lot of heavy weather.

Blume: It certainly has. So over the years we've enjoyed a lot of unusual ocean-going jobs. Another big job we did was the Ventura Marina [California], which was a man-made harbor big enough to berth over 3,000 boats. That involved jetties and embankment and excavation of many millions of yards of soil material to make the harbor basin. Another phase of work has been a study of mooring forces due to large ships tied up at dockside—the surge and the dynamics of ship docking. When we designed the Dammam Pier in Saudi Arabia in the Persian Gulf, we designed the whole structure to act as a spring so that a ship coming in with a hard docking would not be wrecked. Instead the wharf would move over and then move back again like a giant spring.

We have also designed many other small boat harbors, docks, wharves, piers, locks, etc. in various parts of the world, but mostly in California.

Challenging Design Jobs

Federal Office Building, San Francisco, CA

Blume: One building we did that was interesting is the Federal Office Building at the Civic Center in San Francisco. This building virtually occupies a whole city block. It's regular in dimension. It's not a very high building, and it was fairly simple to design earthquake-wise, and should give good performance. The complicated feature of that building was they couldn't decide which architect would get the job, so they gave it to four firms.

Four separate architectural firms cooperated in the design. If you know architects as well as I do, you'll know that none of them were really happy about this, but it's a nice building and is fairly simple compared to some of these others that we've been involved with. It's the Federal Office Building on McAllister Street, San Francisco, done about 15 or 20 years ago [interview date: September 1987]. We've also done some very interesting smaller buildings over the years.

Rehabilitation of the California State Capitol

Blume: Rehabilitation has also been one of our interesting lines of work.

Scott: Rehabilitation of structures?

Blume: Yes, the retrofitting of buildings for earthquake resistance. Probably the most notable example is the state capitol building in Sacramento. The former Division of Architecture[22] had studied this building years ago, and decided that there were a few things wrong with it, earthquake-wise. The State Architect called us in to do our own examination of the structure, which we did. We were honored to be called in on a job of such interest and importance.

22. The Division of Architecture and the Office of the State Architect (OSA) both refer to the same State of California office under different names at different times.

We found that even though the earthquake stability was not good, there were other conditions that were even worse. For example, the steel trusses over the Assembly rooms had been altered over the years and decades by tradesmen. An air conditioning man would be working in the attic, and decide he had to run ducts from point A to point B, where a steel truss would be between A and B. If some members got in the way, it was simple to burn them out and run the ducts through. But burning out diagonal members of a steel truss is not very good practice. Apparently this type of work was done for dozens of years—not just that particular event, but similar events. There were literally booby traps in that building that might have sprung, even without an earthquake. If an earthquake had occurred, even a small one, it would have dislodged the ceilings and roofs over the main Assembly halls.

After years of consideration and discussion, money was allocated to rehabilitate the building. Welton Becket was the selected architect, and we were the selected engineers. It was quite a project, which has been well written up in the literature, and many awards have been given for this job.

Scott: Everybody seems to be proud of that building.

Blume: Yes, it's a lovely building with a long history. The team of designers and builders saved all the artifacts and the exterior of the structure, and essentially rebuilt the rest of the building inside-out.

Stanford Linear Accelerator

Blume: Another project we were involved [1966-1972] with was the Stanford Linear Accelerator Center, which is a 2-mile-long facility for basic research in high energy particle physics. It is one of the largest research tools in the world and it is used by scientists from many institutions. Many discoveries on the interaction of mass, energy, and the nature of matter have been made there.

At one end, the center is as close as half a mile to the San Andreas fault rift zone. It was located there only after extensive studies of that site and several alternative sites. Our firm, with some consultants, made these studies for the Atomic Energy Commission. In fact, we reported not only on site feasibility, but also on earthquake risk, and developed cost estimates for the entire project.

The final cost estimate of $114,000,000 for the selected site was approved by Congress, and the facility was authorized. Many other states or agencies had been competing for the project, and they were not backward about citing the Stanford area's earthquake problem. I doubt very much that Stanford would have gotten the nod without our extensive work on the seismic problem and its solution.

Our firm teamed up with the Guy F. Atkinson Company and the Aerojet General Company to obtain the architect-engineer-management contract in national competition for selection on a qualification basis. Throughout the project I was chairman of the team's management committee. The facility was done on time, within the budget of our $114,000,000 estimate, and it worked beyond expectations.

Engineering Fees

Scott: I'd like to ask about another type of design problem—the problem of compensation. Earlier you mentioned some of the problems of dealing with tall buildings as a designer or an engineer. You said that the possibility of loss could be great. Were you referring to actual loss in terms of contracting to do the design, and the engineering aspects of the design, and simply finding that to do what you felt needed to be done—to do it right—simply took a lot more time and effort than the contract provided for by way of compensation?

Blume: Yes, basically that's correct. I hate to get too much into the matter of structural engineering fees, but I think it's part of the earthquake problem. If an engineer is going to lose money on every job, in order to do a good job, he can't go on very long and stay in business. The fees for structural design are so small, compared to the responsibilities involved, that it's really a shame. For example, the structural fees—at best—range from 1% to 2% of the cost of the building, without the land. There is no comparison in the work done, but consider the fees for selling a building—real estate agents will try to get from 5% or more of the total cost, including the land. So something is crazy somewhere.

I once gave an address to a real estate group about building design, and so on, and I chided them about their fee schedules. In the question and answer period following the talk it came out that they felt their fees were too low because there were so many of them in the business, and they needed that amount of money to average out better. To me that was rather discouraging.

In addition to possibly losing money in designing a special structure, the engineer faces great responsibilities and possible lawsuits. He doesn't have to make a mistake to be sued. Somebody else can make a mistake, or a scaffold can fall during construction. The lawyers, who are probably after 30% or 40% fees on a contingency basis, will sue everybody on the job, seeking the deeper pockets, namely the insurance companies, who in turn increase their premiums.

Chapter 11

The Blume Firm and the John A. Blume Earthquake Engineering Center at Stanford University

"Few clients came to our office in the early years, and it was not necessary to put on a show of expensive furniture. We were selling, and providing, good engineering and good service."

Blume: My original practice started in 1945, at the end of World War II, as John A. Blume, Structural Engineer. I was the sole owner, and for several years conducted all the business affairs, as well as the technical work. I even kept what books there were, did the payroll, tax and withholding reports, etc. After obtaining two degrees from Stanford I took a mail order course in management, which was quite comprehensive. And while at Stanford I had taken courses in economics and law. I rather enjoyed all aspects of private practice, and this gave me a broad view of the problems, actual and potential.

I did not hire a secretary and bookkeeper until late November 1949, after we had already done some rather big jobs. This delay was largely because there was not enough space in our small office for a secretary! The girl I did hire was perfect for the job, and stayed on for about 20 years until "we got too big," in her words.

I invested a great deal of time and energy in starting up and maintaining the business, but very little money. Few clients came to our office in the early years, and it was not necessary to put on a show of expensive furniture. We were selling, and providing, good engineering and good service. Very little was taken out of the office funds for several years—just enough to get by in reasonable comfort.

Incorporating

Blume: As time went on, my then legal and accounting advisors suggested incorporation for the usual reasons--liability, taxes, evaluation, multiple ownership, transfers of ownership. In 1957 the sole ownership was made into four corporations, which I headed. Their purposes were engineering, research, graphics, and management, respectively. There were several reasons for forming the four companies, besides a slight tax advantage—organization, concentration of efforts, personnel training and advancement, overhead allocation, etc. Of course there was increased bookkeeping and expense. Nearly all contracts were assigned to the engineering or the research company called John A. Blume and Associates, Engineering, and John A. Blume Research Division. The tax advantages evaporated after a few years. If I had to do it over, I would settle for only one or two companies, after study.

At the time of incorporation I gave each of four key employees a fractional interest in all four companies and made them officers. In addition, arrangements had been underway for some time on the further transfer of ownership interests to the original four, and to others. These arrangements were never completed, however, largely because of the failure of certain associates to accept reasonable fiscal responsibilities.

Also in 1957 I established a not-for-profit foundation, using personal funds. This organization still exists today, and has given away all of its earnings each year. In fact, it is committed well into the future.

Merger With URS Corporation

Blume: The next big step in organizational matters was in 1971, when we merged with URS Corporation, a national technical services company. All of our stock in all four companies was exchanged for stock in URS, which was then traded over the counter. This was an agonizing decision, reached after much thought and study, and agreed to finally by all of our stockholders. We had been approached by others prior to URS, but did not like the proposed arrangements. There were some excellent eastern professional engineering firms already in URS and we were impressed. After much negotiation, and after we were assured that our operations would continue under our sole professional management, a deal was made. Thus, indirectly, our stock finally was subject to evaluation by the market price of URS. We continued to operate technically as we always had, at least as long as I was chief operating officer in our company (which by the way, kept its name) until 1984.

The price of the URS stock has had its highs and lows over the years, and there have been some good and some bad URS spinoffs. Each stockholder was on his own, and some did well, and some not so well. From my point of view, the money aspect was minor compared to other factors such as financial management, exposure to more or bigger jobs, continuity, simple ownership transfer, etc. I forgot to note that I was on the board of directors of the national URS company for several years, until I bowed out.

Probably the item that influenced me most in this decision to merge was the lack of a Blume company internal vehicle for stock transfer. Also a big factor was the desire to get away from financial and management matters in favor of getting more time for my earthquake matters—I still had more important things to do than to be embroiled in fiscal matters. Another factor to consider was the proliferation of lawsuits in this country. We only had one, which we won, but the trend was awesome.

Given all the circumstances at the time, the merger was logical. If I had to it do over, I would instead try to have better circumstances before reaching a firm decision to merge or not to merge. I did try, for years, but it was not all under my control. How did it all come out? Some good, some bad, like many things in life. At least I got back to earthquake matters, which after all was one of my basic objectives.

What should others do? I would not touch that hot potato—each case is different. If making money is an objective, and you have the right partner, merger may be a way to go, but it may not be all roses.

Associates and Employees

Blume: From my original firm's beginning there was a policy not to be a hire-and-fire organization except, of course, for part-time or clearly temporary employees. Many came to work for us to get earthquake knowledge or experience, but many of these failed to get a comprehensive picture of the complex and changing subject. We were not running a school (although at times it seemed like we were, and our "alumni" are all over the country), and we had jobs to get out.

When the workload was decreasing from time to time, I would work with one or two promising employees on research studies, often "on the house" with no client. Several of these assistants have gone a long way in engineering. A good man or woman likes to be busy, and to work on interesting assignments. If we could not keep them busy enough on design or research, they would find other employment, often with our help. But we did not force an employee to either leave or stay on. Our people were always in demand. Several, over the years, left and came back to us later.

Many employees were with us for years, and even decades. Many became associates and officers of the firm. When we incorporated in 1957, I gave some stock to four associates, and upgraded several others as well. It is interesting, at least to me, to consider the early employees, up to the time of incorporation. I should note first that in 40 years I only had two secretaries, each for about 20 years. So I guess I was not too hard to work for.

My first full-time employee was Donald Teixeira, who came to work on September 9,

1945. Don was with the firm continuously until the time of his death in 1983. He became a vice-president at the time of incorporation. My second full-time employee was Harvey Klyce who came to work on March 10, 1946 and retired in 1984, some 38 years later. He was an associate.

The third was James Arntson, from August 11, 1946 to 1951, almost five years. The fourth was Robert M. Allan, from August 11, 1946 to 1950, when he regretfully had to leave because of poor health.

The fifth full-time employee was Joseph Nicoletti, who started February 17, 1947 and retired in 1987. He served for years as vice-president and later as president of the company, and was chief design engineer.

Other vice-presidents were H.J. Sexton, Roland Sharpe, and Roger Skjei. Roger also served as president for about three years in the 1980s. Other key associates and longtime employees include James Keith, Lloyd Lee, William Nelson, Henry Lee, Bob van Blaricom, Ken Honda, Roger Scholl, Dilip Jhaveri, Lincoln Malik, Marty Czarnecki, Andy Cunningham, Helen Aubermann, Pat Dickinson, Ron Gallagher, Walt Mestrovich, and many others.

The John A. Blume Earthquake Engineering Center

Blume: No history of my technical and earthquake career would be complete without mention of the John A. Blume Earthquake Engineering Center at Stanford University. Several years before this center was founded in the mid-1970s, I had been deploring the fact that after Lydik Jacobsen retired, interest in earthquake matters at Stanford had decreased. I felt, and so advised many at Stanford, that this should be a vital and continuing subject at Stanford, with its record of severe damage in 1906 and its history of pioneering research on the subject.

In 1974, Jim Gere and Haresh Shah talked to me about a center at Stanford to promote research and education in earthquake engineering. This concept was ideal, and I readily agreed to provide seed funds if they would become the first co-directors of the Center. The Stanford University officials approved the concept and provided initial space. I should note that Jim and Haresh worked extremely well as a team, and got the Center off to a fine start. An inaugural symposium was held at Stanford on September 17, 1976. Over 450 engineers and scientists attended the all-day session and evening banquet program. It was like Who's Who in earthquake engineering. The speakers on the program were George Housner, Emilio Rosenblueth, Harry Seed, Henry Degenkolb, Dick Jahns, Nate Newmark and myself. The proceedings were published at Stanford.[23]

The Center has continued to function well and is quite active. It conducts research, provides instruction, publishes reports and articles, conducts seminars and conferences, and provides financial support for students.

23. *The Future of Earthquake Engineering: Proceedings of the Inaugural Symposium of the John A. Blume Earthquake Engineering Center.* Dept. of Civil Engineering, Stanford, CA, September 17, 1976.

Recently, Professors Anne Kiremidjian and Helmut Krawinkler took over the direction of the Center.

I am very proud to have this earthquake center named after me. I like to think that their choice of the name was due more to my work in the subject than to any fiscal support.

Chapter 12

Combining Work and Travel

"I took time out, of course, to visit some of the museums...to see some very beautiful paintings."

Blume: Not all of it has been hard work. A lot of it has been extremely enjoyable. I like research efforts very much, especially after computer aid became available. One can do so much in a short period of time, and study the effect of parameter variations very rapidly. Another diversion that I've had over the years, combined with my work, has been some trips out of the country. I'd like to briefly mention a couple of them.

Saudi Arabia and Around the World (1957)

Blume: I think I noted in a previous interview session that in 1957 I went to Saudi Arabia on a special mission for the U.S. government. I've talked about the technical and political aspects of that, and now I'd like to mention the more enjoyable parts of the trip. I first flew to Amsterdam, out of New York. From there I went to The Hague, where I studied for a few days the records and data in the offices of Aramco—the Arabian American Oil Company—prior to going into Saudi Arabia. I took time out, of course, to visit some of the museums in Amsterdam to see some very beautiful paintings. From there I flew, via Paris and Rome and Beirut, to Saudi Arabia, where I worked for 2-1/2 weeks, and where I was taken virtual prisoner by the Arabs. After that experience was settled and our work in Saudi Arabia was done, I decided to come back home through the Orient, or complete the trip around the world.

So I booked passage with Pan Am to go to the Orient. My first leg was from Dhahran in Saudi Arabia to Karachi, Pakistan. The only problem was a 12-hour delay in getting started, so I sat around the hot Dhahran airport for 12 hours waiting for the plane to come in. In the meantime, the local Arab in charge of departures—I believe he was the equivalent of governor of the region—had refused to sign my departure release until the plane came in. By the time it came in, he was long gone. To make a long story short I had a hell of a time getting out of Saudi Arabia, but I finally made it.

The plane flew to Karachi, but the last 500 miles or so was with one engine less than the two it started with. So we put down in Karachi and they put me up for two days while they got another airplane engine. In the meantime I went all over the place, and even got a ride on a camel. These are the most disdainful animals! I saw a great deal of interesting country life. You'll recall that Pakistan was split off from India a few years prior, when the British left.

From there I continued with Pan Am. I took almost three days off in Bangkok. I hired a samlor for 24-hours a day, so I went night and day in my activities. From there I spent two or more days each at Singapore, Hong Kong, and Kowloon, of course, then to Fiji and Honolulu. So I came home about five weeks after I started, had a tremendously active trip in all respects, and got around the world on a trip I hadn't planned. Pan Am contacted me later to advise me that I had a 10% rebate coming, because instead of coming back the same way I had originally come, I went around the world.

Peru and Ecuador (1958)

Blume: I've made several trips to Peru and Ecuador. I've done a lot of earthquake and other engineering in Peru. I've been all over that country and I like it very much—at least when the politics are reasonable. On one trip I arrived a few days after the Arequipa earthquake of 1958. I managed to get to Arequipa, and apparently I was the only outsider that got there. There was a lot of heavy local damage. It was a very sharp, strong earthquake right below the town. The masonry and adobe construction in some places was not just cracked, but shattered. Still, walls were standing even though shattered, with pieces of masonry no bigger than 2 feet square.

I found a very unusual structure of masonry and adobe—one room, two stories high. In other words there was a ground floor room and directly above it another room. It was a two-story building but with only two rooms altogether, occupied by an old woman. The building was badly damaged, and she couldn't speak any English. I tried to tell her in my poor Spanish that she should move out of the building, that it was apt to fall down in an aftershock. We did get to an understanding and she told me, no, she was going to stay there. It was all the property she owned, and everything she had was in that building. She was going to stay there and go down with it. I learned later that the building did come down. They took her to the hospital, injured, but alive. She would not leave and I had no authority to force her out of there. And no authority figure was interested. I was just a gringo tourist as far as they were concerned.

Alaska (1958)

Blume: On all these trips where I got out of the country, I usually managed to stay over a few days. In Peru I got up to very high elevations. I was up as high as 18,000 feet, working with copper mining companies on the earthquake resistance of their structures. I also did work near Talara, Peru, on the coast, where we were considering new methods of underwater oil recovery for a large oil company. In 1958 I had a nice trip to Ketchican, Alaska. I went there to study some buildings that had been leaking badly under heavy rainstorms. I managed to parlay that into a trip to Anchorage. I enjoyed that very much, and I flew around with some bush pilots to see the country.

Japan, New Zealand, and More (1960)

Blume: In 1960 I went to the Second World Conference on Earthquake Engineering in Japan, and managed to tour Japan in addition to attending that conference. I went north of Tokyo to where the nuclear power plants were being installed—one of which we had worked on. After the conference was over I managed to take side trips to Manila; Sydney, Australia; Auckland, New Zealand; Fiji and Honolulu—about 2-3 days in each place.

We had work in Auckland, where we had designed a 20-story administration building for the City of Auckland, which, however, was not built because the price of butter went down. Butter and wool—that was it in their economy.

India (1977)

Blume: In 1977 I went to the Sixth World Conference at New Delhi, India (1977). After the conference I went on a tour where we'd fly from town to town and be picked up by buses, mostly in central India—the tour was arranged by Haresh Shah of Stanford, and Mrs. Shah. We saw a great deal of Indian life and culture. Near the end of the tour I broke off from the group and went all by myself up into Nepal, to Katmandu.

My objective was to rent a plane and fly around Mount Everest at dawn, which is considered the thing to do. I had a plane and pilot lined up, but they called me at 2:00 a.m. to wake me and tell me that due to cloud formation and fog the plane could not take off. I couldn't stay any longer, I had to leave the next day. So between 2:00 and 4:00 in the morning, working with the hotel desk people, I managed to hire a car and driver to drive me up to a vantage point high up in the Himalayas where I could see Mount Everest from the ground.

That was quite a trip, because the car itself was pretty junky and broke down a few times. It was in January—winter conditions—and to provide drainage across the road, about every mile or two (on dirt roads), they'd simply dug narrow trenches to let the water run across the road. Every time we came to one of these narrow trenches we had to get out and put some rocks in there to drive the tires over. All this in the dark, in the middle of the night, in freezing cold weather. We finally got up to a place the driver knew about, which was at an elevation of 10,000 feet, he told me. The cloud cover was high and cold. Lo and behold at dawn I did see Mount Everest for about 20 minutes, then the clouds came down. They just went up and came down. That was very enjoyable.

Around the World Again (1980)

Blume: In 1980 I went around the world again. This trip was undertaken partly in connection with the Seventh World Conference in Turkey, but also covered a lot more territory. I started out with a polar flight from San Francisco to London. From London I went to southern France and took about four days off on the beach in Cannes. I reverted to my old beachcomber procedures by getting into bathing trunks, getting out and swimming and laying around the beach for a few days. One morning I was lying down in the sun out on the end of a pier. I heard a call for help in the water alongside the pier. I looked over in time to see a boy—I learned later he was about 8 or 9 years old. He was going down. Apparently he had been floating in a waterwing apparatus, the strings broke, and he lost his flotation. He didn't know how to swim.

I proceeded to jump in and save his life, which I did almost automatically because I'd done so much of that in the Hawaiian Islands when I was a beachcomber. I dived off the pier, grabbed the boy, took him to shore, and got the water out of his lungs. Then his father came. He was all excited. He had been talking to some girl in a topless bathing suit and hadn't been paying attention to his son who had paddled into deep water. They don't wear tops over there. The father apparently was some Middle Eastern country citizen. He wanted to give me a big reward but I wouldn't take it. I had his undying thanks. After he learned what happened and how close he was to disaster, he was shaking like a leaf.

From Cannes I went via a Switzerland stopover to Istanbul, Turkey, where the earthquake conference [Seventh World Conference on Earthquake Engineering] was to be held. There the same thing happened to everybody in our group—we were involved in a military takeover. The first we knew about this was on the morning of about the third day of the conference, when everyone at the hotel where I was staying was told not to leave the hotel—just stay in there all day long and they'd feed us at meal times. They said not to go to the meeting. A day or so later they released us from the hotel. I proceeded to walk to the meeting point through a park, where I had covered the same route a few days prior. The only difference was that the military had taken the park over as a depot. I suddenly found myself facing two Turkish soldiers, each sticking a submachine gun in my stomach, telling me in their language to stop.

I got the message very fast. I was all alone because no one else had the foolish idea to walk where I had. I finally managed to explain to them in their broken English and my lack of Turkish where I was headed. They had me go back and take a different route to get to the meeting place. They wouldn't let me go through the park. But it was a strange feeling to suddenly have automatic weapons stuck in my belly.

Official EERI Tour of China (1980)

Blume: After the Turkish incident was pretty well taken care of, I went on to Tokyo, Japan. At Tokyo I rendezvoused with 10 other engineers who were joining me for an invited trip through China. It was called the EERI Delegation to the People's Republic of China, and had been arranged beforehand. I was then president of EERI and I had been asked to lead the delegation, which I did. We had a very busy, very enjoyable, very unusual couple of weeks in China. This trip started out in Beijing. From there we went to Harbin, which is up in the northern part of China. In fact it's far north of North Korea. It was very cold.

Back to Tangshan, where the tremendous earthquake had happened a couple years before, with a loss, officially, of a quarter of a million lives, unofficially, half a million. We spent some time there but there was no place left to stay overnight. From there we went to Tianjin, then back to Beijing, for more meetings and talks. From there we went to Shanghai, from there to Guangzhou, which used to be Canton, and from there to Hong Kong, then back to the United States via Honolulu.

Scott: This was a special trip to take an earthquake-related tour through China?

Blume: That's right, by invitation of the Chinese government. The Chinese government put us up, paid all our meals and everything. We just paid our transportation, and that was covered by the National Science Foundation. The object of the trip was to exchange goodwill and earthquake-related data between the two countries. We lectured in three cities, and listened to talks by them. I met some very interesting and nice people in China. As leader of the group I was asked to talk longer and more often than the others. I spoke in three major cities—I was the only one to speak more than once. Every night there was a banquet of different food from different parts of China. The food was excellent. I had some food over there that I've never enjoyed anywhere else in my life.

At Harbin, I had come down with a bad cold in the chest, with a fever and I could hardly speak. I couldn't come down for dinner. They asked me what I wanted for dinner, so I said a bowl of soup—they make good soup in China, full of sea slugs and all kinds of things. They sent me up a huge tureen (must have been 16" in diameter) full of hot soup. I ate most of it. I don't know whether the soup cured me or the Chinese doctor cured me. They sent in a Chinese doctor to examine me in bed—he took my temperature, my blood pressure, listened to my chest, and prescribed some odd-looking pills. There were some black pills and some white pills. The interesting part is he had me sitting up in bed stripped down to the waist with the entire hotel personnel in the room watching—the cook, bus boys, the gardeners, the clerk at the desk—they all came in to see what was happening to the man from the United States. Very friendly, but curious. It was a very strange situation. I don't know what the doctor gave me, but the very next morning from 10 to 12 I lectured for two hours with a voice I couldn't even use the night before. I think they gave me something that would probably be illegal in this country.

Those are just a few examples of some of the trips I've had that have been rather unusual and interesting.

Chapter 13

Selected Papers and Writings

"Unfortunately, you can't write down everything that you learned and have done over a period of time...."

Scott: I'd like to talk about some of your papers and writings. I think it is important to get you, in your own words, to say something about your evaluation of the significance of the papers. Do most of the writings you will discuss here have some special relevance to seismic safety?

Thesis at Stanford

Blume: Yes. The ones I have in mind can all be connected with earthquake engineering or structural dynamics. Some of them I have mentioned before. My first major work in the field of structural dynamics was the thesis [for Engineer's Degree] I did at Stanford, with my partner, Harry Hesselmeyer. That was published in a hard cover for the Stanford community. It was never published outside the university, but I gave a paper on it to the Seismological Society of America in 1934.[24]

Scott: But it was available within the Stanford system in hard cover?

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24. Blume, John A. and Harry L. Hesselmeyer, "The Reconciliation of the Computed and Observed Periods of Vibration of a Fifteen-Story Building," Engineers' Degree thesis. Stanford University, CA, 1934.

Blume: Right. And it has been referred to by a great many people. Then in 1953 Professor Salvadori published a paper in the ASCE journal, Separate 177,[25] entitled "Earthquake Stresses in Shear Buildings." I read this paper with great interest, and it rekindled my thoughts regarding the work I had done on the thesis years before. Even though Salvadori's procedures, as outlined in his paper, were technically correct, they seemed cumbersome to me, in view of the shortcut methods that we [Blume and Harry Hesselmeyer] had developed previously. So I wrote a discussion of Salvadori's paper, which was published in the 1953 ASCE *Proceedings* and in the ASCE *Transactions*, Volume 119, 1954.[26] I outlined the procedures that we had developed in the thesis study for determining the fundamental period of vibration of a structure, and also a special procedure for determining the higher modes. All these efforts were prior to computer technology being launched upon the world.

Scott: So in 1953 you were referring back to what you had already developed 20 years before?

Blume: Right, there were two reasons why nothing had been published in the interim. First, there was no money to work with, and second, there was no interest by anybody in the subject.

25. Salvadori, M.G., "Earthquake Stresses in Shear Buildings," *Journal of the Structural Division*, Proceedings of the American Society of Civil Engineers—Separate No. 177. ASCE, New York, NY, March 1953.
26. Salvadori, M.G., "Earthquake Stresses in Shear Buildings," *Transactions of the American Society of Civil Engineers*, Vol. 119. ASCE, New York, NY, 1954.

Scott: Why was there no interest?

Blume: I guess mainly because there weren't enough earthquakes. We went from Long Beach in '33 to Kern County in '52. This paper by Salvadori was no doubt written because of the Kern County series in 1952. I won't go into the technical details here, but by hand calculator methods, which were much shorter than the theoretical methods proposed by Salvadori, I was able to come up with the same solutions to the problems as he had, and in a fraction of the time.

Reworking the Thesis Twenty Years Later (1956)

Blume: This effort got me started again on the Alexander Building, the subject of our thesis. So in 1956, by the time of the First World Conference on Earthquake Engineering in Berkeley, I had redone the whole effort, and refined the work in view of the latest knowledge about the vibration periods of structures.

Scott: You mean you redid the thesis effort?

Blume: Yes. I published a paper in the proceedings of that conference, "Period Determinations and Other Earthquake Studies of a Fifteen-Story Building."[27] This established the Alexander Building as the world's first guinea pig structure for earthquake research.

27. Blume, John A., "Period Determinations and Other Earthquake Studies of a Fifteen-Story Building," *Proceedings of the First World Conference on Earthquake Engineering*. Held in Berkeley, CA, June 1956. Earthquake Engineering Research Institute, Oakland, CA, 1956.

Among other things, I had determined—for the first four modes of vibration in each horizontal direction—how much of the natural periods was due to shear, flexure, and ground yielding. In other words, this paper was way ahead of its time, in spite of being delayed a couple of decades.

Scott: And that had not been written up and published anywhere else, until you reworked and published your dissertation?

Blume: Right, in spite of all those years. The general conclusions that I listed in the paper were 14 in number. I won't bother to enumerate them all here, except to say that for the first time the participation of stairways, brick filler walls, floor slabs, and tile partitions in an office building was being determined by dynamic procedures. Also, the effective modulus of elasticity of materials was determined. We found that the ground yielding affected the periods of vibration, and a percentage of this effect was given. Also, the amount of flexure and the amount of shear.

Procedures were developed to calculate the periods of vibration and the mode shapes from drawings of the structure. Correlation was obtained for the periods of vibration—whether caused by earthquake, wind, or forced vibration with the testing machine. The periods were found to be consistent with each other regardless of the type of excitation. This paper was important to me in several respects, but mainly because it got me started all over again.

Scott: You mean it got you started again in the mathematical analysis of dynamic problems in design?

Blume: Yes, and in the mathematical research of dynamic problems in earthquake design. Of course, another factor that got me started was the debate over the 1948 San Francisco code, and the subsequent joint committee analysis and report [*Separate 66*], which was published in an ASCE structural *Journal*[28] and was given the Moisseiff Award.

Scott: That's the one called *Separate 66*?

Blume: Yes, that was *Separate 66*, which came a few years prior. So we had *Separate 66* [published in 1951], the Salvadori paper [1953],[29] and my world conference paper [1956],[30] getting me steamed up all over again on these matters of structural dynamics.

[This interview left off in September 1987, and resumed in October 1987.]

Blume: At our last session I started to talk about some of my engineering writings in the earthquake field. I'd like to continue on that for a short while today, covering initially the earliest writings of 20 and 30 and even more years ago.

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28. Anderson, Arthur W., John A. Blume, et al., "Lateral Forces of Earthquake and Wind," *Separate 66, Journal of the Structural Division*, Proceedings of the ASCE. ASCE, New York, NY, 1951. (Also "Lateral Forces of Earthquake and Wind," *Transactions of the American Society of Civil Engineers*, Vol. 117. ASCE, New York, NY, 1952.)
29. Salvadori, M.G., "Earthquake Stresses in Shear Buildings," *Proceedings —Separate No. 177.* ASCE, New York, NY, March 1953.
30. Blume, John A., "Period Determinations and Other Earthquake Studies of a Fifteen-Story Building," *Proceedings of the First World Conference on Earthquake Engineering.* Held in Berkeley, CA, June 1956. Earthquake Engineering Research Institute, Oakland, CA, 1956.

In today's literature there's such a tremendous volume of writing on earthquake matters that it's very difficult for some people to get a picture of what it's all about. In fact, I sometimes think the field is becoming overcrowded with literature. It's probably amazing to some how much was done in the early days. I think I mentioned briefly in a prior session the U.S. Coast and Geodetic Special Publication No. 201, and the fact that this publication released in 1935 contains a great deal of valuable information on engineering seismology. I have a chapter in that book on the shaking machine that I designed with Professor [Lydik] Jacobsen.

Scott: What is the report called?

Blume: The title of that report is *Earthquake Investigations in California, 1934-1935*, put out by the U.S. Department of Commerce, Coast and Geodetic Survey.[31]

Scott: It has a lot of valuable background information, in addition to reporting on that year's investigations?

Blume: Yes, it's got many chapters by many writers. Shortly after that report came out, a special issue of the *Bulletin of the Seismological Society of America*, dated October 1935, contained several papers on the special work of the Coast and Geodetic Survey, including a paper by me entitled "A Machine for Setting Structures and Ground Into Forced Vibration."[32] That was my first published paper in an outside publication, although I had given talks to the society prior to that time.

Scott: When you say outside, you mean not a campus publication?

Blume: Not a campus publication or part of my work.

"Resistance to Wind and Earthquake Forces" (1951)

Blume: Another early publication was entitled *Modern Building Inspection*, published in 1951.[33] In spite of its somewhat limited title, it was a hardcover book that contained chapters by many people in the earthquake field. Franklin Ulrich and I collaborated on Chapter 13, which was entitled "Resistance to Wind and Earthquake Forces." Part I of this chapter, mainly by Ulrich, was about seismology, and part II, mainly by me, was about structural considerations and design requirements. In looking this ancient book over I'm amazed at how much was known at that time. Even though that book was written for building inspectors, I have been told by many that engineers have frequently referred to the book.

Scott: Who published that? Say a little about the sponsorship.

Blume: It was sponsored by Hal Colling and his wife. He was a publisher and was also the godfather of the Uniform Building Code.

Scott: Would you say a little more about him? How was he the godfather of the UBC?

31. *Earthquake Investigations in California, 1934-1935*, U.S. Dept. of Commerce, Coast and Geodetic Survey. Special Publication No. 201. Washington, D.C., 1935.
32. Blume, J.A., "A Machine for Setting Structures and Ground Into Forced Vibration," *SSA Bulletin*, Vol. 25. SSA, El Cerrito, CA, 1935.
33. *Modern Building Inspection: The Building Inspector's Handbook*. Building Standards Monthly Publishing Co., Los Angeles, CA, 1951.

Blume: He was a person who was dedicated to establishing a uniform building code in the United States—not only for earthquakes, but for all phases of design and construction. He did it mainly working out of the Los Angeles area, over a period of many years, by forming a conference of building officials and engineers and some architects.

Scott: When would he have started that? Obviously that was an extremely important development.

Blume: I'm not sure when he started, but I know this book was not the first thing done. The first thing was a monthly publication. The book was done in 1950—I would estimate that he started his code work in the mid- or late '40s. The official publishers' name was Building Standards Monthly Publishing Company, 124 W. Fourth Street, Los Angeles, California.

Scott: Well, I diverted you from talking about your chapter.

Blume: My chapter, as I said before, was in two parts. The first part was about seismology, what causes earthquakes, and where they occur, and whether or not they can be predicted. It covered a great deal of valuable information in that regard. The second part contained quite a bit of philosophy about static earthquake design, but did not go into dynamics in any detail because of the reading audience I expected. But as far as statics goes, it talks about relative rigidity and many other factors—building separations, building hammering, the need for good construction and good design. In fact in looking it over briefly today I was impressed, if I do say so myself, at how much was known and how much information was given in this early publication, which I had almost forgotten about.

Scott: You say it was mostly on statics, and that was largely because of the audience. Was it because the inspectors really didn't need dynamics, or because they couldn't handle dynamics?

Blume: I wouldn't expect a building inspector or official of that era to get deeply involved in dynamics, although some of the principles were mentioned. On the other hand, they had to enforce these code design requirements—whatever they were—and this book was a great help to them in that regard.

Also in this book is work by Fred Converse, who is the original soil mechanics man at Caltech; Bill Moore of Dames & Moore worked for him at one time. So it's a small world.

"Reinforced Brick Masonry..." (1953)

Blume: Another book came out in 1953 under the joint authorship of Harry Plummer and myself. This book was sponsored by the Structural Clay Products Institute of Washington, D.C. The book was entitled *Reinforced Brick Masonry and Lateral Force Design.*[34] It was the first published book on how to reinforce brick masonry for earthquake resistance. The preface is dated November 1953. This book again is mainly static in concept, but it gives a great deal of very valuable information. In

34. Blume, John A., and Harry Plummer, *Reinforced Brick Masonry and Lateral Force Design.* Structural Clay Products Institute, Washington, D.C. 1953.

Chapter 4 for example, on design criteria, it covers building code requirements—especially of lateral forces on wind and earthquake. It shows how to use the building codes. Chapter 5 gives examples of how to design brick structures that are reinforced under the codes, and gives a great deal of information about the very important principle of relative rigidity. In other words, the most rigid element in a series will gather in the most force, and has to be designed accordingly.

Another interesting thing is that in a wall elevation—a wall punctured by windows and doors—the forces tend to go to the individual piers according to the shape of the piers. A tall slender pier between windows will receive less force than a square pier between windows. But you could have a square pier that would be only say 1' x 1', or it could be 6' x 6', both having the same relative rigidity but one having six times more strength in shear. Combinations like that were explained in detail in this book, in Chapters 4 and 5.

The book was given an award by both the American Institute of Architects and the Producers Council. The award was for the best of its class, and the class was Class I literature. The book was also widely used by structural engineers, and in some cases parts of the book were used as a textbook at schools.

I don't know exactly how I got into this. I didn't know Harry Plummer at all before we worked on the book, but the Structural Clay Products Institute apparently looked me up and selected me to be approached about the writing of the book. It may be that they read something in the building inspectors' book about my writing, but for some reason they came to me out of Washington, D.C.

I enjoyed a great deal working with Harry Plummer. He was from back east, and extremely well known in the technology of masonry, not earthquake—I supplied the earthquake part. With one author back east in Washington, and another out in San Francisco, toward the middle of the work and also when approaching the end of the work, we met at the Broadmoor Hotel at Colorado Springs. We selected that place because we did not want to be interrupted. It was very nice, and we stayed there a week or 10 days at a time, working in the daytime and relaxing in the evening. The book was very popular in its day, although it's probably long since been forgotten.

"Structural Dynamics in Earthquake-Resistant Design" (1958)

Blume: I had many pet theories I had been working on in my spare time for years, but few people were really interested until I published a paper entitled "Structural Dynamics in Earthquake-Resistant Design," which was first published in the July 1958 *Journal of the Structural Division*, ASCE.[35] The paper, with all of its discussion, was finally published in the 1960 ASCE *Transactions.*[36] It also won the Moisseiff Medal.

35. Blume, John A., "Structural Dynamics in Earthquake-Resistant Design," *Journal of the Structural Division*, Proceedings of the American Society of Civil Engineers. ASCE, New York, NY, July 1958.
36. Blume, John A., "Structural Dynamics in Earthquake-Resistant Design," *Transactions of the American Society of Civil Engineers*, Vol. 125. ASCE, New York, NY, 1960.

Scott: You mean it was published after others had commented?

Blume: Yes. They call it "discussion," even though it's formal writing. This paper contained many "firsts," including the results of dynamic analysis of highrise buildings using actual earthquake records.

Among other matters, the concept of ductility and energy capacity beyond the yield point was introduced, through detailed examples. In fact, the detailed procedure for analysis of design in the inelastic range—termed the reserve energy technique—was presented in the closing discussion. Incidentally, that technique had been discussed in two or three of my oral presentations in prior years. I had been holding back on it, waiting to get more field data, but none came, so I finally published it.

Other concepts introduced in this paper included so-called plateau design, wherein two or more levels of resistance to ground shaking were utilized. The first, for example, being under probable conditions of shaking, and the second under extreme-but-possible other conditions of shaking. Another matter that was pointed out in this paper, which to me is extremely important, was the fact that the traditional buildings, such as were present in San Francisco in 1906, were of a totally different character than the contemporary buildings that were being erected in the '50s and '60s. The difference lay mainly in the fact that the 1906 buildings had heavy masonry or other walls, in addition to a fairly light steel frame, as compared to the modern buildings, with a heavier steel frame but no walls, only architectural cladding.

This becomes important to consider, because much of the thinking of the Joint [*Separate 66*] Committee, and subsequently in the Blue Book, was influenced by the results of the 1906 earthquake, without, however, in my opinion, giving completely adequate consideration to the differences in the characteristics of the buildings then and now.

Scott: You mean without considering the additional capacity that the masonry walls gave those older structures?

Blume: Yes. The old buildings were generally designed with a steel frame to withstand wind forces, and the masonry walls were put in without any structural recalculation. When the earthquake came along, the buildings cracked the walls. Everybody said, "The steel frame buildings stood up fine," not giving adequate credit—in my opinion—to the fact that those brick walls probably saved many of those buildings [that had steel frames]. And yet the brick walls are not present in modern buildings, so something has to be done to make up the difference. I think it has been done to a certain extent, but perhaps not enough. The paper covered many other considerations, including the change of period with damage—from the so-called deterioration factors as materials get into the inelastic range.

Three Papers: Second World Conference (1960)

Blume: At the Second World Conference on Earthquake Engineering in Japan, 1960, I gave three papers, including a joint paper by myself and R. W. Binder of Los Angeles, entitled "Periods of a Modern Multi-Story Office

Building During Construction."[37] This was about a first-time effort to determine by measurement the actual periods of vibration of an office building during its construction and in its various stages. You may wonder why this is important. The answer is that it helps to analyze what mass and rigidity going into a building do to create the overall natural vibrational characteristics of that building. It was a very interesting study.

At the same conference I gave what I consider one of my most interesting and far-reaching papers, entitled "A Reserve Energy Technique for the Design and Rating of Structures in the Inelastic Range."[38] Although I had done prior work and given prior talks on this subject, I believe this was the first published effort on how to design in the inelastic range, which range most buildings must go into in order to resist major earthquakes. It is to be recalled that the codes up to that time did not recognize the inelastic range, but required the design to be done in the elastic range. Today, decades later, in fact almost three decades later, inelastic procedures are common, and the reserve energy technique has been used directly by some, and by different names by others.

Again, at the same conference in Japan in 1960, another paper was given by Jack Meehan and myself. Jack Meehan later went with the Division of Architecture, but in the early days he worked in our firm. I believe he recently retired. The title of this paper was "A Structural-Dynamic Research Program on Actual School Buildings."[39] This paper gave the results of research we had done in the field on 15 school buildings in California. The main part of the work was using a small vibrating machine to shake the buildings and determine their periods of vibration and damping characteristics. As a by-product of this work we learned a great deal about school buildings and helped to create better practice in school building design.

Scott: As long as I've known Jack he's been with the State Architect, involved in the administration of the Field Act program. So that early work of his was highly pertinent to his career, wasn't it?

Blume: Right. Though as I recall it, at the time we did this work he was already with the state, but in prior years he had worked with us in our firm on similar programs. He was with us from mid-'47 to mid-'49.

"...Dynamic Analysis of Steel Plants..." Chile (1963)

Blume: In 1963 I wrote a paper, which was published in the *Bulletin* of the Seismological Society of America, dated February 1963.[40]

37. Blume, John A., and R.W. Binder, "Periods of a Modern Multi-Story Office Building During Construction," *Proceedings of the Second World Conference on Earthquake Engineering*, Vol. II. Held in Tokyo and Kyoto, Japan, July 1960. Science Council of Japan, Tokyo, Japan, 1960.
38. Blume, John A., "A Reserve Energy Technique for the Earthquake Design and Rating of Structures in the Inelastic Range," *ibid.*
39. Blume, John A. and John F. Meehan, "A Structural-Dynamic Research Program on Actual School Buildings," *ibid.*
40. Blume, John A., "A Structural Dynamic Analysis of Steel Plant Structures Subjected to the May 1960 Chilean Earthquake," *Bulletin of the Seismological Society of America*, Vol. 53, No. 1. SSA, El Cerrito, CA 1963.

This paper was entitled "A Structural Dynamic Analysis of Steel Plant Structures Subjected to the May 1960 Chilean Earthquakes." Here was a case where a steel plant had been subjected to violent shaking from a very strong earthquake and all the vertical structures were vibrated, including stacks and ovens and other vertical structures. Some were damaged and some were not damaged. I put together a study that went on for about a year and a half in my spare time, often with the help of calculators in the office, wherein we established the threshold between damage and no damage, by period. From this we were able to reconstruct a response spectrum of the ground motion, which was very important because no instrument had recorded the actual motion.

This paper was controversial in its review before publication. In fact, George Housner undertook to review it critically and thought it wasn't scientific enough.

I couldn't agree with him. He was apparently acting as editor of a special Chile edition of the *Bulletin* of the Seismological Society. We couldn't agree on publication. He wanted me to make changes that I didn't want to make. So he appointed a committee of three—Perry Byerly I know was one, Lydik Jacobsen, and I believe Glen Berg was the other one. The committee of three voted to publish the paper as written, and then to publish any discussion that might ensue. The paper was published and was very well received by the engineering community. There was no discussion.

Scott: Interesting. George Housner didn't come in with any critique afterwards, then?

Blume: No. In fact no one did.

Scott: Then it must have stood up pretty well.

Blume: I think it did, and it was quite useful, at least to engineers.

Scott: That work involved a series of earthquakes in Chile. Was the work mostly done here, as a mathematical modeling analysis?

Blume: It was done in two or three stages. The first stage was the measurement of the natural periods of vibration of the structures after they were damaged.

Scott: So you or somebody actually went down and did some tests on the structures.

Blume: Bill Cloud did that (from the Coast and Geodetic Survey). Then there was the detailed analysis of the actual damage or lack of damage observed in each structure. So we had these two things. We knew the periods of vibration, we had damping test results, and we knew whether the structures were damaged or not damaged. We also obtained from the steel company the detailed drawings of all the structures. The bulk of the effort was done in San Francisco in a computational mode, taking one structure at a time. Take for example, a stack or a chimney made of steel. We know that the anchor bolts pulled out at the base and the stack may have started to wrinkle or buckle. By detailed analysis made from the drawings of the stack, we were able to determine at what point it would tend to pull out the anchor bolts, and at what point it would tend to buckle. We also know its period of vibration, let's assume it's one second, for example. That would give us a point at one second.

Then we'd go to another structure where the period may be a half a second, and find no damage. By analysis again, we'd figure out the point at which damage should have occurred if there had been any. That would give us another point at half a second, and a no-damage state. By doing this over and over with every one of the structures—and I think there were about 16 structures—I was able to plot on a period scale the damage points and no-damage points for each structure. By running a line between them, I knew I was somewhere in the right territory. The vertical scale on this plot was the response motion, which we derived from knowing that the structure was either damaged or not damaged. One could work back and estimate the ground motion.

Scott: You estimated the presumed ground motion?

Blume: Yes. We got limits for it—a high limit and a low limit—and the answer was in between. It was amazing—the 16 structures gave us almost a smooth line.

"...Installations Near Active Faults" (1965)

Blume: For the Third World Conference [on Earthquake Engineering] in New Zealand in 1965, I submitted and published a paper that was for nuclear power plants, and other important structures. The title of this paper was "Earthquake Ground Motion and Engineering Procedures for Important Installations Near Active Faults."[41] For the first time, in publication at least, this paper outlined procedures one might undertake for nuclear power plant seismic designs, which as I mentioned before are entirely different from conventional procedures. They are much more conservative and require different techniques. This paper outlined the techniques I had been following in several plants.

Scott: You had already been doing work for the Atomic Energy Commission?

Blume: Yes, for the commission, and also for General Electric and a couple of other suppliers. Instead of taking a lateral force coefficient out of the code for these plants, we would develop the estimated potential shaking at the site. This was determined by a reference to any faults in the region, the distance to the faults, the type of soil, and many other considerations. In fact, I presented formulas for determining site acceleration, given magnitude, and distance and soil conditions. This later became known as the SAM procedure, an acronym for site-acceleration-magnitude, which procedure I updated several times in the ensuing years as more ground motion records became available, and gave subsequent papers on it.

Dissertation at Stanford (1966–1968)

Blume: I'll jump around now, and go to a 1966 publication, done when I wrote my dissertation at Stanford. The title was "The Dynamic Behavior of Multi-Story Buildings With Various Stiffness Characteristics."[42] This

41. Blume, John A., "Earthquake Ground Motion and Engineering Procedures for Important Installations Near Active Faults," *Proceedings of the Third WCEE*, Vol. III. Held in Auckland and Wellington, New Zealand, January 1965. New Zealand National Committee on Earthquake Engineering, Wellington, NZ, 1965.

turned out to be a thick volume, a condensation of a tremendous amount of work, mostly on the computer.

Scott: Is that available from the Stanford campus, or in some other form?

Blume: It's available on the campus, of course, but also I published other papers about it. A year or two later I wrote several papers on different parts of the dissertation effort. The main such paper was in the American Society of Civil Engineers, *Journal* of the Structural Division, February 1968. This was entitled "Dynamic Characteristics of Multistory Buildings."[43] This paper won the Leon S. Moisseiff Award. That was the third time I won that same award from the ASCE, probably the only one to win it three times.

"Structural Dynamics of Cantilevered-Type Buildings" (1969)

Blume: I wrote another paper, which leaned on another part of the dissertation work, for the Fourth World Conference on Earthquake Engineering in Chile, in 1969. That was entitled "Structural Dynamics of Cantilevered-Type Buildings."[44] One of the main things I was working on was the fact that cantilevered-type buildings, or buildings with flexible floor systems, as compared to the vertical elements, tend to have the lowest point of contraflexure well above the first story. I believe that I was the first to point this out, even many years before writing this paper. Traditional (pre-computer) frame design approximation in the old days was often done by various rules of thumb. Most methods put a point of contraflexure in the first story columns. Actually, with the cantilever-type buildings being erected in contemporary times, the point of contraflexure might be several stories above the first. The approximate methods could thus be very seriously on the dangerous side.

Scott: Was this because they had been based on the assumption that the point was actually much lower?

Blume: That's right, lower, and the moments much smaller.

Scott: Would you say a bit more in layman's terms about 1) cantilever-type buildings, and 2) the term "point of contraflexure."

Blume: All right. A pure cantilever is like a springboard or a diving board. It just bends in the shape that you're used to in a diving board. Now just turn the board around, or stand it on end, and you've got the cantilever bending in a building that has nothing but walls, let's say, or a very flexible floor system.

The point of contraflexure applies to the point where the cantilever bending moment is zero,

42. Blume, John A., "The Dynamic Behavior of Multi-Story Buildings With Various Stiffness Characteristics." Ph.D. dissertation. Stanford University, CA, 1967.
43. Blume, John A., "Dynamic Characteristics of Multistory Buildings," *Journal of the Structural Division*, Proceedings of the American Society of Civil Engineers, Vol. 94, No. ST2. ASCE, New York, NY, 1968.
44. Blume, John A., "Structural Dynamics of Cantilevered-Type Buildings," *Proceedings of the Fourth WCEE*, Vol. II. Held in Santiago, Chile, January 1969. Chilean Association on Seismology and Earthquake Engineering, Santiago, Chile, 1969.

changing from positive to negative in the same story. The only way one can get zero moment in all stories of a frame building is to have a very rigid floor system, compared to the vertical elements, which elements might be the walls and/or the columns.

"Spectral Response...From Nuclear Event SALMON" (1969)

Blume: In the middle and late '60s I was getting deeply involved in my work for the Nevada operations office of the Atomic Energy Commission, regarding underground nuclear explosions. One of the first events that we witnessed in that program was a nuclear event entitled SALMON, which was a shot in a salt dome underground in Mississippi in 1965.

Two or three years later I published a paper in the *Bulletin of the Seismological Society of America*, February 1969.[45] The paper was entitled "Spectral Response to Ground Displacement in Hattiesburg Resulting From Nuclear Event SALMON."

This paper was short, but it demonstrated how one could take a few key cycles of ground motion out of a whole lengthy time history of ground motion, and by proper statistical methods create essentially the same response spectrum as though one had used the whole record. In other words, this article was pointing out that under actual motion a small portion of the entire record was the controlling feature of the response to the ground motion.

I published several other papers in the SSA *Bulletin* regarding nuclear seismology, which the subject came to be known as. In the December 1969 issue of the SSA *Bulletin*, I published "Response of Highrise Buildings to Ground Motion From Underground Nuclear Detonations."[46] In 1970, the University of Arizona Press published another paper, "Seismic Signal and Structural Response," in a book entitled *Education for Peaceful Uses of Nuclear Explosives.*[47]

"An Engineering Intensity Scale..." (1970)

Blume: Then in February 1970, coming back to the SSA *Bulletin*, I published a paper that I hope in time may become one of the most worthwhile papers that I have done. But this will take a long time.

Scott: You mean a long time before it's fully recognized?

Blume: Before it's fully recognized, and before there's enough data to put it into proper use. It's entitled "An Engineering Intensity Scale for Earthquakes and Other Ground Motion."[48] The Rossi-Forel scale and the Modified Mercalli scale are widely used, as are

45. Blume, John A., "Spectral Response to Ground Displacement in Hattiesburg Resulting From Nuclear Event SALMON," *Bulletin of the Seismological Society of America*, Vol. 59, No. 1. SSA, El Cerrito, CA, 1969.

46. Blume, John A., "Response of Highrise Buildings to Ground Motion From Underground Nuclear Detonations," *SSA Bulletin*, December 1969. SSA, El Cerrito, CA, 1969.

47. Blume, John A., "Seismic Signal and Structural Response," Chapter 13 in *Education for Peaceful Uses of Nuclear Explosives.* University of Arizona Press, 1970.

similar scales in other countries like Japan, Russia and Italy. But all of those scales have the problem that they are subjective and thus not very accurate. I proposed in this paper an engineering intensity scale (EIS) based upon the partitioning of the response spectral diagram into nine segments. Rather than spread the energy of the spectrum over the whole period range, divide the period range into nine standard groups. I have used this for a large nuclear event named FAULTLESS.

I also published another paper,[49] where I used the method for the 1971 San Fernando earthquake. In order to make it work, one needs to get recorded ground motion at several stations over a wide area and from these ground motions develop response spectra by the nine bands, and then simply plot the results on a map. I envision that this scale, properly correlated with damage statistics, could turn out to be extremely useful in the future when more data is available.

"Motion and Damping of Buildings..." (1970)

Blume: Another paper in the SSA *Bulletin* in February 1970 was entitled "The Motion and Damping of Buildings Relative to Seismic Response Spectra."[50]

Scott: It's in the same issue of the *Bulletin*?

Blume: Yes, I had two papers in that one issue.

"...Poured-in-Place Concrete Structures" (1970)

Blume: Prentice-Hall published a book in 1970 entitled *Earthquake Engineering*. Bob Wiegel was coordinating editor. I did Chapter 18, entitled "Design of Earthquake Resistant Poured-In-Place Concrete Structures."[51] This was based on the ductile concrete theory that I have discussed previously.

"Building Columns Under Earthquake Exposure" (1971)

Blume: Another paper published in the ASCE September 1971 Structural Division *Journal* was entitled "Building Columns Under Strong Earthquake Exposure."[52] This paper was unusual in that I was concentrating on concrete corner columns in tall buildings, on the structural, dynamic and probabilistic bases. I attempted to show that corner columns, even in symmetrical buildings of several stories, could be subjected to stresses considerably over what is normally expected by standard procedures. In other words, I was pointing out a weakness in the system. Unfortunately, no one

48. Blume, John A., "An Engineering Intensity Scale for Earthquakes and Other Ground Motion," *SSA Bulletin*.. Vol. 60, No.1. SSA, El Cerrito, CA, 1970.
49. Blume, John A., "Engineering Intensity Scale Data for the 1971 San Fernando Earthquake," *Proceedings of the Sixth WCEE*, Vol. I. Held in New Delhi, India, January 1977. Indian Society of Earthquake Technology, Meerut, India, 1977.
50. Blume, John A., "The Motion and Damping of Buildings Relative to Seismic Response Spectra," *SSA Bulletin*.. Vol. 60, No.1. SSA, El Cerrito, CA, 1970.
51. Wiegel, Robert, I., ed., *Earthquake Engineering*. Prentice-Hall, Englewood Cliffs, NJ, 1970.
52. Blume, John A., "Building Columns Under Strong Earthquake Exposure," *Proceedings of the ASCE*, Vol. 97 No. ST9. ASCE, New York, NY, 1971.

took much interest in this paper. However, many of the buildings that have been damaged before and since, had distress in corner columns. I think the principles I was trying to point out are very important.

Scott: So, in your estimation, cases of actual earthquake damage bear out your findings?

Blume: At least in part. The corner columns in certain buildings are being stressed more than people realize. This can lead to damage and collapse.

[This interview left off in October 1987, and resumed in February 1988.]

Blume: Since our last session I have been going over my various reference lists and histories of lectures and talks, and have come to the conclusion that I've been involved with at least 190 papers and/or books over the years, and that I have given talks and/or lectures numbering over 300. A few of these talks were published later on and were counted in the publication list. Between myself and my company, over the years we've written reports for the government and for industry that I very conservatively estimate at being over 800—possibly 1,000 reports. These were for private consumption, or for client consumption.

Today I'd like to go back and mention a few more of the key papers, just to get them on the record. It might be of value if somebody wanted to look them up sometime.

"...Dynamic Inelastic Design Code" (1973)

Blume: A paper was published in the *Proceedings of the Fifth World Conference on Earthquake Engineering*, in Rome, 1973. The title of that paper was "Elements of a Dynamic Inelastic Design Code."[53] Just as the title implies, this paper suggested in code format, how inelasticity and dynamics might be combined in a potential earthquake design code. It was a very simplified approach to an extremely complex subject—one of interest to me and to many others. The purpose of this code would be to recognize officially the inelastic excursions of ductile structures. Again, I've been harping on this subject for decades—long before it became popular—on the need for ductility and energy absorbing characteristics in tall buildings. If they're properly designed, buildings can go somewhat inelastic during a severe earthquake, absorb a great deal of energy, and still retain their integrity. The building might be designed so that the damage would be mostly minor.

Scott: This is a way to accommodate large, and maybe partly unknown or unpredictable future forces applied to a building, and yet make the building reasonably safe, but without making it so massive or so expensive that it would be impractical to build it. I was thinking about this in the earlier session when you were describing your work on nuclear power plants, and how their design was so different, had to stay in the elastic range, and not be allowed to go into the inelastic range. I guess that sort of differentiation would probably help future lay

53. Blume, John A., "Elements of a Dynamic-Inelastic Design Code," *Proceedings of the Fifth WCEE*, Vol. III. Held in Rome, Italy, June 1973. Secretariat Committee of the 5WCEE, Rome, Italy, 1974.

people reading this oral history to grasp some of these things a little bit better.

Blume: That's correct. The nuclear power plants are designed to remain elastic, even under severe earthquake demands. This means that they are much stronger—far stronger—than normal office buildings. In a severe earthquake, normal office buildings and other structures that are not nuclear will have to go into the inelastic range, because they're not designed to be strong enough to remain elastic, nor should they be. The risk is so small compared to the demand that there is no reason to design all structures elastically.

On the other hand, we have to make sure that a structure undergoing several excursions in the inelastic range does not lose its integrity, such as happened in Mexico City in a few cases. That's why this subject should be brought out into the open rather than handled as something on the back counter.

"Seismic Design Spectra for Nuclear Power Plants" (1973)

Blume: Another paper was published in the *Journal of the Power Division,* American Society of Civil Engineers, in November 1973.[54] This was a very important paper jointly prepared by Nathan Newmark, John Blume and Kanwar Kapur. The paper was entitled "Seismic Design Spectra for Nuclear Power Plants." This was done for use by the Atomic Energy Commission, and the director of licensing, as a guide toward the earthquake requirements for nuclear power plants from that time on. It embraced the results of the research work done in two places: by Nathan Newmark's group back in Illinois, and by our group in San Francisco.

The work was combined, the results were combined, and one set used as a check on the other. The resulting spectral diagrams were provided for design of nuclear power plants.

Scott: Was that research work actually done on designs of nuclear power plants, or was it independent, theoretical research work? Say a little about where it came from.

Blume: The work was basically from research of a mathematical nature, based on the recorded motion of earthquakes around the world. It had been used to some extent, prior to nuclear power plants, but not officially. This effort combined the work of the two firms in statistical research, and provided a device that the Atomic Energy Commission was looking for—namely a suggested basis for designing power plants for extreme earthquake motion.

Three Papers: Sixth World Conference (1977)

Blume: The next paper I'd like to mention briefly is one that was published in the *Proceedings of the Sixth World Conference on Earthquake Engineering* in New Delhi, India, 1977. The paper, by myself, is entitled "The SAM Procedure for Site-Acceleration-Magnitude Relationships."[55] SAM is an acronym for site-acceleration-magnitude. This was an updating of work I had done long before, on the subject of how ground motion attenuates with distance

54. Newmark, Nathan, John A. Blume, and Kanwar Kapur, "Seismic Design Spectra for Nuclear Power Plants," *Journal of the Power Division,* Proceedings of the ASCE, Vol. 99, No. PO2. ASCE, NY, NY, 1973.

from the epicenter, and how to estimate future conditions where a site or a structure or a plant is at a certain distance from a proposed earthquake epicenter, and what the parameters are, and how to go about estimating future ground motion at any distance from an epicenter.

In conducting this study, I did statistical analyses on computers of 2,713 records of ground motion induced by underground nuclear explosions, not to mention hundreds of records of actual earthquake ground motion. Our parameters were magnitude, distance from the source to the site, and type of soil conditions.

Scott: That was a massive job, wasn't it?

Blume: It was a tremendous job, and it all winds up in a few pages. That's true of a great deal of this work.

The next one is also from the Sixth World Conference on Earthquake Engineering. It's entitled "Engineering Intensity Scale Data for the 1971 San Fernando Earthquake."[56] I believe in the prior session we discussed my engineering intensity scale—EIS. This paper is an application of that scale procedure to the 1971 San Fernando earthquake. It shows maps for various period bands of the engineering intensity scale of the southern California region from that earthquake. It is my sincere hope that in time, with future records of this type—it takes a great many records to do this—that this scale will become more and more useful as time goes on. I consider it the most logical intensity scale that is existent. As I mentioned before, it requires many earthquake records to make it viable.

Scott: On that point, comment on the state's strong motion instrumentation program that's been going on now for a number of years.

Blume: Well, the old strong motion work goes back 50 years.

Scott: Yes, but after the San Fernando earthquake there was an effort to put out many more instruments.

Blume: Yes. Arrays, they call them. That's been going on since about '71.

Scott: The state program, started after the San Francisco earthquake, represented a major augmentation of previous instrumentation efforts. The program is funded by a small fee charged when a building is put up. It took them a long time to get the program to where it was really working well. For a good many years they recorded a lot of data, but it was raw and they couldn't get it out in usable form promptly.

Blume: And many owners objected to having the space taken for instrumentation.

Scott: But apparently in these last earthquakes, particularly the Whittier earthquake of last October [1987], they recorded lots of good data, and they got it out quickly, in forms that engineers felt they could work with. I think we are now to the point where we can get adequate data to make methodologies like yours really pay off.

55. Blume, John A., "The SAM Procedure for Site-Acceleration-Magnitude Relationships," *Proceedings of the Sixth WCEE*, Vol. I. 1977.
56. Blume, John A., "Engineering Intensity Scale Data for the 1971 San Fernando Earthquake," *Proceedings of the Sixth WCEE*, Vol. I. 1977.

Blume: That's good, because it's badly needed.

The next item I have noted here is an invited paper given at the same conference. The title is a little misleading, because I was invited to speak under the title: "Allowable Stresses and Earthquake Performance."[57] The paper discusses allowable stresses, as it should, but the main thrust is a consideration of demand of the earthquake motion, and capacity of the structure to resist that motion on a probabilistic basis, involving the probabilities of safety margins and factors of safety. This paper received a lot of comments and much discussion. It was one of the first of several I have written on the subject of probabilistics and demand vs. capacity.

Scott: For the layman, just say a little about the modifier "probabilistics." Is this using statistics to estimate the likelihood of something happening?

Blume: That's a strong part of it, but it's also a little more than that. It considers such things as earthquake intensity, structural capacity, energy absorption capacity—and all these things—not to have finite numerical values, but to be random variables, which have certain probabilities of being more or less than what you're talking about at any one time. By combining these random variables according to the mathematics of probability, one can estimate very logically and closely the probable performance of a structure in a probable earthquake, and what the chances are of it being less valid or more valid than the calculations show. In other words, it's a method of evaluating risk, based upon mathematics, as well as some judgment.

57. Blume, John A., "Allowable Stresses and Earthquake Performance," *Proceedings of the Sixth WCEE*, Vol. I. 1977.

The Acceleration "Gap" (1979)

Blume: The next paper I have [marked] here was given at the Second U.S. National Conference on Earthquake Engineering in August 1979 at Stanford University. The title was "On Instrumental Versus Effective Acceleration, and Design Coefficients."[58] There's long been a so-called anomaly in the earthquake field, and I have written many papers on this subject, wherein measured acceleration by reliable instruments and measured ground motion of any type in addition to acceleration is far greater than the code coefficients for design of structures. This is what I call the "gap." One approach to this problem is to deal in what is called "effective acceleration," wherein instrumental acceleration values of large numbers are adjusted downward—item by item—in view of the parameters of the situation that tend to make the structure resist the motion. One of the greatest of these many factors is the one I've talked about before, namely energy absorption and ductility. One can add to that the subject of redundancy, the safety factor in stress allowances, and many other factors that are enumerated in this paper.

58. Blume, John A., "On Instrumental Versus Effective Acceleration and Design Coefficients," *Proceedings of the Second U.S. National Conference on Earthquake Engineering.* Held at Stanford University, Palo Alto, CA, August 1979. Earthquake Engineering Research Institute, Oakland, CA, 1979.

Scott: Let me ask a question regarding the gap—the rather wide divergence between code requirements and the actual strong motion record. Is this partly related to the character and duration of the motion. That is, don't dome very strong motions occur as quite high peaks or spikes of energy release over extremely short periods of time, so short that the building does not really respond nearly as much as it might to accelerations that continued substantially longer?

Blume: That's a part of it—probably one part of maybe 20 or more, but it's definitely a factor. In fact, I've written a complete paper on just that subject alone for the LL series for PG&E. It's true if there's a very narrow spike—we call those spikes on the records, where the time dimension is so small compared to the amplitude that the spikes look almost like a vertical straight line in a diagram—that spike is not fully effective in motivating the building to respond. And that is one of the many factors.

"Probabilistic Procedures for Peak Ground Motions" (1979)

Blume: The next paper I thought I should mention briefly was published in the November 1979 issue of the Structural Division *Journal*, American Society of Civil Engineers. The title of the paper is "Probabilistic Procedures for Peak Ground Motions,"[59] jointly authored by myself and Anne Kiremidjian. Anne is now [February 1988] an associate professor of engineering at Stanford University and she was a part-time employee of the Blume firm in San Francisco at the time that we wrote this paper. The paper takes work I had done previously, and various methods of estimating ground motion probabilistically for such sites as nuclear power plant sites, and combines the whole into one concise paper for publication.

The first procedure used was a regression procedure based upon available data. The second procedure has to do with fault dislocation based upon geologic findings. In other words, if the faults around a site can be located and studied by geologists expert in this type of activity, estimates can be made as to how the faults have moved in geologic time. Having this, with proper mathematical treatment, the estimated ground motion at a particular site can be derived. The paper shows how this can be done, and it was done for a nuclear power plant in California by the writer, I believe for the first time.

The third procedure in the paper is called plate boundary. This approach considers the geologic plates in the bedrock in a given locality, and the boundaries of these plates, and again, based upon geologic and seismological data, estimates can be made of where the possible motion will occur sometime in the future.

This sounds pretty far out and I guess it really is, but nevertheless it shows the state-of-the-art in the nuclear power plant field. I cannot in this brief interview go into all the details, but anyone interested can look at the paper itself. The product of the work is often given in the form of a plot showing the peak ground acceleration

59. Blume, John A. and Anne S. Kiremidjian, "Probabilistic Procedures for Peak Ground Motions," *Journal of the Structural Division*, Proceedings of the ASCE, Vol. 105, No. ST11. ASCE, New York, NY, 1979.

plotted against the probability of that acceleration occurring. The paper contains several plots of that description.

"...Attenuation Studies" (1980)

Blume: The next paper is from the Seventh World Conference on Earthquake Engineering in Istanbul, Turkey, 1980. The title is "Distance Partitioning in Attenuation Studies."[60] I'm the sole author. This paper considers the results of more attenuation studies in the computer, consisting of a data set of 816 station component, acceleration, distance and magnitude combinations. Instead of considering all the data regardless of epicentral distance in one procedure, the distance parameter is divided into partitions of certain dimensions and distances, and each partition is studied individually. Later on the results of that study are combined to make the overall distance plot.

This procedure sounds a little far out, like some of the other recent papers, but nevertheless is has very practical significance. One of the results of this study shows that by this partitioning procedure, which makes as much sense as any other procedure, the ground motion close to the epicenter is estimated to be less than when estimated by more conventional methods.

Scott: That's interesting. Do you have any conjectures as to why there is a difference?

Blume: That's a little difficult to answer, but I would say that very close in to an epicenter I've long felt that the motion is not as great as people think, especially if the distance from the epicenter to the site is less than the distance to the focal depth of the earthquake underground.

There's another factor involved too, and that is what I call leverage. If you take a series of data points, such as from the San Fernando earthquake where hundreds of data points were obtained, it would be such massive data at that one distance, or within a certain distance band, that the data has what I call a leveraging effect. It's prying the motion either up or down, due to the fact that the data is not evenly spread, but concentrated too much in one place. The partitioning procedure discussed in this paper is intended to eliminate that bias.

"Protecting ... Museum Collections ..." (1986)

Blume: The next paper I want to mention is an excursion from the most recent papers I've talked about into the realm of artifacts. It seems like a big jump. The title of this paper is "The Mitigation and Prevention of Earthquake Damage to Artifacts."[61] This paper was developed and given by invitation to a large national meeting in Washington, D.C., which led to the publication of a hard-cover book entitled *Protecting Historic Architecture and Museum Collections From Natural Disasters*. Barclay Jones was

60. Blume, John A., "Distance Partitioning in Attenuation Studies," *Proceedings of the Seventh WCEE,"* Vol. 2. Held in Istanbul, Turkey, September, 1980. Turkish National Committee on Earthquake Engineering, Istanbul, Turkey, 1980.

61. Blume, John A., "The Mitigation and Prevention of Earthquake Damage to Artifacts," *Protecting Historic Architecture and Museum Collections From Natural Disasters.* Barclay Jones, ed. Butterworth Publishers, Stoneham, MA, 1986.

the editor. As the title of the book implies, it considers all forms of natural disaster—earthquakes being one of these.

Scott: Who published the book, and in what year?

Blume: The book was published by Butterworth,...Boston, London...a worldwide organization called Butterworth. The copyright is 1986 by Butterworth Publishers, 80 Montvale Ave., Stoneham, Mass. 02180. I'm responsible for one chapter in this book, which bears the same title of the paper I just read. I greatly enjoyed working this material up.

Scott: That does sound like a bit of a departure from some of your more mathematical papers. I'm curious as to how you went about doing the paper and chapter. Did you go to some museums and look over the artifacts and figure how you would go about tying them down?

Blume: Well, I've given a lot of thought over the years to protecting delicate objects, and I've been to many museums myself—not only in this country but in other countries—and I know pretty well what the situation is. But for this invited paper I actually sat down and figured out different classes of exhibits. Thus, some are on display, while some are packed in storage. Some are large statues, while some are small delicate vases.

In the chapter, as I recall, I outlined procedures that might be followed to protect each of these types of artifacts. One of the criteria that has to be faced initially is, is the object to be seen at all times, or is it put on temporary display and then stored away somewhere, and if so how do you store it? If it's out for permanent display, how do you protect it from being tossed over? The paper also discussed the fallacies of the old tombstone argument. In the old days people used to estimate earthquake motion by observations of tombstones falling over or standing up, which is all right as far as it goes, but they didn't consider the soil-structure interaction, three-dimensional effects, traveling, and many other parameters in the problem. I have listed in the paper—interior artifacts of large size, objects in glass-covered cases, objects on shelves, wall displays, stored items, and discussed what might be done under each category.

I found that the people I was working with, and people who read the book or heard the talk, were extremely interested in the subject and the approaches that I outlined. In fact, they had me come back to Golden Gate Park months later to give another talk to the people in the Golden Gate Park museum, and they also had others come in to attend a large meeting.

"...Earthquake Resistance of Tall Buildings" (1984)

Blume: The next paper that I'll mention today is from the Eighth World Conference on Earthquake Engineering, San Francisco, 1984, entitled "Redundancy and Relative Earthquake Resistance of Tall Buildings."[62] In this paper I consider redundancy as not necessarily the same as ductility and energy absorption capacity. They're both extremely desirable in

62. Blume, John A., "Redundancy and Relative Earthquake Resistance of Tall Buildings," *Proceedings of the Eighth WCEE*, Vol. V. Prentice-Hall, Inc., Englewood Cliffs, NJ, 1984.

tall buildings. They are not mutually independent, but you can have a ductile building without redundancy, or a ductile building with redundancy. I much prefer to have both redundancy and ductility, and the ability to withstand severe excursions. If things start to break up, with redundancy another line of defense comes into play. In fact, aircraft are usually designed with this principle in mind—if something gives a bit, then something else comes into play and takes over. For decades I have preached that redundancy, as well as ductility, are not only desirable in tall buildings, but may be essential for survival.

In this paper I considered the various code types of structures, and I wind up with plots of the distortion in inches, versus the relative value of the various code-type structures, and the results show a tremendous range in the allowable distortion of a building before collapse.

Scott: A range between actual buildings, or between hypothetical buildings that have or do not have these redundancy factors?

Blume: The ranges between the different types of buildings set forth in the code. I might just read off what those types are—ductile moment-resistant steel frame, ductile moment-resistant concrete frame, braced frame, braced frame and ductile moment-resistant frame combined, shear wall and ductile moment resistance, shear walls in framing, a box and shear alone without ductile moment resistance, and a box in flexure.

The shear wall structure starts to fail after very small distortions, as compared with the redundant structures, which can withstand large distortions, provided there's no failure from secondary effects such as p-delta, p being the weight above a point under consideration, and delta being the distortion from the static position.

Predicting Ground Motion Effects on Structures (1975)

Blume: There is a publication that our office prepared, based largely upon many of my research efforts for the Nevada operations office for the Atomic Energy Commission (AEC), having to do with underground nuclear explosions and the safety of buildings subject to that induced ground motion. A large book was prepared under the number JAB-99-115, which represents the 115th report prepared by our firm under our contract 99 for the Atomic Energy Commission on structural response to ground motions from underground nuclear explosions.

This book is entitled *Effects Prediction Guidelines for Structures Subjected to Ground Motion.*[63] Even though the work was done relative to nuclear-induced ground motion, almost all of the results are applicable as well to natural earthquake ground motion. In addition to starting off with definitions and discussion of typical conditions in the earthquake ground motion field, this book goes into the analytical damage prediction fundamentals. It covers how to predict damage, and discusses the behavior of dynamically loaded buildings and building ele-

63. *Effects Prediction Guidelines for Structures Subjected to Ground Motion.* Prepared by the John A. Blume Corporation for the Atomic Energy Commission. National Technical Information Service, U.S. Dept. of Commerce, Springfield, VA, 1975.

ments. Then it considers the engineering intensity scale (EIS) method, which I've discussed before, and another method that I have not discussed herein to date, the spectral matrix method (SMM) for predicting structural damage. This method involves using estimated or actual spectral response diagrams partitioned into divisions and correlating and reconciling-ground motion demand and capacity of the building. Much of this is similar to other papers I have given, but also much of it is new and different. And a great deal of it is complementary to what has been done before.

The book also discusses the threshold evaluation procedure (TEP) for engineered buildings. We used this approach in the case of a great many large underground nuclear shots. Before the shot we had to estimate the possible damage, if any, so we used these procedures to estimate what might happen, and prepared written reports in advance of the shot. The thresholds outlined were the code-required threshold, the working stress threshold, the yield limit threshold, human alarm (as when people start to panic, or at least feel the motion and then panic), observable damage threshold, human hazard threshold, and story failure threshold. I'm not implying that all of these were reached with underground nuclear shots, but we certainly have reached them all with natural earthquakes.

Scott: These thresholds are stages of response to stronger and stronger motion?

Blume: That's right. Stages, with each successive one getting more severe as we go along. The first one—code-required threshold—is merely coming up to code stresses, which is really nothing compared to where it might go in a real earthquake. As part of this work, we studied the human response to motion. There have been a few papers on this subject, but we conducted experiments in our laboratory. We used people on swings—subjected to different degrees and extents of swinging motion and got their blind-folded reaction to what they felt and when they felt it.

Scott: You put them in an ordinary swing, or a specially designed swing?

Blume: A special swing where we could measure the motion. We'd pull them a certain distance and let them go. The results checked very nicely with very exotic experiments that were made back east. You see, the human motion threshold is very important in the east, especially for wind. Many office buildings feel the wind motion. I've been in New York buildings during windstorms. When the building moves with the wind you don't feel it very much, but when the wind lets up and the building goes back, you feel that.

This book goes into a great deal of mathematics on probabilistics and risks and how to calculate the risk. The book has a tremendous amount of graphical information and tabular information. This is the condensed result of a great deal of work.

Scott: It sounds like it not only tells how you did those things, but also tries to set up something to guide others who might want to do similar things elsewhere.

Blume: Actually, that's what it is. It's called *Guidelines*, for others who might want to do it. That's its purpose. The reason behind the requirement to do this—we were asked to do this by the U.S. government—I think part of it was that they felt we had been on this project for 20 years. The word got out that we were getting to have a monopoly on our procedures, and that it should be expanded and given to the world at large, because it was government money paying for it, which we agreed to. So they said "Pull together a lot of what you've done that might help a person predict the effects of ground motion from another shot, or by somebody else on the same project." They wanted it put in a book that could be studied to get the advantage of what we'd been doing for all those years. And we did this as well as we could. You can see it's a large book.

Unfortunately, you can't write down everything that you learned and have done over a period of time, so we still have a lot of expertise that we couldn't put in this book. But we tried to, I'll say that. We didn't hold anything back.

Chapter 14

Observations and Retrospections

"I have simply lived and breathed earthquake matters for decades."

Scott: What do you consider your most interesting or most important contributions to earthquake engineering and structural dynamics?

Blume: Some people have been confused about me and what I do and have done. Many can't understand why a structural engineer should be doing research not only in structural engineering but also in such matters as probabilistics and ground motion and dynamics. It's all part of my original intent to try to do something about the earthquake problem.

I have simply lived and breathed earthquake matters for decades. I believe that I'm considered a maverick by some in the field because I have done design, consultation, and research. Some purists probably don't approve of that combination, but I've found it to be extremely useful. My design experience has complemented and improved my research experience and vice versa. Even my early work in the building trades has been useful in practice.

Over the years I've come up with several innovations due mainly to hard work and some luck. I started out studying intensely the characteristics of buildings—both structural and dynamic—and in the days when dynamics was a nasty word to structural engineers. I pioneered in such matters as natural period determinations and period changes under sustained motion, site periodicity, which is something that is now very popular—especially after Mexico City, an extreme case of site periodicity.

Scott: Say just a word about what you mean by site periodicity.

Blume: That certain building sites, due to the nature of the soil and the depth of the soil, may shake in periods that are characteristic of that site.

Scott: Something of a harmonic response?

Blume: Right. In other words, motion coming into a soft site, such as Mexico City, can become very strong or be amplified in the site's own periods of vibration or frequency. Then if on top of that soil you place a building that has a similar natural period of vibration—especially after the building has undergone a few inelastic excursions and is coupling with the soft soil below it—you are heading for disaster. This happened in some cases in Mexico City. A few buildings out of a great many had complete collapse.

This is not a new consideration. For example, in our first thesis in 1934, Hesselmeyer and I discussed briefly the matter of site periodicity. The subject was pretty much ignored for decades. The way it can be approached today is to have design spectral diagrams that show the site periodicity for each site. I believe at the time of our 1934 thesis the feeling was that the dangerous site period range was 1 to 1.5 seconds. In Mexico City it turned out to be 2 seconds, but it's the same principle.

For decades I have preached the matters of energy absorption and ductility and redundancy and soil-structure interaction and ductile concrete, where concrete is designed to withstand inelastic motion without failure. I have done a lot of work in the attenuation of motion with distance from the site, the SAM procedure. I pioneered in reinforcing brick masonry to be earthquake resistant. I've done work in smoothed 5% response spectra, which are now getting to be a standard approach. Code development, plateau design—where you design a building for at least two stages of motion. One is the initial onslaught of the motion, and two, after the cosmetics have failed and little resistance is left except that of the structural frame, how the building will stand up under continued motion. This, again, is something that happened in Mexico City. After many excursions and such long duration of strong motion, some of the structures just ran out of steam. The reserve energy technique is a powerful tool and concept for inelastic design. The Engineering Intensity Scale, the spectral matrix method, the threshold evaluation scale, were pioneering efforts in nuclear seismology, which can be applied to the earthquake field.

Philosophical Guidelines

Blume: I have not followed any particular formula in dealing with earthquake problems, whether in my research, or in designing structures, plants, and installations. I do, however, have a few philosophical concepts which I have found helpful from time to time. I just offer them here for whatever value they might have for others. "When in doubt, take the course that is best for the public welfare." That perhaps sounds corny and obvious, but I've found it very useful in reaching tough decisions over the years.

"Some problems which cannot be solved directly, may be straddled, or approached from the sides to find the solution trapped in a narrow window." This is somewhat of a research tool, especially in parametric studies where, due to the unknowns or the complications, a direct, classical solution may not be possible. But by approaching it with parameter variations and other means, one can often trap the only possible solution in a narrow space. It's got to be in there somewhere. And if the space is narrow enough, for all practical purposes you've got a solution. But you've got to prove that it's the only solution and that it is repeatable.

Another mental tool that I've carried around for a long time is "Do not get bogged down in trivial matters, but on the other hand, have great respect for important detail." There is a distinction between triviality and important detail.

Scott: And it's essential to be able to recognize the difference between the two.

Blume: Absolutely. Another is: "Always do good work regardless of profit, or lack of profit." And: "Nature is always right—try harder to understand it." Those are simple little items, but I've found them very useful at times.

Scott: These are things you keep in the back of your mind?

Blume: That's right. Over the years I've had some tough decisions to make—personal, technical, social, financial—and I've found these philosophies, and others like them, to be very helpful.

Reflecting on Predecessors and Contemporaries

Scott: Do you have some comments on the roles and contributions of your predecessors and contemporaries in earthquake engineering?

In Japan

Blume: A great deal of the important work—at least in static design—started in Japan. The Japanese came to this country in 1906 to study the effects of the San Francisco earthquake. They then went home and proceeded to do something about what they saw and learned. San Francisco did not get a real earthquake code in effect until 1948. There is a message there.

Some of the early Japanese workers, such as Tachu Naito, Kyoji Suyehiro, and Kiyoshi Muto, did a lot of thinking on the problem and did some good work. The main loss of life in Tokyo and Osaka in 1923 was due to fire and panic. The buildings that they had constructed in recent times had stood up pretty well, but they were of the classical, traditional type, somewhat like the early San Francisco buildings that also stood up pretty well in 1906. But as I've pointed out in many papers, the buildings we're putting up today are not the same as the traditional buildings of early Japan or San Francisco. Those early buildings had thick masonry walls and other non-calculated elements that made them a better risk. They had great redundancy in many cases.

Another class of construction that I've seen in Japan that struck me as interesting—and this came along before ductile concrete was introduced—they built a steel frame composite with concrete, using angle shapes and flat bars for braces to build an open-frame column, for example, around which they put forms and poured the whole thing solid with concrete. It made a very interesting and worthwhile type of construction, which we never saw in this country. The reason we didn't have it here is that the labor cost would be prohibitive in dealing with such small pieces of structural steel. I think today they're reinforcing the way we do, but this goes back 20-30 years. A column, for example, would have four corner angles of structural steel and lattice type diagonals.

Dynamic Approach: Jacobsen, Dewell, Blume

Blume: The dynamic approach to the earthquake problem was probably started by my friend and professor, Lydik Jacobsen. As I pointed out, however, he was not a structural engineer, but a mechanical engineer. As his assistant, I had the opportunity to take dynamic theory and apply it in a structural sense. There was one San Francisco engineer who I think would have liked to have done this, but he didn't have the information at the time, and that was Henry Dewell. He took a fairly active interest in dynamic matters and how they might someday be applied.

Scott: Henry Dewell must have been a pretty remarkable person.

Blume: He was. In fact, I recall—I probably have it somewhere in my files—an engineer named [Jacob] Creskoff back east wrote a commentary in *Civil Engineering* about the U.S. Coast and Geodetic Survey forced vibration work, and failed to mention that I'd done it and that Jacobsen was involved. So Henry Dewell wrote a letter to the editor complaining about this lack of proper recognition for prior work. Of course they wrote back and forth a few times, but I was impressed that Henry Dewell would bother to do such a thing. And he hardly knew me then, either. A little later on I got to know him more. He was not a dynamicist in the true sense, but I think he had an appreciation of the possibilities. And he understood Jacobsen a little better than most of the others. You see, Jacobsen was so far out that when he'd lecture he'd leave the typical structural engineer just flat.

So I came along and probably filled the gap, which was one of my intentions, to bridge the gap between theory, dynamics and practical structural design. That was my whole objective. Later on it broadened, but that was the start of it. That goes back to 1932, a year before the Long Beach earthquake.

Other Early California Engineers

Blume: I hesitate to list names, because I am almost certain to omit some deserving person. On the other hand, I have been talking so much in this series about my own work it would be a pleasure to bring others into focus. There were several early pioneers in structural engineering, some of whom were quite interested in the earthquake problem. Essentially all of the latter were in California or Japan. It took quite some time for the public to learn or care what a structural engineer was, much more time to associate earthquake-resistant design with the structural engineer, and even longer to do something about it. Part of the reason for this was the relatively small number of damaging earthquakes in populated California—1906, 1925, 1933, 1940, 1952.

In the San Francisco area structural engineers in the early days included M.C. Couchot, C.H. Snyder, H.J. Brunnier, R.S. Chew, Charles Derleth, L.H. Nishkian, Henry Dewell, Walter Huber, Harold Hammill, E.L. Cope, J.B. Leonard, A.V. "Gus" Saph, Henry Powers. Of these, I would consider Dewell, Huber and Nishkian to be the most advanced in their earthquake interest and approach, and possibly Chew as well. Dewell had a keen interest in the subject and studied the Japanese static techniques. Brunnier did the most to make structural engineering a viable profession. Slightly later came Harold Engle as a strong 10 percenter, and Mark Falk moved from the south to San Francisco—all good engineers.

Stanford, with its large shaking table, was active in the late '20s and the '30s. Arthur Ruge tested elevated tank models at MIT, and R.R. Martel had a modest program at Caltech. There was the U.S. Coast and Geological Survey special California research program in the mid-'30s, and not much else was going on. I was active at Stanford, in the USC&GS program, and later in the decade in my own research.

In the Los Angeles area the early structural engineers included Oliver Bowen, Clarence Derrick, Murray Erick, Rufus M. Beanfield, Paul Jeffers, Mark Falk, R.R. Martel, R.W. Binder, Steve Barnes, Robert Labarre, Fred Converse, Ben Benioff, D.L. Narver. Probably the most advanced in their earthquake thinking were Derrick, Jeffers, Martel, and Binder, with Jeffers doing the most to make structural engineering a viable profession. Jeffers and Falk were the best storytellers at the early meetings. Barnes in later years did a great deal of diaphragm testing as well as code work.

George Housner started his long career at Caltech, I believe in the late '40s. He was later joined by Don Hudson. Ayre and Hollis later worked with Jacobsen at Stanford after I left to use the shaking machine. Williams and Benjamin also conducted tests at Stanford.

Not structural engineers, but important in the picture were Bailey Willis, Andy Lawson, Lydik Jacobsen, Perry Byerly, Beno Gutenberg, Carl Richter, Hugo Benioff, Maurice Biot, Frank Neumann, Frank Ulrich and later, Clarence Allen and Bruce Bolt.

More Became Involved

Blume: As time went on, more engineers became earthquake-conscious. Several began to call themselves earthquake experts or specialists, although most had little more than a static code philosophy, and perhaps some experience from inspection of damage. But some were beginning to probe deeper, especially after Kern County, 1952. Among these were Karl Steinbrugge, Henry Degenkolb, John Rinne, Emilio Rosenblueth, Nathan Newmark, Roy Johnston, Martin Duke, Mike Pregnoff, Glen Berg, Harry Seed, Ray Clough, Joe Penzien, and a little later, Edward Wilson, Egor Popov, Vitelmo Bertero, et al, at U.C. Berkeley, and Haresh Shah and James Gere at Stanford. I should also mention Bob Hanson at Michigan, and Bob Whitman at Massachusetts.

As I feared, the list grows long, and the subject gets more involved, and I am certain to be overlooking some important persons. As far as contemporaries are concerned, there are now hundreds of them—a sign of progress. Moreover, there are now a dozen or more specialties within a specialty—another sign of progress. And not listed are all the many seismic workers in other countries, especially in Japan, India, New Zealand, Mexico, Europe, South America, etc.—another sign of progress.

A Selected Listing

Blume: In order to make some sense of this, let us consider the most industrious, the most durable with seniority in advanced technology, the most innovative, the most productive, the most versatile, the most recognized in the fields of earthquake engineering and structural dynamics.

In earthquake engineering, alphabetically, there would be: Barnes (diaphragm testing, codes), Blume (various), Degenkolb (damage surveys, codes), Engle (static analysis, early code), Housner (various), Muto (various), Newmark (various), Rinne (codes, organization), Steinbrugge (damage study, codes, risk).

In structural dynamics, alphabetically, there would be: Blume (various), Clough (analysis), Duke (soils, lifelines), Housner (various), Hudson (instruments, theory), Newmark (various), Rosenblueth (early probabilistics, analysis), Seed (soils, codes).

Those on both lists, and also those with the most aspects of the subjects, are Housner, Newmark, and Blume. Blume started in the early '30s, Housner in the late '40s, and Newmark in the '50s. Apologies to the many not listed.

The Future

Scott: What will or should the future bring?

Blume: Of course, no one knows the future. However, the past and the present, and especially trends, help us to look ahead, albeit with uncertainty. As time goes on and the population increases, the probability of major earthquakes affecting major cities increases. The event does not have to have magnitude 8, or even 7, to cause a lot of damage to old pre-code buildings. A close-by 6 is a strong earthquake. In fact, I have a feeling that a 7 or an 8 will not shake much harder than a local 6 to 7, but will shake much longer and over a larger area. Because codes vary and designers vary, and conditions vary, there will be some damage, or worse, to a few code buildings. This is a problem of a very small probability of a major event or events.

Earthquake Prediction

Blume: We can be sure that earthquakes will continue—the questions are when, where and how much. Judging by the media and public reaction to the small but sharp 1987 Whittier shock, there is going to be a great problem with human reaction, including panic.

Prediction of time and place within reasonable tolerance is, in my opinion, a long way off. Even if it can be done someday, and this is not certain, the handling of people will be a major task. Buildings will still be damaged unless they are well designed and well built, so prediction should not be considered an alternative to good structures.

Earthquake-Resistant Design and Ground Motion

Blume: It is possible now, and has been for some time, to create earthquake-resistant structures and also to estimate the potential ground motion for a given site. This is best done probabilistically. The degree of expected damage, if any, can be adjusted in the design stage to suit the requirements for safety and cost. In other words, risk can be controlled where it is essential to do so. Many of these techniques have been presented in the publications [I have authored] over the years. It is hoped and expected that in the future such techniques and procedures will receive wider application.

It is hoped as well that the Engineering Intensity Scale (EIS) will come into general use someday as a simple but logical and very useful scale of earthquake intensity.

Two-stage design (plateau design) should be used for important structures. So should energy design, and controlled ductility, and redundancy. Base isolation will increase and be watched closely for performance, with and without major earthquakes.

I think the missing link has been, and still is to some extent, lack of acceptance or general knowledge of the true nature of the subject. The subject has been put back considerably by not facing up directly to the situation that ground motion can be very severe compared to building code forces, and you have to go into a ductile range to withstand it. It's been under cover. I've been trying to bring it out in the open for decades. I think the only way we can understand anything is to know what makes it work.

Tying It Together Is Not Enough

Blume: But most engineers of the earlier class, even after these things became known to some degree, would take the approach that a 2 percent or 3 percent base shear design for lateral force is adequate if you tie the whole thing together and make the connections good. This helps, of course, but it is not always enough.

I don't think it will ever be a pure science, not for a long, long time. But to make it a state-of-the-art practice combined with some science, people have to recognize the true nature of what makes the darn thing go, and that is that the ground motion can be extremely severe. You can design structures to withstand this motion, but it's not going to be done by ignoring the need for ductility under sustained motion.

Buildings have to be tied together—there's no question about that. That's one thing they all agree on. By that I mean the connections of the floors, roof, columns, framing, walls, etc. They have to be well connected at all points to remain tied together. There's no argument about that, there can't be. But beyond that one gets into this rather strange world where you design with 5 percent gravity at the base shear, and yet the instruments record the spectral responses of 100 percent of gravity. I've been working on that gap between the two—code forces and actual earthquake forces—for decades, and I'm not sure I've got the message across yet, because people are still reinventing the wheel.

Scott: Basically you've got to try to design your building so that if it's forced beyond that lower code percentage, nevertheless it can somehow absorb the energy, respond, and withstand the greater motion without collapsing.

Blume: That's right. An analogy is a football player. A good football player who gets tackled and swamped by the other team falling on top of him rarely gets injured. He's tough and ductile. He's got to be to survive. On the other hand, a player who's going to break a leg or an arm or a jaw the first time he gets tackled is brittle. He's no good as a football player. It's the same with buildings. They have to be tough, ductile, roll with the punches, absorb shock and energy, and still hang together. It's the only form of engineering that I know of—outside of tornado, cyclone, and hurricane—where in dealing with earthquake problems you *do* one thing but expect something else to happen.

Lateral Force Gap

Blume: You design according to a code for a relatively small amount of lateral force, and yet the actual force can be enormous. We're kidding ourselves. I'm not saying we have to design for that great force, but you have to understand what is needed to reconcile the gap.

Scott: You have somehow to design so as to deal with that lateral force gap, and without getting a collapse.

Blume: That's right, and some of these papers I've published are based on that subject alone—how to take care of the gap, what makes the gap, when is it present, and when are the parameters missing? I think that's something that has to be worked on in the future. Although I think we know most of the answers, it's still not generally understood.

Using Past Research

Blume: In my opinion, a lot of the research that is going on since the federal government has come up with all this research money is valuable if for no other reason than educational purposes. I don't think it's all necessary, but I don't object at all because I think it's helping to educate. What I do object to is when they do something today, but forget all about what has been done in the past. So I think what we need is an open policy of recognizing that the earthquake problem—although big earthquakes may happen very rarely—is a real problem for the public. And nothing is as dramatic as Mexico City and Tangshan, China, to point that out. Tangshan was a complete disaster. Of course their buildings weren't like ours. Our buildings would stand up much better than those in Tangshan, by far. Incidentally, a great many of those buildings were designed by Russian techniques. The Russians used to be very strong in that country, and they showed the Chinese how to build industrial buildings, and some of the techniques were not so good for earthquake resistance.

Improving Codes

Blume: I think there has to be constant effort to improve the code. Codes should be considered live objects, and not allowed to sit around too long without some indicated improvement. On the other hand, I don't think they should be changed just for the sake of change. There should be some good reason for doing so.

Awards and Honors

Blume: I have received many awards and honors for my work in this area, and I'm very appreciative and proud of them all. One other thing has stood out in my mind over the years: the fact that I've had hundreds of engineers come up to me after talks or at meetings and tell me how much they enjoyed the talk or reading my writings, and how much they've gotten out of my efforts. The apparent feeling is that I've been successful in combining theory and practical know-how into something that is understood and useful to the practicing engineer. That makes it all worthwhile.

I've worked very hard, and I sincerely hope that I've been able to contribute something that has saved lives and will save lives in the future.

Acknowledgments

Blume: In closing I would like to note my appreciation for the contributions over the years and decades of the Blume company employees, associates, and officers toward the production and innovations of the firm.

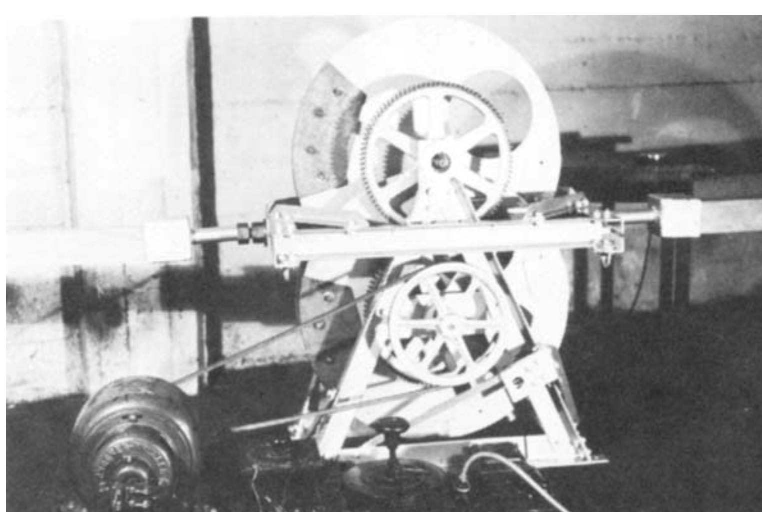
It should also be noted for the record that my wife of 42 years, Ruth, who passed away in 1984, not only put up with me and my workaholic ways but encouraged my efforts. And I thank Jene, my wife today, for her help and encouragement.

Stanley Scott, his secretary, Maria Wolf, and student typists, Stacy Furukawa and Sheila Rose, deserve the credit for putting this oral history together. Thank you!

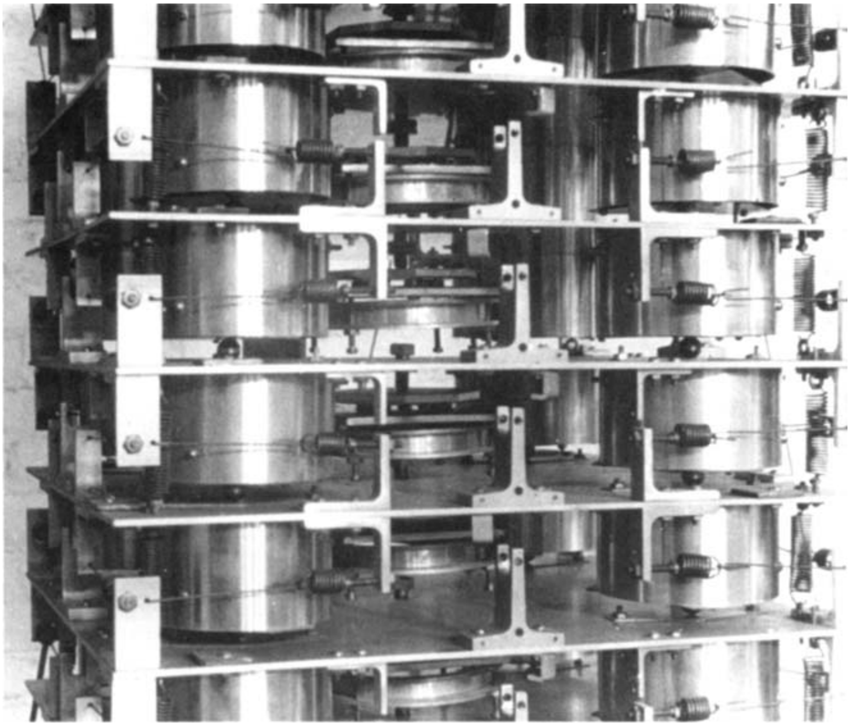
Photographs

John A. Blume, 1957. (photo: Moulin Studios)

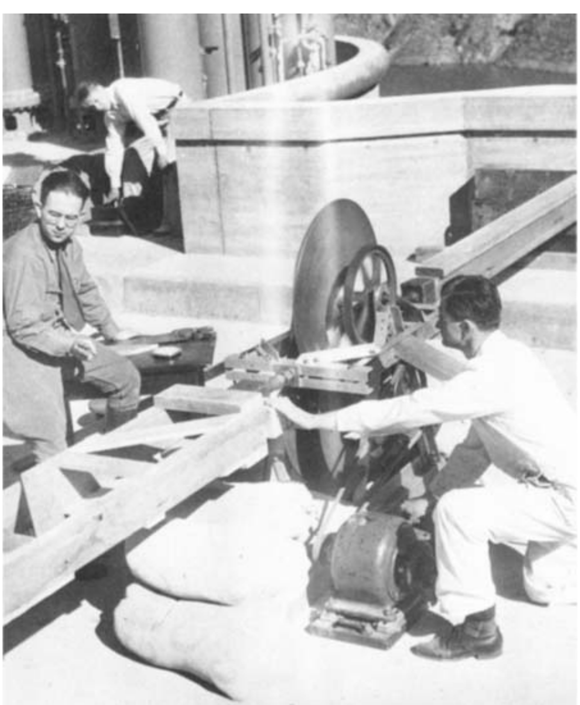
Working as a carpenter in the Santa Cruz Mountains during summer of 1928 (following his freshman term at Stanford University).



Building and ground vibration machine designed and built by Blume while working for the U.S. Coast and Geodetic Survey, 1934-1935. (photo: U.S. Coast and Geodetic Survey)



Close-up of Alexander Building dynamic model—five degrees of freedom per story. Designed and built by Blume in the Stanford Vibration Laboratory with Lydik Jacobsen, 1934.



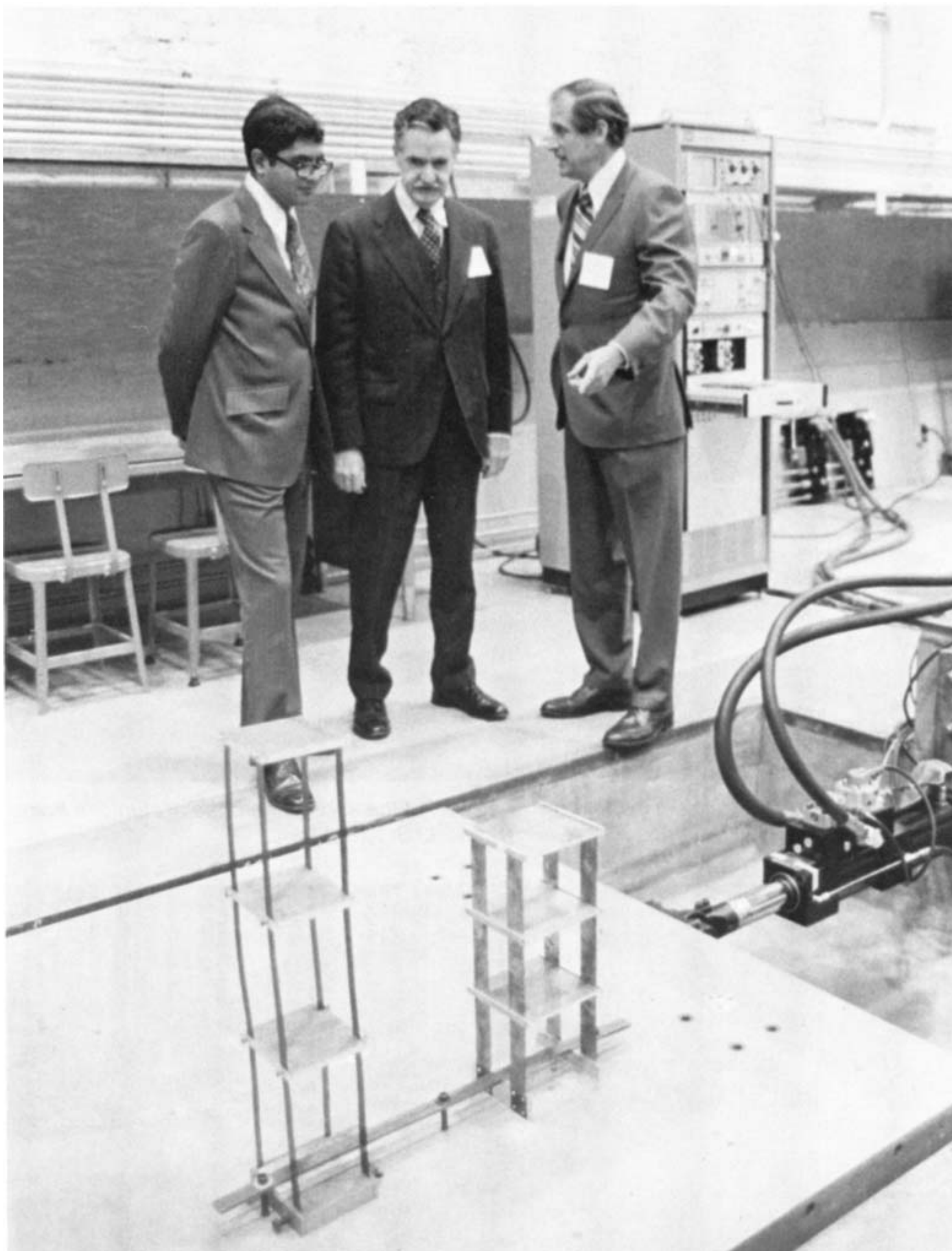
Measuring forced vibration on Morris Dam on the San Gabriel River. Left to right: Ralph McLean, George Pickett, John Blume, 1935. (photo: U.S. Coast and Geodetic Survey)

The San Francisco-Oakland Bay Bridge field engineering staff prior to bridge opening. Blume is in top row, far right, 1936.

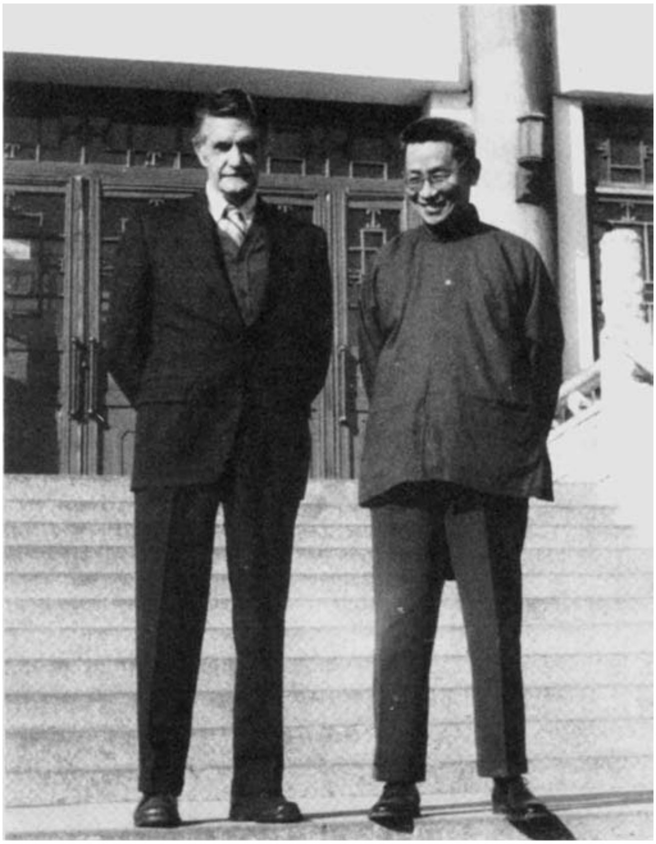
Inspecting a hydrostatic tiltmeter on the San Francisco-Oakland Bay Bridge, 1936. At left is R. Robinson Rowe, Blume on right.

John Blume and Joe Nicoletti inspect the quay wall of the submarine base at Hunter's Point Naval Shipyard, San Francisco, CA, 1952. (photo: U.S. Navy)

Panel discussion at the First World Conference on Earthquake Engineering, held in Berkeley, CA, 1956. Left to right: John E. Rinne, S. Okamoto, John Minami, Kiyoshi Muto, John A. Blume, V.A. Murphy, Emilio Rosenblueth, Jorge Barco, Steve Barnes.



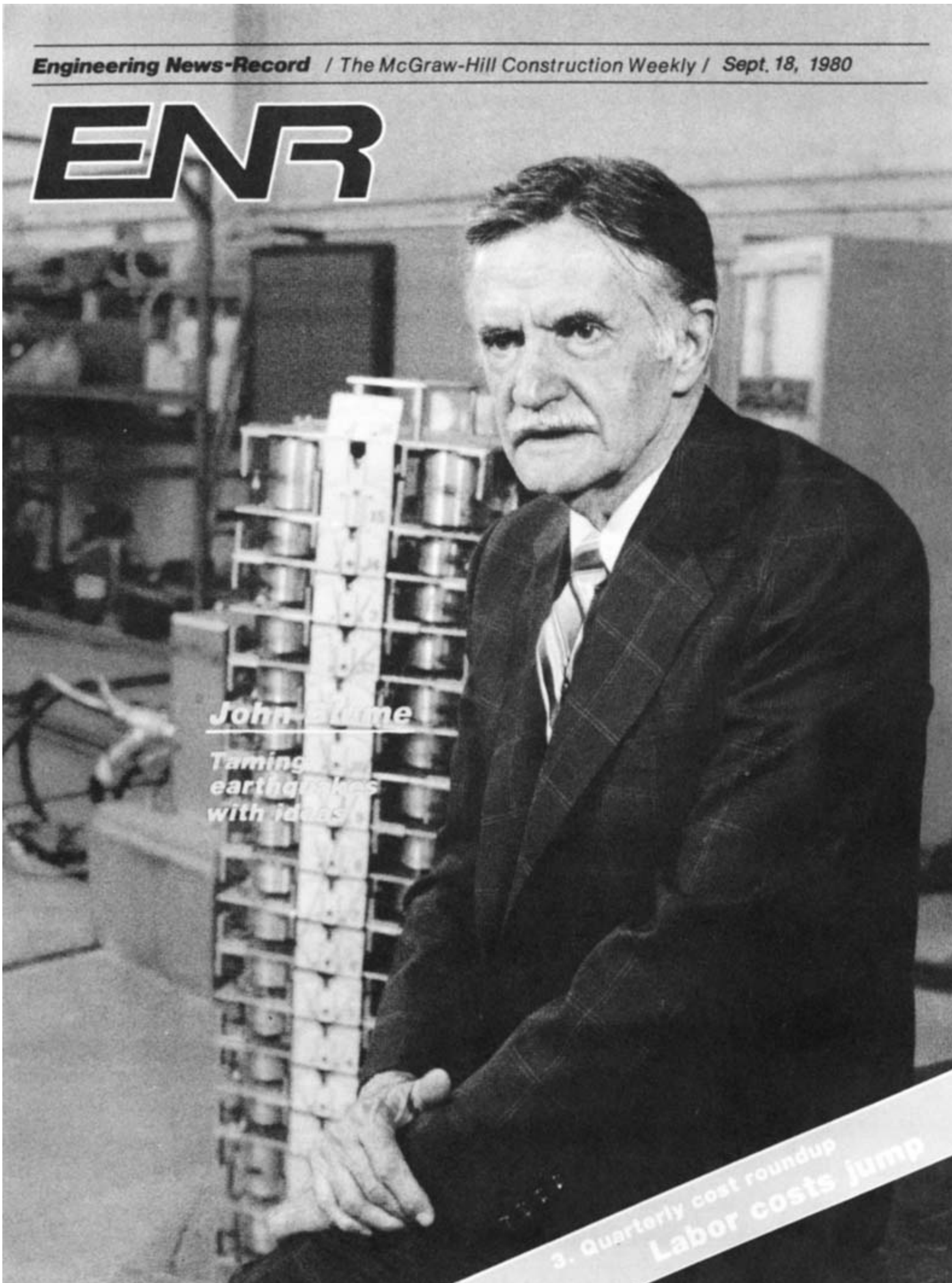
Blume with Haresh Shah (left) and Jim Gere (right) at a Stanford University party celebrating the Blume gift to establish the John A. Blume Earthquake Engineering Center at Stanford University, December 5, 1974. (photo: Stanford University News Service)



At the Institute of Engineering Mechanics, Harbin, China during the EERI Delegation to the People's Republic of China tour, 1980. Liu Huixian, Institute Director, on right.

Blume makes notes between meetings at the China Institute of Seismology during the EERI Delegation to the People's Republic of China tour, 1980.

EERI Delegation to the People's Republic of China, 1980. From right to left, back row: Hank Lagorio, Neville Donovan, Robert Hanson, Roger Scholl, Willard Keightley, Kalman Lee Benuska. Front row: Leon Ru-Liang Wang, Roy Johnston, John Blume, Helmut Krawinkler, Anestis Veletsos.



Cover of Engineering News Record, *September 18, 1980 featuring John Blume. Article within entitled "Taming Earthquakes With Ideas."*

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