CONNECTIONS

The EERI Oral History Series

Edward L. Wilson

Robert Reitherman, *Interviewer*

with an Appendix on

Ray W. Clough

Stanley Scott, *Interviewer*

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Earthquake Engineering Research Institute

Editor: Pam McElroy
Composition: George Mattingly, Berkeley, California, www.mattinglydesign.com
Book design: Laura Moger, Moorpark, California, www.lauramoger.com

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Tel: (510) 451-0905 Fax: (510) 451-5411
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The EERI Oral History Series

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

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

The EERI oral history program was initiated by Stanley Scott (1921-2002). The first nine volumes were published during his lifetime, and manuscripts and interview transcripts he left to EERI are resulting in the publication of other volumes for which he is being posthumously credited. In addition, the Oral History Committee is including further interviewees within the program's scope, following the Committee's charge to include subjects who: 1) have made an outstanding career-long contribution to earthquake engineering; 2) have valuable first-person accounts to offer concerning the history of earthquake engineering; and 3) whose backgrounds, considering the series as a whole, appropriately span the various disciplines that are included in the field of earthquake engineering. Scott's work, which he began in 1984, summed to hundreds of hours of taped interview sessions and thousands of pages of transcripts. Were it not for him, valuable facts and recollections would already have been lost.

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

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

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

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

EERI Oral History Committee

Robert Reitherman, Chair
Thalia Anagnos
Roger Borcherdt
Gregg Brandow
Robert Hanson
Laurie Johnson
Loring A. Wylie, Jr.

Foreword

My interviews with Ed Wilson began in March 2014 and extended to March 2016, all of which were held in his study in his home in El Cerrito, California. The conversation with Ed Wilson and Ashraf Habibullah occurred at Ashraf's Computers and Structures Inc. office in Walnut Creek, California, June 26, 2015.

Oral History Committee members Roger Borcherdt and Loring Wyllie reviewed the manuscript. Pam McElroy edited and indexed the volume, and the page layout was accomplished by George Mattingly. Stephen LaBounty, EERI Manager of Membership and Communications, shepherded this volume through to completion.

Robert Reitherman
EERI Oral History Committee
June 2016

Personal Introduction

It isn't a stretch to say that Professor Edward L. Wilson significantly altered the course of our engineering profession. His contributions to the industry—and thereby to humanity—are so great that it's almost impossible to fathom the engineering world without the impact of his work.

What's remarkable about Professor Wilson is not only that his work proved to be groundbreaking and industry-changing, but that he shared his work freely with anyone who wanted it. This generosity is not just an element of his personal character, it is a professional trait that has become the foundation of his legacy. His core belief that his work should be shared is unwavering and resolute. His commitment to sharing it ensured that it proliferated rapidly and widely in immeasurable ways, not just throughout the structural engineering profession, but across many other disciplines as well.

The Finite Element Method, for example, which Edward Wilson was instrumental in developing with his mentor Ray Clough, changed the course of engineering analysis methods in structural engineering, mechanical engineering, automotive engineering, aerospace engineering, and geotechnical engineering. There is hardly a remote corner of engineering that hasn't benefited from his work, and the practical impacts are far-reaching beyond the earthquake engineering applications familiar to EERI members: from the design of the Apollo Command Module, which carried humans to the moon and back, to the design of artificial limbs—the world is unquestionably a better place because of him. Even the humble potato chip, whose grooves can be designed for maximum crunch using the Finite Element Method, is better off.

Professor Wilson is, of course, the original developer of SAP2000 and ETABS, the bedrock software products that my firm now licenses to engineers in more than 160 countries. His pioneering work in numerical methods on the relatively inexpensive personal computer made earthquake engineering computer technology readily available for the first time to all engineers, including many in developing nations that previously did not have access to this sophisticated technology, allowing engineers throughout the world to build safer structures that preserve life and property.

When I met Professor Wilson more than 40 years ago, I could not have known how his work would change the world, or even my own life. Had it not been for him, I would not have gotten my first job. I would not have learned computer programming, nor would I have stumbled upon the inspiration to found Computers & Structures, Inc. Had it not been for his generosity, SAP2000 and ETABS might not even exist today, and my own life most certainly would bear no resemblance to the life I lead now.

Edward L. Wilson has been my friend and mentor for nearly half a century. He is a bright, beaming star of our profession, a true visionary, and a guiding light for us all.

Ashraf Habibullah,
May, 2016

CONNECTIONS

The EERI Oral History Series

Edward L. Wilson

Chapter 1

Growing Up Near Ferndale

We got our water from a well. We had a windmill to pump the water. If there was no wind, we climbed up the tower and made it spin by hand.

My First Seven Years

Reitherman: Begin with your early years and say a few words about your parents and relatives.

Wilson: I was born in September of 1931 in a small dairy ranch house, located on Ambrosini Lane, within walking distance from downtown Ferndale, California, near the coast. It is approximately 20 miles southwest of Eureka, the capital of Humboldt County.

Reitherman: For the benefit of the readers, let's add that Eureka is about a 300-mile drive north of San Francisco on the freeway that exists now.

Wilson: Ferndale is the most westerly city in the first 48 states and is a very active earthquake area. There is little population in that region, which makes its numerous earthquakes less newsworthy, but it is one of the more active seismic regions of the world.

On a seismological map, you can see it is the region where the San Andreas Fault makes a bend to extend out into the Pacific.[1]

My father, James C. Wilson, was born in 1882 in San Francisco and worked as a carpenter with his father. My Mother, Josephine J. Christen, was born in 1892 in Petrolia, in Humboldt County, the oldest daughter of Edward Christen, (1860–1940) and Maria Regli (1866–1912). When Jo was four years old, the Christen family moved from Petrolia to Pleasant Point near where Fernbridge would be constructed in 1910. My father was the only child and my mother was the oldest daughter of 15 children. I was the youngest of seven children of Jim and Jo Wilson. Also, I was one of 59 grandchildren of my grandparents Edward and Maria. All the grandchildren were born in the area where the Eel River comes out to the ocean.

The house I was born in had a living room, because every house had a living room in the front of the house, but we spent our time in the kitchen/dining room, because it was warm: Ferndale has a cool climate and is one of the foggiest places in the world. This was also where we had our only radio. We listened to *The Lone Ranger*, mysteries, and the news. My earliest memory is sitting under the kitchen table and looking out to see sixteen feet. We doubled up in the sleeping rooms. Being the youngest, I slept in the crib until I was five or six.

Reitherman: How did your parents meet?

Wilson In approximately 1908, the Humboldt County Board of Supervisors voted to fund the construction of a bridge across the Eel River, near Pleasant Point, for $225,000. The contractor brought all his crew, including my father, up from San Francisco by ship to work on the construction of Fernbridge. My father recalled being deathly seasick and vowed to never make another ocean trip. The construction of the bridge lasted from March 1910 to November 1911, only a little over a year and a half, even though it was a large structure. The construction crew then returned by ship to San Francisco. However, my father did not board the ship due to his seasickness. Also, during the construction on the bridge, my father made many local friends and decided to continue his carpentry work in the Ferndale area. He could build a building or water tank structure without needing to look at a plan. He built a loading dock that received very heavy loads, and through experience, chose the right size timber members. Since he had relatives in the San Francisco Bay Area, he did make three bicycle trips (three days and two nights each way) during the next five years. In 1916, he and my mother were married and they rented a small dairy ranch and started to raise the Wilson family. Over the next 13 years, the following children were born: Margaret 1918, Jimmy 1920, Alice 1922, George 1924, Blanche 1928, Wilfred (Bill) 1930, and myself, Edward (Ed) 1931.

At that time, Fernbridge was the longest poured-in-place concrete bridge in the world, a multiple arch bridge. It still holds that record. It has been through many earthquakes, but hasn't been severely damaged. The only

1 This region is known to geologists as the Mendocino Triple Junction, the intersection of three tectonic plates: the North American Plate and two plates lying under the sea, the Pacific Plate and the Gorda Plate.

damage it has suffered is from debris hitting it during a flood in 1955. The sides of the arches—as you can see from the exterior—are reinforced concrete, as is the bottom, but then they put gravel in the middle and built that gravel surface up as the side walls went up. The gravel loading has the effect of prestressing the bridge in compression. It helps keep the concrete from cracking, thus preventing deterioration. The bridge has no temperature expansion joints—in its mild coastal environment, they are not needed. Joints have proven to be a recurring problem with the earthquake performance of bridges. I've collected some information on the bridge on my website. As I get older, I try to put some of the interesting historical information I've collected on the Ferndale Bridge.[2]

Reitherman: You lived your first decade during the Great Depression. What was life like back then?

Wilson: Everybody in the family worked and helped out. From an early age, in my case four years old, children living on a family ranch like ours had work to do. My task at that age, for example, was to fill the wood box for the stove every day.

In 1937, we were living on the small dairy ranch at the end of Ambrosini Lane and paying $100 per month rent. At that point, we were unable to pay the rent because the price we received for our milk sold for less than the rent. The price of butterfat had dropped to five cents a pound. My oldest brother, Jimmy, dropped out of school to work on larger dairy ranches so he could help the rest of the family. My oldest sister, Margaret, after she graduated from high school in 1936, moved to Eureka to support herself by becoming a hairdresser. My dad continued to do carpentry work whenever possible. My mother, who was trained as a professional seamstress, earned a few more dollars by doing extra sewing work in addition to taking care of the family.

To make matters worse, in 1936 I had broken my right elbow by falling off of one of the ranch buildings. It was a compound fracture and required major surgery, which could not be done by the local doctor. After three days, a doctor took the train up from San Francisco to Eureka to operate on the arm. It was a serious injury, and when I entered the first grade the following year, I could only write with my left hand. I was naturally right handed, so it took me three years to complete the first two grades of grammar school. I still have a limited range of motion in my right arm, but it hasn't limited me in participating in work or my sporting activities, in particular my college sport of running on the track team.

The Depression affected different people differently. You have to recall that while unemployment reached a high level, most people still had steady jobs, and they weren't affected all that much.

Reitherman: What was daily life like, living on a dairy ranch?

Wilson: Besides working seven days a week? There were two types of dairy ranches in the Ferndale area, because automation was being introduced. The large ranches had several portable milking machines that could be moved

2 www.edwilson.org

from cow to cow; therefore, the milking was no longer a major time-consuming daily task. In addition, they replaced the work horses with powerful tractors and larger farm equipment. Our family operation just had a horse and wagon. To convert a small dairy ranch to a large ranch required a large amount of capital expenditures. Needless to say, the Wilson family did not have the money to buy such modern equipment.

The owner of a large ranch could run the ranch with one or two hired men. A hired man was not paid very much money. However, he was given free room and board seven days a week. On Sunday, the hired man had the day off, except for milking the cows night and morning. Therefore, a hired man could save money if he spent most of the time on the ranch.

My brother Jimmy worked as a hired man for many years after he dropped out of school. Also, my brother Bill worked over a year as a hired man after he graduated from high school in 1947 to save money to go to Humboldt State College. In the summer of 1949, I worked as a hired man at a large ranch, getting up at five o'clock and working three hours, then having breakfast, then going back to work. There were 105 cows to milk. There was always irrigation, hay to gather, a million things to do. We used to milk in the morning and again in the evening. Cows are so used to their daily cycle of eating, sleeping, milking, that when you change to daylight savings time, you don't change their schedule a whole hour all at once, you do it gradually.

Moving to Seaview Ranch

Wilson: In 1938, we moved to an abandoned 180-acre secluded ranch on a hill approximately one mile off Centerville Road and two miles from the ocean. The elderly owner of the ranch had moved into the town of Ferndale since he was no longer able to take care of the ranch or make any money operating it. My father made an agreement with the owner that he would buy the ranch by paying him $500 a year for the next five years. The ranch had been developed by a wealthy European gentleman in the late eighteen hundreds who named it Seaview Ranch since it had a great view of the Pacific Ocean.

I lived on the ranch during the next 10 years. In 1941, my sister Alice graduated from Ferndale High School and enrolled in a nursing school at Mercy Hospital in Sacramento. At that time, the nursing students worked in the hospital without pay in order to get room, board, and a nursing education. In 1942, George graduated from high school, was drafted into the military in January of 1943 and KIA (killed in action) in July of 1944. In 1946, Blanche graduated from high school and entered the nursing school at Mercy Hospital in Sacramento and became an RN in 1949. Bill graduated in 1947, worked as a hired man until 1948, and then entered Humboldt State College. Therefore, I lived alone on the ranch in 1947 and 1948 with my mother and father until we sold the ranch in 1948.

Reitherman: How did your brother George die?

Wilson: He volunteered for the Army Air Force (the "Air Force" wasn't a separate branch of the military until after World War II) and became a gunner on a B-17, flying bombing missions from England to Germany. On his seventh mission, the plane was hit by flack as they crossed into enemy territory. The plane attempted to return to England, but it exploded over the English Channel. The only survivor reported that three of the crew managed to bail out. The naval vessel sent to rescue them retrieved George's body from the sea. Needless to say, the Wilson family was devastated by the news.

Another setback to the family occurred in 1946. My father had a serious stroke. I saw him out in a field stumbling and falling. I went over to him and realized something was drastically wrong. I probably weighed all of 95 pounds but got him to the house. He was in a coma by the time the local doctor came to the house. He said that he could not help him. My sisters Margaret and Alice came home from Sacramento to help. After a week, he slowly came out of the coma; however, his left arm was completely paralyzed. A few days later, he asked me to cut him a walking stick that he used for the next fifteen years, until he died in 1961. As the only child at home, I had the pleasure of his company until I transferred to Berkeley in 1953. He needed help shaving and bathing. He continued to follow the news and enjoyed visits from his children and grandchildren.

Although it was the Depression, life was good living on the very hilly Seaview Ranch. We always had enough to eat during the depression, since we basically lived off the land. Most of my mother's brothers and sisters lived on ranches also. Therefore, we shared the fruit, berries, and vegetables we produced. Also, we all raised chickens, ducks, pigs, veal, and beef in addition to milk cows. And we were able to fish in the Eel River or the Pacific Ocean. On our hill ranch, I was able to kill a delicious cottontail rabbit with my 22-caliber rifle by walking a few hundred yards from our house. If we killed a deer, we would dry or can the meat. My older brother George and his friends were always hunting or fishing.

Reitherman: What was daily life like, living on a ranch?

Wilson: At the Seaview Ranch, we only had about 10 cows, and two of the boys were able to milk the cows by hand in less than an hour. Dad was almost able to support the family by his work as a carpenter, until his stroke. The ranch produced most of the food we needed. A significant part of our time was used to cut firewood for the cook stove and fireplace. The 180-acre ranch was approximately 75 percent trees, therefore, we cut only the trees that were near a road and close to the house so we could easily haul the wood by the car and trailer. Most of the trees were spruce or alder. My brother Bill and I could cut a cord of wood in less than two days.

Reitherman: I assume you used handsaws? This was before chainsaws?

Wilson: Yes, we would use a two-man saw, a "push-pull" saw, to fell a tree about two feet in diameter, then we'd saw it in four-foot lengths, then split each of those four-foot lengths so one person could carry that to the wood shed. We were always burning wood to keep warm, heat the water, and do the cooking. Years later, when I went back to visit my relatives there in Humboldt County, the first thing I noticed was the smell of wood, and I don't like it. I never noticed it growing up; it was always there. Of course, now we know that the smell of burning wood is a sign of the air pollution it causes. It's a pollutant. I also recall taking the Greyhound bus back to my relatives after I had moved away, and when I got off at Fernbridge I noticed the smell of cow droppings. I thought the whole place could blow up from all the methane!

Reitherman: Did the ranch have electricity?

Wilson: Yes. There was one wire coming to our house a long way from the road. We didn't even put lights in the barn because we didn't have enough wattage. We put pennies in the electrical box to bypass the fuses. After Jimmy gave my mother an electric washing machine, we had to turn off everything else to run it. We got our water from a well. We had a windmill to pump the water. If there was no wind, we climbed up the tower and made it spin by hand. The cook stove in the kitchen had water pipes going through it to heat the bathroom water. Up until I was a teenager, I took baths with my brother Bill because there wasn't much hot water. The bath water was used more than once. The stove was the center of the house, and we spent most of our time there in the kitchen.

Reitherman: How did you get back and forth to town?

Wilson: We had an old Ford Model T and later an old Model A. We walked three miles to school. Bill and I only had one bicycle, which was a Christmas present from our older brother Jimmy. In high school, we took the school bus.

Reitherman: I assume it was the same as today in elementary school, even up through high school: nobody taught engineering or even mentioned that word to students?

Wilson: Right, there was nothing like engineering that was taught. I liked math, but I didn't actually have much interest in school, because if ranching was your life, you didn't use an education for much. It was just assumed I would grow up working on a ranch or in construction. I got interested in construction early on, seeing my father work. And on a ranch, you build small buildings, fences.

At Ferndale High School, there were some very good shop classes in woodworking, welding, sheet metal, and drafting. I've always valued what I learned in shop classes and in my experience building things. I first began to learn about what I would later find out was called structural engineering and earthquake-resistant construction by working with my father as a carpenter's helper. He used to move wooden frame houses and had faith in their ability to undergo earthquake motions. The exception, he said, was if you had a brick chimney. That's still generally true.

During 1947 and 1948, when I was living on the ranch alone with my parents, we only had a few cattle and chickens, so I could easily take care of the ranch in a few hours a day. My dog Lindy was my constant companion, and I started running with him all over the ranch just for fun. In the spring of 1949, I won the 660-yard race at the Humboldt County high school track meet.

Reitherman: You and Lindy running together brings to mind the stories about the great runner Wes Santee, running with his dog across the plains of Kansas.

Wilson: I can tell you a story about running against Santee a little later.

In 1948, we sold the ranch and moved into my grandfather Edward Christen's old Victorian house in Grizzly Bluff, which had not been lived in since he died in 1940. My father, mother, and I lived there for a year.

Earthquakes in Humboldt County

Reitherman: Do you remember the first earthquake you experienced?

Wilson: We always had one or two small quakes every year. The largest one I remember was in 1948. I slept in the upper story of my grandfather's old Victorian house and I recall falling out of bed during the night. That morning, we were sitting at the breakfast table, and I asked, "Was there an earthquake last night?" And my parents, without taking any particular notice of it, said, yes, there was another one. They weren't unusual.

My parents lived through the 1906 earthquake. My mother was living in Pleasant Point in Humboldt County, my father in San Francisco. And they recall the shaking being about the same in both locations, which makes sense because the San Andreas ruptured for 300 miles, sending out its vibrations all along that length.

Reitherman: It seems like the '06 earthquake should really be called the Northern California Earthquake, not the San Francisco Earthquake, because it was such a long stretch of fault that ruptured. For example, the city hall in San Francisco had shaking and then fire damage, but the city hall in Santa Rosa over 50 miles to the north completely collapsed, caused only by the ground shaking.

Wilson: My father told me that the damage from the earthquake itself was worse in Santa Rosa than San Francisco. Of course, Santa Rosa was a smaller city, but proportionally it was worse.

Reitherman: Have there been any large earthquakes in Humboldt County recently?

Wilson: Yes, the Cape Mendocino earthquakes of 1992. There were three within an 18-hour period: a magnitude 7.2, 6.6, and 6.5.[3] One result, besides the shaking, which caused a lot of damage to the few buildings in that rural area, was a permanent uplift of the land of almost a meter and a half, almost five feet. In 2010, there was a magnitude 6.5 and in 2014 a 6.8 located offshore.

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3 The California Division of Mines and Geology (now Geological Survey) report on the strong motion records collected from the earthquakes notes: "The [mainshock] earthquake produced some of the highest accelerations ever recorded. The ground response station at Cape Mendocino station recorded a peak acceleration near 2 g." R. Darragh, T. Cao, C. Cramer, M. Huang, and A. Shakal, *Processed CSMIP Strong-Motion Data from the Cape Mendocino/Petrolia Earthquake of April 25, 1995: Release No. 1*, Report No. OSMS 92-12, California Division of Mines and Geology, Sacramento, CA, December 4, 1992.

Chapter 2

Moving to Sacramento

The Structural Engineering option, under Civil Engineering, sounded close to being a carpenter to me, so I started to major in that.

Wilson: When I was seventeen, in 1949, we moved from rural Ferndale to Sacramento. My three sisters, two of them nurses, were living in Sacramento, and my mother decided to move there so they could help take care of my father. Suddenly I was in the state capital, a big city, and I was overwhelmed at first.

My brother-in-law arranged to have me enrolled in Christian Brothers High School for my senior year. The academic program was the same degree of difficulty as Ferndale High School. I did very well in the physics class, which is the closest thing to engineering in high school. However, I was not able to make any close friends there.

We bought an old house in need of repair near 35th and H Streets, using the money from the sale of Seaview Ranch. The house had only two bedrooms—one for my mother and father and one for my sister Blanche, who worked as a nurse at nearby Mercy Hospital. I slept on a couch in the living room for the first few months.

Within a week, I cleaned up the yard, worked up the flowerbeds, and did a little painting. We didn't have a car, so I remodeled the garage into a room for myself, putting in a door, window, and insulation. The neighbors were impressed by my work and started to ask me to do yard work and small construction jobs. I worked very fast, did a good job, and charged $1.00 per hour—the same pay rate I received working on a ranch. Therefore, I had no problem earning spending money for myself. However, my parents, Blanche, and I did not have the combined money to buy and maintain a car. My other two sisters, Margaret and Alice, were always close by to help us with transportation problems. Therefore, the decision to move to Sacramento worked out very well for my family and me.

Starting to Take College Classes

Reitherman: You ended up at the University of California at Berkeley. Did you go straight from high school to there?

Wilson: No. When I graduated from high school in 1950, I had no intention of going to college, but I looked at the catalogue of Sacramento Junior College. The Structural Engineering option, under Civil Engineering, sounded close to being a carpenter to me, so I started to major in that. I didn't know what a civil engineer was. The only engineers I ever knew about growing up were driving trains. I took math, physics, chemistry, and engineering classes. In addition, I made many new friends in my engineering classes, who all planned to transfer to the University of California at Berkeley. Also, I started running seriously on the track team. In my sophomore year, I won all my 880-yard races including the state meet.

During the summers after my freshman and sophomore years, I worked at the American Can Company, which was only several blocks from our house. During one period of time, I worked 57 days straight. I was paid $1.45 per hour, with time and a half on Saturdays and double time on Sundays.

The machinery making the cans went at a constant rate, and I had to take the cans and put them in boxes on a pallet. It took me a few days to get fast enough to keep up, to fill a box in 40 seconds, but then I got efficient enough to do one box in five seconds, or ten seconds at a comfortable rate. I've always been obsessed with the most efficient way to do a job.

Reitherman: I had a similar summer job, with U.S. Gypsum, being the guy who stacked the cans of "mud" or gypsum board compound onto pallets. I had to work as fast as I could to get ahead of the flow so that I had time to tie a rope around the top row of cans so a fork lift could carry the pallet away. There really are some assembly line jobs that are like the one Charlie Chaplin had in "Modern Times." What did you do with the money?

Wilson: Other than living and school expenses, I saved my earnings, because I knew then that I wanted to transfer to U.C. Berkeley after my second year at Sacramento City College.

Reitherman: What sticks out in your memory about your engineering courses?

Wilson: The surveying and drafting courses were very good. I loved the physics lectures and laboratories, because we solved real problems using calculus and differential equations. I believe we had better teachers at Sacramento Junior College than the teaching assistants who taught these same courses at Cal. The teachers at Sacramento were devoted to just teaching, whereas, the teaching assistants at Cal were young and inexperienced.

Most of my friends at Sacramento Junior College did not have much money. We just shared expenses for gas and food. Therefore, we had to entertain ourselves with inexpensive activities. Almost every Saturday we would play tennis at the public courts. Also, playing pool or going bowling was not expensive. A few of us went on long weekend fishing trips to various streams in northern California, which some of us kept doing after we transferred to U.C. Berkeley.

Running on the Track Team

Wilson: In a physical education class in the spring of 1951, we all ran a one-mile race, and I won easily. I had engineering laboratory classes four days a week, so I couldn't join the team in all their workouts, but I ran on my own and competed in a few meets. The next year, I trained by myself, running with a stop watch in my hand, again having engineering labs in the afternoons. One of the lifelong friends I met was another runner, Al Baeta. The event I ended up specializing in was the 880-yard race, which is a half mile, or about 800 meters. When I left Sacramento City College to transfer to U.C. Berkeley, I had some additional chances to see how fast I could run.

Chapter 3

An Undergraduate Civil Engineering Student at Berkeley

I didn't take an earthquake engineering course because none was given.

Wilson: Now we're up to the point where I entered Cal. In January 1953, I transferred to U.C. Berkeley as a junior, majoring in civil engineering. The next two years were one of the most enjoyable periods in my life. It was inexpensive to have our meals at a student cooperative facility nearby. Most of the other students living in the house were engineering students from Sacramento and were in a position to give me good advice. I could live for a little under $100 a month including room, food, and books. Also, I eventually had a job getting $1.19 an hour as a grader of papers. I got scared if my bank account got down to $200 or $300, anticipating room and board and other expenses. I didn't want to live without having any cash, the way my parents were during the depression. I'd tell myself, "I'll go without." The difference between wanting and needing was set in my character from an early age.

Reitherman: What courses did you take?

Wilson: The basic mechanics course, using Professor Egor Popov's book on the mechanics of materials, was the most important. It was the prerequisite for all the other structural engineering courses. The structural analysis course from Professor Eberhart was important and very entertaining. However, the concrete design lecture and laboratory courses given by Professor Polivka proved to be of immediate value the next summer when I had a job as a concrete inspector on the construction of a bridge. After I took a course called Aircraft Structures from Professor Ray Clough, I knew I wanted to know more about structural engineering and structural mechanics and started to think of graduate school.

Along with civil engineering courses, a course on electrical engineering on power generation was required. A course on engineering economics taught me the basic principles of investing that I still use today. I recall enjoying reading Alexis de Toqueville's *Democracy in America* in a political science class, and I have used several quotes from him ever since. My wife Diane and I took the same course at Cal, a few years apart using that book, and we still have it on the shelf here at home. Have you read it?

Reitherman: Yeah, still a classic. A combination of political science theory and sociological observation. He was interested not only in the form of government in the United States but also the characteristics of the population behind that institutional layer.

Wilson: I have re-read it from time to time. He was impressed with the local American social systems, mostly rural ones, where neighbors helped neighbors, and he was distrustful of big governmental systems. The French sometimes have a reputation for being rude to foreigners, but at that time in the 1800s, they admired the United States.

Reitherman: Let me ask you a leading question, because I think I know the answer. In all of your undergraduate years at Berkeley, did you ever take a course on earthquake engineering?

Wilson: I didn't take an earthquake engineering course because none was given. And in fact, in the bridge design course, I still have a copy of the 1954 AASHO[4] highway bridge design specification handbook and the word "earthquake" is not mentioned. According to George Housner's EERI oral history,[5] Professor Romero R. R. Martel at Caltech was giving courses on earthquake-resistant design at Caltech in 1934. At that time, it appears Caltech was about the only academic institution in the USA to theoretically address this problem. Prior to this point in time, Japan and Italy were working in this area.

Reitherman: What engineering buildings were on the Berkeley campus then?

Wilson: In 1953, Etcheverry Hall did not exist. The civil and mechanical engineering

4 AASHO, American Association of State Highway Officials, later became AASHTO, American Association of State Highway and Transportation Officials.

5 *George W. Housner*, Stanley Scott, interviewer, Earthquake Engineering Research Institute, Oakland, CA, 1997, p. 5.

groups were in several temporary buildings: the Engineering Building, Engineering Material Laboratory, and a Hydraulics Laboratory where Moffitt Library is now. The hydraulics laboratory was converted from an old brick building built prior to 1900. I believe it was initially the Men's Gym. The new Hydraulics Laboratory is now O'Brian Hall. Cory Hall had just been completed or was under construction. The Engineering Material Laboratory housed the big testing machine.

Reitherman: You mean the three-story-high monster nutcracker that was later moved out to the Richmond Field Station?[6]

Wilson: Yes, it was on the campus until Davis Hall was expanded in the mid-1950s. That was the era when the College of Engineering was established at Berkeley. As an undergraduate, there was instead a Department of Engineering, and when I began graduate school there in 1957, a Department of Civil Engineering had been created within the College of Engineering.

By the time I came to teach at Cal in 1965, the three main departments in the engineering college were civil, mechanical, and electrical. Within each department, there were further changes, such as the unification of sanitary and hydraulic engineering to be the "environmental" part of civil and environmental engineering.

6 The testing machine that has a capacity of four million pounds in compression and three million in tension.

Working Summers for the State Bridge Department

Reitherman: During your upper class years at U.C. Berkeley, you worked summers at the state's Bridge Department in what is now the California Department of Transportation, Caltrans. What was that like?

Wilson: I was a field engineer. My first summer, they shipped me up to Fort Bragg, back up in northern California where I was from. They had just finished a steel bridge at Noyo, and the next big bridge across the coastal river valleys was the Ten Mile River Bridge. It was at the stage of driving piles, the concrete mix plant having just been completed. The resident engineer, George Hood, later a department head at Caltrans, was my boss. In that summer job on that highway bridge, I was given a lot of work responsibilities, because I did them, and because I did them fast and accurately. I could set up and level a transit in less than a minute. I inspected pile driving, took materials samples of the concrete. We only had to get 3,000 psi in 28 days, but we were getting 5,500.

I recall one confrontation with the contractor. I inspected the reinforcement in a large column and found about two inches of sawdust and wood fragments at the foundation level of the column. I told the contractor to make sure that all the wood material was washed out prior to the column being poured the next morning. At eight the next morning, I was on the construction site and asked if the foundation of the column had been cleaned. He replied that it had been cleaned as I directed. Since the column was over 30 feet long, I could not see the base from the top. Therefore, I climbed down the reinforcement to the base and saw it was the same as the previous day. I climbed back up and told the contractor to "clean it." They had to stop the job and send one man down to clean the foundation. It took about 45 minutes to clean the base to my satisfaction. In the meantime, the concrete mix plant had to be closed down and approximately 10 men had to be paid for doing nothing. The contractor did what I told him to do the rest of the summer.

In the summer of 1954, I was assigned as a field engineer for the construction of a skewed Highway 4 overpass structure near the town of Pittsburg, in the delta where the San Joaquin and Sacramento Rivers join. By that time, I had purchased a very old and used 1941 Chevy for $150. I drove from Sacramento to the construction site in less than two hours and met the resident engineer, who was just a few years older than I was and had graduated from the Polytechnic Institute of Brooklyn, now part of New York University. The two concrete abutments of the bridge were poured. The resident engineer said they were planning to pour the main beam at the center of the freeway, which was on the top of three previously poured columns. We walked over to the beam to check the steel reinforcement. The resident engineer looked at the reinforcement and said that the beam would be poured the next morning. I looked at the reinforcement and said "it looks like the contractor had forgotten to put a layer of reinforcement at the top of the beam above the columns." The resident engineer said "the contractor is very good and I do not believe he would make a mistake—let us go over and check the construction drawings." The drawing clearly showed that a layer of steel was required. The resident engineer then informed the contractor they would not be able to pour the beam until the problem was corrected. I thought, "Professor Polivka would be proud of me" because he always emphasized the need of extra reinforcement at the top of the continuous beam above all columns, where you have a hogging or negative moment. This was a serious error. Without the added reinforcement, the beam would have definitely cracked above the center column. I did most of the theoretical work in checking the strength of the timber concrete forms.

Reitherman: I'm guessing we'll get to the topic of the new East Span of the San Francisco-Oakland Bay Bridge, because I know you may have something to say about that.

Wilson: Oh, yes, we'll have to talk a bit about that. What a boondoggle, so much politics, the architects, the overruns, the delays, the choice of structural systems. You know, they built the Empire State Building in eighteen months, in 1931. It's unbelievable—not how short that was, but how long we have gotten used to. At Caltrans, if you said there was a little research needed, they would assume it would be a year or more, when what I had in mind was an afternoon.

Reitherman: We can also touch on your consulting work on some of the major bridges in the region, such as the Richmond-San Rafael and Golden Gate Bridges, as well as the new section of the Bay Bridge.

Taking My First Class from Ray Clough

Reitherman: Talk a little more about your professors at Berkeley.

Wilson: When I got to the University of California at Berkeley, the most impressive professor I had was Ray Clough. In fact, I went there in 1952 to get advice on what to take in my junior and senior years, when I was transferring from Sacramento. I took the Greyhound bus from Sacramento for the appointment. I had tentatively selected hydraulics, but Ray said, "I'm structures, not hydraulics, but I can advise you anyway." A lot of professors would have just shunted me off. He was my advisor for my junior and senior years, though I only took one course from him. It was called Aircraft Structures.

Reitherman: Later on we can get to the story about Clough doing research at Boeing during summers, work that led to the Finite Element Method.

Wilson: Clough's aircraft structures course was fascinating. There is no question that his interests and my interests were closely aligned, although we have our differences in approach. He never gave his personal opinion of the best method to use, whereas I'm quick to state those judgments. He had a very simple and direct way of doing things that I admire in engineering. With almost all his writings, the first draft was the final draft. He had that ability. I saw him think about a paper for a while, write an outline, and then write the paper.

Reitherman: When you say "writing," was that handwriting and having a typist type it up?

Wilson: He could type, but usually there was a typist.

Reitherman: Was dynamics introduced in the aircraft structures course?

Wilson: Not much, except for impact factors upon landing, things like that. We did take a dynamics course along with a statics course as a civil engineering major, but it was taught by mechanical engineers and was mostly about rigid bodies, parts of machines that moved and interacted along fixed paths. To a structural engineer, dynamics has to provide answers for what the forces are within a structure's elements and account for the deformations.

I was later hired as a reader for the aircraft structures course when it was taught by another faculty member who had wartime airplane design experience. He taught only about one-third of what Ray had covered. Ray had the ability to cover a lot of material. He tended to teach for the best students in the class, rather than trying to win a popularity contest. I don't think he ever got an honor as a teacher, though he was a great one.

Reitherman: Another question about Clough's aircraft structures class: was inelasticity a topic that was covered?

Wilson: No. Generally, an aircraft structure fails by fatigue, but up until that point it behaves linearly. The aeronautics industry had come up with simplified methods for the computations.

Reitherman: Computations were done on the slide rule at that time?

Wilson: Yes. There were a few mechanical calculators available on campus, but the slide rule was the dominant tool. When I came back from Korea in January of 1957, that's when the digital computer started to become available, and within a year of that, I was adept at programming on the high-speed computers as they came along. I pretty much skipped the phase of using the hand-held electronic calculator.

As a student, I didn't take many notes but read the textbooks thoroughly. Sometimes I would end up with a *Daily Cal* newspaper at a lecture and write down the homework assignment on a corner of it. I lived a block from campus on the north side, so I would walk home after an eight o'clock class and do the homework—no wasted time.

Reitherman: One follow-up to the topic of inelasticity. Sometimes even engineers who should know better use the terms inelastic and nonlinear interchangeably. Do you have a short explanation of the difference for the general reader?

Wilson: Take this rubber band [picking one up off his desk at his home]. I pull on it, and as it stretches, it gets stiffer. A lot of force is needed to stretch it another fraction of an inch after it's stretched tight, whereas a small amount of force was needed to get that same amount of deformation when it wasn't so stretched. The force-deformation relationship, as you would see it on a graph, isn't linear. But when we remove the force, the rubber band returns to its original size and shape, because the material remained elastic. The rubber band is an example of a nonlinear elastic structural system.

Another example engineering professors have been using for years is a paper clip. If I take a straight length of it and bend it to a point where the material does not yield, it will return to its original position. This is a linear elastic structural system. However, if we apply a force large enough to yield the material and there is a permanent displacement, we have a nonlinear structural system because of the yielding or inelastic behavior of the material. A base isolator, made of layers of rubber and thin metal, is an example of a nonlinear elastic structural system: its stiffness changes as it deforms, it behaves nonlinearly, but after the load is removed it returns elastically to its initial displacement.

Running on the Cal Track Team

Reitherman: Did you continue your running activities at Cal?

Wilson: Yes. When I transferred to Cal in January 1953, I started training as I did in Junior College—between classes and after five o'clock when my afternoon laboratory classes ended. Apparently, Coach Brutus Hamilton assumed I was not taking my running seriously. When I did run with the other 880-yard runners at three o'clock in the afternoon, I clearly demonstrated that I was faster than all but one of the other runners, the great Lon Spurrier. Another reason Coach Brutus didn't enter me in any meets was probably to save my eligibility for my senior year.

In 1954, my senior year, I had only one Friday afternoon laboratory bridge design class; therefore, I was able to work out at three o'clock in the afternoon four days a week. I ran in all the meets that year, won the 880 in the Stanford meet and set a record in the UCLA meet. In addition, I ran in the 4-by-400 relay team in every meet—each of the four runners having a 400-yard or quarter-mile segment to run. In the Los Angeles Coliseum Relays, we broke a world's record in the 4-by-880 yard relay; however, we were in second place. My best running time in 1954 for the 880-yard event was 1 minute and 51.5 seconds and in the 440-yard event my best time was 47.5 seconds.

The popularity of track in the United States has declined over the last 60 years. This year, 2014, I went to a track meet at Berkeley. Unfortunately, there were very few spectators in the stands. I was pleased to find that my best times 60 years ago were close to the winning times at the meet. A few of my teammates from the 1950s teams were there and we had a good time watching the young men and women of today participate and enjoying the great sport of track and field.

I recall a meet at Berkeley in April of 1954 when Wes Santee competed.

Reitherman: The miler from Kansas? He came close to being the one to break the four-minute-mile barrier, didn't he?

Wilson: Yes, he was the American hope to win the four-minute mile record. In this meet at Berkeley in 1954, Santee cruised to a win in the mile, forty-five minutes later won the 880, and then forty-five minutes after that was on the winning University of Kansas mile relay team. He may have given up a chance to run a four-minute mile that day by instead scoring so many points for the Kansas team in different events. A month later, Roger Bannister was the first to break through the four-minute mile barrier in England.

A week after our meet in April, the Berkeley team traveled to Los Angeles to compete with UCLA. It was my first ride in an airplane. I finished first in the 880 in one minute and fifty-four seconds. The finish was rather memorable. The finish line tape was held at one end by Robert Gordon Sproul, president of the University of California. For some reason, he held on tight to the tape, and as it ripped across my neck as I crossed the line, I stumbled and sprained my ankle. Lon Spurrier ran to my side and I said "Who was that son of a bitch who didn't let go of the tape?" and Lon just said "be quiet, it was President Sproul."

At the end of the fall semester of 1954, I was going to complete all my requirements for the BS degree in civil engineering. Therefore, I had to make a decision to either leave the academic life and take a job as a structural engineer or to continue my education and work for the next degree, the MS. During the previous four years, I paid for all my living and education expenses without a scholarship or loan. In fact, I had been able to buy an old, reliable car and I still had saved enough money to pay for graduate school. In the spring semester, 1954, I was academically in the upper twenty percent of the senior students in the Department of Engineering and was awarded membership in the Tau Beta Pi honor fraternity. Therefore, I anticipated having no problem being accepted in the graduate program at Berkeley. However, the main reason I decided to go to graduate school is that I was having a great time as a student athlete at U.C. Berkeley.

I made an appointment with Professor Howard Eberhart, the only graduate advisor for the Structural Engineering Graduate Program. He looked at me very sternly and said “you have been in school for over sixteen years, don’t you think it is time for you to leave the university and go out and get a productive job?” I smiled and said “I have a lot more to learn, and if I don’t do it now, I may never come back.” He smiled and said “you are absolutely right.” He then offered me a teaching assistant position, which meant my salary would pay most of my expenses. Looking forward to the spring semester of 1955, life looked very good for Ed Wilson.

Chapter 4

In the Army in Korea

When you're young, you look at the military more as an adventure rather than as something that can get you killed, but your mother looks at it differently.

Drafted into the Army

Wilson: As it happened, serving in the military in Korea was a two-year interval between my undergraduate and graduate studies.

The Korean War started in June of 1950, almost on the date of my high school graduation, when I was eighteen. Within 30 days of turning 18, you were supposed to register with the local draft board. I don't know if they still have something like that today. Do they?

Reitherman: The "draft" per se isn't in effect now, but the Selective Service System has never been changed. Everyone turning 18, i.e., every male turning 18, has to register for the draft. There's a national database of all the men, and if you don't register, they can find that out and prevent you from being employed by the federal government. When my two sons registered a few years ago, as I did a generation before, the signs that are still in all the post offices noted that the potential penalty for not registering is similar to getting caught robbing a bank: a $250,000 fine and up to six years in prison. But as I say, while there is mandatory registration, no one has gotten drafted since the 1970s.

Wilson: So, it's still like a social security number that follows you through life.

Reitherman: In 1950, the draft was a huge issue for young men, as it was during the Vietnam War. The Korean "conflict" was a full-scale war. What was it like for you?

Wilson: You could either get drafted or enlist. My brother Bill enlisted. In fact, one of my sisters, Blanche, also enlisted. They were both in the Air Force. I ended up being drafted just as I was completing my senior year in college. I got my notice in late 1954 and reported in January 1955. My two years of service were basically 1955 and 1956. I took the physical exam, and because of my right arm—broken in a farm accident when I was a kid and it never straightened and healed properly—I was a 3C. It turned out I should have been 4F, unfit for service, because of the limited mobility and range of motion of my arm, but I was classified as a 3C. I had a year's deferment, going to junior college. Every year I got a draft notice and responded to them, and depending on how many men they needed to mobilize into the armed forces, they kept metering out the induction notices. I got called up, as I said, getting inducted into the Army in 1955, hurrying to get paperwork done in order to complete my undergraduate degree from the University of California. When I would have been receiving my diploma in a June ceremony that year, instead I was in a U.S. Army uniform as a private.

My mother of course was greatly worried, after having lost her son George in World War II, and now her son Bill was flying in a B-26 in Korea. When you're young, you look at the military more as an adventure rather than as something that can get you killed, but your mother looks at it differently.

I did my basic training at Fort Ord on the central California coast, choosing to get drafted and serve for two years, rather than enlisting for three years. When I got my physical, the doctor looked at my arm and said, "What are you doing here? You're 4F. You don't have to go in the Army." I had 30 seconds to make a decision. I had said good-bye to all my friends and family and had been wished well. I had sold my car. I had friends in the military. So I said, "I'll go." One benefit of my decision was that when I got out of the military, I had GI benefits to help pay for graduate school. However, if I had worked two years as an engineer in California I would have made several times as much money as a private in the Army.

The worst part of my four months at Fort Ord was catching pneumonia. When we were classified for assignments, I of course wanted to go into the Corps of Engineers, because I had engineering and construction experience. But the man classifying me totally ignored that, and he was about to put me in the infantry until he saw I was 3C, and then apparently he looked down the list and said I would be a radio repairman. So off I went to Fort Knox, Kentucky, to learn about radios.

I recall arriving there the first Saturday in May—the Kentucky Derby. The plane flew over the Derby and I could see the stands filling up.

Reitherman: What was radio repair school like?

Wilson: The civilian teachers and the manuals were excellent. Also, we had lectures and laboratories 40 hours a week with no homework. It surprised me that they could take a seventeen-year-old and make a pretty good radio repairman out of him. But I found that you can't make everybody shoot a rifle straight.

I pulled a shooting range detail to get the ordinary Army office workers to qualify for their annual rifle rating. With all my hunting experience on the ranch, I found it strange that a master sergeant didn't know how to adjust the sight. If you move the back sight up, you're going to shoot higher. You move the back sight the way you want to correct–lower it to shoot lower, higher to shoot higher. Every ten-year-old kid in Ferndale knew how to do that. I was down in the pits, behind the target. After somebody fired, you put up a small black marker showing where the shot went. If they didn't hit the target at all you waved a big red flag, called Maggie's drawers. I got a call that a general was trying to help someone hit the target, and we, the ones marking the target, kept getting chewed out because the guy completely missed the target. The guy must have shut his eyes. I don't know how he could have shot so badly. That was the first time I got chewed out by a general. In the military, you lost all your usual American civil liberties. You were under military law. A commanding officer could put you in the stockade for weeks or months just on his say-so.

It was announced that they were going to have a track meet for any of the men who wanted to run. I won the half mile, also the quarter mile, and the 220-yard distance. So I qualified for the next meet at a higher level. I was in the top ten half-milers in the nation. Then I was ordered back to Fort Knox, where a colonel told me I could apply for the all-Army track meet. He also said that if I didn't make it and go on to higher levels, he would have me come back and go to tank driving school to drive a tank. I knew there were at least five people in the nation who had better times, so I said I would just go on to do radio repair.

Reitherman: You had to make a lot of hard decisions as a young man because of the war.

Going to Korea

Wilson: We took a trip by train to Fort Lewis near Seattle, then embarked on a ship for Korea in September of 1955. The Korean War technically ended in 1953, though the two sides have been armed on the two sides of the armistice line ever since and there have been combat incidents. It was Saturday, September 4, the day before my twenty-fourth birthday.

About six the next morning, somebody came into the barracks and touched me on the shoulder and said come with me, you have KP (kitchen duty) for the day, and he took my ID dog tags. You couldn't do anything without them. It didn't occur to me until about noon when I was peeling potatoes that this was my birthday. I recall thinking that the Army wasn't making the best use of my talents.

Reitherman: What was it like going across the Pacific on the ship?

Wilson: It was a typical troop ship. It had four or five bunks high, with a total of about 600 men. I had never been on a big ship on the ocean before, but I loved it. They had a good library on the ship, and I read about a book a day. I enjoyed myself.

We landed at Yokohama and then went on to Korea. The voyage over and the voyage back, with some tourism in Japan, were the best parts of my two years in the Army.

In Korea, at Inchon, we got loaded into landing barges because there wasn't really a decent port. Then we were taken to Camp Casey, relatively close to the 38th latitude parallel, which was the prewar boundary between North and South Korea. At this time, the Korean War was officially over, at least there was an armistice, with a demilitarized zone about three miles wide at the cease fire line in 1953, where it still is today. That's where I started repairing radios that were used in tanks, jeeps, and trucks. The walkie-talkies had batteries that were about this big [gesturing with his hands].

Reitherman: About as big as one of those plastic water bottles, about a liter in size.

Wilson: At that time we were buying a lot of goods from Japan, to boost their economy. We would get cases of twelve of these big batteries from Japan, and less than half would work. The Japanese had a lot to learn about quality control, which they did, a few years later.

After a couple of months, it was recognized that I could write and organize things. There was a horrible problem of keeping track of the inventory. I was eventually sent to Inchon at the central Signal Depot facility to clean up all the record-keeping. Engineers are born to solve problems. It was enjoyable work to solve the problems, even though it was tedious.

Reitherman: What were the living conditions like?

Wilson: The winters in Korea are cold, but we had good equipment, like thermally insulated boots and hooded parkas, so we never really got cold. I remember walking guard on New Year's Eve—that's the kind of duty a private gets. There was a little shack with a small stove. I would take a canteen cup and make hot tea on the stove, walk the post for five minutes, then come back and drink the tea. That night I made the tea, and when I came back it was frozen solid.

Reitherman: What about the barracks?

Wilson: We had the typical four- or five-man tent, or a larger tent with a wooden floor and a diesel oil stove. Later, when I was assigned to the Signal Depot in Inchon, we lived in Quonset huts.

Reitherman: Was the scene the way the 1970s *M*A*S*H* television show depicted it?

Wilson: Yes, they showed it the way it was. There were jeeps running all over the place, and a few helicopters. Everybody wore fatigues. Alan Alda, who played in that TV show, apparently looked like me when the show was running because people would tell me that.

I recall a senseless inspection we had when General Paul Caraway visited. It was about 15 degrees Fahrenheit. We had to wait in our ordinary uniforms, not weather gear, for about two hours. I thought, "can't you organize these things better," but in retrospect, I think these things are part of the military to demonstrate that they have control over you, so when they say, "take that hill" you'll do it. He walked along inspecting uniforms and weapons, and when he got to me—I'm about six feet two inches tall and he was a short man—I looked down, looked him right in the eye. His aide-de-camp came up to me and chewed me out (we had to have our eyes looking straight forward). That gave me a sense of the power of the military, but also a sense of power over them. That was the second time I got disciplined for irritating a general.

A few months later, I was pulling guard duty at the gate, and General Caraway drove by the road outside the camp. I hadn't had much sleep, and I didn't salute when his vehicle drove by with the general's flags on it. So I got chewed out again. Combined with my experience on the shooting range at Fort Ord, that made three times I was chewed out by a general, a fact I am proud of. If it was a shooting-war environment, I would have had a different attitude, but the war had already reached its stalemate.

After close to two years, somebody said that there was a regulation saying I shouldn't still be a buck private, the lowest possible. I wasn't even a private first class. Running the radio inventory, not having anything against me on the record, they had to give me my PFC stripe—but I never sewed it on.

Reitherman: What about the food?

Wilson: The food wasn't bad, but every once in a while they would have us use up World War II C ratios. About the only thing we had to drink when we went through the chow line was strong black coffee, without sugar or cream, which was never my "cup of tea." I still drink tea, not coffee.

When I was at Inchon, next to the Signal Depot was the Quartermaster Corps, which distributed the food. There were filet mignon steaks. But I saw that the higher up you went in the military, the more they took care of their friends. The men on the front lines didn't get the steaks.

We had an NCO, non-commissioned officer bar. It was a tent. We even had the Kim Sisters perform for us, who later became famous. The drinks were cheap. The end of the month they had drinks for a nickel.

I also was eligible for a one-week rest and recuperation leave in Japan, and I went twice. Even though it was ten years after the Second World War, the spending of the American GIs was a big source of income in Japan. I went with another soldier and stayed in a Japanese inn where he had stayed before. You could get a room and negotiate for a companion for a week. We slept on mats on the floor. When I was there the cherry blossoms were in bloom, and there were lots of things to see and cultural events to attend. I remember that on my second trip, we stayed in the Imperial Hotel in Tokyo for only a few dollars.

Reitherman: Wow, the original Imperial Hotel. I've been to the large fragment of it that they have saved and installed out in Meiji Mura when Frank Lloyd Wright's hotel was torn down in 1968, but that's all. What was it like?

Wilson: It was a little disappointing. The rooms were small. I took the train to see Mount Fuji and stayed in a nice hotel for a dollar a day, and I played on the golf course there for almost nothing. Now I think it costs $1,000. But that was the last time I played golf, and I think I'm going to keep that as my record.

Reitherman: Being in the earthquake engineering field later on, you must have been back to Japan a few times.

Wilson: Oh, yes, in the 1970s and 1980s, to give lectures and courses. And also to China in the 1980s, seeing the Great Wall, the buried Terracotta Army in Xian.

Reitherman: Back in Korea, what else did you do for a pastime?

Wilson: In Inchon, we could get books ordered through the library. I read a lot of historical books.

Reitherman: What kind of history?

Wilson: I read about Genghis Khan, European history, philosophy. I remember reading several volumes of a set of books by Will Durant.

Reitherman: Me too, scholarly but very readable work. Will and his wife Ariel Durant produced that set together. Like de Toqueville, books you still enjoy?

Wilson: Yes. I've got the complete set here in the house now. I think they wrote more volumes after I got back from Korea and I bought the set. Every once in a while I look something up in them.

Reitherman: To summarize, other than learning some electronics and how radios worked—let's call that electrical engineering—you learned zero engineering all through your Army years?

Wilson: Right. Near the end of my time when I had about two months left in the Army, I wrote my mother and had her send me two books. One was an advanced book of calculus, and the other one was an advanced book on differential equations. I wanted to brush up.

On the ship coming home, lying on the bunk, I worked every problem in the books and got my mind back into the physics and mathematics underlying engineering. Before I got out of the military, I knew I had employment as a graduate student when I got back to Berkeley working on a research project, on alternative designs for the Oroville Dam.

There's one last important point I would like to make about this period in my life. When we got off the ship in Seattle, the troops that were to be discharged at Fort Ord were loaded directly onto a train car at the end of a passenger train that would drop us off at Fort Ord on its way to Los Angeles. I decided to go up to the Club Car and have a martini to celebrate my return to the good old USA. When I walked toward the front of the car, the sergeant in charge of our group announced we were not to leave our car and mingle with the civilian passengers, and that we would eat as a group after the others had completed their meals. Since that time, I have talked to many other Korean veterans who had the same type of welcome home. I had a cousin who was a medic in the Korean War. He was right at the front and went through it all up close. He couldn't believe it when he got off the ship in Seattle—in the middle of this war—and there were only about half a dozen people there to greet them. There's a reason why it is called the "forgotten war."

Chapter 5

Working for a Master of Science Degree

I told Professor Eberhart, "I have no plans to use a digital computer." He replied, "That is right; if you don't know how to use a computer, you will never use one."

Reitherman: Pick up the story when you returned from Korea in January of 1957 to do your graduate work at the University of California, Berkeley.

Wilson: When I started working for my MS degree, I did not need financial aid. There was no tuition required by Cal at that time. I only needed living expenses, which were covered by my GI Bill. The additional income from working on the Oroville Dam project allowed me to have my own apartment located near the north side of campus—less than 50 yards from the Engineering Materials Laboratory, EML. It did not take long for me to forget the two years I lost in the military and to take responsibility for my own life.

There were many administration changes during the time I was in the military. First, the Engineering Department became the College of Engineering. The College was subdivided into the four Departments of Civil, Mechanical, Electrical, and Material Science. Within the Civil Engineering Department, three Divisions were created—Structural Engineering and Structural Mechanics (SESM), Hydraulics and Sanitary, and Transportation. Each Division had a budget and was responsible to hire new faculty. The SESM faculty, structural engineering analysis, design, and material testing laboratories were in the EML. This historic building was located on Hearst Avenue near the intersection with Leroy Avenue.

My initial objectives were to spend three semesters at Berkeley to obtain an MS degree in structural engineering while working on the model studies for the Oroville Dam. Then it would be possible for me to start my professional engineering career, such as being a design or field engineer for the State Bridge Department. However, these short-term goals did not occur. I spent the next six and one half years at Berkeley, which were the most productive years of my life.

My Masters Degree in Structural Engineering

Reitherman: What courses did you take while working for your MS degree?

Wilson: In my first spring semester I took an advanced strength of materials course from Professor Popov who was an excellent teacher. He received his PhD from Stanford University under the direction of the legendary Professor Timoshenko. Therefore, his lectures always contained a significant amount of information on the history of mechanics. In addition, I took Professor Scordelis's excellent course on Advanced Structural Analysis. This course was so popular, his hour and a half lectures were given on Tuesday nights at 7:00 p.m. and at 8:00 a.m. on Saturday mornings. This allowed members of the structural engineering profession to take this important graduate course through University Extension. Also, I took a math course that semester that was of little value.

At the end of the spring semester, I had an appointment with my graduate advisor Professor Eberhart. I proposed to take another mechanics course from Professor Popov and the Experimental Stress Analysis course from Professor Clough in the fall. In the following spring, I would take the Soil Mechanics course from Professor Seed in addition to the Dynamic of Structures course from Professor Clough. This proposal, along with a three-unit CE299 research course, would complete the minimum requirements for the MS degree. Professor Eberhart approved my proposal.

Professor Eberhart then informed me that Professor Clough, who was on sabbatical leave in Trondheim, Norway[7], had informed him he intended to offer a new course in the fall on "Computer Analysis of Structures" and Eberhart recommended that I take this additional course. I then made a very stupid

7 Ray Clough spent his first sabbatical in 1956-1957 in Trondheim, Norway, at the Skipstekisk Forskning Instituut, a naval architecture research center, where he did some analyses of rectangular and triangular elements.

statement, "I have no plans to use a digital computer." He replied, "That is right; if you don't know how to use a computer, you will never use one." I took the new course. It changed my life. Also, I realized it was human to avoid change.

Wilson: Professor Ray Clough returned from his sabbatical in Norway at the start of the 1957 fall semester. He walked into his new graduate class on computer analysis of structures and said, "There are only three equations in structural analysis: force equilibrium, displacement compatibility, and material properties, which relate forces to displacement." Also, he stated "there were only two different methods of structural analysis: the force method and the displacement method." I thought, "Why didn't somebody tell me these simple facts ten years ago?" Based on these simple principles, after writing several structural analysis computer programs during the next few years, I realized the "displacement method" was the best method to use for all types of structural systems. Furthermore, it became apparent the method could easily be extended to dynamic response analysis.

In all my structural engineering courses, prior to that point in time, I had been taught to use many different methods, classical theorems, and clever tricks for the analysis of structures. Also, there were many methods for the analysis of different types of structures. Now, I believe a young high school student, with elementary courses in physics and algebra, can be taught the fundamentals of structural engineering in a very short period of time.

At that time, design engineers preferred to build structures that were statically determinate. This type of structure could be designed using the equations of statics only. Only simple hand calculations were required to determine the design forces in all members within the structure. However, if one member fails in a statically determinate structure, the entire structure collapses.

The I-880 Viaduct in Oakland was designed and constructed during the 1952 to 1958 period. The bridge design engineers placed (very weak) pinned joints at the base of the top-deck concrete columns in order to make the structure statically determinate and to avoid additional hand calculations. There was no earthquake loading required by the bridge design specifications at that time. The viaduct was located approximately seventy miles north of the epicenter of the relatively small 1989 Loma Prieta Earthquake and was subjected to approximately 15 percent of gravity. Therefore, if the design engineers had eliminated the pinned column joints and made them rigid, the cost of construction would have been reduced, and I believe the structure would not have been significantly damaged. The "rigid joints" would have yielded progressively and dissipated the strain energy. We now call this approach "performance-based design."

Reitherman: Did your courses from Professors Seed and Clough consider earthquake analysis?

Wilson: Professor Seed's lecture and laboratory course on soil mechanics was an excellent summary of the advanced state-of-the-art as of the spring of 1958; however, he did not mention earthquake loading. This was seven years prior to the large 1964 Alaska Earthquake; therefore, very little government funding for earthquake engineering research was available for both soil mechanics and structural engineering. However, by 1966 he was specializing in the field of geotechnical earthquake engineering and is recognized as the "father" of the field[8].

Wilson: Professor Clough was also a great teacher. His Experimental Stress Analysis and Dynamics of Structures courses were very clearly presented and proved very valuable to me while working on the Oroville Dam project. However, he only presented a few lectures on the earthquake analysis of structures. After Clough presented his 1960 Finite Element Method paper, he was recognized as the "father" of the FEM[9].

In summing up my MS courses, I took one course from Professor Scordeles, two courses from Professor Popov, three courses from Professor Clough, and one course from Professor Seed—and they were all recommended by Professor Eberhart. All five of these Professors were elected to the National Academy of Engineering and received many other national and international awards. I was a very, very lucky student.

Reitherman: What topic did you select for your MS degree research?

Wilson: When Professor Clough returned from his sabbatical leave in September 1957, he immediately posted a notice on the SESM bulletin board announcing he was looking for MS students to do research using the Finite Element Method, FEM, of analysis for the solution of plane stress, plate bending, and shell structures. I had no idea what FEM was; however, I made an appointment with him to obtain more information. When we met, he informed me of his summer work at Boeing using the Direct Stiffness Method and his intention of extending the method to the determination of displacements and stress concentration in classical problems in continuum mechanics. This was the reason he coined the new name "Finite Element Method" to unify the methods used in structural analysis with those used in continuum mechanics.

He gave me a copy of the Boeing paper published in 1956[10] and a series of papers written by Argyris.[11] Also, he asked me to find out more information on the IBM 701, which had been recently installed in the basement of Cory Hall. We also established a meeting time each week in order for me to report the status of my research.

Reitherman: What type of computer?

8 H. B. Seed and K. L. Lee, "Liquefaction of Saturated Sand during Cyclic Loading," *Journal of the Soil Mechanics and Foundations Division*, ASCE, Vol. 92, No. SM6 Proceedings Paper 4972, November 1966, pp. 105-134.

9 Ray Clough, "The Finite Element Method in Plane Stress Analysis," *Conference Papers of the 2nd Conference on Electronic Computation*, American Society of Civil Engineers, Sept. 1960.

10 M. J. Turner, R. W. Clough, H. C. Martin, and L. T. Topp, "Stiffness and deflection analysis of complex structures," *Journal of Aeronautical Science*, vol. 23, no. 9, pp. 805-823.

11 J. H. Argyris, "Energy Theorems and Structural Analysis," *Aircraft Engineering Journal*, Vols. 26 & 27, October 1954-May 1955.

Wilson: An IBM 701. It was a first generation Scientific Computer with 5,000 vacuum tubes. I looked it up recently, and only eighteen of them were delivered between 1953 and 1956.[12] Ours was a used one.

Reitherman: Eighteen, in the world? IBM delivered only eighteen machines and one was in the College of Engineering on the Berkeley campus?

Wilson: Yes. The 701 took up an entire room, with about a dozen refrigerator-sized components. It was one of the last vacuum tube computers produced. It was unreliable and very difficult to program. Ray Clough and I spent nine months to develop an incomplete finite element program, which produced displacement only.

I finished my master's degree research report in June 1958 without making any significant contribution to structural engineering or structural mechanics. However, it was the first time the term "finite element" was ever used in any report or paper. Also, this report was typed by a student friend of mine, Diane Farrington, a journalism major. I found my writing had significantly improved after she produced my final report.

Reitherman: What was the title of your master's thesis?

Wilson: A Study of the Application of Finite Elements to the Problem of Plane Stress.[13]

Life as a Graduate Student

Reitherman: Say a little about your life as a graduate student.

Wilson: I was a free man for the three years after I got back from Korea and lived very close to the north side of campus. Therefore, I did not waste any time traveling to and from work or school. There were 168 hours in every week and I always got at least eight hours of sleep every night. That left more than 110 hours each week for work, study, and to have an active social life. After the first year, I was living in an apartment by myself on Hearst Avenue on the north side of campus. It was a studio apartment with a fold-out bed and a small kitchen and bathroom. When the building was built in 1930, they charged $40 a month. When I lived there from 1958 to 1960, rent was still $40 a month. Particularly when I got into experimental work, I was at the lab in the middle of the night, and it was very convenient to live near the campus. Also, I always had a good office on campus and did most of my studying there.

La Vals Gardens and its basement beer bar opened in 1957 and I spent a lot of time there in addition to the Northside Coffee Shop, where the Asian Restaurant is now located. Also, there was Cody's Book Store with Mr. Cody giving suggestions of what to read. The architecture school was still on north side then and we had a good mix of people with different interests.

12 *Encyclopedia of Computer Science and Engineering*, Van Nostand Reinhold Co., 1983.

13 Edward Wilson, *A Study of the Application of Finite Elements to the Problem of Plane Stress*, Earthquake Engineering Research Center (now PEER) report, University of California at Berkeley, June 1958. Available from the NISEE-PEER library at: http://nisee.berkeley.edu/documents/elib/www/documents/201312/wilson-plane-stress.pdf.

When you went through architecture school at Berkeley, the architecture building was on the south side. As I recall, they moved into Wurster Hall in 1964, which was the year before I joined the faculty.

Reitherman: Yes, but occasionally I had a class in the old north side architecture building, a warm and welcoming wood building as compared to the cold and impersonal Wurster Hall built on the south side.

Wilson: The south side was liberal arts, the north side was engineering and architecture. The university was smaller then. I could easily find a permanent parking spot for my 1957 Chevy, the turquise color. If I still had that car today, it would be worth a fortune.

Reitherman: Wow, a '57 Chevy, the classic one. What did you pay for it?

Wilson: $2,750. I bought it brand new in September 1957 with money I had saved. I would never have borrowed money; in fact, I never borrowed money through college. I just worked and saved to pay the bills and I always had $500 to $1,000 in the bank. Today, the fees are so high and students borrow too much money. Something has to be done about the high cost of tuition. Also, the University has an obligation to produce students who are employable.

The graduate students and faculty members in SESM became involved in several athletic activities between other groups on campus. The serious one was a softball game between Chemistry and Engineering. Bob Taylor, Len Herrmann, and Stan Dong were students of Karl Pister. Jim Tocher and I were students of Ray Clough. The five of us were all good friends and were involved in most activities, including beer drinking. I believe all of us got doctorates because we were having too much fun to leave after our MS degrees.

Evolution of Computer Power at the Berkeley Campus

Wilson: My experience with programming the IBM 701 (with 16-bit storage cells) was so negative it was a joy to program the IBM 704 (with 32-bit storage cells and a floating point processor) when it was installed in Campbell Hall in 1959. The IBM 7094 in 1961 (which was a transistorized version of the IBM 704) and the *new* CDC 6400 (with 60-bit storage cells) in 1963 finally gave the Berkeley campus the computer power it deserved. Also, in 1961, the FORTRAN (*For*mula *Tran*slation) program language was developed. This was a very important step in scientific computing. It allowed research engineers, who developed new methods of analysis or new finite elements, to send the program (a box of ASCII[14] punched cards[15]) any place in the world. Then, the

14 ASCII stands for American Standard Code for Information Exchange. A string of 8 zeroes or ones translates into the various letters, numerals, and punctuation marks. As the name (Information Exchange) indicates, it was a development in the early 1960s that allowed for the soon-to-burgeon number of computers to talk to each other.

15 The card with punched holes to sort data was invented by Herman Hollerith in 1890 to tabulate results of the U.S. census of that year, using machines (not yet computers) that could sort the cards into categories. He later founded a company, which, after Thomas J. Watson Sr. joined it, became International Business Machines Corporation. (Anthony

computer program could be used on any of the many different manufactured digital computers if they had a FORTRAN compiler. The recipients could easily modify the program to meet their needs and then give or sell the modified program to other structural engineering firms.

Punched cards remained the standard form of computer input on the Berkeley campus until approximately 1980. They were then replaced by multiuser computer systems with remote terminals or smart personal computer systems. Now, all personal computers have 64-bit architecture. Most have multi-processors and are very fast. Memory is very inexpensive. Also, Intel has developed very high-speed equation solvers that are built into their FORTRAN compilers. However, most young engineers do not want to learn a programming language.

Reitherman: From your account, it seems that your time in Korea delayed your education, but it delayed it to the right point in time. It allowed you to start your research career using powerful, modern, scientific computers that were capable of solving significant and real structural engineering and structural mechanics problems.

Ralston, editor, *Encyclopedia of Computer Science and Engineering*, Second Edition, Van Nostrand Reinhold Co., New York, NY, 1983, p. 706.) The perforated paper tapes to "program" the operation of weaving looms was an analogous invention by Basile Bouchon going back to 1725 in Lyon, France.

Chapter 6

Model Studies of Oroville Dam

No one believes an analytical solution except the individual who derived it. Everyone believes an experimental solution except the individual who conducted it.

Wilson: In 1957, one common proverb used to describe structural engineering research was: No one believes an analytical solution except the individual who derived it. Everyone believes an experimental solution except the individual who conducted it.

I became known as "a computer guy," but I'm proud of my experimental work on the Oroville Dam project, and in fact there was a very active local chapter of "Society of Experimental Mechanics."[16] Most of the senior staff and faculty working in the Engineering Mechanics Laboratory belonged to this group. My reaction was "I will do both and then decide which solution best satisfies the fundamental equations of structural mechanics."

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16 The Society for Experimental Mechanics (SEM) was formed in 1943 by engineers and scientists in a wide variety of disciplines who were doing experimental work. It currently publishes three journals.

The construction of the Oroville Dam was a major project for the State of California Department of Water Resources. It is still the highest dam in the United States. They were considering five different designs for the 730-foot-high dam with the water surface elevation of 900 feet above mean sea level. The five different designs were a concrete gravity, buttress, multiple arches, arch buttress, and a rock-fill embankment dam.[17]

Reitherman: The scope of the study was to investigate the alternative designs?

Wilson: Yes. The arch-buttress dam was a new type of dam and required a three-dimensional analysis. Therefore, the board of consultants recommended a model study of the arch-buttress structure, because, at that time, it was not possible to perform an accurate hand analysis solution. It had a large concrete arch in the middle flanked by buttress segments. Professor Jerry (Jerome) Raphael was responsible for the model analysis of this proposed dam. Jerry was brought on to the faculty as Raymond (R. E.) Davis retired. Frank Baron was hired in 1953 to replace Davis as the head of the Engineering Materials Laboratory. Davis had a personal telephone line to his Berkeley office for his consulting work. I don't know how it worked, but you didn't go through the university switchboard to access it, which was unusual as of then. He contributed money when the lab needed to get things done. He was the one who got the money from the Hoover Dam project for testing. And later on, that's why Berkeley got the Oroville Dam project research work, because the experience had been built up, and the lab had been built up. You need some steady income to keep the labs and the research going. Some of the faculty were jealous of Davis because the employees in his consulting office were covered by Kaiser health insurance, at a time when the faculty had none.

There were three other research areas in the contract between the University and the State in addition to the model studies. Professor Eberhart conducted the photoelastic analysis of the largest underground powerhouse ever built. Professor David Pirtz conducted adiabatic temperature properties of the proposed concrete mixes and the testing of the concrete cylinders that were four feet high and half that in diameter using the four-million-pound testing machine. When these specimens failed, they exploded rather dramatically.

The contract with the State started in March 1957 and I was the first employee with the title Junior Engineer. During the next two and one half years I worked on all four areas of the project and learned more about the field of experimental mechanics. I enjoyed the experimental work and may have developed some new experimental techniques. Also, it was the start of an over 50-year career as a very active consultant on mass concrete hydroelectric structures subjected to static, temperature, and seismic loading. By chance, a few years later, working with Ray Clough, the finite element method made a big step forward because of its use in analyzing a dam.

17 Jerome M. Raphael, Structural Model Investigations for Oroville Dam, Structures and Materials Research Department of Civil Engineering, Series 100, Issue 6, University of California, Berkeley, February 1960.

Design of the Model Material

Wilson: The state board specified that the dimensions of the model would be 1/200 of the dimensions of the real dam. The construction of the arch-buttress-foundation model was the most labor-intensive phase of the project. Therefore, under the direction of Professor Raphael, our goal was to design a model material that had stable and predictable properties at the time of testing. At the start of material study, we assumed that standard plaster material, with a weaker filler material added, behaved like concrete. After it is poured, concrete normally increases its strength and stiffness for many years. Also, the curing conditions have a significant effect on the mechanical properties of concrete. Therefore, Professor Raphael designed a very elaborate series of tests and graphs in order to create a model material with predictable linear elastic modulus. At that time, the filler material "celite," manufactured by Johns-Manville Company,[18] was the standard ingredient used by model makers to reduce the stiffness of the model material. Also, we found plasters with almost identical properties made by different manufacturers. Finally, we found if we mixed the plaster with 25 percent of celite by weight and added a fixed amount of water, then placed it in an oven at 90 degrees for two weeks, we obtained the modulus of elasticity we wanted.

18 The Johns-Manville Corporation entered bankruptcy in 1982 because of large claims on its assets from asbestos-related legal cases and later was re-organized (without the hyphen in its name).

Appointment of Project Engineer

Wilson: Within a month after I started working, Professor Raphael hired a full-time project engineer, Stuart Bartholomew, with several years experience in the design and construction of concrete dams in many areas of the world. As the project engineer, his function was to organize the fabrication of the Oroville model and to supervise six or seven individuals, including me. He and Professor Raphael made the decision to construct a mock-up of each of the major parts of the model, foundation, and transition plug between the foundation and buttresses. Using these mock-up structures, fiberglass forms were made and plaster model material was poured to form the foundation in layers. This phase of the project took approximately 15 months. At that point in time, the Oroville Consulting Board had made the decision to select the rock-filled dam with a thick clay core. The dam was to be sited in the Sierra foothills where so much mining was done in the 1800s and early 1900s. They would dam up an area and float a dredger there that would mine the ore, leaving behind excavated rock—many, many piles of rocks. Dredger mining efficiently mined the ore, but it turned farmland into rocks. Because there was so much rock nearby, just about free for the asking, it was feasible to build a rail line to transport the material to the site of the dam. This was the deciding factor as to what type of dam to build.

Stuart Bartholomew stated he had no desire to work on a project that would not be built. However, the State decided to fund the completion of the model studies in order to develop the technology for other dams they were planning to construct. It was then the summer of 1958 and I had completed my MS degree and was working full time. When Professor Raphael offered me the position as project manager and a promotion to assistant engineer, I was pleased to accept. I held that title for the next five years and found that when I retired from teaching in 1991 those five years would be added on to my retirement as a full professor. How lucky can a man be?

This was the first opportunity in my life to be responsible for telling people when and how to do different tasks. I thought about my father who supervised his four sons on our ranch near Ferndale. The one thing I remember was he never asked or told any of us to do anything he could not do himself. During the previous year, I had taken Professor Clough's Experimental Stress Analysis course; therefore, I knew the fundamentals of model similitude, photoelasticity, strain gage, and data acquisition systems. Also, my army experience as a radio repairman would be very helpful in the creation of the strain gage measurement system.

First, we applied the hydrostatic loading to the complete dam model. We couldn't let the water get in contact with the celite or it would turn that hard substance into mush, so we used water-filled plastic tubes. After completing the hydrostatic stress testing, we just cut the model down, from top to bottom, to produce horizontal surfaces. At each surface we applied a vertical load proportional to the weight of one foot of concrete dead load and recorded the stresses. After completing the process, we integrated the stresses at each level from bottom to top. This produced stresses at each strain gage as a function of the construction height above that point. Professor Raphael named this approach "the method of integration for dead load analysis."[19]

This simple method for evaluating dead load stresses in dam models proved to be far more accurate than all the other approaches used at that time. In a few years, all of the major laboratories in the world were using this new method. Professor Raphael wrote a very nice paper comparing the new method with other methods. However, within the next 15 years the need for model analysis was eliminated and replaced by finite element computer programs developed by Clough, Wilson, and many others.

Reitherman: It sounds like your computer programming work helped to make your experimental method obsolete.

Wilson: In a way, yes. Today, the CSI version of SAP2000, one of the most widely used structural engineering programs in the world for the earthquake analysis of structures, allows you to analyze any type of structure and include the effects of incremental construction. Also, the program can calculate the stresses due to temperature changes, creep, soil structure interactions, reservoir dam interaction, and pre- and post-tensioning. The laboratory work on the Oroville Model was some of the most creative work I ever did, and it was about the development of new experimental methods, not numerical work on the computer.

19 Jerome M. Raphael, "Dead-Load Stresses in Model Dams by Method of Integration," *ASCE Journal of the Structural Division*, Vol. 87, No. ST 6, August 1961.

People don't know that I did all this experimental work. They assumed I just developed computer programs all my life.

In July 1959, we were ready to test the fully instrumental model. By that time, most of the student helpers had completed their degrees or left for summer vacation. As I recall, Professor Raphael, Gene Croy, and I were conducting the test. In addition, several other spectators were present. When the loading was approximately 50 percent of the maximum value we all heard a loud bang and saw that the central arch section had moved downstream approximately one inch. I immediately reduced the pressure in the airbags that were applying the loading. The reaction of everyone in the room was "what happened!?" Within a few hours, everyone in EML knew that the two-year-old project had a major failure.

I knew it would not be possible for me to sleep that night unless the reason for the failure was found. I started to dig into the foundation near the corner of the test pit and found that below 6 inches from the surface, the water in the material continued to increase. Near the bottom of the test pit the material was essentially mud.

If the foundation had been dry, the solution would have been very simple. We could just epoxy the arch section back on the foundation and continue to test the model. No big deal. However, it was now apparent the complete foundation had to be removed and replaced. We removed the arch section with its strain gages and the switching system and moved them away from the test pit. Within three days after the failure, my plan was to cast and dry 20 by 30 by 4 inch blocks. We had oven capacity to cast and dry approximately 4 blocks every day. We needed approximately 60 blocks to form a new foundation; therefore, it would take approximately 15 days to replace the foundation.

Approximately four weeks after the failure, we were ready to apply the hydrostatic load for the second time. During this phase of the project, I worked seven days a week and did not keep track of my time. Some days I worked a few hours and other days I worked 10 hours. Gene was always there to help when two people were needed. The hard work was no big deal for me since I had worked 57 days straight at American Can Company and longer hours as a hired man on a dairy ranch. Also, I was a single man and lived on Hearst Avenue only 50 yards from EML. The second time the model was tested everything worked fine.

I found that my radio training in the military allowed me to modify the instruments and get more accuracy in the measurements. I could walk up to one of the strain gauges and the heat from my hand would affect it. To see if my approach to incremental loading to simulate the construction process worked, I would cut off a layer of the model and then put on a uniform pressure proportional to the weight of one foot of concrete at that level. We used lead bricks to simulate the weight of the concrete. We borrowed the bricks from "the hill," from Lawrence Berkeley Laboratory, where they used them for radiation shielding. We would place them very precisely on the model after we had cut off each layer, which took a few hours for each layer. The next morning we would take a zero reading of the gages and then take the bricks off the layer quickly and then read the gages again. This difference in the gage readings was proportional to the strain of adding or removing one foot of concrete at that level. By reading all the strain gages below the loaded level, we obtained a curve of the dead load stresses caused by placing the concrete above that level.

In August 1959, after we completed the dead load testing, my work on the Oroville Dam model was essentially completed. Within the next few months, Gene Croy completed all of the data reduction and prepared plots so that Professor Raphael could complete his final report.[20] I continued to work with Professors Eberhart and Pirtz on the other phases of Oroville project, which went on for several more years.

Reservoir-Induced Earthquakes

Wilson: In 1975, eight years after the completion of the dam, the Oroville earthquake of magnitude 5.8 occurred. This was a surprise because the Sierra foothills were not considered seismically active. The USGS has been monitoring the area since then. They have determined smaller earthquakes have occurred every few years since that time. Earthquakes of smaller magnitudes tend to correlate in the summer and fall after the reservoir is lowered rapidly. The behavior has been termed a reservoir-induced earthquake.

Reitherman: It's fascinating that puny little humans can do something to change the stress underground in vast amounts of rock to induce an earthquake, but then again, dams, or rather the reservoirs they create, are the largest and most massive man-made objects on Earth.

Wilson: The earthquake that brought attention to the phenomenon of reservoir-induced earthquakes was the magnitude 6.0 Koyna Earthquake in 1967, several years after the completion of the Koyna Dam in the area of Koymanagar, India. It was also associated with a large drawdown of the water level, and then refilling. That earthquake killed nearly 200 people and did significant structural damage, including a large horizontal crack near the top of the concrete gravity Koyna Dam. In 1975, I was considered an international expert on the earthquake analysis of dams, and therefore, the California water resources agency asked me to check the earthquake safety of the concrete spillway of Oroville Dam. I ran one analysis for them. Then I showed them how to use SAP IV, so they could check the safety of the other concrete structures connected to the dam.

Adiabatic Temperature Measurements

Wilson: Another aspect of dam engineering I worked on at Cal, with Professor David Pirtz, was research on adiabatic temperature increases for a large number of proposed concrete mixes proposed by the state.

Reitherman: I can't recall what adiabatic means—something to do with heat transfer?

Wiilson: If you can approximate a system's thermal behavior as steady, as not losing heat to outside the system at least on a short time scale, it's adiabatic. Many different parts of the

20 Jerome M. Raphael, *Structural Model Investigations for Oroville Dam*, Structures and Materials Research Department of Civil Engineering, Series 100, Issue 6, University of California, Berkeley, February 1960.

rock-filled Oroville Dam were made of concrete, such as spillways and the large underground powerhouses, and the curing process generated heat. This work continued after the completion of arch-buttress model studies.

I was heavily involved in the adiabatic temperature tests. Professor David Pirtz had built a large insulated box to hold one large two-foot-diameter concrete cylinder four feet tall within a temperature-controlled room. For 10 days prior to pouring the cylinder, we stored all the material, including all water, gravel, and cement in the control room at 60 degrees Fahrenheit. Prior to pouring the cylinder, we instrumented the mold with temperature gages and Carlson strain gages at various locations. The concrete mix was poured without moving or damaging the gages. Dave had built a small heating system that would heat the air in the insulated box to the same temperature as the temperature at the center of the concrete cylinder. We then would read the gages periodically for the next 28 days. Since most of the heat of hydration of cement took place in the first few days, the gages required reading every few hours. Also, I had to check if all the electrical equipment was functioning properly. These adiabatic temperature curves are very important and are used to prevent cracking of the concrete during and after construction. Eight years later, for the construction of Dworshak Dam, I wrote a heat transfer finite element computer program to predict construction stresses within mass concrete structures. In fact, this 50-year-old program is still being used by many professional organizations.[21]

Reitherman: Explain why the heat created in the chemical process of concrete curing could harm a big structure like a dam.

Wilson: It's not the temperature itself, which never gets the concrete as hot as in a fire. It's the differential temperature. Part of the dam's concrete is hotter than other areas based on when it was poured, then it cools, with this expansion and contraction affecting contiguous areas of concrete. In the case of Norfolk Dam, it was being built so fast that the heat made it expand, but that was when the concrete was soft. When it was cooling, the concrete was fairly well cured and hardened and more brittle. I recall Roy Carlson saying they put railroad ties across the big crack that was forming, but the dam kept prying itself apart.

One method is to run water piping through the mass of the concrete to keep its temperature down as it cures. In the case of Libby Dam, at a high altitude in Montana, it was the opposite problem. The concrete on the exterior would have gotten too cold, so they heated and insulated it. The point is that there should be a small temperature gradient through the cross-section of the concrete. When you run water pipes through the dam, you design the piping to transport the water that is heated by the curing process to the cooler areas and recirculate that cooled water to the warmer areas.

In the case of the St. Lawrence Seaway locks, which I did some consulting work on with Roy Carlson, the locks that cracked the most were the ones they drained in the winter for inspection and maintenance. Exposed to the air, they

21 Edward Wilson, "The determination of temperatures within mass concrete structures," SESM 68-17, Department of Civil Engineering, U.C. Berkeley, December 1968.

were the ones that got the coldest, while the ones that weren't drained cracked less.

Jerome Raphael and Earthquake Engineering

Reitherman: Raphael was the one who wrote the state-of-the-art paper on dams for the first of the World Conferences on Earthquake Engineering, in 1956.[22]

Wilson: That was before any thorough consideration of the dynamics was incorporated into the seismic design of dams. A static lateral force coefficient was applied to the dam back then.

Reitherman: His 1956 paper is an interesting benchmark because insofar as it deals with the dynamics topic, it is preoccupied with how the earthquake vibrations affect the water and the hydrodynamic loading, not how the earthquake directly shakes the structure. It also preceded a modern understanding of the geotechnical factors involved in earthfill dams, a topic only lightly addressed in his state-of-the-art paper in 1956. Especially since the 1971 San Fernando earthquake, the vulnerabilities of earthfill dams became a central topic in the design of dams.

Wilson: The Lower San Fernando Dam, which failed in 1971, was constructed by the hydraulic fill method. In simple terms, you make a little pond on top of the earth fill and put a barge in it that pumps a slurry mixture out to where you want to place the soil and build up the cross section of the dam. You add more earth and more water as the dam increases in height. A few days after the earthquake, Professor Harry Seed and I visited the failed dam. We both agreed if the reservoir had been filled to capacity at the time of the earthquake, thousands of homes below the dam would have been destroyed and many people killed. Because the water level was fortunately low, even though the dam slumped and reduced in height by about 30 feet, the water just barely didn't spill over. At that time, there were over twenty hydraulically filled dams in California, several in the Bay Area. They all were replaced within ten years after the earthquake.

Jerry Raphael and I were friends for about thirty years until he died in 1989 from heart problems. We went sailing on his boat in San Francisco Bay often, the last time only a month before he died. Because I was twenty years younger, I was his crew and the one climbing the mast when he was in his 70s. Less than a year after he died, I had a heart attack in 1990 at the age of 58. I retired from teaching in 1991. Jerry was a very unique individual. I still miss him. By the way, after 25 years, my heart is in great shape.

Marrying Diane

Wilson: In June 1959, my friend Diane Farrington received her BA in Journalism and returned to her home in Callahan, California. After a few weeks, she returned to Berkeley and was hired as an assistant to the merchandizing manager at the headquarters of Longs Drugs Stores. I was not surprised she returned

22 Jerome Raphael, "Design of Dams for Earthquake Resistance," *Proceedings of the World Conference on Earthquake Engineering*, Earthquake Engineering Research Institute, Oakland, CA, 1956.

to the Bay Area since her job in Callahan was to help her parents run their general store, gas station, and post office. I had been to Callahan during the previous year. It's a small town in northern California near Trinity Lake, a nice setting, but it was not an exciting place to live.

During the next several months, Diane and I saw each other nearly every day. We were married in 1960. We rented a two-bedroom apartment on Dwight Way near Milvia Street, still within walking distance of the campus. Diane worked at Long's until a week prior to the birth of our son Mike in 1961. Because we only had one parking spot, we had to sell one of our cars. Diane had a 1959 Ford when we got married and I had an older 1957 Chevy. Hers was newer, so I sold mine. After being married for nearly 55 years, we still maintain a nice home in Callahan and visit the large Farrington family several times a year. However, the hunting and fishing are not what they used to be.

Chapter 7

1959 to 1963 Working with Professor Ray Clough

After completing a research project, always write a paper or report to summarize your results.

A Very Difficult Decision

Reitherman: What did you do after the Oroville model testing was completed?

Wilson: I was working with Gene Kroy converting the strains measured on the model to stresses on the concrete version of Oroville Dam. Also, I continued to work for Professors Pirtz and Eberhart doing routine concrete testing associated with the construction of Oroville Dam. In fact, after working in EML for over two years, I knew all the staff and how to use nearly all the test equipment in the building. One day, Professor Eberhart and I met in the hall and he said to stop by to see him. That afternoon when I sat down in his office, expecting him to have me work on one project or another, he said "I think you should get a doctor's degree." He had been watching me since I was an undergraduate student in 1953 and he concluded I had the ability to solve complex structural engineering problems. I recall him saying something to the effect that I might make a good fit in the faculty at Berkeley. I was very surprised. After a significant pause, my response was "I will think about it."

A few days later Clough came to my office and said he had received NSF funding for his research on computer analysis of structures. He asked me to work with him on the project as a doctorial student. Eberhart and Clough were the two professors I had the most respect for in the structural engineering group. At that time a doctor's degree required passing two different language examinations and completing minors in two other areas. I told Clough I would like to work with him; however, I thought the requirements for the degree would take too long to complete. At that time, it was a few weeks before my 28th birthday. He suggested that we meet Professor Popov to minimize the requirements for a D. Eng. doctoral degree. Popov agreed to my proposal to pass only the German examination and have the second language requirement replaced with my experimental stress analysis contributions while working on the Oroville Dam project. Also, I would take a few more courses in dynamics and numerical methods.

Reitherman: What was it like working with Professor Clough?

Wilson: Within a few weeks after I started to work with Professor Clough for my D. Eng. degree, we were solving problems. Within a few months I was developing new programs for the IBM 704. Also, our meetings were very productive because he treated me as an equal. We would clearly define the problem we wanted to solve and then discuss the fastest and most accurate numerical methods to use within our computer programs. For many problems it was necessary to develop new numerical methods that were not possible by hand calculations. I enjoyed our computer research work and tolerated taking many obsolete graduate courses that neglected the existence of the modern digital computers on the Berkeley campus.

One thing unique about Ray, he was an optimist. He believed anything was possible. You just had to find the right way to solve the problem. He must have developed this approach from his experience climbing mountains as a teenager. He was very athletic. In 1953, when I was an undergraduate student, the faculty would play tag football in an annual picnic normally held in Tilden Park. I thought I was in great physical shape since I was the fastest half-miler on the track team. I believe most of the faculty considered the traditional football game a chance to really teach the students a lesson. I played table tennis with him, and he was intense about that. Ray was an excellent squash player. He lived down the street from here (El Cerrito hills, the top ridge), and then moved to a similar hilltop location in the Berkeley hills on Grizzly Peak Boulevard. If it was decent weather, he rode his bicycle to the campus. Even after he retired and was near seventy years old, he would ride his bike to and from campus on nice days. He also skied until he was almost eighty.

Force equilibrium, displacement compatibility, and material properties are still the essence of structural analysis. The compatibility aspect is critical: as members deform, they still fit compatibly together, affecting each other differently as the earthquake progresses. With the advent of automated analysis, sometimes people forget the fundamentals of structural analysis, that it is important to compare our methods and assumptions with physical reality, to see if our methods really describe what is happening.

Reitherman: Many people think of Clough as an analyst, not an experimentalist, while many think of you as a developer of computer programs, without knowing your first research project was with experimentation. Clough wrote a paper in 1980 in which he notes how he saw the need to develop a physical understanding through testing of how structural materials and assemblies behave and devoted his research to experimental work.[23] He wrote that "... it is important to express my concern over the tendency for users of the finite element method to become increasingly impressed by the sheer power of the computer procedure, and decreasingly concerned with relating the computer output to the expected behavior of the real structure under investigation."

The Symbolic Matrix Interpretive System–SMIS[24]

Wilson: After his experience at Boeing and his sabbatical leave in Norway, Professor Clough was convinced the "three fundamental equations of linear mechanics" could be written using matrix notation for finite element structural systems. Furthermore, he believed that all linear continuum mechanics systems could be accurately approximated by finite element systems. Therefore, if we wrote a simple program to manipulate matrices and sub-matrices, in user-specified order, the student could solve structural engineering problems. This simple educational program would eliminate the need for students to spend a large amount of time trying to learn a complex computer programming language. Within a few months, we developed a primitive version of the SMIS educational language, which was first used in early 1960. We then added SMIS commands to solve for mode shapes and to integrate the dynamic equations motion. These operations clearly illustrated that structural dynamics was a simple extension of the static analysis of structures. We then found the later versions of SMIS became a great research tool since users could easily add or suggest new operations. In the next 30 years, various versions of the program migrated from Berkeley to over 100

23 Ray Clough, "The Finite Element Method After Twenty-five Years: A Personal View," *Computers and Structures* vol. 12, no. 4, 1980, p. 361–370.

24 Edward Wilson," SMIS—Symbolic Matrix Interpretive System," University of California, UCSESM 73-3, April 1973; and, "CAL—A Computer Analysis Language for Teaching Structural Analysis," *Computers and Structures*, Vol. 10, pp. 127–132, 1979.

universities and many companies throughout the world.

The 2nd ASCE Conference on Electronic Computation

Wilson: When I started to work with Ray, I moved into an office with another of his doctorial students, Ari Adini, who was also using the IBM 704. Ari was working on solving all of the examples that were included in "The Finite Element Method in Plane Stress Analysis,"[25] which was presented by Ray at the second ASCE conference on computers held in Pittsburgh in 1960. This was the first time the phase "Finite Element Method" was used at a major conference in which all papers were refereed.

At the same time I was programming SMIS, I worked on a new computer program for automated nonlinear analysis of plane frame and truss structures of arbitrary shape. I used a nonlinear moment-curvature relationship defined by the Ramberg and Osgood equation.[26] A new incremental loading numerical algorithm was developed and my program presented very impressive results. After I wrote the paper and showed it to Ray, he suggested I submit it to the Pittsburg conference for publication. To my surprise, it was accepted. One of the reasons it was accepted may have been because my friend Diane improved the readability of the paper. By that time, she had changed her name to Mrs. Edward Wilson.

Reitherman: How were Ray and your papers received by the engineers at the conference?

Wilson: To my surprise, it appeared no one at the conference was greatly impressed with Ray's new "Finite Element Method." Most of the examples were solved on the IBM 701 and the meshes were very coarse; therefore, the constant stress plots within each element were not impressive. Also, most of the civil and structural engineers in the audience were not interested in solving problems in continuum mechanics. It was apparent to me that we needed to solve very fine mesh problems in order for the method to be accepted.

The ASCE conference in Pittsburg was the first professional technical meeting I ever attended. From my copy of the conference proceedings, I see that thirty-seven papers were presented and 375 engineers attended the three-day conference. Most of the papers presented simply used the computer to solve one structure with a certain geometry shape and the user had to hand calculate a significant amount of input data before using a computer to solve the equations. In my paper, I emphasized hand calculations could be minimized if you defined the geometric location of each joint and assigned a unique identification number to each joint. Each frame or truss member could then be located by referring to just two joint numbers and the material and section properties. Tables of joint and member loads would then complete the definitions of any two-dimensional truss or frame

25 Ray Clough, "The Finite Element Method in Plane Stress Analysis," *Conference Papers of the 2nd Conference on Electronic Computation*, American Society of Civil Engineers, September, 1960.

26 Edward Wilson, "Matrix analysis of non-linear Structures," *Conference Papers of the 2nd Conference on Electronic Computation*, American Society of Civil Engineers, September, 1960.

structures. My nonlinear program was based on an incremental tangent stiffness approach. Therefore, it also could be used for any linear elastic structure by applying only one increment of load. The paper was well received after I presented several example structures with different geometry. Also, it was the only paper on nonlinear analysis. There were no papers on the earthquake analysis of structures.

Ray and I flew to and from the conference together on the same flight. We also shared the same room to save his research funds. While at the conference, he introduced me to several other faculty members from other universities. For example, I met Nathan Newmark, Al Ang, Steve Fenves, Bill Schnobrich, and Andy Veletsos from the University of Illinois. On our flight home, Ray and I discussed how we could increase the capacity of the finite element method in order to obtain larger capacity. In one of my numerical analysis courses in the Mathematics Department, they had suggested using the Gauss-Seidel iteration method to solve equations. The main advantage of the method was only the nonzero terms of the stiffness matrix needed to be stored in high-speed storage, which was limited on all computers of that generation. The day after I returned from the conference, I started modifying the frame program by adding the plane triangular element to the program. Within a month, I had a working version of "The First Automated Finite Element Program." It definitely had larger capacity on the IBM 704 than using a direct solution equation solver. All I needed was a real structure to illustrate the capacity and accuracy of the program.

The Analysis of Norfork Dam

Wilson: Professor Raymond (R. E.) Davis and Roy Carlson, from U.C. Berkeley, were on the consulting board for the U.S. Army Corps of Engineers for the repair of Norfork Dam, located near Norfork, Arkansas.[27] Roy was an Adjunct Professor at U.C. Berkeley for many years. His connection with Berkeley started in the 1930s studying concrete for the Hoover Dam. Roy was a physicist who had worked on the Manhattan Project in World War II. He was the one who steered the Corps of Engineers to our work. Roy was the cousin of Chester Carlson who invented the Xerox machine and process. Roy invested $10,000 worth of stock in the original Xerox company.

Reitherman: Wow. People speak jokingly about wishing they had gotten in on the ground floor of such an investment, but this is the first time I've heard about someone actually doing it. That explains why Carlson was the one who donated some of his own money to house the hydraulic pump house when Ray Clough and Joe Penzien were getting the university's shake table facility built.[28]

Wilson: He also donated funds for two endowed professorships; one is for the Dean of the College of Engineering and the other for a Professor in the SEMM area. I will tell you more about Roy Carlson later. Now, let us get

27 Ray W. Clough and Edward L. Wilson, "Stress analysis of a gravity dam by the finite element method," Reunion Internationale des Laboratoires et Experts des Materiaux, Bulletin RILEM, No. 12, June 1963.

28 *Joseph Penzien: Connections*, The EERI Oral History Series, 2004, p. 42.

back to the analysis of Norfork Dam. The dam was about 300 feet tall. It was constructed very quickly during World War II. The dam developed a large vertical crack due to the cooling after a high adiabatic temperature increase. The crack ran 200 feet vertically, from the foundation up to more than half the height of the structure.

The consulting board was about to award the analysis project for Norfolk Dam to Caltech to use an analog computer to solve finite differences equations to determine the safety of the dam. They would not have been able to include the foundation or evaluate the stress concentrations near the top of the crack. Roy knew I had conducted work on the Oroville Dam project and Ray's 1960 paper referred to a dam analysis. Roy asked Ray to prepare a proposal for a finite element of the Norfork Dam. Since Ray was going out of town the next day, he asked me to prepare the proposal. Quickly, literally in one day, I made a coarse mesh analysis of the dam, without the crack, subjected to gravity and hydrostatic loading. Roy Carlson took that solution with him to the board meeting. The board recommended that the contract be awarded to the University of California at Berkeley. For Ray and I to get the chance to do the analysis of an existing dam was an opportunity to show the profession that the FEM could analyze real complicated structures. The Corps added Ray to the Norfork Dam consulting board and he transmitted the result of our analyses to the Board at every meeting. Ray realized that the routine analysis work, requested by the Board, was more than a part-time student employee, like me, should be asked to do. Ray then hired Ian King, his new doctorial student from Oxford, England, to help me with the preparation of the stress contour plots. Ian and I became great friends. I helped him program the computer and he taught me the British sense of humor. Also, with his cricket experience, he became a productive member of our SESM softball team.

Ray, Ian, and I finally completed all of the Oroville Dam analysis work in 1962, approximately 15 months after the start of the project. Ray wrote an excellent report that illustrated the real power of the FEM.[29] Based on our analyses, the Board recommended the Dam did not require major repair.

At that time, no one in the world had ever solved such a complicated structural analysis problem. The Finite Element Method allowed us to accurately model the geometry of the concrete dam and the foundation rock, which had orthotropic properties. The triangular elements allowed a fine mesh to be used in areas of high stresses, and larger elements were used where the stresses were low or constant. Temperature changes were used to predict the crack opening.

By the time Ian joined the Norfork Dam project, the IBM 704 was replaced by the IBM 7090, which had the FORTRAN language compiler. Therefore, Ian did not have to learn the complicated machine language in order to write structural analysis programs. Also, I converted SMIS and the Finite Element program to FORTRAN, which allowed the programs

29 R. W. Clough, " The Stress Distribution of Norfork Dam," University of California, Berkeley, SESM 62-19, August 1962.

to run on any computer that supported that language.

Analysis of High-Rise Buildings

Reitherman: When did your research start on the analysis of high-rise buildings?

Wilson: It started right after I developed SMIS in 1960. Using sub-matrices, we could model thirty-story buildings. As I recall, one of the first applications was the static analysis of a twenty-story condominium concrete frame and shear wall building. It was located in San Francisco and was being designed by T. Y. Lin's firm. Ray was a consultant and I prepared the data. It only required a few days work. Ray and I checked the results and everything was in equilibrium and the displacements were realistic. After preparing a few additional plots, I drove to San Francisco to give the results to T. Y. He was very impressed with the large pile of paper that summarized the forces in all members. I showed him a plot of the lateral forces in the shear wall. The plot of shears in the wall indicated zero shears at about the sixteenth level of the building and negative shears at the top of the shear wall. He immediately indicated that the results were impossible and the analysis was wrong.

I had prepared myself for his reaction. It was time for me to give the master a short lecture on modern structure analysis. I drew the lateral displacement shape a building would have if it only had shear walls. Then I drew the lateral displacement shape of a building with frames only. The two drawings were very different. I then explained to him that in a building with both shear walls and frames, both the frames and shear walls must have the same lateral displacement at each floor level. Therefore, because of the lateral displacement compatibility requirement at each floor, there is a complex interaction between the two types of structural systems. The frame is pulling the shear wall back at the top, opposite to the deflection pattern the wall would have on its own. T. Y. Lin was smart enough to understand in five minutes. In his early days, in 1931, he had made a major contribution to moment distribution.[30] He was not a stubborn man at all, he was open to new ideas. Most successful people are that way. You have to admit when you're wrong and move ahead and learn from it. You have to be willing to change.

In the next few years, I did several more analyses for his firm as an independent consultant. Also, I did a few time-history earthquake response analyses just for fun. Also, I wrote a program for them to automatically design prestressed continuous beams and girders.

In 1962, Steve Johnston, the chief engineer for Skidmore, Owings, and Merrill (and a classmate of Ray Clough at MIT), needed some analysis help with a very tall steel frame building in San Francisco for a well-known insurance company. A consultant to the insurance company had told Steve that his design used too much steel compared to the buildings he had constructed back east, where he was from. Steve had to convince him that the additional cost was due to the larger seismic environment of San Francisco. Since SMIS could

30 See the oral interview with T. Y. Lin conducted by Alex Scordelis and edited by Elearnor Swent in 1999: http://content.cdlib.org/view?docId=kt4w1003s9&brand=calisphere&doc.view=entire_text.

not analyze the large structure, Steve asked Ray if he could have Ian and I develop a very accurate new multistory building program. We had approximately a month to do the job. At that time, I had just completed my D. Eng. thesis, which was based on the fastest iteration method I could develop. I had concluded if I had used a direct band matrix solution method it may be faster. Ian and I would write the new multistory program with the option given to the user to use the Band Solver or an Iterative Solver. We found that the iterative method was only faster for a small number of load cases and the banded method was faster for many load cases. Since the iterative method could not be used for dynamics we both concluded we would use direct sparse solver for all of our future development work. This research was published in the 1963 Third ASCE Conference on Electronic Computation, held in Boulder.[31]

Lectures and Teaching as a Graduate Student

Reitherman: Did you give any lectures or teach any courses as a graduate student?

Wilson: Yes. After completing the Oroville Dam Model project, I gave a SESM Monday afternoon seminar on the project and the experimental techniques that were used. After completing my D. Eng. degree during the 1963 spring semester, before leaving for Aerojet, I gave another SESM seminar on the linear and nonlinear finite element analysis of structures. Also, after members of the local structural engineering profession started using our multistory building programs, the SEAONC asked me to give a lecture after one of their monthly dinner meetings.

During the spring semester 1962, Professor Clough was invited to go on a four-week UNESCO mission to the Mediterranean and Middle East. I was appointed a lecturer and taught four weeks of Ray's Matrix Analysis Course using SMIS. It was then I realized that teaching was hard work. What I most remember about that course were Ed Keith and Bob Feibush, who were two very smart students. After they received their MS degrees, they took SMIS to John Blume's firm and started to use stick models for the earthquake analysis of nuclear reactors. By the time I returned to teaching in 1965, they had formed their own firm, EDS Nuclear, and obtained master's degrees in business administration from Stanford. They always hired our best students from Cal. Within the next ten years, they sold the firm and both became multi-millionaires.

My First Earthquake Engineering Paper

Reitherman: Up to this point, your research has been on non-seismic topics. When did you start your earthquake engineering research?

Wilson: If you read Ray Clough's Oral History [see the appendix to this volume], you will find he was hired at Berkeley to start the Earthquake Engineering Program in the Civil Engineering Department. Therefore, since my D. Eng. was nearly completed, and I was back working on his NSF research grant, he asked

31 Ray W. Clough, Edward L. Wilson, and Ian P. King, "Large Capacity Multi-Story Frame Analysis Programs," ASCE Conference on Electronic Computations, Vol. 89, No. ST-4, August 19, 1963.

me to read Newmark's 1959 paper[32] on step-by-step dynamic analysis. He wanted me to see if the method could be extended to non-linear earthquake structural analysis.

Our son, Michael Wilson, was born in June 1961. He's now a science teacher at a local middle school. As I recall, when he was about a year old, on a sunny Sunday afternoon, Diane, Mike, and I went to Tilden Park to enjoy the nice weather and barbeque hamburgers. While the barbeque was heating, I decided to start my assignment from Ray by reading Newmark's paper. It was a very good paper and was easy to understand. However, it required time consuming iteration at each time step. After we had our hamburgers, I rewrote the Newmark algorithm in matrix form and found I could eliminate the iteration by replacing it with a banded equation solver. At that time, all of the methods used by engineers to integrate the linear dynamic equations used iterative approaches. It has been over 50 years since that afternoon in Tilden Park; however, I still clearly remember looking up at the tree tops and realizing I made a significant contribution to the field of dynamic analysis of structures. The next day, I showed the new method to Ray and he was equally surprised that it was possible to eliminate the iteration. Within a few days, I implemented the method into SMIS. We carefully compared the new method with the classical mode superposition method using the 1941 El Centro Earthquake record as the loading on an eight-story building. The results from the two methods were nearly identical. Ray and I presented the new method at a conference in Lisbon, Portugal.

32 Nathan N. Newmark, "A Method of Computation for Structural Dynamics," *Journal of Engineering Mechanics Division*, ASCE, 85, 1959, pp.67-94.

The 1962 Conference in Lisbon, Portugal

Wilson: The Symposium on the Use of Computers in Civil Engineering, SUCCE, was an International Symposium sponsored by NATO (North Atlantic Treaty Organization) and gave travel grants to young researchers from NATO countries to attend the conference and a week-long course prior to the conference. Ray was invited to be one of the lecturers and I received a travel grant to participate in the course and conference. It was a great opportunity to meet more professionals from the international community who were conducting research on the computer analysis of structures.

Ray presented our work on Norfork Dam[33] and I presented the new method for the step-by-step earthquake analysis of structures.[34] Ray's paper, based on my computer program, was perhaps the most significant paper presented at the conference. The editor of the RILEM

33 R. W. Clough and E. L. Wilson, "Stress Analysis of a Gravity Dam by the Finite Element Method," Proceedings, Symposium on the Use of Computers in Civil Engineering, Lisbon, Portugal, October 1962. (Also, published in Bulletin RILEM, No. 10, June 1963.)

34 E. L. Wilson and R. W. Clough, "Dynamic Response by Step-by-Step Matrix Analysis," Proceedings, Symposium on the Use of Computers in Civil Engineering, Lisbon, Portugal, October 1962.

bulletin[35] heard the paper and realized that the Finite Element Method was a new and very significant approach for the solution of many different types of civil engineering problems. Both Ray and the chairman of the conference gave their permission to reprint the paper in the next issue of the RILIM Bulletin.

The resulting publication in the Bulletin RILEM[35] was very significant. The recognition by this large and respected international group of structural engineers was one of the major reasons the FEM was accepted within a few years. Prior to 1963, experimental physical models, photo elasticity, and finite difference analysis methods were used to solve problems of this type. Within the next several years, most of these methods were replaced by the FEM.

The First Nonlinear, Dynamic Analysis Computer Program for Tall Buildings

Wilson: I had another consulting job in 1963 for the Los Angeles office of T. Y. Lin. A former Cal student, Lee Benuska, was employed at their LA office and had been awarded a large government contract to study the nuclear blast response of tall buildings outside the ground zero zone. At that time, there was no program in the world that could conduct such an analysis. I had completed and filed my doctor's degree thesis and was in a position to work as a full-time employee of T. Y. Lin and Associates. Ray was also retained as the chief consultant to the project. He was extremely interested in the project since he could use the same program for the earthquake analysis of tall buildings. I would use the CDC Oakland data center and work from home most of the time. Lee Benuska and I would meet at Ray's office as needed. However, it would not converge for some problems. Lee Benuska would forward these difficult problems to me for further study. After a few months, we added a series of options that improved the reliability of the program significantly, and all the structures required to be studied by the project were solved.[36]

The numerical methods and the engineering significance of a seismic nonlinear analysis were not documented until our paper was presented at the 3rd WCEE, which was held in New Zealand.[37]

Based on the nonlinear analysis of a 30-story steel frame, the New Zealand paper indicated the following three conclusions:

1. The displacements, obtained from a nonlinear time history or response history analysis, were significantly greater than a linear analysis of the same structure subjected to the same earthquake record. This conclusion is contrary

35 RILEM stands for Reunion Internationale des Laboratoires et Experts des Materiaux, Systemes de Construction et Ouvrages (International Union of Laboratories and Experts in Construction Materials, Systems, and Structures).

36 T. Y. Lin and Associates, "A Computer Program to Analyze the Dynamic Response of High Rise Buildings to Nuclear Blast Loading," Report to OCD Protective Structures Division (OCD-OD-63-44) October 1963.

37 Ray Clough, Lee Benuska, and Edward Wilson, "Inelastic Earthquake Response of Tall Buildings," 3rd WCEE New Zealand, January 1965.

to the equal displacement results based on the analysis of a one-story building that was presented by Veletsos and Newmark[38] at the 2nd WCEE in Tokyo.

2. The linear moment deformations did not provide a direct estimation of the deformations obtained from a nonlinear analysis. In addition, they varied significantly between different members of the structure.

3. If tall buildings are designed for elastic column behavior and restrict the nonlinear bending behavior to the girders, it appears the danger of total collapse of the building is reduced. This was one of the first statements on capacity-based design.

It has been over fifty years since we have proven the "equal displacement rule" has no theoretical or experimental justification. At the present time, however, far too many professional structural engineers and structural engineering faculty members continue to use this erroneous rule to justify old and new methods of "approximate" earthquake analysis. It would take only a few hours, using a program such as SAP2000, to run both a linear and nonlinear earthquake time history analysis of a complex structure to check if the rule is correct. It has been my experience that the nonlinear behavior can cause significant redistributions of the design forces and member deformations when a nonlinear analysis is used. If an engineer wants to conduct a "performance-based design," nonlinear dynamic response analysis must be conducted so that all members of the structure are in equilibrium at all points in time.

Leaving Berkeley for Aerojet

Wilson: I consider the six and a half years I spent at Berkeley the most productive period in my life. Our daughter, Teresa, was born in Berkeley on July 18, 1963. I started to work on the Apollo Space Program at Aerojet in Sacramento on August 2, 1963.

In late July 1963, a few days before I left for Aerojet in Sacramento and before Ray left for his second sabbatical leave in Cambridge, England, we met to review the research we were working on. For the first time since I met him in 1952, as a young undergraduate student, he gave me some personal advice. He said, "After completing a research project, always write a paper or report to summarize your results." I have not always followed his advice.

38 S. A. Veletsos and N. M. Newmark, "Effect of Inelastic Behavior on the Response of Simple Systems to Earthquake Motions" 2nd WCEE in Tokyo in January, 1960.

Chapter 8

Aerospace Engineer at Aerojet

If Aerojet will not allow me to write my own computer programs, I will return to the Bay Area and continue my consulting business.

Wilson: I finished my doctor's degree in December of 1962, but that was when my wife Diane was pregnant with our second child, Teresa, who was to be born in July 1963. So we decided to stay in Berkeley until that time, and I wouldn't start a permanent job until August. During that time, I would have no problem making good money doing consulting work in the Bay Area. At that time, there were very few positions available in civil engineering for a person with a doctoral degree. It was a few years later, as the nuclear power plant business began to grow, when that industry started to employ civil engineers with a doctor's degree. PhDs were already being hired in aerospace, however. T. Y. Lin, who was chairman of the U.C. Berkeley Structural Engineering and Structural Mechanics division of the civil engineering department (SESM) at that time, wanted me to stay as a temporary lecturer and teach Clough's matrix analysis course while Ray was on sabbatical leave in England. I was thirty-two years old at that time. Diane and I wanted a home of our own as soon as possible; therefore, it was easy to reject the part-time teaching appointment that had a very low salary.

Choosing an Aerospace Firm

Reitherman: How did you end up working for Aerojet, near Sacramento?

Wilson: In February and March 1963, I interviewed with three different aerospace companies. I first considered Aerojet near Sacramento, since my parents and three sisters were still living there and I was familiar with the area. Also, my former classmates Len Herrmann and Stan Dong, former students of Karl Pister, were working in a research group at Aerojet's Solid Rocket Plant, and they had a position open for me in that group.

I then interviewed with Lockheed in Sunnyvale. They offered me a position in the loads group. I asked them if I would be able to develop numerical methods and computer programs for dynamic analysis of aerospace structures. The answer was absolutely "no." Another department did all the dynamic analysis, and the research group that was located in Palo Alto did the program development.

Next, I took a flight to Los Angeles the day prior to my interview with Douglas Aircraft Company. I rented a car and drove to a nice hotel and had time to drive around LA for a few hours. Since they were paying all expenses, I had a very nice dinner and returned to my room to watch TV. In 1963, there were only local stations available. After watching the local news and other programs for the next few hours, I realized that the LA Area and the people living there were very, very different than those in the San Francisco Bay Area. Also, the smog was everywhere. By the time of the interview the next morning, I had decided that LA was no place to raise a family. However, the interview went very well and they indicated I could do almost anything I wanted. Also, their salary offer was more than 25 percent greater than Lockheed or Aerojet. But I still decided to take the offer from Aerojet. During the next 30 years, I made many consulting trips to LA. For most LA trips, however, I took an early morning flight down, and as soon as my business was complete, I took the next flight home.

Reitherman: Others on your faculty who were prominent in earthquake engineering had aeronautical experience like you, including Ray Clough, Joe Penzien, Boris Bresler, Egor Popov, Karl Pister, Jerry Sackman, and Bob Taylor. Maurice Biot, who was at Caltech in the 1930s working on the response spectrum method, had von Kármán as his advisor for his aeronautical thesis, and the earthquake content of his thesis was only one chapter. Back in your undergraduate days, you explained that the first course you took from Ray Clough was on aircraft structures. In that historical context, going into aerospace engineering and then becoming a faculty member at Berkeley doing earthquake engineering research wasn't really an anomaly.

Wilson: When I ended up in the aerospace industry, I found that the best structural analysts were educated as civil engineers. If you understand dynamics, the loads of aircraft and rockets taking off, flight loads, earthquake loads, et cetera, it's all the same, the fundamentals are the same. One of my best PhD students from the 1980s, Charbel Farhat, is now chair of the department of aeronautical engineering at Stanford, though his degree is in civil engineering. By the way, Ray also taught a naval architecture course on dynamic aspects of ship design. In some ways, I think the Berkeley faculty has become too focused on earthquake engineering, rather than applying methods to a variety of problems.

Reitherman: When you went to Aerojet, did you know what specific projects you would be working on?

Wilson: I had two classmates from graduate school at Berkeley, Stan Dong and Len Herrmann, who were already working at Aerojet, so I had some idea of what the structural people were doing there. I joined them in the same department, the research department devoted to solid rockets. There was a separate department for liquid fuel rockets. Ray Clough recalled from his summer work at Boeing that there was also a division between engineers working in different departments, in his case he worked on the displacements and forces of an aircraft, while a separate group calculated stresses. One reason I didn't go to Lockheed, in addition to my relatives being in Sacramento, was that they were going to put me in the loads department. I had already analyzed thermal stresses, dynamics, and other complex problems, and just calculating aircraft loads didn't sound very interesting to me. You have to analyze the whole structure to understand it.

Reitherman: What's the difference between rockets running on solid fuel as compared to liquid fuel? Does a solid rocket basically work the way fireworks do—you light it and it burns till it's out?

Wilson: Yes, a solid rocket is basically a controlled explosion. It's a bomb going off in a way that you've designed to make something shoot off in a particular direction.

Once you're up there in space going at a good speed, you need very little propulsion, but you need a huge lift to get you up away from Earth.

Reitherman: When you see a rocket taking off on television, you get preoccupied with the visual impact of the flames and smoke coming out the tailpipe of a rocket and maybe forget for a moment that it operates under Newton's third law, that for every action (propelling the mass of the rocket forward) there is an equal and opposite reaction (throwing mass out the back end).

Wilson: Whether it's liquid fuel or solid fuel, you need mass. You have to throw mass out, and in a rocket, it's not just the products of combustion that you can expel to make it go forward, it's whatever special blend of materials you add that the rocket motor accelerates and throws out the back.

Reitherman: What are the forces at work in a rocket?

Wilson: When you launch a rocket, you get large forces from the acceleration, for example a 10 *g* acceleration, as compared to an earthquake shaking the ground with one or maybe two *g*. In a large magnitude earthquake, the area shaken strongly can be many times that of a smaller earthquake, but the acceleration doesn't go up equivalently.

Reitherman: What about vibrations in the rocket?

Wilson: Yes. Multi-stage rockets have been the standard way to put spacecraft into orbit or to go to the moon, and at the times of separation and ignition of the next rocket motor, you get additional forces from vibrations.

My First Day at Aerojet

Wilson: During the first week in August 1963, I started to work at Aerojet. After receiving my security badge, I had a short conversation with the head of our research group, Dr. John Zickel, who received his degree from Brown University in continuum mechanics. It had been five months since I had accepted the position. The first thing he told me was that Aerojet did not receive a major development contract for a new solid rocket from NASA. He also mentioned that some groups were laying off engineers. He said Aerojet was in the production phase of building the Minuteman Rocket fleet and was making lots of money. However, they may have to reduce the size of the research group. After this cool reception, he gave me some reports to read and I sat down at my new desk in a small cubicle that I shared with another engineer I had not met before.

My old classmate, Stan Dong, was nearby; therefore, I went to his office to get the true story. He confirmed what Zickel had told me. Also, he told me that an older professor from Brown University had recently informed Zickel that the Finite Element Method, FEM, was a theoretically incorrect approach for the solution of problems in continuum mechanics and that he had made a bad decision in hiring me. Both Stan and I had a little laugh. Stan also told me the majority of the Aerojet contracts were with the federal government on a cost-plus-7% basis. Apparently, up to a few months prior to when I joined, they put many people on overtime, not because they needed the extra labor but because the company could pick up that 7% on the higher labor cost. This was the military-industrial complex at its worst. People told me that they came in on Saturday as they were told to do, but ended up sometimes playing cards.

Using Computers at Aerojet

Reitherman: What was the computer equipment you were using at Aerojet. Could you sit at a terminal in your office and interact with the computer?

Wilson: At Aerojet, we had an IBM 360 computer with a little larger capacity than the CDC 6400 I was using at Cal. The input was all based on punch cards. In fact, I didn't switch to terminals until I had a personal computer on my desk at home in 1979.

Reitherman: Because you were so heavily involved with computers at Berkeley, doing your own programming, what did you do with computers at Aerojet?

Wilson: I was informed that only the Computer Center Programming Staff was allowed to write programs. Engineers who wanted to develop a program would have to give their equations to a programmer. I also was told I'd be lucky to get any results within six months. At that point in time, I reached a conclusion about my career. If Aerojet will not allow me to write my own computer programs, I will return to the Bay Area and continue my consulting business.

Reitherman: When you went to Aerojet, did you know what specific projects you would be working on?

Wilson: No. The group I was working with was charged with conducting research that would benefit all projects within the Solid Rocket Plant. However, I decided to look for a real engineering problem that real engineers were trying to solve. I went to see my old friend Gene Kroy, who worked with me on the Oroville Dam project at Berkeley. Gene was working on the approximate analysis of a two-dimensional plane stress problem using a finite difference program provided by the Computer Center. I told him my D. Eng. thesis program could easily solve the problem with a minimum of errors. Gene and I took the FORTRAN deck of cards of my program to the Computer Center and asked the programmer to compile it and put it on the system so Gene could use it. I saw what was involved in compiling and loading a program on the computer system. Within a few days, Gene had solved the problem and wrote a report that acknowledged my program and my research group for producing it. In a few weeks, my boss, John Zickel, showed me a copy of the report and said "this is the first time, since our research group was formed three years ago, that anyone has acknowledged our work."

Analysis of Axisymmetric Solids

Wilson: I continued to work with Gene on other problems that he typically dealt with. It was apparent most of his problems were axisymmetric structures such as a rocket nozzles subjected to large internal pressure. However, in order to solve this type of structure, it would be necessary to modify my program. I found it was very easy to bypass the Computer Center system that was designed to prevent me from writing or modifying my programs. I just added a few extra cards in front of my job to execute an acceptable Computer Center program that did nothing but give me control of the operating system. The program then returned to the card reader to execute my real job that was coded on the rest of the deck. I continued to modify and develop programs for the next several months without any objections.

Also, I helped Len Herrmann and Stan Dong write their own computer programs without using the Computer Center programmers. It took Len less than a week to master the FORTRAN language, which did not surprise me. When Stan Dong, Bob Taylor, and I were at Cal, we considered Len one of the smartest engineers we ever met. Also, he was a great third baseman on the softball team at that time.

Photoelastic Stress Analysis

Wilson: When I got to Aerojet, the group of engineers doing photoelasticity analysis was in the same building as our mechanics research group. We were both interested in comparing the results of stresses calculated by the finite element method with those calculated from photoelasticity. Therefore, we started to work on the same problems together. The first result of our joint research was the development of a special purpose program for the analysis of solid propellant rocket grains.[39] Solid rocket fuel is composed of a number of carefully formed grains that combust in a planned way.

Reitherman: While the photoelasticity method has become somewhat obsolete, it's pretty amazing that you can load a model and actually see, in a sense, stress, whereas usually you can only see displacements or deformations and you have to conceptualize stress as represented by numbers.

Wilson: The color bands produced by a loaded photoelasticity model are not directly proportional to the stresses at the location. In order to convert the color to stresses, a significant amount of hand calculations was required. Whereas the triangular element used at that time produced a good approximation of the stresses at the center of each element. Photoelasticity only works for plane stress problems, so you can't figure out stresses in the other direction. You might have a model that's a quarter of an inch thick representing the real structure that might be six inches thick. Because it's unconfined, the stresses in the photoelastic model would come out to be less than what I got out of my finite element analysis.

The problem we used to compare the accuracy of the stresses produced by the finite element method with the stresses obtained from a photoelasticity analysis was of a solid rocket grain. The photoelasticity method had limitations. It cannot easily be used to calculate dead load or temperature change stresses. For many problems, the photoelasticity approach can produce accurate results. However, creating the plastic model and loading equipment is time consuming compared to the preparation of a finite mesh. After this study was made, I created a special purpose finite element program for the analysis of rocket grains. The user was only required to define the location of the nodes on the internal boundary. The program automatically generated the finite element mesh, applied the loads on the internal boundary, and produced a finite element model.

Photoelasticity had been a standard laboratory approach for several decades, but by the time I left Aerojet in August of 1965, after two years there, the photoelasticity analysts were essentially converted into finite element modelers.

Later when I was on the faculty at Berkeley, a visiting Russian scientist explained he was from a laboratory that had 500 engineers doing photoelastic analysis work. They were very slow to change. When the cold war ended, they had lots of factories building tanks, and to keep the workers employed, they had them keep on building tanks. People are slow to adapt to technology and don't like to change, so they find reasons not to. They say, "I can do that too, using my familiar approach."

Reitherman: What was your daily work life like at Aerojet compared to the Berkeley environment you had just left?

Wilson: No one took any work home. Their weekends were completely free. When I was a graduate student at Cal, I worked right through

39 Edward L. Wilson, "Two-dimensional Stress Analysis of Solid Propellanbt Rocket Grains," Chemical Propellant Information Agency Bulletin of the 3rd Meeting of the Working Group on Mechanical Behavior, November, 1964.

the weekend, maybe 60 or 80 hours a week. During the first year at Aerojet, I had time to teach an evening course at Sacramento State College on Matrix Analysis of Structures. It was a 90-minute lecture on Monday and Wednesday nights. On the weekends, I prepared the lectures for the week and handed out ditto copies to all the students. Also, I gave them homework problems requiring the use of SMIS.

After one year of renting a house in Rosemont, we purchased a nice home in Carmichael, a suburban area of Sacramento. Since I liked woodworking and yard work, I started to enjoy the low-pressure job at Aerojet. In September of 1964, at the age of thirty-three, I got the first paid vacation in my life. Diane, Mike, Terri, and I went up to Callahan in the mountains. Callahan is a small town in the far north of California, about in the east-west middle. My brother-in-law, Steve Farrington, and I went hunting and fishing for an entire week. It was a new sensation, getting time off for a vacation and still getting paid.

General Purpose Program for the Analysis of Plane and Axisymmetric Solids

Wilson: My programs that were modifications of my thesis work at Cal were being used extensively by many engineers at Aerojet. After several months of working with engineers from both the solid and liquid plants, I decided to develop a completely new structural analysis program that would better meet the needs of the engineers. A typical problem that my old program could solve was the rocket nozzle one I mentioned earlier. The user was required to specify the location of each node and only triangular elements were possible. Also, the computer capacity was relatively small, and the computer time for a solution was relatively large. By introducing the quadrilateral element, mesh generation was easy. In addition, fully orthotropic material properties and temperature-dependent material properties were possible.

I was very pleased with my new program and was looking forward to the response from the users of my old obsolete program. As soon as I completed the user's manual, I made an appointment with my old friend Gene Croy, the first user of my old program. When I walked into his office, he was very busy attempting to meet a deadline for his current project. I gave him the user's manual of the new ASOLID program and tried to tell him all the great new options it had. He immediately responded that the old program was working great and he did not want to learn how to use a new program. I was a little upset with Gene's response; however, since I was also a typical human I understood his response. After I returned to my office, I called several other users within Aerojet and found that none were interested in the new program. Or, "if it's not broke, why fix it?" So I just continued to work on my other projects, such as solving heat-transfer problems using a new finite element method.[40]

Two weeks after trying to give my program

40 Edward Wilson, "A digital computer program for the steady-state temperature analysis of plane or axisymmetric bodies," Report No TD-44, March 1965, Aerojet, Sacramento, CA.

to Gene Croy, he called me one morning and was in a panic mode. He asked me if I could immediately increase the capacity of my old program. It appeared he prepared a finite element mesh that was larger than the capacity of the old program and wanted me to modify the old program to run the existing mesh that had taken him several days to prepare. I softly replied that it was not possible for me to increase the capacity of the old program; however, I had a new program that had larger capacity and I would immediately come to his office and show him how to use it. Fortunately, the geometry of his mesh could be defined by just a few nodes and a few more data cards would generate the mesh. Within one hour we had prepared all the data for his problem. He just had to drop off a few cards of input data to define the large problem at the computer center and pick up the results the next morning.

The next morning, prior to lunch, Gene called me and said "I just plotted up the results of the problem and everything looks great. This is the greatest program in the world. Why didn't you tell me about this program before?" I thought, how lucky can I get? Many researchers wrote papers that were recognized as significant many years after they had died. My work was recognized by the engineering profession a few weeks after it was completed. Gene was quick to tell other structural engineers at Aerojet about the capabilities of the new ASOLID program. However, I continued to add more options to the program until I left Aerojet. The final modification was to allow the material properties to be nonlinear as the load was applied.

Working on the Apollo Project

Reitherman: How did you get involved in the Apollo Project?

Wilson: The headquarters for all Aerojet operations was located in Azusa near LA. The computer programming group there had a contract with NASA to solve for the thermal stresses within the heat shield for the Apollo Spacecraft when it reentered the earth's atmosphere. After a significant amount of time, they had failed to get their program to produce any results. It became an embarrassment to the management at Aerojet since they could not complete the project. They heard of our work at the Sacramento plant and contacted us for help. The heat shield was an axisymmetric structure and could be modeled by ASOLID. However, my program could not consider the nonsymmetric thermal loading. Len Herrmann proposed expanding the non-symmetric loading into harmonic functions. This required a very minor modification to the ASOLID program to solve the heat shield problem. John Zickel and I took Aerojet's private airplane to Azusa to present them our proposal, which they approved. After two trips to the Houston Space Center during the next three months, we delivered a computer program that satisfied all the requirements of the NASA/Aerojet contract. I believe Aerojet's corporate policy of not allowing engineers to program was never enforced again.

Following the suggestion given to me by Professor Clough, I wrote a significant paper on my work on the "Structural Analysis of Axisymmetric Solids," which I presented at American Institute of Aeronautics and Astronautics (AIAA) 2nd Aerospace Sciences

Meeting in New York during January 1965. Also, it was published in the AIAA Journal in December 1965[41] after I returned to Berkeley.

Reitherman: What were your general impressions of the Apollo program?

Wilson: When I first heard of all the complexity involved in putting a man on the moon and returning him to earth alive, my first impression was: this will never work! [Laughter] There were just too many pieces to the problem. Then I realized that every little step was being tested in sequence from liftoff to landing on the moon. However, the first landing and first liftoff on the moon could not be tested in steps. It had to work the first time.

I only worked on the Apollo heat shield, whose purpose was to prevent the spacecraft from burning up as it reentered the earth's atmosphere. For the Apollo program, we were using ablative material that peeled off, taking the heated material away. That, of course, changed the geometry of the spacecraft with time. It was like building a dam, where the heat of the curing concrete being continually added to the structure was the reverse process of the heat protection process for the spacecraft. I said at Aerojet that the fancy aerospace problem I was working on was actually similar to building a dam, which by comparison to a spacecraft is a very primitive structure. Aerospace engineers didn't take it kindly when I compared dams to spaceships.

41 Edward L. Wilson, "Structural Analysis of Axisymmetric Solids," AIAA Journal, Vol. 3, No. 12, December 1965.

Reitherman: What were the other projects Aeroject was working on when you were there?

Wilson: At the same time that the Apollo program was underway, Aerojet was in the production phase on the Minuteman missile, which had a solid rocket. One advantage of solid fuel for a military rocket is that the solid fuel is almost instantly ready to be ignited, whereas a liquid fuel rocket takes time to prepare for launch. The space shuttle had solid rockets that were used in the first phase of the launch, but the shuttle's internal liquid fuel motors were also used, and after the solid fuel boosters were jettisoned, the liquid fuel motors took over and could be controlled, turned on and off, and so on. Aerojet also made the rockets for the Titan rockets that powered the NASA Gemini, Viking, Voyager, and Cassini programs. It also made the Polaris submarine missile.

The NASTRAN Proposal

Wilson: In May 1965, we received a Request for Proposal, RFP, from NASA for the development of a general-purpose structural analysis program. Len, Stan, and I reviewed the RFP. It was a three-to-five-year project to develop a *NA*SA *Str*uctural *An*anlysis computer program (NASTRAN). The NASTRAN program would have several different types of elements and have the ability to conduct both static and dynamic analysis of many different types of complex structural systems. You can look up "NASTRAN" on the web and you'll get information about it. One version of NASTRAN had over one million lines of code, whereas the program SAP IV developed at Berkeley had fewer than 2,000 lines of code.

Aerojet management directed Len, Stan, and I to prepare a proposal, and we did. However, none of us were excited to work full time for over three years on one project, which was one reason we left Aerojet. Two days before Stan and I physically left Aerojet, NASA notified us that Aerojet was one of only three companies that were asked to attend an oral interview to determine their final selection. Needless to say, no one from Aerojet attended the oral interview: the people who developed it were gone.

Returning to Berkeley

Reitherman: Why did you leave Aerojet and return to Berkeley in 1965?

Wilson: There were several reasons for leaving Aerojet. One day at work, Len Herrmann told Stan and me he was working on updating his résumé. That night I started updating my résumé. If Len and Stan left Aerojet, I would have no one to learn from. You always want to work with people who are smarter than you. Also, it was clear Aerojet was on the decline. When I started, there were 20,000 employees. Twenty months later, there were fewer than 14,000. Len Herrmann received a faculty position at the University of California at Davis, and Stan Dong joined the faculty at UCLA. This was in the fall of 1965. Exactly ten years later in 1975, Len and I had been promoted to full professors. Len was in charge of the structural engineering group at Davis, Stan had a similar appointment at UCLA, and I became Chairman of the SESM Division at Berkeley.

The most significant reason for leaving Aerojet was that Diane and I both missed the Bay Area. We had made a large number of friends while we were there. I would have no problem earning money. In the five years since we were married, we had saved a reasonable amount of money. So we decided to move regardless of a job offer. I then went down to Berkeley and talked to Ray Clough telling him I was quitting Aerojet. He said the department needed someone like me, so I applied. If approved, I would start in the Fall Semester 1965.

I quit my job at Aerojet, we sold the Sacramento house, came back to the Bay Area, and rented a house in Orinda up on a hill not far from the Cal campus. I immediately started to work with Ian King on the development of a program for the dynamic analysis of offshore structures for Shell Oil Company.

Jerry Raphael had become chairman of SESM while I was at Aerojet. A few weeks prior to the start of the semester, Jerry had me over for lunch at the faculty club. George Maslach was dean of the college of engineering at the time. He was a mechanical and aeronautical engineer. We happened to sit at the same table. The engineers had a couple of tables where they usually sat.

Apparently, my appointment was held up in his office. We talked a bit about the department, new areas, how it would develop, et cetera. Because I had been an undergraduate and graduate student there, I could converse knowledgeably about U.C. Berkeley, not just my own work.

Within a week, my letter arrived. I don't know how much that lunch had to do with it. By the way, Maslach had only an MS in mechanical engineering, and the man who was president of the U.C. system at that time, Robert Sproul, had only a BS in civil engineering.

When I left Aerojet, I was making about $15,000 a year, with great benefits; when I started on the faculty at Cal, my salary was $9,200 with meager benefits. Obviously, I didn't join the faculty at Berkeley to make money, but I've never regretted the decision. I was motivated to continue my research interests, including dynamics, and to be in the Bay Area.

Chapter 9

Back to Berkeley, on the Faculty

One professor had said, "Wilson is just like Ray Clough. We don't need another Ray Clough." And I laughed when I heard that: it was the biggest compliment I have ever received in my life, believe me!

Reitherman: Did you ever find out the reason your appointment to the Berkeley faculty was delayed?

Wilson: A few years later, Jerry Raphael told me a little of what went on in the faculty discussions about me. One professor had said, "Wilson is just like Ray Clough. We don't need another Ray Clough." And I laughed when I heard that: it was the biggest compliment I have ever received in my life, believe me! Also, I heard that a few professors thought computers were just a fad, a replacement for the slide rule. Many other universities at that time were using the same approach as Aerojet. The professor or graduate student just gave their equations to a computer programmer and waited for the results. In fact, one earthquake engineering professor in SESM, in his oral history, indicated writing a computer program was a waste of the student's time. After I joined the faculty, however, I got a very warm reception from the majority of faculty members in the civil engineering department.

The Berkeley Campus

Reitherman: What was the Berkeley campus like in 1965?

Wilson: It was during the Vietnam War, so there were frequent protests. The students didn't want to get drafted as I had been during the Korean War and its aftermath. At first, the draft board in Oakland was one of the focal points for the nonviolent protests. However, after the Alameda County Sheriffs men physically beat up the students without arresting them, the students moved back to Sproul Plaza and the Southside area. However, the entire campus was affected.

For example, I remember teaching classes when helicopters were dropping tear gas to break up protests that were blocking streets near the campus. I recall one lecture in a finite element course when I was on the stage in North Gate Hall and some of the students started coughing. I told them it was a minor disturbance and I continued with the lecture. Finally, the tear gas seeping into the room built up to the elevation where I was on the stage. I quickly said it was time to end the class. [Laughter]

This was when the new Davis Hall was being built, so we faculty were dispersed around various buildings, like Hearst Mining and the T or temporary buildings. I had a pretty good office in McLaughlin Hall. Davis Hall was completed in the fall of 1968, and we moved in. We were then called the Structural Engineering and Structural Mechanics division of the civil engineering department, SESM. Twenty of us were on the seventh floor of Davis Hall, and the four from the material group were on the fifth floor.

Structural Dynamics Research at Berkeley 1950 to 1990

Reitherman: In your paper that outlines the history of earthquake engineering at Berkeley,[42] you cite seven reasons for why Berkeley became so successful in earthquake engineering:

1. the hiring of very capable faculty in the post-World War II years who had both analytical and experimental experience;
2. the digital computers and the development of the finite element method and other numerical analytical methods;
3. California and federal funding for studies of bridges, because of the rapid expansion of the state's freeway system;
4. Cold War research funded by the Defense Department on blast analysis;
5. research on the manned space program;
6. offshore platforms and the Trans-Alaska Pipeline; and
7. the 1964 Alaska Earthquake.

It's interesting that only one of the factors is related to an earthquake, while several of the others were not directly related to earthquakes

42 Edward Wilson, "The History of Earthquake Engineering at the University of California at Berkeley and Recent Developments of Numerical Methods and Computer Programs at CSI Berkeley," *Proceedings of the NATO ARW Conference*, Opattia, Croatia, 2006.

but provided research funding and consulting experience to tune up the faculty's talents that could be applied to earthquake engineering. When National Science Foundation funding in that area began to increase, the faculty were ready to capitalize upon it.

Wilson: Personally, while I did a lot of earthquake engineering work, I never considered myself just an earthquake engineer. I was interested in many engineering areas. I believe wind engineering is a more important research area, with respect to property damage and loss of life, than earthquake engineering. However, it receives very little funding. You are correct in stating faculty members at Berkeley and many other universities with earthquake engineering programs capitalized on the significant increase in NSF funding. Another fact, which is seldom referred to, is the research of Professor Robert Wiegel. Bob joined the Berkeley civil engineering faculty in 1946 in the areas of hydraulics and coastal engineering. He was a member of the committee that recommended the establishment of the Pacific Tsunami Warning Center (PTWC) in 1949 and still remains active in the field of earthquake engineering. During the December 2004 Indian Ocean earthquake and March 2011 Japanese earthquake, over 75 percent of the life loss and property damage was due to tsunamis that were initiated by the earthquakes. Diane and I were vacationing in Waikiki during the 2011 Japanese earthquake. When the 6 foot high tsunami wave arrived at 3 am, as predicted by the experts from PTWC, the hotel management had evacuated the first four floors of the hotel and turned on all the beach lights to make sure no one was on the beach. From the 12th floor we were able to see the tsunami waves come in and out every 15 minutes.

Reitherman: Wiegel was the editor of one of the first textbooks in earthquake engineering, a compilation of chapters mostly by Berkeley professors.[43] Wiegel wrote the tsunami chapter, but beyond that, didn't he organize a Berkeley short course on earthquake engineering that was the inception of the textbook?

Wilson: Yes, it was only a short course, but it brought together a strong nucleus of the civil engineering department's earthquake engineering expertise. The first point in that paper I wrote in 2006 is that the early hires in the civil engineering department in the 1940s and 1950s had both experimental and analytical experience. Clough's career is obviously a case in point, doing finite element research and later heading with Joe Penzien to the Earthquake Engineering Research Center, EERC, and being one of the chief developers and users of the big shake table.

Having experience both in the lab and with analyses is still valuable, but maybe it is less common today. I understand the shake table that so many professors and doctoral students used in their research is today mostly used for what are called service to industry tests to certify the earthquake resistance of commercial products.

43 Robert Wiegel, ed., *Earthquake Engineering*, Prentice-Hall, Englewood Cliffs, NJ, 1970. The U.C. Berkeley authors of chapters in that book were Bruce Bolt, Jack Bouwkamp, Ray Clough, T. Y. Lin, Joseph Penzien, Dixon Rea, H. Bolton Seed, and Karl Steinbrugge.

Reitherman: When ASCE 7 was revised to include mandatory certification requirements for essential equipment in critical occupancies in high seismic regions, and by reference those requirements became part of the International Building Code, it caused a demand surge for many of the shake table facilities in the country. In particular, California's hospitals, which at the same time have been undergoing mandatory seismic retrofit requirements, comprised a big enough market for seismically certified products such as back-up generators that the manufacturers began to get in line to test their equipment.

Let's return to your point about how developing computerized engineering methods made Berkeley's civil engineering department stand out in those early years as Berkeley was becoming such a leader in earthquake engineering. Your career is obviously a case in point.

Wilson: In fact, over 50 percent of the programs the department produced were from SESM students and faculty. The Geotechnical Group sold their program directly to the users. Very few other universities with NSF grants contributed programs.

Reitherman: The EERC library, now the NISEE-PEER library, had a list of computer programs it published that anyone could acquire, in addition to its collection of books and papers.

Wilson: Over the years, a number of Berkeley doctoral students and faculty worked on most of the computer programs EERC distributed. I always thought if you develop a useful computer program, which was funded with public funds, you had an obligation to give it to the engineering profession. I use the library extensively. Unfortunately, most of the computer programs are out of date.

Reitherman: The third reason you cite for Berkeley's early rise to prominence in earthquake engineering is the simple fact that Berkeley is in California, where many freeways were being built, which required overpass bridges, and California is earthquake-prone.

Wilson: Yes, and the overpasses became more complex, with longer spans and curving in plan. The older designs for freeway bridges are the simpler type you have seen hundreds of, with columns only tall enough to provide clearance for a truck to drive under. In the 1960s, many complex interchange overpasses were built significantly taller and had many joints. Some of these structures failed in the 1971 San Fernando earthquake. With respect to the design of new roadways, I helped one of Professor Carl Monismith's students, and we wrote a paper together on the finite element analysis of pavements.

Reitherman: The fourth reason you cite is the Cold War.

Wilson: In 1963, Ray Clough and I developed numerical methods and the first large capacity computer program for the nonlinear dynamic analysis of tall buildings. That research and development project was funded by a government agency that wanted to minimize the loss of life and the collapse of tall buildings subjected to a nuclear blast located some distance away. We completed that project as consultants to T. Y. Lin International. After that work was completed, we subjected the same tall buildings to earthquake motions and found that if you designed a tall building with strong columns and weak beams, the building's resistance to collapse would be increased significantly. Ray published these research results in the proceedings of the Third World Conference on Earthquake Engineering over fifty years ago.[44] It was one of the first statements on performance-based design. It also proved the "equal displacement rule" is invalid for most building. However, it is still common practice for uninformed engineers to ignore Ray's research.

Reitherman: The fifth factor you mention is the space program, which we've talked about with respect to your Aerojet work.

Wilson: Yesterday [December 9, 2014] I had lunch with Karl Pister, Bob Taylor, Jim Kelly, and Jerry Sackman, and we talked about those old times and how much money there was. We were saying that NASA funding was like a blank check. A lot of graduate students were educated on that money.

Reitherman: Then you list the development of engineering for the petroleum industry, like offshore platforms and the Trans-Alaska Pipeline.

Wilson: Yes. Jack Bouwkamp conducted many tests, using the big machine at the Richmond Field Station, on long sections of pipeline that were filled with water. Also, Graham Powell developed a computer program for the nonlinear analysis of the pipeline. One of my students, Eduardo Rukos, wrote a computer program to simulate the dynamic behavior of the hot pipeline interacting with frozen soil. Also, I gave some lectures for Shell and Chevron on the dynamic behavior of offshore platforms subjected to wave and earthquake loading.

Reitherman: The last reason you cite is the 1964 Alaska Earthquake.

Wilson: I was working for Aerojet at that time; therefore, I really cannot comment on the reaction of the Berkeley faculty. However, I understand after that earthquake the federal government got involved in earthquake engineering research and in 1977 the National Earthquake Hazards Reduction Program was enacted to fund earthquake engineering research. Ray refers to the importance of the Alaska Earthquake in the fragment of his oral history. [That incomplete oral history is provided here in the appendix.] I visited the Magnitude 6.5 February 9, 1971 San Fernando earthquake damage right after it happened with Harry Seed and several others engineers, and that was another influential earthquake.

Reitherman: What impressed you the most?

Wilson: How close the Lower San Fernando Dam came to total failure. I had done work on a variety of dams, and of course Harry Seed was interested in the soil failure that caused damage to the dam. Harry was more interested in the damage to the Upper San Fernando Dam that had recently been rebuilt to modern

44 Ray Clough, Lee Benuska, and Edward Wilson, "Inelastic Earthquake Response of Tall Buildings," *Proceedings of the Third World Conference on Earthquake Engineering*, New Zealand, January 1965. All of the World Conference papers are available as free pdf files from the Indian Institute of Technology Kanpur: http://www.nicee.org/wcee/.

standards and had no significant damage. We agreed it was important to look at structures that survived an earthquake, not just the heavily damaged ones.

The Olive View Hospital was an example of a new structure but one with a design that just needed strong shaking to fail. The five-story reinforced concrete main building was constructed using the soft first-story philosophy. Shear walls only extended from the top down to the second floor, making the ground story a soft story. The design was based on linear elastic thinking, but reinforced concrete ground-story columns behaved nonlinearly, and they failed. The top four stories were not significantly damaged compared to the weak ground story. I remember crawling under a collapsed portion of the hospital. I thought it was ironic that in the area I was in, where the ambulances had entered under the main building, there was a small door sign that said Emergency Area. I couldn't resist picking up the memento, and I had it in my faculty office for many years, and now it is here in my home office. I remember what Ray Cough often said: "You do not make a structure more earthquake-resistant by making it weaker." For this reason he did not like to use seismic joints unless they could be justified by a very accurate nonlinear analysis—which is now easily conducted.

Teaching Assignments

Reitherman: What was your first year teaching assignment?

Wilson: In 1965 U.C. Berkeley was on a two-semester system with a break at the end of January. I taught CE 130 and CE 290G in the fall semester and CE 118 and CE 130 in the spring semester. CE 130 was the basic course in mechanics using Professor Popov's book.[45]

CE 118 was the introductory structures course for the architecture students, a big class with about 120 students that was basically on statics. They didn't have a strong background in mathematics, so it was different than the way the subject was taught to the civil engineering majors.

Reitherman: What did you think of teaching the architecture students?

Wilson: It was enjoyable. They started out not knowing what a force or a stress was. I would point out that a young lady who weighed maybe 100 pounds wearing high-heel shoes could exert a large stress over that small area of the heel when she walked. In those days, students dressed up more. Wearing tennis shoes to class wasn't quite socially acceptable then. By comparison, shortly before I retired in 1991, I was supervising a final exam for about 60 students. As I walked around the room, I started to count the number of shoes with leather soles on their shoes. There was one tall cowboy fellow with cowboy boots, and one Asian woman with soft leather shoes, but all the others had tennis shoes on.

The architects could think better in three dimensions than the engineers. Engineers still often look at a building, at least for seismic design, as having two axes, and they often analyze them separately, without having a clear understanding of its three-dimensional structure.

45 Egor Popov, *Introduction to Mechanics of Solids*, Prentice-Hall, Englewood Cliffs, NJ, 1968.

Reitherman: I taught an introductory structural engineering course to architecture students for a year at Berkeley. I thought a visual approach would appeal to the architecture students, so for every lecture I had handouts with hand-drawn sketches illustrating the loads, reactions, and so on, complemented with a few minutes of slides of that type of structure.

Wilson: I used handouts in most of my courses. I always thought it was a waste of time to write detailed information on the board and then have students copy it onto paper. The only graduate course I taught in my first year was a computer analysis course, CE 290G, previously taught by Professor Charles Scheffey. He got a government job in Washington DC a few years before my appointment, and I was hired as his replacement. I would give the students a starter set of cards to get them going on a Monday and say I wanted results by Friday. Just get the hang of it, then we'll do more things with the computer. Give the students some success to start with.

I had been a teaching assistant in 1962 for the course and had concluded it was not a modern course. I had a numerical analysis minor and had taken a few courses from the Mathematics Department. In one of the math courses they used a book that was the best numerical analysis textbook of that time. It was a book by Stephen Crandall at MIT;[46] therefore, I used his book as the basic reference book for my course and my research. I finally met him several years later when I participated in a summer course at Union College in New York. He was a great educator and a gentleman.

Reitherman: Here are the first two sentences from Crandall's 1956 numerical procedures book you just handed me: "The advent of high-speed automatic computing machines is making possible the solution of engineering problems of great complexity. This is a text devoted to such problems and the methods for organizing practical programs for their numerical solution." Explain a little more about what Crandall called "high-speed automatic computing machines."

Wilson: As you know from my recent lectures, I have defined "one computer operation" as the time to multiply two numbers together and to add the result to a third number, move the three numbers into the CPU,[47] and the time to move the result back to storage. I got this idea directly from Crandall's book when I first read it in 1960. After considering the reduction in cost of computers and the increase in speed of computers, I have concluded the cost to perform engineering calculations, during the last 55 years, has been reduced by a factor of over 100 billion times.

Crandall's 1956 *Numerical Procedures* book devoted three chapters to the static and dynamic analysis of problems with finite number degrees of freedom and three chapters on the numerical solution of continuous

46 Stephen H. Crandall, *Engineering Analysis: A Survey of Numerical Procedures*, McGraw-Hill, New York, NY, 1956.

47 CPU is the Central Processor Unit where the arithmetic operations are performed. Recently, most new personal computers have multiple processors.

systems. Of course, in 1960, Clough's Finite Element Method of modeling of continuous systems illustrated that both problems could be solved by the same method. We could easily spend several hours talking about Clough's and Crandall's contributions to both civil and mechanical engineering. But let's return to the chronology, teaching at Cal in 1965.

Reitherman: After the first year did you continue to teach the same courses?

Wilson: In 1966, we changed to the quarter system where we had fall, winter, and spring quarters. Therefore, all the courses were reduced in content or expanded into two courses. Bob Taylor, Graham Powell, or I always taught a graduate course on numerical methods at least once every year. By 1969, we finally developed a graduate finite element course and a new undergraduate course on computer analysis of structures. Ironically, I was never asked to teach the graduate or undergraduate course on dynamics, even though it was one of my major research areas. I always considered dynamics as a logical extension of statics. In fact, if you look at structural analysis computer programs, such as SAP IV or SAP2000, approximately 90 percent of the program statements are associated with creating element stiffness equations and stresses and the solution of the global equilibrium equations; whereas, approximately 10 percent of the program statements are associated with solving the dynamic response problem.

After I retired, I wrote the book *Static and Dynamic Analysis of Structures*[48] in an attempt to illustrate that statics is much more complicated than dynamic analysis. After Taylor, Powell, and I retired, they dropped the course on numerical methods. Now, most graduate students do not want, or do not know how, to write computer programs. They are very happy to use the programs developed by others. Many of the structural engineering students and professional engineers, who use these commercial programs, are unaware of the numerical methods and approximations that are used within those programs.

Fellow Faculty

Wilson: We were called the Brown Bag Mafia, because several of us faculty got together with lunch bags at lunchtime to eat. We never talked about the university. Frank Moffett, who did the surveying courses, always had jokes to tell. We did things socially together. Frank Baron at one time nicknamed us the Brown Bag Mafia, as if we were trying to run the structural group in the civil engineering department, but we were talking about anything but the university.

Graduate Students

Reitherman: Starting out in 1965 as a new professor with no research funding, how did you attract graduate students?

48 *Three-Dimensional Static and Dynamic Analysis of Structures: A Physical Approach With Emphasis on Earthquake Engineering*, Edward L. Wilson, Computers and Structures, Inc., Berkeley, CA, 2002.

Wilson: My first PhD student, and one of my best, did not require funding. David W. Murray was a professor on leave from the University of Alberta, and his goal was to get a doctor's degree from U.C. Berkeley conducting research using the finite element method. He was teaching part time in SESM and was receiving additional support from the University of Alberta. A few weeks after I arrived, he came to my office in search of a dissertation topic. Also, he was a student in my numerical methods class and was learning how to use the campus computer center.

I had a long list of analysis problems that engineers were solving with very approximate methods in the aerospace industry. One of the topics I suggested to Dave was the finite element analysis of plates subjected to large displacements. He had several years of research experience at Alberta and knew how to work independently. All I told him was "the three fundamental equations of elasticity must be satisfied in the large displaced position of the structure." He was two years older than me and had more experience in teaching and research; therefore, I also learned from him. I was able to teach him how to use modern computational methods and how to do practical research using the FEM. Less than two years after I came back to Berkeley, Murray filed his thesis in September 1967 and became my first doctoral student. I did not change one word in his thesis. He set a very high standard for my next 28 doctoral students. He returned to his teaching position in Alberta, and I was pleased to attend his retirement conference several years ago. Also, he published papers in an ASCE journal [49] and in the aerospace journal, AIAA[50] based on his dissertation research. Again, Ed Wilson was a very lucky assistant professor to have a student write two very good papers including my name as co-author.

My luck of working with very good graduate students continued until my retirement in 1991. One of the reasons was that Ray Clough established Berkeley as the birthplace of the finite element method. Also, the fact that I wrote the first automated finite element method may have attracted a few students who wanted to meet the crazy professor who wrote his own structural analysis programs and then gave them away.

Reitherman: What methods did you use to advise them?

Wilson: I typically had only one or two doctoral students at a time. You have to realize that many of my good students were on scholarships, rather than being funded by a research project. A lot of the foreign students came with a scholarship from their home countries. It gave them freedom to select their own research area and the professor they wanted to work with. I never had a university project that required the development of a computer program to be completed at a specific time. I did that type of work as a consultant. A student

49 David Murray and Edward Wilson, "Finite Element Large Deformation Analysis of Plates," *Journal of the Engineering Mechanics Division*, ASCE, Vol. 95, EM1, February 1969.

50 David Murray and Edward Wilson, "Finite Element Post-Buckling Analysis of Thin Elastic Plates," *AIAA Journal*, Vol. 7, No. 10, October 1969.

thesis should not be a project report. A good graduate student teaches you the most. All of my students were free to develop their own ideas. I did not rewrite their theses. After all, it was the student's thesis, not mine.

Because most of my good students were scholarship students, I realized I didn't need to grub around for research grants all the time. One of my Japanese students spent five years working on research with me, and all of his costs were paid by Shimizu. I had a Chinese student whose father died right before he took his qualifying exam and had to go back to Hong Kong to take care of the family and the family business. About ten years later, he came into my office when he had a full-time job at Lockheed and wanted to finish up his doctor's degree. That worked out very well, and he didn't need Berkeley funding. Most of my best students were like that, whereas some who were on Berkeley funding didn't finish up cleanly, and I would have to write up the final research reports. I love students who teach me something and tell me when I'm wrong. They have confidence in themselves.

Reitherman: Say a few words about this list of your doctoral students.

PhD Students of Edward Wilson, 1968–1991

1. David Murray, "Large Deflection Analysis of Plates," (1968). He joined the faculty at the University of Alberta.
2. Ranbir Sandhu, "Stress Analysis of a Porous Media Subjected to Fluid Flow," (1968). He joined the faculty at Ohio State University.
3. Sukamar Ghosh, "Dynamic Stress Analysis of Axisymmetric Structures under Arbitrary Loading," (1970). He worked for a nuclear reactor analysis company.
4. Peter Smith, "Membrane Shapes for Shell Structures," (1970). Smith worked for Westinghouse. Unfortunately, he died young.
5. Irag Farhoomand, "Nonlinear Dynamic Stress Analysis of Two-Dimensional Solids," (1970). Initially he worked for General Electric in San Jose. Eventually he returned to teach in Iran and was jailed and executed without trial after the Islamic Revolution of 1979.
6. Eduardo Rukos, "Earthquake Analysis of Interacting Ground-Structure Systems." (1971) Eduardo returned to Mexico, taught at the University of Mexico, and then formed his own company.
7. Jamshid Ghaboussi, "Dynamic Stress Analysis of Porous Elastic Solids Saturated with Compressive Fluid," (1971). Funded by the US Corps of Engineers. Ghaboussi joined the civil engineering faculty at the University of Illinois.
8. William Doherty, "Dynamic Response of Human Tibia," (1971). He went on to earn his medical degree at U.C. San Francisco and became an orthopedic surgeon.
9. Klaus-Jurgen Bathe. "The Structural Eigenvalue Problem," PhD Research funded by UCB Scholarship, (1971). He has been a professor at MIT for over 40 years.
10. Lindsey Jones, "Unification of the Ritz and Finite Element Method," (1973). Jones

worked as a consulting structural engineer in the San Francisco Bay Area and is currently retired.

11. Harvey Dovey, "Extension of Three-Dimensional Analysis to Shell Structures Using the Finite Element Idealization, (1974). Another of my PhDs who decided to get his medical degree; he practiced as a urologist.
12. Ronald Polivka, "Finite Element Analysis of Nonlinear Heat Transfer Problems," (1976). Polivika worked as a principal for several different local consulting structural engineering firms.
13. Jeffery Hollings, "Use of Substructure Technique for Linear Elastic Analysis," (1978). I've described earlier how he formed a local consulting structural year firm using his own software to solve large and complex structures.
14. John Dickens, "Numerical Methods for Dynamic Substructure Analysis," (1980). Dickens was employed as a research engineer at Lockheed Sunnyvale.
15. Martin Button, "Numerical Techniques for Dynamic Stochastic Structural Analysis," (1980). He is working as an independent consultant on the analysis of large unique structures, including earthquake loading.
16. Tetsuji Itoh, "Adaptive Finite Element Methods in Two-Dimensional Structural Problems," (1980). Itoh represents CSI in the distribution and support of their software in Japan.
17. Mehdi Khalvati, "Finite Element Analysis of Interacting Soil-Structure-Fluid Systems with Local Nonlinearity," (1980). He worked for EDS Nuclear in San Francisco.
18. Eduardo Bayo, "Numerical Techniques for the Evaluation of Soil-Structure Interaction Effects in the Time Domain, (1982). Bayo returned to Spain where he is teaching at University of Navarra in Pamplona.
19. Hassan Saffarini, "New Approach in the Structural Analysis of Building Systems," (1982). He returned to Jordan to teach at a university.
20. Marc Hoit, "Computer Program Development Techniques for Structural Engineering," (1984). Hoit became Vice Chancellor for Technology and CIO and Professor of Civil, Construction & Environmental Engineering at North Carolina State University in Raleigh, North Carolina.
21. Van Jeng, "Dynamic Analysis of Base Isolation Systems," funded by NSF Field Test program, (1985). He returned to Hong Kong to practice engineering.
22. Pierre Leger, "The Use of Load-Dependent Vectors for Dynamic and Earthquake Analysis," (1986). Leger is teaching at Ecole Polytechnique de Montreal, Montreal University.
23. Javier Cartin, "Build-86: A Computer Program for the Preliminary Design of Buildings," (1986). He became one of Costa Rica's leading experts on the design and construction of earthquake-resistant structures.
24. Nielen Stander, "Analysis of Prismatic Structures by Means of a Recursive

Substructure Technique," (1986). He works for Livermore Software Technology Corporation, which produces software for dynamic crash analysis of vehicles.

25. Charbel Farhat, "Parallel Computations in Structural Mechanics," funded by a UC scholarship (1987). He pioneered the use of parallel computing in the aerospace industry. I mentioned earlier that he is now chairman of the Department of Aeronautics and Astronautics at Stanford University. In 2013, he was inducted into the National Academy of Engineering.

26. Kuan-Jung Joo, "Elastic-Plastic Finite Element Analysis with Mixed Adaptive Mesh Refinement," Korean Government Scholarship, (1988). He returned to Korea to practice.

27. James Hart, "Simplified Earthquake Analysis of Buildings Including Site Effects," (1989). After receiving his degree, he has been working in the general area of dynamic analysis of structures.

28. Adnan Ibrahimbegovic, "Dynamic Analysis of Large Linear Structure -Foundation Systems with Local Nonlinearities," (1989). He is a Professor at Ecole Normale Supérieure Cachan, France. He has earned an international reputation in the general field of computational fluid mechanics.

29. Y. C. Yiu, "Reduced Vector Basis Method for Dynamic Analysis of Large Damped Linear Structures," (1990). He is now in a management position at Lockheed Sunnyvale.

The Wilson Donation Fund

Wilson: A few years after I joined the faculty in 1965, I learned it was possible to establish a Various Donor Account in a faculty's name and have it administered by the Civil Engineering Department. Therefore, in my case, if an individual or company found my programs were of value to them, it would be possible to make a donation directly to the faculty's fund. The donor must specify the faculty member could use the money for academic purposes only and that the donor did not expect to receive anything in return. To me, the "Wilson Donation Fund" was a savings account because it did not have to be spent in a specific amount of time. It was possible to pay a student salary or other fees. Or, a student and I could go to a conference and have the registration and travel expenses reimbursed within the limits specified by university policy. Best of all, I did not need to write a proposal or a final report to do research. Therefore, if a student and I had an interest in a new area of research, it was possible for us to initiate research. The fund also was able to pay for the catering services for the memorials of Professors Eberhart and Raphael.

The major donors to the fund were users of my software, which they had modified to meet their special needs. Other times, I would help a company with the application of my program to an analysis of an important problem. I would not bill them for my consulting, however, I would ask them to send a donation to my fund. One of the major donations, $25,000, was from the Ford Motor Company. The most consistent donor was Ashraf Habibullah of CSI, Computers and Structures, Inc., who added both graphics and design post processors to SAP IV and ETABS in order for the programs to be more useful to the profession. We need to talk further about the importance of Ashraf's work with his CSI company.

The most amusing donation to the University occurred in 1992 after I retired. A Japanese software company had made significant modifications to SAP IV and wanted permission, in writing, to market it in Japan. The obsolete program was over twenty years old and many other companies had resold the program without my approval. I told them they could not refer to the program as a version of SAP, or refer to the University of California, or use my name, unless they donated $25,000 to the Department of Civil Engineering, and then I would give them written permission to market their program. I did not expect to hear from them again. A few months later I received a phone call from a lawyer working in the President's Office of the U.C. system stating the software company in Japan had written to the President along with a copy of my letter. I told him that I had written the first SAP program and I had been giving it away for over 20 years, and I considered it worthless and did not expect the Japanese software company to reply to my request. Another few months passed and the same lawyer called me and asked if I would come down to his office and sign some papers. Apparently, he had convinced the Japanese software company that the program was my "intellectual property" and I would approve their request if they donated $45,000 to the Civil Engineering Department.

He then asked what account I wanted to put the money in. Since I no longer had students, there was no reason to put the money in the Wilson Fund. During that period of time, I had been donating $1,000 each year to the Professor Alex Scordelis Graduate Scholarship fund; therefore, I told him to put the $45,000 in Alex's fund. Looking back now, I believe the total donation from the Japanese Company was $50,000 and the President's Office took 10 percent for all the work required to negotiate the settlement.

I was very careful not to use university resources for my consulting work. One of the first things I did when I joined the faculty was to establish a personal account at the computer center to pay for consulting activities. Also, the department would send the telephone bill to each faculty member so they could pay for their personal calls. Rather than try to figure out what was attributable to my non-university consulting work, I wrote a personal check for the total bill including the cost of the equipment. Diane typed all my consulting reports and handled many of my university secretarial requirements.

Consulting Work While on the Faculty

Reitherman: Talk a little bit about your consulting work after you were on the faculty. While it is not very common for natural science, social science, or humanities faculty to do much consulting work, engineering faculty often do. Would you agree that experience with solving practical problems as a consultant makes the professor a better educator and researcher?

Wilson: Yes. Here's an example. Within a few months after I started teaching, Marvin Braemer, an engineer from the Walla Walla

District of the Corps of Engineers, called me to help them with the construction of Devorshak Dam.[51] I had met Marvin in the summer of 1963 when Ray Clough, Ian King, and I gave a two-week course on how to use my thesis program for analysis of structures. There were about five engineers in the course, all from the Walla Walla District. We suggested they bring in the topic of a structure they wanted to analyze with FEM. Ian or I would give a lecture at nine o'clock each morning and at four in the afternoon. The course was a great success. They all went back home and used my program on the design of Devorshak Dam and many other concrete structures.

Marvin's new problem was associated with the placement of cooling pipes within each layer as the Devorshak Dam was constructed incrementally. The use of cooling pipes to remove the adiabatic heat of hydration was common at that time. Typically, the construction specifications stated the cooling pipes would be located at some fixed interval and that water at a specified temperature would be pumped for 28 days after the layer of concrete was placed. However, the method had no theoretical basis and did not consider weather conditions at the time of concrete placement or the conduction of heat between layers of concrete. Since they were ready to start placing concrete for Devorshak Dam, Marvin wanted to know if I could develop a computer heat tranfer program to help them predict the temperatures as the dam was constructed.

I told him about my heat transfer program, which had never been used at Aerojet and that it could easily be modified to add new elements at the time a new layer of concrete is placed. Then we would continue to solve the new structure, using the previously determined temperatures as the initial conditions for the new layer. Also, it would be possible to add or remove the insulation forms at specified times. Adding the cooling pipes would be possible in the same manner. The final values of heat conduction and capacity for the various elements would be determined by field measurements. When he asked me when I could finish the program and what would it cost, I thought for a few a minutes and checked my schedule, then I said less than a month and the cost would be approximately $1,000. He said a letter of authorization would be in the mail in a few days. The next weekend, I started the program modification and finished all options the following weekend. Diane typed up the user's manual, and it was in the mail less than three weeks after I talked to Marvin. Marvin and I had a few phone calls, and he made a few changes to the program. After about six months, he called and said they were getting good agreement with temperature measured in the field. That was my first consulting job after I joined the faculty. Most important, however, the solution of a complex and real engineering problem gave me great personal satisfaction. It was similar to the feeling I had after winning a foot race over ten years previously.

51 Dworshak Dam is a concrete gravity dam on the North Fork Clearwater River in Idaho. It has a height of 717 feet and is the third tallest dam in the United States. Also, it is the tallest straight-axis concrete dam in the Western Hemisphere. Construction of the dam by the U.S. Army Corps of Engineers began in 1966 and was completed in 1973.

A few months later, I received a copy of a purchase order from the Walla Walla District to the University of California that ordered "$100,000 worth of finite element research associated with the construction of Devorshak Dam during the next five years." I went to see Ray to figure out how to use the money. At first, we were both dumbfounded. Then, we realized the cost of the dam would be hundreds of millions of dollars; therefore, they had realized our finite element programs developed at Berkeley had already saved the U.S. Army Corps of Engineers millions of dollars in construction costs. This mid-1960s experience Ray and I had was an early demonstration of the cost reductions FEM could provide by supplanting less efficient analysis methods, as well as being a tool to find answers that simply couldn't be found by other means.

The first thing we decided to do was to hire Dr. Carlos Felippa as a postdoctoral research engineer. Carlos had just completed his degree with Ray and was one of the brightest and most productive students in SESM history. Also, he enjoyed teaching our students numerical methods and the power of the Finite Element Method. Ray and I started having project meetings with the students. Later, Professor Bob Taylor and his students joined us, and structural analysis and mechanics were unified at Berkeley. We did not have a formal FEM course at that time; however, I stated "students learn the FEM by walking through the hallways of Davis Hall." A few years ago, Tom Hughes, one our best graduate students at that time, stated that "life at Berkeley was like living in an intellectual candy shop."

The Corps of Engineering project would guarantee my summer salary for the next three or four years. Also, it was the beginning of the research that allowed me to create the SAP program, which I wrote in 1969 and was published the following year. The development of the SAP[52] program just involved putting all of the elements, developed by our students, together into one program. The students I acknowledged in the report were Dr. Carlos Felippa for the development of the quadrilateral shell element, Peter Smith for the development of the two-dimensional plane element, Harvey Dovey for the development of the dynamic options, William Doherty for parts of the equation solver and beam element, and Kenneth Kavanaugh for the three-dimensional isoperimetric element. All of the work was done under my or Ray Clough's direction.

Reitherman: I've always liked the foreword to your 1970 SAP report, in which you say, right there on page one, that SAP not only stands for Structural Analysis Program, but also has the meaning of don't be a sap, or don't be foolish: "The slang name SAP was selected to remind the user that this program, like all computer programs, lacks intelligence. It is the responsibility of the engineer to idealize the structure correctly and assume responsibility for the results."

Wilson: When I was a young assistant professor, the consulting income was a necessary supplement to my meager salary at the university. Later, however, I wanted my students

52 Edward L. Wilson, "SAP: A General Structural Analysis Program," Report to the Walla Walla District of the USA Corps of Engineers, Report No. UC-SESM 70-20, September 1970.

involved in local consulting work in order to give them experience and confidence working on real structures—similar to when I worked with Ray Clough as a student. After I retired from teaching, however, I referred most consulting jobs to former students who I knew would do the best job.

Earlier in this Oral History, I promised to tell you more about my work with my millionaire friend Roy Carlson, who was born in 1900 and died in 1990. I first met Roy on the Norfork Dam project. He later recommended me for many consulting projects all over the world on the analysis of mass concrete structures. In 1969, however, we were consultants on the thermal cracking of the Snell and Eisenhower locks on the Saint Lawrence Seaway near Massena, New York. These two locks were built and managed by the U.S. Army Corps of Engineers. Their policy was to reimburse for coach airfare. For my first consulting meeting, I took an afternoon flight to New York and then transferred to a Mohawk flight to Massena and landed in a snow storm at midnight. After taking a taxi to the hotel and getting a few hours of sleep, I attended the Board of Consultants meeting at 8:30 a.m. Roy was at the meeting and appeared to have had a good night's sleep. That evening, Roy and I had a nice dinner and compared notes on how we came to the meeting and how we planned to travel home. After I told him of my long flight, he told me the best way to get to Massena was to take a coach fight to Chicago. Then, take an Air France flight going to Paris that stopped in Montreal to pick up additional passengers. On the fight from Chicago to Montreal, one could upgrade (for less than $20) to first class and have a great French dinner. After he arrived at Montreal he had arranged Corps of Engineering personnel to meet him and drive him to Massena.

The Seismic Analysis of Tarbela Dam

Wilson: In 1980, my first application of Eddie, my communication program, was associated with a consulting project on the seismic analysis of the 485-foot-high Tarbela Dam in Pakistan. It was the largest earthfill dam in the world and second largest by structural volume of concrete. The dam was completed in 1976, but earthquake loading had not been considered. We had one consulting meeting in New York City with the Engineering Group TAMS, which is now a defunct company, conducting the analysis and design of a retrofit if required. The three consultants were Clarence Allen from Caltech (who would provide information on the design earthquake motions), Harry Seed from Berkeley (who was the expert on dynamic behavior of earthfill dams), and myself (who would give them guidance on the dynamic analysis of the finite element model of the concrete structures associated with the dam using their version of SAP IV). The part of the meeting that was most interesting and entertaining for me was when Harry and Clarence negotiated the magnitude of the earthquake. Finally, the design earthquake was approximately one-half the value initially proposed by Clarence.

Reitherman: Bob Whitman once told me that if you argued with Harry Seed, you usually lost.

Wilson: After the ground motion discussion, their local structural analysis consultant, a young professor who taught at a local university in New York City, made a presentation on the results of an analysis of one of the structures using the response spectra method. He pointed to the base of one of the concrete structures and stated the stress at that point was 3,200 psi in tension using the response spectrum method. The chief engineer on the project was upset and stated "that is impossible—that concrete will crack at 200 psi." That discussion continued for several minutes. Finally, I explained to the young professor his statements were based on the assumptions concrete was a linear material and the method he used to combine the model stresses was not based on the fundamental equations of mechanics, whereas, the 200 psi maximum tensile stress estimate was based on laboratory tests of the concrete cores from the Tarbela Dam. We then took a break for lunch.

After lunch the young professor was not there. They asked me if I would do the dynamic analysis. I declined and suggested we use SAP IV for the time history analysis. I would help an engineer within TAMS, who previously used SAP IV for static analysis, to model the concrete components of the Tarbela Dam. The engineer explained he did not know much about dynamics and he did not understand the mathematics. I told him after you do one dynamic analysis, and check dynamic equilibrium at any point in time, you will realize dynamic analysis is a simple extension of static analysis.

After the formal consulting meeting was over, the engineer and I set up a dial-in account on the TAMS computer in order for me to send small text and FORTRAN files to him. Also, he could post text messages and computer output for me to review. We also could talk on the telephone if required. I suggested the best time to call would be 10:30 a.m. California time, which would be 1:30 p.m. in New York. The first program file I sent to him, after I returned home, was the input data for the dynamic analysis of a simple dam structure using SAP IV. For the next few months, I developed simple post-processing programs and taught him how to develop his own programs and subroutines. Also, he learned how to perform linear dynamic time history response analysis of any structure and to verify the results. Best of all, I did not need to return to New York for another consulting meeting. I thought "I can educate a smart structural engineer to perform seismic analysis of structures from my home office."

The week after my consulting meeting with TAMS, I attended one of our traditional "brown bag lunches" with Professors Ray Clough, Jack Bouwkamp, and Frank Moffit in Alex Scordeles's office. After lunch, I had the opportunity to ask Ray about the use of the response spectrum method to calculate the maximum tension stress at the base of a dam structure. He had many years experience as a consultant on the seismic response of concrete dams and answered my question with one word—"impossible." I responded by telling him I had concluded the same thing after the examination of the basic equations used in the method.

Field Testing

Reitherman: You were involved in the dynamic field testing of real structures at Berkeley. Who funded this work?

Wilson: NSF supported most of the work. One of my students was always involved helping with the field tests and comparing the test results with the computer results. Senior development engineer, Roy Stephens, did all the organization and field work. The project lasted over 10 years and provided a unique experience for students to evaluate the mode shapes and frequencies by shaking a real structure. The student would create the computer model and try to explain why there was always a small or large difference in the results of the analysis and the measurements.

It is interesting that we never were able to vibrate a full-scale timber structure and get decent results. There was always a large amount of friction energy dissipation in the joints near the shakers. The earthquake resistance of lightweight wood structures has been demonstrated in all earthquakes in California during the past 150 years. However, the collapse of a non-reinforced brick chimney can seriously damage a home and has resulted in deaths. My father walked the streets of San Francisco as a guard after the 1906 earthquake and observed this.

The modification by Jeff Hollings of TABS to ETABS was supported by the field test program, and we acknowledged NSF for their support. Therefore, when my request to NSF for an extension of the project was rejected, I could not understand the reason why. At the same time, NSF was funding private companies and other universities to make minor modification to ETABS. It was at this time I made up my mind I would never give away the FORTRAN statements of SAP 80 to anyone, which was 100 percent written by me. I also reaffirmed my intention to continue to give away all FORTRAN programs developed on projects funded by the university.

Earthquake Analysis of Buildings

Wilson: Shortly after I returned to Berkeley, Steve Johnston from SOM called and wanted me to serve on a dynamic analysis committee of the Structural Engineers Association of Northern California, SEAONC. I told him to send me an application for SEAONC and I would be happy to serve on the committee. I was looking forward to meeting additional local structural engineers.

Reitherman: Steve Johnston was the lead structural engineer in the San Francisco office of Skidmore, Owings, and Merrill, correct?

Wilson: Yes. I had done consulting work for him before I went to Aerojet. Also, I had shown him how he could prepare the data and run the response history analysis himself. After he did this, he started examining the results to find out which member yielded first. Then he would change the design and found that there was a redistribution of forces and other members would yield as he increased the magnitude of the earthquake record. He immediately realized the power of a response history analysis. One could easily locate the weak points of the structure for a given earthquake record. He stated, "if I can do it, anybody can conduct time-history analyses and learn how a structure behaves during a real earthquake." After that, he was a great advocate for the use of the response history method.

At that time, most of the buildings were symmetrical and rectangular; therefore, the two-dimensional programs FRMSTC and FRMDYN gave a good approximation for most three-dimensional buildings. However, I was too busy to write a good three-dimensional building program. I was maintaining several different finite element programs at that time. Every new element required the student to write a new special purpose program, and then we would write a paper on how good the element was. This was the typical approach at all universities at that time.

EERC-NISEE and the Distribution of Computer Programs

Reitherman: When did EERC start to distribute not just reports about programs but the programs themselves? I say EERC, for Earthquake Engineering Research Center, which is now PEER, Pacific Earthquake Engineering Research Center. The library also has the name NISEE, for the National Information Service for Earthquake Engineering.

Wilson: Before EERC was created in 1968,[53] we were already distributing FRMSTC, FRMDYN, SMIS, and several other programs directly from the SESM division. After I joined the faculty in 1965, I created the ability to pay for postage from my various donor accounts. Then we distributed a large number of programs prior to the existence of EERC. Since the programs at that time were decks of FORTRAN punched cards, the mailing cost could be significant.

I believe it was in 1972 when NSF initially funded the NISEE operation and the distribution of computer programs.

Reitherman: Wasn't Ken Wong the person at the EERC library in charge of that?

Wilson: Yes. Ken was yet another former Aerojet engineer. He migrated to Berkeley with Fred Peterson and they were sharing an office. At first, Graham Powell was the faculty person overseeing the initial operation of EERC's distribution of programs. After he found out part of the job involved the duplication of decks of cards and user manuals, mailing them to a customer, then billing them for the postage and duplication charges, he changed his mind. He thought the money was for research. After he failed to respond to the first request, I took over the project and hired Ken Wong. He did a great job for many years. By the time Ken retired in the 1990s, the distribution was done over the internet. At one time, the distribution of SAP IV was bringing over $100,000 a year to the university in just the handling charges, more than paying for Ken's salary.

Reitherman: I looked it up, and in the quarter century from 1972 to 1998, the EERC-NISEE library offered 112 different computer programs.

53 U.C. Berkeley established the Earthquake Engineering Research Center in January of 1968 with Joseph Penzien as the first Director. As Penzien notes in his EERI oral history, at that early point "we had the center approved, and had a director, but that was all. The whole thing was all on paper." (Joseph Penzien, *Joseph Penzien: Connections, the EERI Oral History Series,* EERI, Oakland, CA, 2004, p. 39. The shake table was up and running by 1972.

The Development of the First SAP Program

Reitherman: When did you start to develop the SAP program?

Wilson: The thinking underlying the program goes back to 1968. One day some engineers, including Joe Nicoletti from John Blume's firm, came to my office and asked me if I had a program to conduct a dynamic analysis of the Hyatt Regency Hotel to be built in San Francisco. It had members that were neither vertical nor horizontal, the in-plane rigid floors approximation was not valid, and plane stress elements would be required in addition to 3D beam elements. For the first time since I received my doctorate, I had to admit it was not possible for me to modify any of my programs or to rapidly develop a new program to help them. I was very embarrassed. I thought there must be a simple method to write a general-purpose structural analysis program for such three-dimensional problems. I thought about the problem, off and on, for over a week. Then I found the simple method I was looking for.

Most groups who were working on the general structural analysis program assumed six degrees of freedom existed at each joint—three displacement and three rotations. Then they formed the global stiffness matrix, which required a very large amount of storage and limited the capacity significantly. After they formed the global stiffness, they eliminated the equations that were not required.

My approach was to form an integer ID array that was 6 by the number of joints (or reference points) defined by the structural model. Then, as the program formed the element stiffness matrices in sequence and placed them on a low speed disk file, the program also put an integer number 1 in the ID array for the displacement degrees of freedom that existed. The program could then assign an equilibrium equation number at every location where the number 1 existed. All degrees of freedom were defined prior to the formation of the global stiffness matrix, which was then formed in banded form. This may sound very complicated; however, it required less than 50 FORTRAN statements and allowed all types of elements to be easily incorporated in the same program. To this day, every time I walk by the Hyatt Regency Hotel, I think: "this is the building that finally motivated me to write the SAP program."

A Problem I Could Not Solve

Wilson: Our daughter, Teresa, was born in Berkeley in July of 1963, two weeks before we moved to Sacramento and I stated working for Aerojet. During her first year, she developed slower than her brother Mike had, who was two years older. During her second year, she started to wake up at night crying for extended periods of time for no apparent reason. By the time we returned to Berkeley in 1965, when she was two years old, we knew she definitely had developmental problems. Therefore, we looked for help from the limited number of professionals in the field, and they confirmed what we already had concluded: Terri was mentally retarded. The professionals referred us to the local Association for Retarded Citizens, ARC. In our area, the Contra Costa ARC, or CCARC, with headquarters in Walnut Creek, was operating a preschool for mentally handicapped children within two miles from our home in El Cerrito. When Terri was four years old, she started to attend this parent-operated school. A very kind, elderly lady was the only paid teacher, and she was assisted by volunteer family members. In addition to her own volunteer activities, Diane asked me to volunteer as a facility manager who would fix anything in the classroom that needed repair. As I got more involved in the CCARC program, I realized Terri was not getting the best treatment possible.

I wrote a few letters to the experts in the Bay Area and received no reply. After follow up telephone calls, it was apparent they considered me as a parent who could not accept the fact that my child was mentally retarded, for which there was no cure. Perhaps they were correct. It was apparent the medical profession had given up on the problem. I then realized that if Terri's life was to be improved, I had to help all of the mentally handicapped obtain better care.

The CCARC was one of the most active in the state, and I was happy to join the board of directors when requested. At that time, 1970, the Association was operating two preschool programs in Contra Costa County, the one in El Cerrito and one in Danville. Also, we were operating three adult workshops in Richmond, Walnut Creek, and Pittsburgh. In the hopes of producing income, we operated a thrift shop in Richmond. For the next 10 years, I spent approximately 25 percent of my time volunteering to help various organizations serving the mentally retarded. Many weeks, including my university responsibilities and consulting activities, I worked over seventy hours. Solving human problems was much more difficult than writing complex computer programs for solving structural engineering problems. The best part of that time was meeting other parents with the same problems we had and to learn how to work with bureaucratic government officials to assume the responsibilities to fund programs for the mentally handicapped. As an engineer, one of the arguments I could make was "the money we are asking for is less than one tenth the cost of a highway overpass."

My First Sabbatical Leave

Wilson: In June 1972, I had completed six years of teaching at Berkeley and took advantage of the sabbatical leave opportunity—a year off at two-thirds pay. Also, after spending three years as an assistant and then three years as an associate professor, I was promoted to professor. The sabbatical leave program, which exists at many academic institutions, some government agencies, and a few private companies, is a great opportunity for an individual to evaluate their professional and scientific contributions in the past, to study the current problems that need to be solved, and to prepare a new approach or direction for the next phase of one's professional life. The previous six years of my life, as a professor at U.C. Berkeley, had been very enjoyable and satisfying. However, I did not have time to fully evaluate the impact of the worldwide effect of the new computers on the engineering profession. Therefore, during my sabbatical leave, I proposed to travel to Europe and Asia to attend conferences and give lectures to learn how other engineers were using the new high-speed computers at that time. This travel year was completed with a very enjoyable trip to the Fifth World

Conference on Earthquake Engineering held in Rome in June 1973.

A large group of structural engineering faculty from Berkeley attended the five-day conference, which attracted more than 1,000 engineers from all areas of the world. However, most of the brief papers (including my two 10-minute presentations) were of little technical value. Therefore, we decided to enjoy the city of Rome. Ray and Shirley Clough, Jack and Marianna Bouwkamp, and Diane and I had some great Italian dinners together. A small group of American engineers, led by Harry Seed, had front row seats at an audience with Pope Paul VI where he acknowledged the contributions of our earthquake engineering group to improve the safety of structures. Also, Jack and I joined the ladies program to attend a fashion show one afternoon. That was the first and last world conference I attended. However, I have attended most of the annual conferences of the Structural Engineers Association of California, SEAOC.

After visiting many university and research centers in different areas of the world on my sabbatical leave, I concluded that the SESM group at Berkeley was very strong in the computational mechanics area, in earthquake engineering analysis, and dynamic testing of structures in the laboratory and field.

Appointment as Chairman of SESM

Wilson: Prior to my trip to Rome in the spring of 1973, Professor Eberhart, the chairman of the Department of Civil Engineering, came to my office and asked me to be chairman of the SESM division. That was long before all the civil engineering departments in the United States began to change their names to civil and environmental engineering. Since I had been promoted to professor the previous year, I expected to be asked to be vice chairman of graduate student admissions. When I tried to change his mind, he reminded me that all professors had an obligation to accept administrative appointments. Also, he stated I had demonstrated administrative ability when I was appointed project engineer for the Oroville Dam project in 1958. I finally accepted the position when he told me Bob Taylor had accepted the vice chairman position. Bob and I had worked with each other since we first met as undergraduate students twenty years before. In addition, Janet McDonald, the secretary to the chairman, and Judy Ambrose, the secretary to the vice chairman, were very smart and capable. Therefore, the administrative job was not as difficult as I expected. Unfortunately, there was no time for me to personally develop computer programs. As I recall, over the next three years I attended many meetings, attempted to solve personnel problems, and wrote memos and letters as chairman of SESM. Also, during the same period of time, I was treasurer and president of the Contra Costa County Association of Retarded Children, which required many evening meetings and talking with the politicians and bureaucrats in Sacramento.

Chapter 10

The Evolution of SAP, NONSAP, and TABS Programs, 1972–1979

The local structural engineers were not ready to conduct three-dimensional structural analysis of buildings.

The First Version of SAP

Wilson: When the first version of SAP was released in 1970, only a few large companies, such as Bechtel and General Electric, liked it since it replaced my old ASOLID program that I had developed at Aerojet and had a dynamic analysis option. Also, the local structural engineers were not ready to conduct three-dimensional structural analysis of buildings since they were very happy to continue to use the two-dimensional programs FRMSTC and FRMDYN. I must admit, the data input for the SAP program, which required the x, y, and z coordinates of all joints, was far more complicated than the frame programs, which only required the elevation of each floor and the location of the column lines.

In addition, since each element was developed by a different student, the SAP documentations were not consistent. Also, the calculations of the mode shapes and frequencies used the approximate Ritz method. Therefore, the SAP program needed additional development work.

It took me a year, working part time, to clean up the program and the user's manual. Also, I removed the dynamic options since it was based on the use of Ritz Vectors, which most engineers did not understand. This static version of SAP was named SOLID SAP. In 1972, ninety-five percent of the structural engineers only did static analysis.

Contributions of Jürgen Bathe

Wilson: In November 1971, one of my very best graduate students, Klaus-Jürgen Bathe, completed his PhD degree thesis on "The Structural Eigenvalue Problem." His contribution was the development of a new numerical method and FORTRAN software for the calculation of the exact mode shapes and frequencies of very large mathematical models of structural systems. He had named the numerical approach the "Subspace Iteration Method." Both Ray Clough and I had been looking for a student since I joined the faculty in 1965, to create a numerical method to accurately calculate the mode shapes and frequencies for large structures subjected to earthquake loading. We had suggested this topic to several other students, however, all had failed. Jürgen Bathe solved the problem in less than one year. Also, Professor Parlett from the Mathematics Department, who was on his thesis committee, agreed the Subspace Iteration Method was a significant contribution to the field of applied mathematics.

I immediately suggested Jürgen publish his dissertation as a report in our Structural Engineering and Structural Mechanics group, the SESM series of reports with blue and yellow covers,[54] including the FORTRAN listing in the appendix. Therefore, his new Subspace Iteration Method was immediately made available to all members of the engineering profession throughout the world.

Jürgen was supported by a scholarship during the time he was working on his research. After he filed his dissertation, I requested he be appointed as an assistant research engineer to be funded from my various donor accounts to improve the dynamic analysis capabilities for the SAP program. Also, many structural engineering companies hired him as a consultant to help them implement his software in their proprietary programs.

Engineering Analysis Corporation was a Berkeley software corporation that was formed by Fred Peterson and two other employees who had worked with me at Aerojet. They had developed a relatively simple three-dimensional static analysis program called EASE, which was offered by Control Data Corporation on its CDC computers worldwide on a royalty basis. EASE had a competitor called STAR-DYN, which had dynamic analysis capability. Therefore, Fred hired Jürgen as a consultant to add dynamic capability to EASE. However, one of Fred's partners, with a degree in computer science, was in charge of making

54 K. J. Bathe, "Solution Methods for Large Generalized Eigenvalue Problems in Structural Engineering," Report UC-SESM 71-20, Department of Civil Engineering, University of California, Berkeley, November 1971.

the modifications in EASE; therefore, Jürgen and Fred made modifications to SOLID-SAP, which was a clean static version of SAP. The new program produced by Jürgen and Fred was SAP IV,[55] completed in June of 1973. In my opinion, the program and documentation was a work of art and a very significant advancement in the field of computational mechanics. Jürgen had put his heart, soul, and many nights at computer centers into the effort to produce the first large-capacity linear dynamic structural analysis computer program.

Reitherman: In the acknowledgements of the SAP IV report,[56] one sees 28 companies or agencies credited with making contributions to support the work, including big name consulting engineering firms like Agbabian Associates, Dames and Moore, Martin Associates, Pregnoff Matheu Beebe, and big corporations like Fluor, General Electric, Lockheed, and Bechtel.

Wilson: NSF basically paid just for the printing and distribution of the report. However, I was informed that later on in making budget requests to Congress, NSF referred to SAP as one of their successful projects.

During this period, it was apparent Jürgen loved his research and consulting work at Berkeley. For example, in 1970 I had developed a program for the nonlinear analysis of a rocket launch site concrete closure for the U.S. Army Corps of Engineers branch located at Vicksburg, Mississippi, where they had a large blast load simulator. The nonlinear analysis program produced very good agreement with the experimental results. I had submitted a final report and the project was complete. However, I was having difficulty completing a paper on the new time integration method. I wanted to show mathematical proof that my method would converge and had failed to do so. I finally gave it to Jürgen and asked for his help. In less than a week, he walked into my office with a smile on his face. He not only added a simple proof that I could understand, he added a discussion on the limitations of the method. The paper was ready for publication.[57] Eventually, the method was called the Wilson Theta Method and was used extensively in other programs in the world since that time. However, within a few years, Jurgen and other researchers developed more accurate methods.

The Development of the NONSAP Program

Wilson: In late 1972, the Bureau of Mines (BOM), near Denver, contacted me to develop a complex nonlinear analysis computer program to simulate the behavior of mine structures as they were excavated, reinforced, and

55 K. J. Bathe, E. L. Wilson, and F. E. Peterson, "SAP IV—A Structural Analysis Program for Static and Dynamic Response of Linear Systems," Earthquake Engineering Research Center Report No. 73-11, University of California, Berkeley, June 1973.

56 Klaus- Jürgen Bathe, Edward L. Wilson, and Fred E. Peterson, "SAP IV: A Structural Analysis Program for Static and Dynamic Response of Linear Systems," U.C. Berkeley EERC Report 73-11, June 1973.

57 E. L. Wilson, I. Farhoomand, and K. J. Bathe, "Nonlinear Dynamic Analysis of Complex Structures", Earthquake Engineering and Structural Dynamics, Vol. 1, pp. 283-291, 1973.

expanded during the sequence of operations of the mining area. Clearly, it was a multiyear project that required many man-years of time and a large staff to respond to the requests of the sponsors. Normally, I would have told them such a project could not be developed by inexperienced graduate students while they were doing their own research for a doctor's degree at the university. Then, I thought, if Jürgen was willing to be project engineer, I believed it would be possible to develop a very good, general purpose, nonlinear, dynamic analysis program that would be able to solve many different types of structures.

I talked to Jürgen to check if he was willing to accept the project engineer position, with the authority to hire the staff he needed. I made it clear to him I had other projects and had accepted the position as the chairman of SESM. Therefore, I would have a minimum amount of time to help him. Jürgen agreed to accept the responsibility of being project engineer. Then, Jürgen and I had a meeting with the BOM in Denver and they agreed with our proposal. He hired two post-doctorate engineers and, in approximately one year, produced the program NONSAP.[58] At that point in time, we became colleagues and life long friends.

I met Stephen Crandall at a 1974 summer course at Union College in New York, where I gave lectures on the finite element method. A few weeks later, Steve called me and asked whom I would recommend to be hired by MIT to teach and conduct research in the FEM area. Of course, I suggested Klaus-Jürgen Bathe. They hired him, and Jürgen has been there ever since. We were able to transfer the BOM project to MIT where he developed the powerful linear and nonlinear computer program ADINA. Jürgen has earned a very significant international reputation as a researcher and educator in mechanical, civil, and bio engineering.

After Jürgen moved to MIT, we continued to work together. He completed a book in 2008 on our work together.[59] In addition, we gave several two-day courses in Paris, Tokyo, and other places on the capability of SAP IV and ADINA. Most recently (2006), we were keynote speakers at a NATO Workshop on "*Extreme Man-Made and Natural Hazards in Dynamics of Structures*" in Opatija, Croatia. He has written a very interesting book about his life. We both had hard physical work when we were young—he in a gold mine in South Africa and I on a dairy ranch in California. Also, we are very satisfied that our research and computer programs have been used to solve real engineering problems. And we both want, to use the title of Jürgen's book, *To Enrich Life.*

The TABS Program

Wilson: As I mentioned, in 1970 after SAP was released, the structural engineers who were conducting earthquake analysis of buildings continued to use the old two-dimensional frame programs. Sometime in 1971, John A.

58 Klaus-Jürgen Bathe, Edward Wilson, and Robert Iding, "NONSAP: Static and Dynamic Response of Nonlinear Systems," Department of Civil Engineering, University of California, Berkeley, 1974.

59 Klaus-Jürgen Bathe and Edward L. Wilson, *Numerical Methods in Finite Element Analysis*, Prentice-Hall, Englewood Cliffs, NJ, 1976.

Martin,[60] who at that time had several building design offices in the Western United States, contacted me. He wanted to have a meeting with me concerning the development of a special purpose program for the earthquake analysis of buildings. The following week I had a late afternoon meeting in his Los Angeles office, with "Jack" and his senior structural engineers. Since they had a computer programming group in their office, I attempted to convince them they could add the dynamic options to my three-dimensional static building analysis program. All they had to do was move the FORTRAN statements from the dynamic analysis program FRMDYN to the three-dimensional static building analysis program. I was ready to leave the meeting and catch my 6 p.m. flight back to the Bay Area when Jack suggested we have dinner at his club, after which he would drive me to the airport.

It was a dinner I will never forget, and not because of the delicious food. I found John A. Martin was a very unique and wonderful human being. He was a U.C. Berkeley graduate of the class of 1943 and a baseball player; I was a graduate of the class of 1955 and a runner, and we both loved U.C. Berkeley. We talked about the old engineering faculty at Cal and the future importance of the earthquake analysis of structures. He stated that engineers who had used the response history option in FRMDYN were able to investigate many different designs in a short period of time. He convinced me the entire structural engineering profession would benefit from the program and that I had a responsibility to develop a three-dimensional version of the program in order for the designer to see the torsional behavior of buildings due to real earthquake displacements. Also, he said he would provide whatever funds were required to complete the project. By the time we arrived at the airport, I agreed to develop the program.

Reitherman: How do you go about writing a new computer program? I mean, what are the specific steps, where do you start?

Wilson: My approach in the development of a new program, at that time, was to write a draft of the user's manual for the program, prior to writing code. This defined the type of structures to be solved, method of solution, approximations made, definition of the input data, and the options for what results would be requested by the user. One of my rules on input data was "never ask the user to calculate a number that can be calculated within the computer program."

The development of the three-dimensional building analysis program was not a big effort. With the help of one of my very smart graduate students, Henry Harvey Dovey (Harvey), we worked two weekends and converted FRMDYN into a Three-dimensional Analysis of Building Systems, TABS, computer program.[61] Also, my good friend, Fred Peterson, typed up a clean

60 John A. Martin & Associates, Inc. is a large structural and earthquake engineering consulting firm based in Los Angeles. John A. Martin, Jr. ("Trailer"), the son of the firm's founder, John (Jack) A. Martin, has served as president of the company.

61 Edward Wilson and Harvey Dovey, "Static and Earthquake Analysis of Three Dimensional Frame and Shear Wall Buildings," UCB/EERC-72-1.

user manual along with a few examples. The final step was to give the program to a few smart local FRMDYN users to help check out the program and to get their feedback. For me the project was over at that point. I mailed an invoice to John Martin for $2,000 to cover the computer time and the consulting fees for Harvey, Fred, and myself. One of the "smart local FRMDYN users" I gave TABS to was Ashraf Habibullah.

Contributions of Ashraf Habibullah

Wilson: I first met Ashraf in my Numerical and Computer Analysis of Structures course and my Finite Element Analysis course in 1969 and 1970. Both of these courses had over 50 graduate students. However, he often dropped by my office to ask questions about something I stated in one my lectures. He was not afraid to ask a simple question, indicating that he did not understand something I said. Therefore, I got the impression that he was not very smart. However, after working with him the next several years, I realized his ability to admit he did not know everything was really a very smart approach to learning.

After Ashraf received his MS degree, he went to work for two well-known engineers in Oakland.

Reitherman: David Messinger and Frank McClure? I heard Ashraf talk about that experience when he said a few words about Frank at Frank's memorial service back in 2004.

Wilson: Right. Dave Messinger and Frank McClure. Ashraf came to my office when he started working there and asked how to run the FRMDYN program. At this point, he seemed to still be learning programming slowly. About three weeks later, I got a call from Frank McClure about something else, but he said, "That guy Ashraf—he's a real worker. He has solved a lot of analysis problems for us." I thought to myself, if Frank had called me as a reference when they were thinking of hiring Ashraf, I might not have recommended him!

Reitherman: I've heard you tell a story about being a young boy climbing up the tower to the family's windmill to move the vanes by hand to pump water to the farmhouse when the wind wasn't blowing. And you end up famous for your work in a high-tech computer field. To hear that Ashraf initially was hesitant about using computers is another surprise.

Wilson: That's right. And he was shy. When he was a student, he seemed hesitant in the way he was learning about computers. He appeared to lack confidence.

Reitherman: Wow, that's also hard to believe. We've seen Ashraf descending the grand stairway at San Francisco City Hall at his annual CSI party, wearing a glittering jacket that usually has dozens of little lights on it, singing loudly, greeting everyone with exuberance—it's hard to imagine him a shy graduate student. When did Ashraf, the shy graduate student from Pakistan attending U.C. Berkeley, turn into the confident entrepreneur we know today?

Wilson: I think confidence comes with success. He became very successful with CSI, and that gave him confidence. I said earlier that my running successes on the track in college gave me more confidence in my studies, and my grades improved. There's no direct connection between track and engineering, except that when you are confident in one area of your life it tends to spread over other areas.

Reitherman: Ashraf is also known as a big supporter of the arts.

Wilson: Ashraf is a great lover of music and dance, in particular. In fact, I hear he recently advocated that engineers should first get a liberal arts degree and then proceed with their engineering education, but that would add at least three years to their educational sequence, and it's already an economic burden on many families for a student to complete the undergraduate degree in four years. I think engineers should be well-rounded, but you can learn about music, dance, theater, art, and history without studying it with professors in college. You can't pick up engineering that way.

When they didn't have enough work for him, they let him work in the office on other firms' projects. By 1975, he set up CSI as his own company. He had fallen in love with the computer and became very good at it.

Of course, originally, there was only one developer, me. And then with Ashraf, we were a two-man firm. I should mention Fred Peterson. He was a co-author of SAP IV.[62] He was another former Aerojet engineer. I met him in 1963 in graduate school just before going to Aerojet. He came to Aerojet a year later than I, and ended up maintaining my programs there when I left. By about 1970, he decided to move back to the Bay Area from the Sacramento area, and he joined forces with Ashraf. Peterson had started a little firm called Engineering Analysis. We three used to have a beer after work on Friday afternoons. Ashraf and Peterson shared an office down on Shattuck Avenue in Berkeley. He had a stroke and was in a coma for six months. Remarkably, he came out of the coma and was recovering quite nicely. Ashraf visited him at his home. The doctors decided he needed an operation in his head to correct some kind of circulation problem they thought would lead to another stroke. Unfortunately, the surgery made him worse and he declined thereafter. Ashraf at this time moved to an office on University Avenue, and he moved Fred's office with him with the hope Fred would come back, but he never did. Fred was an excellent programmer and did very clear documentation. We worked together but no money was exchanged.

In 1974, he started doing a U.C. Berkeley Extension course on the use of the programs. He was a good teacher, partly because he was a professional engineer. I'm not. I don't keep up with all the many building code changes, for example. I'm not interested in all the details that are the results of negotiations in committees, which are judgmental. I've tried to tie my work to the essential physical principles such as equilibrium, compatibility, and force-deformation relationships.

Reitherman: When you gave two lectures for the Structural Engineers Association of Northern California in 2014, you said that you worked with the development of programs with Ashraf partly because he was willing to send money back to the university.

Wilson: When he first started working after his masters degree, he was in no position to

62 Bathe, Klaus-Jurgen, Edward L. Wilson, and Fred E. Person, "SAP IV: A Structural Analysis Program for Static and Dynamic Response of Linear Systems," EERC 73-11 June 1973.

send money. Within the past year, however, CSI established the Ed and Diane Wilson President's Chair in Structural Engineering at Berkeley. Also, he established a CSI Graduate Fellowship to support students in structural engineering.

Ashraf also helped the University in many different ways. He started to modify the computer programs distributed by NISEE and added new options to the program and gave the modified programs back to NISEE so that all engineers would have the latest version of the program. Also, he helped Jürgen Bathe verify example problems for NONSAP, just to learn how to use the program. Second, he developed post processors such as graphics and design code checks to add to the programs. Of course, he distributed some of these post processors directly to companies for a fee to cover his development cost. This is when he started to send money back to my Donor Fund.

He also gave university extension courses, from 1974 to 1991. Prior to 1974, I was spending a lot of time giving lectures to local groups, and I encouraged Ashraf to continue his teaching. He needed the approval of a faculty member for the extension course to be offered. Since I was chairman of SESM in 1974, he needed my approval. He called me one day and asked me to have lunch with him to discuss a proposed extension course on the use of ETABS. Since he had been using the program for over two years, he understood the theory and approximations used in the program. I realized that he was the ideal person to bridge the knowledge gap between my research and the needs of the structural engineering profession. I approved the course with optimism about what he would accomplish. He taught the course over the next ten years.

Reitherman: In the 1980s, for a few years I taught a U.C. Berkeley Extension course on introduction to earthquake engineering, with structural engineer Bill Holmes and geologist Bob Nason. You don't make any money doing that, but you have an interesting audience. Everybody is in adulthood beyond their college years, they're sitting there because they went out of their way to spend their evening that way, they already have jobs, and you're giving them some information they can use right away.

Wilson: I think the Extension Division course Ashraf gave was especially good in that same way because a lot of engineers hadn't had the opportunity to learn about computer applications when they did their structural engineering university work. He enjoyed seeing high-up engineers in the class, senior engineers. I recall him saying he had the president of the Structural Engineers Association of Northern California in the audience, for example. It was rewarding to him to spread his knowledge widely.

Reitherman: Did you teach any Extension Division courses?

Wilson: No, I continued to give educational lectures to large groups of engineers in structural engineering in many different areas of the world and special lectures like I did in the two lectures for the Structural Engineers Association of Northern California in September 2014 at the age of eighty-three. In 1969, when I gave the first finite analysis course at U.C. Berkeley, a two-hour lecture on Thursday afternoons, the Extension Division allowed any engineer, for a fee, to take my course with consent of the professor. As I recall, an engineer from

Southern California registered for my class through the Extension Division and flew up from Southern California each week to attend the Thursday afternoon lecture.

CSI is one of the few software development companies that is ISO-certified. You have to document communications about maintenance of the program. Someone comes around once in a while to audit you. Program verification, notifying customers, and so on, is a lot of work. There are only about twenty engineers on the payroll in the Berkeley headquarters office, maybe 40 worldwide. This is not counting the sales force.

Ashraf has accomplished a lot since he came here in 1969 as a student who was initially afraid of computers. Being the head of CSI, with its worldwide prominence in the field of computer programs for structural engineering, is quite an accomplishment.

Jeffery Hollings and the Development of ETABS

Wilson: The basic approximation made in the development of the TABS program was to model a building as a system of two-dimensional plane frames interconnected by an in-plane, rigid diaphragm at each floor level. If a column was common to more than one frame, the user was required to manually combine the forces from adjacent frames to determine the total loads on the columns. Ashraf had developed post processors to reduce the human calculations required. Also, there was a fundamental problem with the compatibility of axial deformations for the columns that were common to more than one frame. If the engineer had used SAP IV, none of these problems would have existed.

One of my very good students, Jeffery Hollings, who was working on the field testing of a tall building, reported he was having difficulty with the comparison of the frequencies measured in the field with those produced by TABS. When he used SAP IV, the agreement was very good.

Reitherman: Did you convince the profession to stop using TABS and tell them to use SAP IV?

Wilson: At that point in time, I had nearly 20 years of experience working with my engineering colleagues, and I was convinced that as long as they got reasonable numbers from TABS, they would not invest in learning a completely new program. Therefore, I asked Jeff to replace the TABS beam and column elements with the three-dimensional frame member from SAP IV. The new version was named ETABS[63] since it was an *E*xtended version of TABS. The input data for ETABS did not change significantly; therefore, most users changed to ETABS. During this project, Jeff learned how to program efficiently in FORTRAN. For his PhD, he developed a very large capacity structural analysis program using a new method of substructure analysis. After graduation, Jeff formed his own software company. For a short period of time, from 1975 to 1984, Ashraf, Fred Peterson, and Jeff shared space in the same office building in Berkeley and used the same prime multiuser computer.

63 Edward Wilson, Jeffery Hollings, and Harvey Dovey, "Three dimensional analysis of building system (extended version)," UCB/EERC-75/13.

Chapter 11

The Transition from Main Frame Computers to Personal Computers

In 1979, I was forty-eight years old and had lost interest in using expensive mainframe computers. Being able to work on my own personal computer at home rejuvenated me.

Replacement of the CDC 6400

Wilson: In 1978, the most powerful computer on the Berkeley campus was the 16-year-old CDC 6400, and all users had to submit their jobs in the form of punched cards or magnetic tapes. At the Richmond Field Station and on the fifth floor of Davis Hall, there were remote card readers and printers. I was chairman of the Academic Senate Committee on Computing during the period 1976 to 1979. The committee was composed of Berkeley faculty members who were the most significant users of computers for both research and teaching. Therefore, I had a working knowledge of what was the state-of-the-art of computing on the Berkeley campus. Our assignment was to make a recommendation to the vice chancellor in charge of computing on what computer should be purchased to replace the old CDC computer.

Many departments had purchased multi-user mini computer systems, such as the VAX-11/780, which was first produced by Digital Equipment Corporation (DEC) in 1977. The VAX had a 32-bit memory and instruction set with "virtual address extension" where blocks of memory were automatically transferred to and from lower speed disks. The approximate cost of a VAX was $100,000 to $200,000 and could support any number of individual users. I personally started using the VAX in 1978 and found that the basic floating point speed was approximately the same as the old CDC 6400, with only one person using the computer. However, when over 10 people were using the VAX, it ran very slowly. Sitting in front of a terminal waiting for results for simple problems was unacceptable. For the next several months, I returned to using the old CDC 6400 and found I was more productive using small decks of cards, preparing them with an old key-punch in my office. Also, running down the stairs, two stories, to the fifth floor and back several times each day was good exercise.

Our computer committee was unable to make any recommendation on what new mainframe computer to buy for general use by all departments. However, the vice chancellor continued to reject requests to use department funds to buy computers. Finally, he developed health issues and resigned. We had no vice-chancellor impediment to departments buying computers, but the purchasing department required a signature before they would process an order. Someone in the administration decided if the chairman of the Academic Senate Committee on Computing was willing to sign the order, the purchase was approved. From then until 1979, when I took my next sabbatical, I approved approximately two computer orders each week. Approximately four years later the administration purchased a used Cray Computer for $3 million. By that time, the researchers on campus found a way to use the large computers at the Lawrence Laboratory in Livermore. Therefore, the Cray on campus was not used significantly.

Introduction of the Personal Computer

Wilson: During the same period of time, a few students and faculty were using personal computers, such as the Commodore Pet, Apple, or CPM System with an Intel 8080 processor. In the spring semester 1979, a local structural engineer, Ron Kraft, who had a one-man office in Oakland, called me and told me he had just assembled a CPM computer system from a kit and he wanted a simple structural analysis program to check out his new computer. I said "does it have a FORTRAN compiler?" He said "Yes, Microsoft sold me one for $250."

My First Personal Computers

Wilson: After checking out Ron Kraft's CPM computer system, I discovered it was very fast compiling FORTRAN programs compared to the multi-user VAX 780, which was a $200,000 computer system with the UNIX operating system. Therefore, I assembled a CPM PC system to be used in my home office for approximately $6,000.

Reitherman: The first computer I owned was also built from a kit, I believe it was a Heath product, back in about 1984. It was like putting a model airplane together, except that you had tiny circuits to connect, wires here and there, and eventually you fit the plastic case over it and suddenly it looked like a personal computer. I think it had 64K of random access memory. Did you have one kit, or did you build your computer from various components?

Wilson: I put components from different manufacturers together, and it was an ugly mess of wires connected to different colored boxes. This messy problem was solved after several hours in my woodshop. I created an attractive walnut piece of furniture that could be easily rolled from room to room.

Communication Program

Wilson: Prior to obtaining my personal computer at home, using my terminal and low speed telephone modem, I had been using the CDC 6400 in Evans Hall or the VAX 780 in Cory Hall on the Berkeley campus. However, if I wanted to transfer a program or file from the CDC to the VAX, I had to go to campus and physically carry a magnetic tape from Evans Hall to Cory Hall. This was the state-of-the-art method of transfer of electronic information in 1979 on the Berkeley campus. Therefore, the first FORTRAN program I wrote for my PC was to transfer files from any remote computer and store it on an eight-inch floppy disk at home. Then I could send it to any other computer in the world that could connect to a telephone line. This Communication Mode program was called C-Mode and was based on the 256 different character set as defined by the US ASCII standard. This was very compatible with the 8-bit binary central processing unit within my Intel 8080 CPM computer system. Therefore, it was an immense improvement over the 80-column punched data card, which only contained capital letters, numbers, and a few other symbols.

This was a very simple form of communication between two individuals using two computers. The speed was relatively slow—only 300 bits per second. To send and receive verification of one 8-bit character could take over 0.10 seconds over standard telephone lines. However, this was prior to the beginning of the development of the modern internet. It was not until 1981 that NSF started funding the development of the Computer Science Network (CSNET), which did not become operational until several years later.

Eddie—The Full-Screen Editor and Word Processor

In writing the C-Mode program, I learned how easy and fast it was to work with 8-bit character data using the FORTRAN language. Therefore, it was very easy to display over 20 lines of data on the screen. Then, I could move the cursor from line to line and move back and forth on the line deleting or inserting characters. Therefore, I could prepare or edit a file very fast without being connected to a slow telephone line. This editing option was incorporated into C-Mode and the new program was renamed Eddie. At that time, the

UNIX system had a program named Troff[64] used to print documents (files) to different printers. I added several of the most important Troff commands, indicated by a period in a column, to Eddie. This allowed me to print technical papers and reports, as shown in a typical EERC Report.[65] After over 30 years, many of my former students still tell me Eddie was the easiest editor and word processor they ever used.

Impact on SESM Educational Program

Wilson: In the summer of 1980, Radio Shack was selling a desktop computer using the CPM operating system and had one eight-inch drive for approximately $3,000—50 percent less than what I had paid for my home computer. Bob Taylor was chairman of SESM at that time and obtained $20,000 of equipment funds to buy six Radio Shack computers and a printer. This new equipment was used to create a personal computer lab on the sixth floor of Davis Hall, where the punched card terminal to the CDC 6400 was previously located. Therefore, when I returned to teaching in the fall of 1980, all of my undergraduate and graduate classes could use the power of personal computers to solve real structural engineering problems. At that time, the Unix terminal only used a line editor and ran very slowly during most of the day; whereas, the personal computer ran at the same fast speed 24 hours a day.

Several SESM doctorial students purchased PC computers in order to conduct their research at home. I then decided to stay home approximately one day a week. At that time, Diane was working full time; therefore, I had a quiet place to work and meet with my graduate students who could easily park in front in my house. After a few years, I concluded graduate students who owned a personal computer always finished several months earlier than those using the Unix system on the Berkeley campus. Of course, the students who were also married and had children always completed in the shortest amount of time possible.

Development and Support for SAP 80

Reitherman: Why did you decide to develop a new structural analysis program, SAP 80, to replace SAP IV?

Wilson: First, I realized the development of computer programs or subroutines on my PC could be accomplished approximately ten times faster than using any other computer system that existed at that time. Second, the numerical methods and finite elements of SAP IV were based on ten-year-old technology. Being able to work on my own personal computer at home rejuvenated me.

During my sabbatical year, 1979 to 1980, I worked a minimum of 60 hours a week and

64 Troff can trace its origins back to a text-formatting program called RUNOFF, written by Jerome H. Saltzer for MIT's CTSS operating system in the mid-1960s. The name allegedly came from the phrase "I'll run off a document." Bob Morris ported it to the GE 635 architecture and called the program ROFF (an abbreviation of runoff).

65 "New Approaches for the Dynamic Analysis of Large Structural Systems," E. L. Wilson, I UCB/EERC-82/04, June 1982.

loved every minute of it. I had converted my teaching and research program, SMIS, to the PC program CAL (Computer Assisted Learning). In addition, this was when the first version of SAP 80 with frame elements only and with the ability to conduct dynamic earthquake response of small structures was created. SAP 80 was a completely new program based on new and more accurate finite elements, and fast numerical methods, with the ability to run on inexpensive personal computers. I did not use parts of previous versions of the first SAP series of the programs. Ninety-nine percent of SAP 80 was personally written by me in my home office. Therefore, if a bug existed in the program, I knew whom to blame. When I was marketing SAP 80 and anybody found an error, I felt like I should pay them, because they were helping me. A lot of programmers and developers have that human element of not wanting to change. I must admit I came from being a very bad student as a young person, so I was wrong a lot. Even today, if I run into somebody at the grocery store and we slightly bump, I immediately say I'm sorry, assuming it's my fault. Other people immediately assume it was the other person's fault.

The first version of SAP 80 was produced in less than nine months, and I had a user's group of approximately 100 users of the program. These users required support. I helped them with modeling their structures. They were small firms designing relatively simple structures who had been using large expensive computer service bureaus to do their structural engineering work. I worked from my home office and did not charge a large fee. However, they gave me valuable feedback to tell me what the program needed. Also, there were several international users who wanted to be resellers and support the program in their own countries. These were problems I did not know how to solve.

IBM Changed the PC World

Wilson: In 1981, the PC world changed significantly when IBM launched the Personal Computer (IBM 5150) with FORTRAN and the DOS operating system developed by Microsoft. My first reaction was it was more expensive than my CPM system and the 5.25-inch disks had less capacity than my 8-inch disks; therefore, I was not immediately interested in the new IBM PC. However, Ashraf Habibullah was very excited and stated "IBM will sell millions of these computers to large structural engineering companies who would never have Radio Shack computers in their offices." The IBM entry into the PC market caused all the small PC companies to start making PC clones or go bankrupt. Ashraf started to convert the latest version of ETABS to the DOS operating system and asked me about forming a new structural software corporation named SAP, Inc. where we would be equal partners.

Modification of Partnership Offer

Wilson: After thinking over the partnership offer from Ashraf for a few weeks, we decided it would be best to form separate corporations where SAP would conduct most of the research and CSI would enhance the products, support the users, and market the software. This would allow me to maintain my position as a professor at Berkeley and continue to do research and consulting not related to CSI. During the next thirty-five years, as times changed, we have modified our royalty agreement several times to our satisfaction. This would not have been possible without the mutual respect and trust between Ashraf and me.

The Rapid Development of Personal Computers

Wilson: Within a few months after the release of the first IBM PC, the user was given the option to purchase a high-speed floating point chip, which made the computer run at a speed comparable to the CDC 6400. In addition, IBM had added a hard disk, which increased the size of structures that could be solved on the PC. However, I did not buy the new IBM computer since my old CPM system was still able to compile and verify FORTRAN programs very fast. I then gave my new FORTRAN statements to Ashraf to incorporate into the DOS version of SAP 80 that he was beginning to market. Finally, one day, one of my PhD students, Pierre Leger, came to my office and told me he had just purchased an AT&T PC, which was faster than the IBM PC and half the price at $2,200. It was a PC that was made in Italy by Olivetti. Needless to say, I immediately purchased two—one for my University office and one for my home office. Clearly, Moore's Law was working as predicted.

Reitherman: By the way, there's a mansion owned by Gordon Moore that has some window panes in it that were taken from a humble little old country house I owned in Half Moon Bay. My house and the Moore place were being remodeled at the same time by the same contractor, and Moore wanted some old-fashioned glass with the optical ripples in it for some purpose. We should note here that Moore's Law, projecting the exponential increase in the number of transistors on a chip, doubling every year and a half or two years, explains a great deal about the tremendous growth in computer devices and their capabilities. Moore was a co-founder of Intel and Fairchild Semiconductor. Fairchild was such an incubator of spin-off computer companies that they have sometimes been called Fairchildren. Moore, like you, grew up in a rural coastal town, Pescadero in his case, and, like you, ended up having a major impact on the world because of his work with computers.

Wilson: Gordon Moore graduated from CAL Berkeley in 1950 with a BS in chemistry prior to completing his graduate studies at other universities. While at Cal, he boarded at Cloyne Court, a student co-op facility on the north side of campus. I did the same thing when I transferred to Cal in 1952. Gordon and I washed pots in the central kitchen for three hours each week. Several years ago, I was asked by George Proper, then executive director of the co-op, to help him with the earthquake retrofit of their buildings. I was asked to a give an earthquake engineering lecture at the kick-off fund-raising dinner. At the end of the lecture, I started the fund-raising drive with a generous donation of $1,000. Several weeks later, George told me he called Gordon Moore and received a check for $100,000.

Kryder's Law

Wilson: In 2005, Mark Kryder, who was Seagate Corporation senior vice president of research and chief technology officer, predicted disk drive density would double every

13 months. With respect to the cost of low speed storage, I recently purchased a 32-gigabit "thumb disk" for less than $10. This device can easily store every word, program, and photo I ever produced. Therefore, for all practical purposes, the cost of data storage is zero.

Reitherman: You hear about how supercomputers are used in the sciences, to study the atmosphere, or what goes on inside a star, or the detailed sequence of events in a nuclear explosion. But I haven't heard of supercomputers being used in structural engineering.

Wilson: No, they're not used, not necessary. There aren't that many numbers to crunch. The speed of ordinary desktop computers is at the point where many analyses can be done almost instantly, and the most difficult ones are solved overnight. The cover of my book[66] contains a photo of the tallest building in the world, which was analyzed by SAP2000 on an inexpensive PC.

In approximately 2011, Intel released a single chip capable of addressing 264 locations of memory. In addition, we now can buy multiprocessor computers at a very low price. As you add more processors to a personal computer, it becomes a supercomputer. After over 50 years, the cost of conducting engineering calculations has reduced by over 100 million times.

Fred Peterson

Wilson: Fred was an excellent programmer and did very clear documentation. We worked together but no money was exchanged. This was during the time when the computer industry was just transitioning from multi-user computers like the VAX to the personal computer. The programs I developed for the personal computer would also run on the mainframes, but it wasn't the other way around.

We found that making my own programs was fundamentally different than using a program that had already been developed, at least for structural engineering research. You have the ability to evaluate different numerical procedures in order to obtain the most accurate and fastest methods to solve problems. Bob Taylor and I are in our 80s and we are still programming.

66 Edward L. Wilson, *Static and Dynamic Analysis of Structures: A Physical Approach with Emphasis on Earthquake Engineering, Computers and Structures, Inc.* Berkeley (now Walnut Creek) California, first edition 1998.

Chapter 12

A Conversation with Ashraf Habibullah and Ed Wilson

I audited your courses, Ed, but didn't take them for credit. I was scared that if I took your computer courses for credit, I would lower my grade point average!

Coming to the United States to Study at Berkeley

Reitherman: Ashraf, why don't you start off by talking about how you got to Berkeley and then got to know Ed.

Habibullah: I came to Berkeley for my masters degree in the winter quarter of January, 1970, the university then being on the quarter rather than semester system. I audited your courses, Ed, but didn't take them for credit. I was scared that if I took your computer courses for credit, I would lower my grade point average! I graduated a year later in December of 1970.

I came to the United States from Pakistan in December of 1969 and stayed briefly with a family in Marin County in Greenbrae through a program operated by the university for foreign students. It was a little strange, because I think the parents wanted to have me in the home to give their kids the experience of knowing somebody from an underdeveloped country. But for the first seven years of my life, I grew up in England, and all of my schooling after that in Pakistan was in Catholic schools, with my high school being affiliated with the University of Cambridge. The exam I took to graduate high school was a University of Cambridge exam. My teachers were very particular about the students' command of English, so my English was very pure. When I arrived, I think my host family expected someone who didn't even speak English. [Laughter]

It was even worse, in a sense, because I was good in math; both my parents had been math majors. My host family in Marin had two teenage children, and they were struggling with calculus. I had already had that subject, and my schooling emphasized memorizing things so that you could just look at a problem and solve it. I also tutored them in chemistry. It was an experience opposite to what the parents were expecting.

They were extremely impressed that I had gotten into Cal, and then later on when I was teaching your material, Ed, through U.C. Extension, they were even more impressed. Who is this guy from an underdeveloped country? I kept in contact with those parents to the ends of their lives, for another 20 years. The father worked for the Bank of America, then retired and had a job with the Warriors basketball team. He would take me to Warriors games, and I had no idea what those guys were doing on the court running around chasing a ball.

Ashraf's First Job with Messinger and McClure

Habibullah: Coming back to how I met Ed, it was really when I had already finished my masters and was out working that I got to know him.

I went out looking for work right away after I finished my degree at Berkeley and got a job with McClure and Messinger in January of 1971. It was January 28. I recall the date because within a couple of weeks, the February 9 San Fernando Earthquake occurred. The last class I finished up was Professor Anil Chopra's dynamics of structures class. Two days before the final exam I received a telegram at my apartment telling me to telephone home in Pakistan. In those days, you didn't just pick up the phone and dial an overseas number; you had to book the call. They would call you back in a couple hours to say your call was arranged. I found out that my father had just died. I went in to talk to Professor Chopra to tell him I had to leave right away for home. Anil said that I had done good work in the class, so I should just go and he would give me an A.

I was ready to stay in Pakistan with my mom, but she said no way—you go out there and earn a good living—we need the money. My mother emigrated from Pakistan to the United States.

There I was back in the United States rather than in Pakistan, so I went to talk to Professor

Chopra to tell him that I felt I should now take his exam. I took the exam, and on that basis, my A changed to an A-minus! [Laughter]

I think the unemployment rate in California then was 12 percent. There were no jobs. I would go to San Francisco knocking on the doors of engineering offices. At the bigger firms, the secretaries would never let you talk to anyone. I even played games, like saying I had an appointment with so-and-so, one of the names of the partners. It turned out the gentleman had passed away four years before! [Laughter] I remember thinking, then why does the firm's name still have his name in it! [Laughter]

On the corner of 40th Street and Telegraph Avenue in Oakland, there used to be a Safeway supermarket, and across the street was a pay phone. I had a pocket full of dimes and made a lot of calls. One of them was to McClure and Messinger. Frank McClure picked up the phone. I had fifteen bucks left in my checking account. I was desperate. I had a $60 rent payment coming up in two weeks. He had me come by a couple hours later.

When I came into Frank's office, he asked, "You know this professor Ed Wilson? We're using one of his programs on a big hospital job, STC3DF, and we're having trouble with it. Have you heard of this program?" I said, "Of course I know that program," but actually I didn't have the foggiest idea of what it was. It was a statics program, a three-dimensional version of FRMSTC that Ian King and Ed developed. Frank said he could give me a job for two weeks. He asked how much money I wanted, and I said $875 a month. He took out his slide rule and said, "That's $4.76 an hour, where did you get that figure?" I told him that was how much I needed to pay my bills.

I still had to go get a work permit, so I took the drawings and went up to the campus to see Ed.

Getting to Know Ed Wilson, Programming for Messinger and McClure

Habibullah: You handed me a three-page user's manual, Ed, and told me to go downstairs to the computer center and gave me the names of people to talk to there. When I went to work, I used the computer center on campus at night when the rates were lower; Frank had an account there. My first day of work was February 8. The next day, the San Fernando Earthquake occurred, and everybody in the office left for Los Angeles. I was alone in the office for a week to ten days. I couldn't find a problem that was making things not balance out, and Ed showed me the error. The program required the lateral forces to be put in at an angle that was clockwise from the vertical axis, whereas I was putting it in from the horizontal axis, counterclockwise. I changed a sign and it worked out.

When McClure and Messinger came back and saw that I had it all figured out, they were impressed.

Wilson: By the way, some consultants took my SAP IV program, compiled it on the Cray, and sold that product for a good sum of money. It didn't occur to them to give any money back to the university. That was about when NSF cancelled the field-testing program at Berkeley, and without research money, I said "No more free FORTRAN decks."

Habibullah: I told McClure and Messinger I had that program. I set up the model and got results that checked out in less than half a day of work.

They had problems on this job with the piling. The piles were off center. There was a lot of eccentricity on the pile caps. They were analyzing each of hundreds of piles. I wrote a small program where you put in the moment and it would add in all the other effects and check it to see if the stresses were okay. That's when they decided they were going to keep me, doing all their computer work for them.

When Frank came back from the San Fernando Earthquake, he had lots of data that we put on cards to create a database and run analyses. Somehow that work was incorporated into the state's Urban Geology Master Plan report.[67]

Reitherman: That was the study that projected what the losses would be over the next several decades from earthquakes, landslides, and other hazards. I recall the surprising finding that one of the losses that was almost as big as the earthquake loss was the loss of the mineral resources that were going to be built over by urban development.

Habibullah: The maps were all printer plots done on the computer.

Reitherman: How did you keep up with computer and other developments, when you were working full time for McClure and Messinger?

Habibullah: After I graduated in late 1970, I came back to campus over the next two years and asked some professors like Alex Scordelis and Frank Baron if I could just sit in on their classes. They said that was fine. I audited your finite element class, too, Ed. McClure and Messinger then were kind enough to let me be at Berkeley for those 8:00 a.m. classes, then take the bus to Oakland and show up at the office at 10:00 a.m. to work into the evening. I did that for two years. That was when I met Jürgen Bathe, then working on NONSAP. He was teaching a U.C. Extension class that was basically about SAP IV. It was a tough class. When I got to know Jürgen, he had me help him test NONSAP for him. That was also when I got to know Fred Peterson.

Wilson: Jürgen's thesis was a real breakthrough. I had him include the coding in it, because I knew how much circulation my thesis had because it included information that people could use. In the commercial and professional world, no one was sharing their coding.

Habibullah: When SAP IV was released, it had no graphics. McClure and Messinger had a job that included a big gymnasium. I modeled that gymnasium with over 1,700 elements. There was no way to see what the results were. I wrote a small program that read that data and made a plot.

Wilson: At that time, you could only get a paper plot, a printout.

Habibullah: And it was only printed out in Palo Alto, then shipped up to the Oakland

67 John T. Alfors et al., *Urban Geology Master Plan for California: The Nature, Magnitude, and Costs of Geologic Hazards in California and Recommendations for Their Mitigation*, California Division of Mines and Geology (now California Geological Survey), Sacramento, CA, 1973.

office the next morning. If you noticed a mistake, you had another 24-hour turnaround. I got a beautiful picture that was very helpful. It occurred to me that there were about a thousand users of SAP IV, acquiring it through the university, and that if I could get ahold of that list and contact them, I was sure that enough of them would buy the graphics package. This was 1974. At night, I bought some computer time from the CDC facility in Oakland on Grand Avenue near Lake Merritt.

My add-on program would read the deck of cards of SAP IV, which had eight types of elements. It would plot the whole structure from the coordinates. I managed to obtain the list of SAP IV users, and I mailed a letter out to them. In six weeks I sold 150 copies at eleven hundred bucks a piece: over one hundred and fifty grand in my pocket, in 1974! My pay at the office was $1,600 a month. By that time I had left McClure and Messinger, after working there three and a half years. In the fall of 1974, I got a job in Ben Kacyra's engineering company, Earthquake Engineering Systems. At the same time, you gave me a class to teach, Ed. I built a U.C. Extension course around TABS and FRAME 2 and called the course Static and Dynamic Analysis of Conventional Structures.

Teaching a U.C. Berkeley Extension Class

Habibullah: I recall approaching Ed to teach the U.C. Extension course we've talked about. Ed was the civil engineering department chair at the time. I called him up and asked him to lunch. I was still working at Ben Kacyra's office, before I started Computers and Structures, Inc. I had this little car, it was an early Honda, 500 cc, two cylinders. I took Ed to lunch at what was Solomon Grundy's, now Skates, and I was thinking he wasn't going to let me teach the course after driving him to lunch in my cheap little car, with Ed, who is so tall, trying to cram his legs in. I was anxious about my car, but at the same time I was going to be able to say, "I drove Ed Wilson out to lunch." You have to realize, Bob, that Ed was a superstar, the guy who was creating all of these computer programs that were the beginning of the use of computers in structural engineering, so I was very lucky.

I called up Nanette Pike at U.C. Extension who told me I had 187 people registered for my course, and that usually the enrollment for their courses was only about 30. They moved me to the big room in Dwinelle Hall on the campus. When I walked into the room on that first Wednesday evening—and note that I had never taught at all—there was a sea of people, and I froze. I cleaned that blackboard for ten minutes before I could turn around and face them. What made it worse, in the front row were a few engineers who had been presidents of the Structural Engineers Association of California. Somehow, the audience ended up getting it, crystal clear, and I ended up teaching that course for 14 years.

Reitherman: What did you teach with? A blackboard and photocopied handouts?

Habibullah: Yes, no slides. I took care in making beautiful handwritten handouts. Engineers who took the course would then come to me to do computer analysis for them. It occurred to me I should just concentrate on consulting to other engineers.

Starting Computers and Structures, Inc.

Reitherman: Was that the beginning of Computers and Structures, Inc.?

Habibullah: The first name of my company was actually Computers/Structures International. I wanted to use the name Computers and Structures, but there was a journal in England by that name, and I didn't know if it was legal to use that same name. I didn't want it to be called Habibullah and Associates. In 1980, I incorporated the company, which had been a sole proprietorship. I wrote the Secretary of State in California to ask if Computers and Structures, Inc. was a name I could use, and I was told I could. I recall friends saying, "What are you doing? You are going to starve on the streets." I didn't care. They say you have to have a certain level of stupidity to do things differently.

When I decided to open CSI, I was looking for office space. That was when changes were happening at McClure and Messinger. Frank had decided that he wanted to get a job with the university when they were doing a big seismic review of buildings, and he could get a pension that way. Dave called me up and said I could use Frank's old office, and for $175 a month, I could use his secretary too. In the whole creation of CSI I never took out a loan. I needed some furniture so I went down to a salvage place in Oakland and bought a desk that had fallen off a truck. I realized that if you put it up against a wall, you couldn't see that it was all broken on the backside. My mother was with me and told me I had to sit on a new chair, and she took me to Montgomery Ward in Richmond and we bought a new chair. My office was in Dave Messinger's space till 1980, when I moved to SSD, Structural Software Development. Jeff Hollings was there then. It was a big company, at 1980 Shattuck in Berkeley. I paid for time on the Prime computer they had.

Reitherman: What was it like, working with Frank McClure? I knew him from doing work for an earthquake loss estimation panel at the National Research Council, and he could be rather gruff.

Habibullah: Frank had high standards and demanded the best from his employees, but was also gracious with his time and help. When I started with Frank, I did not yet have my U.S. citizenship. In order to apply for citizenship, I had to write an essay, so I wrote it and gave it to Frank to review. He said it didn't read correctly, that "we in America don't do this, or don't do that." He spent some time over several days and completely re-wrote it for me. He did a lot for me.

Reitherman: He was a regular at EERI annual meetings, standing up near the front of the audience asking questions, or perhaps it was more like he demanded answers, during the open comment time. Do you remember that?

Habibullah: No, because in those early days EERI was a closed club, membership by invitation only, and I was not yet a member. However, I do remember him not being interested in my business plans for a software business—he thought that only professors and students at the university developed software and that there was no money in it, which was mostly true at that time.

Wilson: Ray Clough and I both thought that we had to get our programs out of the university and out in practice. You learn so much from the practicing engineers. But not everybody did that.

Habibullah: When Frank wasn't interested in the computer industry, that's when I left to work for Earthquake Engineering Systems in San Francisco. When I saw what the sophisticated nuclear industry people were doing with computers, I realized I knew as much or more than they did. If Frank had said yes, everything that has become CSI would have been Frank's. It was a decade later when the World Conference on Earthquake Engineering was held at the Fairmont Hotel in San Francisco in 1984. We had a little display set up in the exhibitors' area. I saw Frank McClure walking up to me, carrying a little red book, which turned out to be one of the very first ETABS manuals. He wanted me to autograph it. Frank McClure asking for my autograph! And then he complimented me on what I was doing with personal computers, and he went on to say that of all the mistakes he had made in his professional life, the biggest was when he said no when I went to him to offer to go into the software business with me.

Some of the things I did were so simple, and they filled a need. I took ETABS and added automatic stress checking to the analysis. Of course that saved time for the design offices. I could sell that program for $25,000 a copy. That was when I was able to start sending money Ed's way for the university. Ed and I got together for lunch and he penciled up how to do the P-delta analysis with a few lines of code. It was well worth the cost engineers paid to buy the programs and save time.

Reitherman: Both of you seem to emphasize how important it has been to understand and talk with the practitioner.

Habibullah: From working three and a half years at Messinger and McClure, I knew what the bottlenecks were in the design office process. It opened the door to the top people in the area's engineering offices. Teaching the U.C. Extension course, I gained confidence because I saw all these big shot engineers in the audience anxious to learn what I had to offer.

SAP 80

Habibullah: When Ed developed SAP 80, a completely new program, he came to me to make it marketable, with graphics and other useful features.

Wilson: SAP 80 was a totally new program. The university had nothing to do with it.

Habibullah: We created a company called SAP Inc. in 1981, which we owned 50-50. As soon as we started making money, we found that I wanted to spend all the money on enhancing the product while Ed wanted to save the money, so I kept CSI and we still split the proceeds 50-50, and each of us could do what we wanted with the money.

Wilson: Ashraf bought a Radio Shack desktop personal computer when they were just coming out, and he realized it was the future. We both found that technological development was a big stimulus for us. That was when CPM was a big operating system, but that only lasted until IBM came out with their PC, using Microsoft's operating system, DOS.

Habibullah: Remember when we put on a PC seminar at a local Marriott Hotel? We told them we sold the software for $2,500, but we would sell it to them there on the spot for $1,000. That afternoon, we came home with sixty grand in our pockets! It was magical.

Wilson: We were so far ahead of everyone else.

Expanding Internationally

Habibullah: The IBM PC opened up the international market for us. Developing countries could afford them. In Latin America, Asia, places with big earthquake problems where they hadn't been able to afford mainframe computers, they were now acquiring computers. Our software ran through those countries like wildfire. When I go to Thailand, Mexico, et cetera, I tell them they are the ones who gave CSI a big boost.

Reitherman: Aside from the personal computer's ever increasing "horsepower," storage capacity, and other improvements you read about in a computer's specifications, what was the big change it caused in engineering practice? Wasn't the ability to iterate quickly, test out assumptions and design changes, rather than visiting a mainframe computer center to do a single run in a day, a big change?

Habibullah: The engineer could run the program many times, because they weren't paying for the time. When I was working with Fred Peterson, we were using mainframes, and we had to strictly budget how many runs we could do on a consulting job. Every run could be $500 or more, so ten runs would be $5,000, not counting storage, which was a big number then. I recall one job where I left a file in storage, and six weeks later the engineering firm we were doing the job for got a bill for $15,000. The companies and employees running the big computer facilities fought the personal computer trend, it was against their economic interest. But in the poorer countries, where they had no computers to start with, they would quickly adopt the PC and use our software.

Reitherman: It sounds analogous to the way developing countries that had very little landline telephone infrastructure leapfrogged that technological step and just went straight to cell phones.

Wilson: When I went to China in the early 1980s, the U.S. Defense Department would not let Control Data sell their big computers there. I told the Chinese: you're lucky, that equipment is rapidly becoming obsolete. Skip that step. The personal computer is the future.

Reitherman: When you talk about software in China, it brings to mind the subject of piracy.

Habibullah: The amount of our software that has been pirated and re-sold is huge. You might be surprised that I think there are some advantages that have come from that. Our software has such a big market partly because of that. The underground market is a training ground, everybody knows about our programs and they become a standard. I can't actually tell you how many pirated copies of various versions of our software are out there.

Reitherman: Do you have a guess as to the proportion of legal and illegal copies?

Habibullah: I would guess one to a hundred, or perhaps one to a thousand. If you do a search for "SAP2000 cracked," you get 50 webpages of sites. We are currently focusing on ways to lock up our products and prevent piracy. If we completely do that tomorrow, we'd be a billion-dollar company. We want our own encryption technology. Now, you buy that from a third party, and if people hack their way through that product, they have access to lots of programs of other companies. If our protection technology is just CSI's, for one thing it's not worth the time of someone to try to break that open, because they only gain access to one product, not hundreds or thousands. I think some of the protection things we're doing now have resulted in a big increase in our sales.

We have different pricing for the developing countries. We used to have the same pricing all around the world, and then one year I went to south Asia to do a seminar. I knew the bigger buildings were being designed with ETABS. As keynote speaker at their convention, I asked for a show of hands, how many were using ETABS? Only about five hands went up. But I had already been to the market and saw stalls selling all kinds of DVDs of pirated movies, they even had copies of movies that hadn't been released yet. And sitting right there among the movies was a SAP2000 CD. I bought five copies, because their packaging was better than CSI's, with a hard copy of the manual, gold embossing on the cover, beautiful stuff.

I told the engineers at the convention that we sell this product for $15,000. I will give it to you for $1,500, if you buy it before I leave tomorrow night. We came home with $600,000 in our pocket. I realized we had to change the pricing. If it's beyond the limit of affordability, people just won't buy it.

We now have a formula. You take what an engineer with five years' experience makes in a month, multiply it by two, and that's the cost of the software. In America, an engineer might get $5,000 a month, so we sell the software for $10,000. In a place where the engineer is making $500 a month, we sell it for $1,000. Of course the rules get broken, international companies buy it in one country, use it in another, and so on.

Wilson: But it's better for the world if they use a pirated version of our software rather than some lower quality software.

Habibullah: People only pirate the high quality stuff. It's a backhanded compliment. It's also market share. If they are using your product, they are not using somebody else's. That's a big plus. They're training each other, entrenching our product.

What's Next?

Reitherman: When you look back, when the connection was made between Wilson and Habibullah, it was the perfect combination. Who could you hypothetically substitute for Wilson, or Habibullah, and have the story come out so wonderfully? If you two had been interested in some other topic, say design of concrete or steel structures, there would have been a number of well-qualified professors and practitioners in that area. But in this particular field of software for structural engineers, you two have been unique.

Wilson: With the PC, we didn't need to give them the source code, just the executable version, because they ran on the same operating system, unlike the previous era when you needed FORTRAN to span across different systems. And now, there is income from support.

Habibullah: We charge a maintenance fee for every product. The client gets free upgrades, free support, for 20 percent a year of the software purchasing price each year. After a while, that becomes a big source of your revenue.

Reitherman: What about BIM, building information modeling?

Habibullah: We are integrating with a lot of the model-generating products, like Revit or Tekla. The whole BIM trend came from the aircraft industry, where one manufacturer did everything—design, construction, maintenance. In a structural engineering environment, there is a great variety of processes used by the construction industry, and it's hard to standardize.

Reitherman: What about the nonstructural components in a building? Have you thought up ways to use software for their design and analysis, perhaps predicting their earthquake performance?

Habibullah: That's where most of the property loss comes from in earthquakes. It's a great opportunity for structural engineers, if they can look at the whole building and evaluate all its potential earthquake losses. It's something we're looking at. Ed and I have been talking about those wonderful, exciting early years of developing software for structural engineers, but the present era is also exciting. And now, I think we're onto something really big over the next five to ten years.

Reitherman: Not getting too specific about CSI products, let me ask what will come in the next five to ten years? Will it be the application of software to more and more buildings and structures?

Habibullah: Even in the next year, the product we have for bridges will extend throughout the bridge design market, which is much larger than the building design market, in terms of software, because the designers of bridges can spend more on software. When we go to a convention of engineers, you can see this in person. The structural engineers designing buildings, working on a commission that is a fraction of the architect's as the consultant to the architect, ask for discounts. The bridge designers say, with these incredible features, how come it's so cheap? Bridges are designed usually by government agencies, and if they save money on the job, the savings don't go into their own pocket. The costs and savings are just passed along to the public. Most building designers are usually smaller companies owned by the engineers running them and paying the bills, making the profits, and paying for the software. We also have a completely new product for piping of all kinds—nuclear, petrochemical, factories—a very big market.

How CSI Generates Goodwill

Reitherman: Do you get a warm feeling at the EERI annual meetings when you see these "kids," these undergraduate young adults of age 17 to 21 or so, our future engineers, already using your software to analyze their balsa wood buildings for the shake table test competition?

Habibullah: I was just in the Dominican Republic, and I had 1,700 people come to my performance-based design seminar. A lot of them are young students. You get a lot of warmth from them. We spend a lot of money to throw parties and seminars for young students. There are thousands of universities around the world that we give free software to for their use with students. We've been doing this since day one—that's the approach Ed started. We've carried on that tradition. People who were young students back then are now running engineering companies, and they have complete loyalty to us. It took us a while, for example, to get into Windows to use that for our programming, and our clients waited. Some may have strayed and tried other products that were on the market earlier, but they came back to us.

Reitherman: Are you getting suggestions for product upgrades or revisions from the practicing engineers?

Habibullah: We are very attentive to our customers. When they use our user support, they complain about this feature or that, and it all goes into a database we use for improving what we do. We identify what takes more time, what is more intuitive or harder to use. We improve our product because we listen to our customers, it's very simple. We invent things they didn't know could be done, but we do that to meet their practical needs.

Wilson: Sometimes what the practicing engineer wants is so trivial, maybe five minutes of time on the computer to program it. In the first version of SAP 80, I calculated the weight of the steel in the building. Soon, an engineer I knew came up to me and said, "thank you, we have been doing that by hand, what a blessing." In different countries, the relative cost of labor and material changes. Typically labor is expensive and materials inexpensive in the United States, but it's the converse in poor countries. The engineers can begin to figure out cost implications as they do their analysis.

Habibullah: I had a young woman come to me saying she remembered when I visited Stanford ten years ago. I spent two hours there and she remembers it a decade later. Human interaction is very important. People will forget what you said, they will forget what you did. But they will never forget how you made them feel. We have generated more than name recognition; I think we have generated a lot of goodwill. We donate heavily to the various structural engineering associations for their conventions, we donate to EERI. We do things other companies don't, like our party every year when we rent San Francisco City Hall. How much money does a person need? What do you do with your money? Leave it in the bank, die, and have somebody else spend it twice as fast because they didn't work to earn those hard-won dollars? No, spend it now, along with having a comfortable reserve, spend it now on things like the big San Francisco party. Spend it now, give it away. I give twenty iPads away at engineering conventions. Sometimes I have the ones who have won one of those come up on stage, while I am in my lighted jacket, and sing a song. It leaves a mark on a person's mind.

Reitherman: Everybody who is invited to your annual party says, "This is the party of my life." Ed and I compared notes earlier about what an experience it is—the brimming-over variety of food and beverages, the entertainment, not to mention the MC, you, with your jacket of many lights.

Habibullah: Every person leaves the party with a good feeling about CSI. It's an unquantifiable value. Having Ed's name associated with CSI from early on was important. It gives the company credibility. At our annual party, for eight hours straight, everybody in City Hall is having a good time.

Reitherman: Eight hours of happiness multiplied by how many people?

Habibullah: The 2015 party was attended by 1,600 people.

Reitherman: Let me see, eight times 16 is 80, plus 48, equals 128, with two zeroes; that comes out to 12,800 person-hours of happiness. That's a lot of happiness.

Habibullah: I just got this letter today from a young woman who said that when I gave a talk at her university, it inspired her and changed her life. It is an inspiring profession, not a boring one. I'm going to be 70 in two years, but I feel better than when I was twenty. I was overweight back then.

Wilson: And you didn't have much confidence, you were shy.

Reitherman: And you didn't like computers. [Laughter] I'm ignorant about all the technical aspects of structural engineering software, but it seems to me that—unlike the early era you two have described, when work done at the university became a popular product—most of the software is generated outside of the university today. NSF spent millions on the information technology aspects of NEES, Network for Earthquake Engineering Simulation, to provide tele-collaboration IT tools, such as teleconferencing by researchers in different locales, but mostly the researchers ended up using the off-the-shelf commercial product WebEx, made by Cisco.

Habibullah: Sometimes the professors say to me, where does that leave our function? CSI does research, and universities do research, but the work CSI does ends up on the engineer's desk in the next release. Our work has a pressing need behind it, and that motivates us. We have brilliant people here who can do research beyond what they were doing when they were in the university. I hire the right people and can give them a difficult problem and have it quickly solved.

Reitherman: What is the reason why in the 1960s and 1970s and 1980s, a university like Berkeley was generating so many important programs, and you don't see that today?

Habibullah: One factor is that money is still extremely important. Once I said to a convention of engineers, I don't know about you but I'm in this field to make money. And there is a reaction against that, because there is the idea that engineers just do good and shouldn't consider compensation. I tell them, I think you could serve the world much better if you had some extra money in your pocket. Is money important? I don't want to be in the situation of worrying about how I am going to make payroll. That's why I have cash reserves. All of our development is paid for, we don't owe anybody anything. In the forty years of CSI, we have never asked anybody to take a pay cut when revenues fell, we have never laid off anybody because we are short of money. The company is supposed to give the employee job security, and that comes from money. When there was a real estate mortgage crisis in 2008, we helped some of our employees with free loans. It builds loyalty. Loyalty has to be earned; you can't demand it. We have lots of long-term employees, whereas a lot of software development companies have developers who stay for maybe two years and then go elsewhere. Our communications director, for example, who took over all our sales, was put through her MBA program by the company. Money can't buy everything, but lack of money causes a lot of misery. I have been poor; I am never going to be poor again. I have been fat; I am never going to be fat again. [Laughter] Rich and thin is better. [Laughter] Money and health are the two essentials.

Wilson: I have a car that's forty-four years old, and it's all I need. Diane and I still live in the same house. And I realized that with regard to health, money wasn't going to solve our daughter's mental retardation. I think that in our lives, Ashraf and I have done what interested us, and that led to making money. We didn't first set out just to make money.

Chapter 13

Review Boards

People in the earthquake engineering field continually preach doom and gloom, seeking funding, but we should keep the earthquake hazard in perspective.

Reitherman: You have worked in the fields of civil, mechanical, and aerospace engineering for over sixty years. You're widely known for your computer analysis programs. How would you describe yourself?

Wilson: I prefer to be called a "solver of structural engineering problems using the fundamental laws of mechanics." The use of the modern digital computer is a tool that allows engineers to analyze and design structures very rapidly. Therefore, many more possible design options can be investigated in a short period of time. But the computer is merely carrying out the instructions of the engineer for how to solve a problem.

Reitherman: In your *Static and Dynamic Analysis of Structures* book,[68] you make this statement: "The static and dynamic analysis of structures has been automated to a large degree because of the

68 Edward L. Wilson, *Static and Dynamic Analysis of Structures: A Physical Approach with Emphasis on Earthquake Engineering*, Computers and Structures, Inc., Walnut Creek, CA, first edition 2000, fourth edition 2010, p. x.

existence of inexpensive personal computers. However, the field of structural engineering, in my opinion, will never be automated. The idea that an expert-system computer program, with artificial intelligence, will replace a creative human is an insult to all structural engineers."

Wilson: When someone refers to me as a computer person or a programmer, they often imply I am not really an engineer. My good friend and colleague the late Professor Joe Penzien often told me "anyone can write a computer program—it is the theory that is important." Professor Vitelmo Bertero would say "I do not want my students wasting their time writing computer programs." Both Joe and Vit never realized digital computer programs could produce more accurate results and were very easy to develop.

I gave my programs away so civil, mechanical, and aerospace engineering could use my work immediately. Only after a program solved a real problem would I write a paper on the accuracy of the numerical methods and the new finite elements that were developed. My objective was always to produce programs that were faster and more accurate than other existing programs.

Reitherman: You've also described how you have worked on a number of different kinds of problems, many of them unrelated to earthquakes.

Wilson: I remember trying to talk people on the faculty into going into wind engineering, but couldn't get people to diversify. People in many cases are overly fearful of earthquakes. We had people offered faculty positions at Berkeley after the 1989 Loma Prieta Earthquake who were hesitant to come to California. And where were they coming from? Parts of the country where there are tornadoes that demolish parts of several towns several times a year, hurricanes along the Gulf and Atlantic Coasts. Snow and ice cause dozens of fatalities a year, as do lightning strikes. People in the earthquake engineering field continually preach doom and gloom, seeking funding, but we should keep the earthquake hazard in perspective. At least in the United States, life loss from earthquakes is rather low. The situation is different in other countries, as shown by the life loss for the 1976 Tangshan Earthquake, which is usually estimated at over 600,000. As I have noted in the brief paper on this topic that I have posted on my edwilson.org website, called "We Should Not Fear Earthquakes as Compared to Other Natural Disasters," in the past 500 years, more Americans have been killed by insect bites than from earthquakes.

Engineering Criteria Review Board

Wilson: From 1985 to 2014—twenty-nine years—I served without pay, on the Engineering Criteria Review Board, ECRB, an advisory committee to the Bay Conservation and Development Commission, BCDC. This Commission was formed for protecting and improving the environment of the San Francisco Bay. During a 29-year period, this group of 12 members reviewed the construction and retrofit of hundreds of structures near and over the Bay. After the 1989 Loma Prieta earthquake, over 50 percent of our time was associated with the retrofit or replacement of many bridges in the Bay Area. After several years of Caltrans attempts to retrofit the eastern span of the San Francisco–Oakland Bay Bridge, it was recommended to replace that section with a new structure.

The Bay Bridge East Crossing Review Board

Wilson: In February 1997, over seven years after the Loma Prieta Earthquake, a task force called the Engineering and Design Advisory Panel (EDAP) was created. Joe Nicoletti, a highly respected local engineer, did an outstanding job as the chairman of Panel. I was one of 32 members of that panel. Joe, in his EERI Oral History, summarizes how the panel recommended a self-anchored suspension bridge. Professors Ben Gerwick, Alex Scordelis, and T. Y. Lin from U.C. Berkeley were also members of EDAP.

Reitherman: What was your reaction to the selection of the self-anchored suspension bridge? It's a rare type of bridge. I have only heard of a few others, such as the one in Cologne, Germany, built around the time of World War I.

Wilson: The Bay Bridge east span decision to select the self-anchored suspension type was chaotic. There was the bicycle lobby, the architects, too many advocates with their narrow interests. The group of engineers on the panel only wanted to build a bridge that was highly resistant to a very large earthquake as soon as possible.

At the first public meeting of the Panel, I stated: "We look out to the Bay and see three major Bay Area bridges: the Richmond–San Rafael is ugly, while the Golden Gate and Bay Bridge suspension bridges are beautiful. Like Hippocrates, we engineers should first do no harm." The next day my quote was published in one of the local San Francisco newspapers.

However, the self-anchored suspension bridge ended up doing a lot of harm from the financial standpoint, with its excessive cost to the tune of five or more times the estimated cost and several years longer than the predicted time of construction. The cable-stayed bridge was economical and almost got voted in. There was another meeting scheduled and I was absent on vacation. I thought it was a rubber stamp decision for the cable-stayed design. Then the architects and others lobbied heavily and got the self-anchored suspension bridge voted in, not knowing what expense and complexity it entailed in the construction process.

The design of the new east span of the Bay Bridge set out for itself multiple problems that other designs would have avoided. I thought the site didn't merit some kind of heroic structure. Let's face it, the Golden Gate is a beautiful site. The suspension bridge there graces and enhances that site. The double suspension bridge structure of the Bay Bridge from Yerba Buena Island to San Francisco is beautiful. But going from Yerba Buena Island to the mudflats of Oakland isn't. In the end, painting the new east span structure white is about the only thing that makes it significant, along with lighting it up like a Christmas tree, which it resembles in shape. The final cost was over a million dollars per foot of length. This made it the most expensive bridge ever built. It tarnishes the reputation of the structural engineering profession. In terms of seismic reliability, I'm a little worried that just under its own dead load there is a tremendous amount of compression strain energy stored in the deck of the bridge. I would assume that the engineers checked the local buckling possibilities of the deck plates. However, if one plate buckles, the deck section is no longer symmetrical and large lateral displacements may be initiated. The architects on the bridge project arbitrarily changed its form and made it unnecessarily complex. One example is the light poles, which are unnecessarily prominent compared to the slender cables.

Reitherman: People often think the overall efficiency of a structure is just how efficient it is, standing there. But it had to get built in a particular sequence. Isn't the self-anchored type more difficult to build than the cable-stayed or suspension bridge types, or a cantilever truss like the section of the old bridge it replaced?

Wilson: The self-anchored design's expense has a lot to do with its construction sequence. You have to build a temporary bridge, end to end, as shoring, until all the pieces of the self-anchored structure are in place. Suspension bridges and cable-stayed bridges can be erected much more efficiently. The look of the bridge became all-important, mostly driven by the architects and a few engineers on the panel. My colleague T. Y. Lin called it "a monument to stupidity."[69] Also, the new steel bridge is showing signs of corrosion. Therefore, in addition to the astronomical initial cost, the self-anchored east crossing bridge will require very large maintenance expenditures for every year of its existence.

Reitherman: "Sustainable" is a popular term these days. But it seems you don't rate the new bridge high in sustainability.

Wilson: New words are invented to refer to old concepts. Engineers have always worried about using the minimum amount of material and minimum upkeep, which is sustainability; environmental impacts have been considered for decades, and civil engineering departments have all been re-named civil and environmental engineering departments.

69 *The Father of Prestressed Concrete: Teaching Engineers, Bridging Rivers and Borders, 1931 to 1999*, Eleanor Swent, Interviewer, Bancroft Library, University of California at Berkeley, 2001, p. 355.

Chapter 14

Retirement and Time to Think

The response spectrum method is very widely used for seismic analysis. Most structural engineers do not understand it is restricted to linear analysis and is incapable of predicting earthquake damage of a structure.

Heart Attack in 1990, Retirement from Berkeley

Wilson: On April 22, 1990, approximately six months after the Loma Prieta Earthquake, I had my first and only heart attack. After six days in the hospital and angioplasty six weeks later, I felt great. However, I cancelled all my travel plans for the next several months—including a lecture tour and safari in South Africa. Also, I applied for and received a Sabbatical leave for the following year at 50 percent pay.

At that time, the faculty retirement fund had developed a large surplus due to a high return on their investments. The university decided to encourage senior faculty members to retire by adding five years of service to individuals who retired by July 1, 1991. Then, the university could replace the high salary senior professors with young professors at a lower pay. Three months prior to my sixtieth birthday, I accepted their offer and immediately started to think about how best to use my time to solve structural analysis problems faster and more accurately.

Development of the FNA method

Reitherman: What did you conclude was the biggest need for improvement in earthquake engineering analyses?

Wilson It was the extension of the linear analysis program, SAP 90, to conduct nonlinear analysis. It required almost ten years to fully develop the Fast Nonlinear Analysis, FNA, method. Ninety percent of the research and development work was conducted by me. The final method was incorporated into SAP2000 by CSI. At that time, the FNA was approximately 100 times faster than other existing nonlinear programs used by the structural engineering profession. In addition, it is very accurate and reliable. The latest version of the FNA method is described in detail on my website (edwilson.org). After retirement from U.C. Berkeley, I had time to work closely with members of the profession, without pay, to point out the many limitations existing in the current methods of analysis. Some of these limitations have to do with basic assumptions that are often not thought about. Engineers need to understand the assumptions underlying a method of analysis, not just run analyses efficiently.

Fundamental Assumptions in Seismic Analysis

Wilson: By 1991, I had worked in the field of earthquake engineering analysis for over 30 years and had produced linear analysis computer programs and numerical methods—such as ETABS, SAP IV, and SAP 90—that were used in most countries throughout the world. However, I realized most of these programs were based on the assumption that the earthquake displacements and accelerations, acting at the base of the structure, could be represented as applied forces equal to the mass of the superstructure times the base horizontal accelerations—if one used the relative displacement formulation for the equilibrium of the computer model. This mathematical transformation of the displacement input into a force input acting on the superstructure is based on the physical assumption that the base of the computer model of the structure moved, as a rigid body, in only the two horizontal and the vertical directions. Therefore, the three directional rotations at the base of the structure are set to zero in the mathematical model of the structure. Some of the errors introduced by using the relative displacement formulation are the following.

One cannot accurately model soil-structure interaction due to the fact the relative displacements are set to zero at the base of the structure.

All of the horizontal forces and the energy of the earthquake are applied to the superstructure high above the base of the structure, whereas the real earthquake displacements and forces are applied at the base of the structure—where energy can move in from the ground and out of the structure to the ground during a real earthquake.

Our analysis model should resemble a shaking table test where earthquake forces are not applied to the superstructure, but rather earthquake displacements are applied to the base of the structure.

However, both the analysis model and the shaking table structure have neglected an important energy dissipation phenomenon: radiation of energy from the structure to the infinite earth below the structure.

Energy-Based Seismic Analysis

Reitherman: Engineers almost always calculate seismic loads in units of acceleration times mass. Back in 1956, in a paper for the first of the World Conferences, George Housner clearly stated the more fundamental energy basis of the seismic response of a structure: "The effect of the ground motion is to feed energy into the structure. Some of the energy is dissipated through damping and nonlinear behavior and the remainder is stored in the structure in the form of kinetic energy of motion of the mass and in the form of strain energy of deformation of the structural members. Therefore, at any instant, the sum of the kinetic energy, plus strain energy, plus energy dissipated through normal damping, plus energy dissipated through permanent deformation is equal to the total energy input."[70] Do you satisfy this conservation of energy equation in your FNA method?

Wilson: We check the energy balance equation at the end of each time step and compare the error in energy to the total energy in the system at that time. Therefore, we evaluate an equation of the form:

Tol = (time step error)/(total energy)

Because energy is always positive, and if we want to be accurate to six significant figures, we will iterate and reduce the size of the time step until *Tol* is less than 10^{-6}. Since our inexpensive personal computers are so fast we can use a smaller tolerance until we are confident we have satisfied Housner's criteria and have solved the nonlinear mathematical model correctly. However, the mathematical model may not be an accurate idealization of the real structure for two reasons.

First, the total energy input to the structural system may have been calculated by using the free-field ground acceleration and the mass of the fixed-base structure. Therefore, the displacements, velocities, and accelerations produced will be relative to the fixed-base structure and all soil-foundation-structure interaction energy has been be neglected. This is considered a conservative assumption.

Second, in all real structures subjected to earthquake motions, the mathematical model soil nodes below the foundation must be subjected to three displacement components of earthquake motion. This type of model was used on the retrofit of the Richmond–San Rafael Bridge in 1997, and it was possible to accurately calculate a total value of energy supplied to the model for each three-dimensional earthquake. Also, the earthquake displacements were different at each pier since the bridge was over five miles long. However, it was not possible to calculate the "radiation

70 George Housner, "Limit Design of Structures to Resist Earthquakes," *Proceedings of the World Conference on Earthquake Engineering*, Earthquake Engineering Research Institute, Oakland, CA, 1956, p. 5-4.

energy," which is the loss of energy at the soil nodes after the bridge starts vibrating.

Housner's conservation of energy criteria are absolutely true for all real structures; however, at this point in time, after thinking about the problem for over 50 years, we have not been able to exactly satisfy the criteria within our mathematical models. Also, we continue to assume a large amount of energy is dissipated by linear viscous damping during the earthquake response of our mathematical models. Do you know we have never built a perfectly linear viscous device or material under very accurate controlled laboratory conditions? Therefore, we still must improve our numerical methods in order to eliminate the need for the assumption of linear viscous damping in our mathematical models.

Response Spectrum Method

Reitherman: You've written on your edwilson.org website about the limitations of the response spectrum method. Please expand on that topic.

Wilson: The response spectrum method is very widely used for seismic analysis. Most structural engineers do not understand it is restricted to linear analysis and is incapable of predicting earthquake damage of a structure.

Reitherman: Is your basic criticism that it plots a single peak response number to represent how a structure responds to tens of seconds of different input motions?

Wilson: Yes, and you use a worst case for a member or connection, but the maximum responses throughout the structure don't all occur at the same time. I first got into this in detail on the seismic retrofit of the San Mateo–Hayward Bridge across San Francisco Bay. The seismologists had developed about eight different earthquake records and did some sort of average of them. It would have cost a fortune to do the retrofit project. I told them to run each record individually. When that was done, it cut down the forces by a factor of three. The response spectrum doesn't have the element of time in it, the response history does. It makes a big difference with multi-mode response spectrum analysis. You apply it to a nonlinear structure and the approximations become very large. As soon as part of the structure yields, the loads all get redistributed. And I'm the guy who helped popularize the response spectrum analysis by getting it to run on the computer! But it's now my responsibility to educate engineers about the limits of the response spectrum method.

The response spectrum method became common in the 1960s, when we essentially only had three decent records to use: the 1940 El Centro; the 1965 Olympia, Washington; and the 1952 Taft. Now we have thousands of earthquake ground motion records to use in response history analyses.

Reitherman: What are some of the other limitations in current seismic design practice?

Wilson: When I vibrate this little model on my desk, you can feel the vibration propagating through my desk. This is because energy in the model is being radiated into my desk. This is still one of the unsolved problems in earthquake engineering and is one George Housner pointed out in the 1956 World Conference paper you quoted. Energy goes into the structure from the base, from the shaking soil, and it has to go somewhere and be dissipated. Quite frankly, I don't know how to solve this problem. This can only be solved by including the structure and foundation in the same model. The profession isn't doing that yet; however, with the recent increase and speed of inexpensive computers we may be able to solve this problem within the next few years.

Most engineers use 5% for the level of damping to create response spectra. That's actually a large figure. You can't just add in viscous damping to make a correction. A simple little term like the center of stiffness of a multistory building isn't clearly defined. Is it the stiffness when you apply a lateral load to a story with the stories above and below fixed? Or is it when a load at that story doesn't make it rotate? You find cases where that story doesn't rotate but the ones above and below do. The concept is used in codes all the time. ASCE 7[71] is filled with these kinds of assumptions.

The standard design spectrum has an artificial short-period plateau. A short-duration earthquake generating the same single peak response is treated the same as a long-duration earthquake with that same peak. I can't believe the equal displacement approach has been in use for fifty years. It originated in the paper by Andy Veletsos and Nathan Newmark for the World Conference in Chile.[72] Their conclusions were based on a one-degree-of-freedom system. They completely neglected the redistribution of forces and displacements in a real three-dimensional nonlinear structure. Any engineer who knows how to use SAP2000 can prove that a linear dynamic analysis is a very poor approximation of the dynamic analysis of a nonlinear system. The equal displacement approximation has no physical or theoretical justification.

There are too many unjustified approximations made by many structural engineers today. Back in the '60s, people like Clough understood all of this, but it's an insight that seems to have been lost.

71 ASCE 7 (current edition ASCE 7-16), *Minimum Design Loads for Buildings and Other Structures*, American Society of Civil Engineers, Reston, VA, 2016.

72 A. S. Veletsos and N. M. Newmark, "Effect of Inelastic Behavior on the Response of Simple Systems to Earthquake Motions," *Proceedings of the Second World Conference on Earthquake Engineering*, International Association for Earthquake Engineering, Vol. 2, pp. 895-912.

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36. "Generation of Ritz Vectors for Adaptive Finite Element Dynamic Analysis," *Proceedings of the U.S.-Korea Seminar Workshop on Critical Engineering Systems*, Seoul, Korea, 11-15 May 1987.

37. "Load Dependent Vector Bases for Earthquake Response Analysis" (with P. Leger), *5th Canadian Conference on Earthquake Engineering*, Ottawa, Canada, July 6-8, 1987

38. "Numerical Methods for Solution of Finite Element Systems," *ASCE Seventh Structures Congress*, San Francisco, CA, May 1-5, 1989

B. Non-Refereed Publications

Technical Reports

1. *Finite Element Analysis of Two Dimensional Structures*, UCB/SESM Report No. 63/2, University of California, Berkeley, June 1963 (PhD Dissertation).
2. *Finite Element Analysis of Axisymmetric Structures*, Aerojet-General Corporation Technical Paper No. 138 SRP, April 1964.
3. *Thermal Strain Analysis of Advanced Manned Spacecraft Heat Shields*, Final Report to NASA Manned Spacecraft Center, Houston, Texas; Report No. 5654-02-FS, Aerojet-General Corporation, October 1964.
4. *A Digital Computer Program for the Steady-State Temperature Analysis of Plane or Axisymmetric Bodies*, Report No. TD-44, Aerojet-General Corporation, March 1965.
5. *A Digital Computer Program for the Finite Element Analysis Solids with Non Linear Material Properties*, Technical Memorandum No. 23, Aerojet-General Corporation, July 1965.
6. *Two-Dimensional Stress Analysis with Incremental Construction and Creep* (with R. S. Sandhu and J. M. Raphael), UCB/SESM Report No. 67/34, University of California, Berkeley, December 1967.
7. *Maximum Temperature Stresses in Dworshak Dam* (with J. M. Raphael), UCB/SESM Report No. 67/14, Structures and Materials Research Laboratory, University of California, Berkeley, August 1967.
8. *Finite Element Stress Analysis of Axisymmetric Solids with Orthotropic, Temperature-Dependent Material Properties* (with R. M. Jones), Air Force Report No. BSD-TR-228; prepared for Space and Missile System Organization, Air Force Unit Post Office, Los Angeles, California.
9. *Structural Behavior of Mass Concrete Beams* (with R. S. Sandhu and J. M. Raphael), UCB/SESM Report No. 67/16, Structures and Materials Research Laboratory, University of California, Berkeley, August 1967.
10. *A Computer Program for the Dynamic Stress Analysis of Underground Structures*, UCB/SESM Report No. 68/1, Structures and Materials Research Laboratory, University of California, Berkeley, January 1968.
11. *The Determination of Temperatures within Mass Concrete Structures*, UCB/SESM Report No. 68/17, Structures and Materials Research Laboratory, University of California, Berkeley, December 1968.
12. *Elastic Dynamic Response of Axisymmetric Structures*, UCB/SESM Report No. 69/2, Structures and Materials Research Laboratory, University of California, Berkeley, January 1969.
13. *Stress Analysis of Axisymmetric Solids Utilizing Higher-Order Quadrilateral Finite Elements* (with W. P. Doherty and R. L. Taylor), UCB/SESM Report No. 69/3, Structures and Materials Research Laboratory, University of California, Berkeley, January 1969.
14. *Dynamic Stress Analysis of Axisymmetric Structures Under Arbitrary Loading* (with S.

Ghosh), UCB/EERC Report No. 69/10, Earthquake Engineering Research Center, University of California, Berkeley, September 1969.

15. *Large Displacement Analysis of Axisymmetric Shells* (with T. Hsueh and L. Jones), UCB/SESM Report No. 69/13, Structures and Materials Research Laboratory, University of California, Berkeley, March 1969.

16. *Dynamic Response Analysis of Two-Dimensional Structures with Initial Stresses and Non-Homogeneous Damping* (with I. Farhoomand and E. Rukos), UCB/SESM Report No. 69/21, Structures and Materials Research Laboratory, University of California, Berkeley, November 1969.

17. *Stability Analysis of Axisymmetric Shells* (with T. Hsueh), UCB/SESM Report No. 69/22, Structures and Materials Research Laboratory, University of California, Berkeley, November 1969.

18. *Three Dimensional, Steady State Flow of Fluids in Porous Solids* (with R. L. Taylor and W. P. Doherty), UCB/SESM Report No. 69/29, University of California, Berkeley, July 1969.

19. *A Nonlinear Finite Element Code for Analyzing the Blast Response of Under ground Structures* (with I. Farhoomand), U.S. Army Waterways Experiment Station, Contract Report N-70-1, Vicksburg, Mississippi, January 1970.

20. *SAP—A General Structural Analysis Program* UCB/SESM Report No. 70/20, University of California, Berkeley, September 1970.

21. *A Computer Program for the Analysis of Prismatic Solids* (with P. C. Pretorius), UCB/SESM Report No. 70/21, University of California, Berkeley, September 1970.

22. *Non-Linear Heat Transfer Analysis of Axisymmetric Solids* (with I. Farhoomand), UCB/SESM Report No. 71/6, University of California, Berkeley, April 1971.

23. *Flow of Compressible Fluid in Porous Elastic Media* (with J. Ghaboussi), UCB/SESM Report No. 71/12, University of California, Berkeley, July 1971.

24. *SOLID SAP—A Static Analysis Program for 3-Dimensional Solid Structures*, UCB/SESM Report No. 71/19, University of California, Berkeley, September 1971.

25. *Three Dimensional Analysis of Building Systems—TABS* (with H. H. Dovey), UCB/EERC Report No. 72/8, Earthquake Engineering Research Center, University of California, Berkeley, December 1972.

26. *Finite Element Analysis of Mine Structures*, Final Report to U.S. Department of the Interior, Bureau of Mines, September 1972.

27. *Computer Program for Static and Dynamic Analysis of Linear Structural Systems*, UCB/EERC Report No. 72/10, Earthquake Engineering Research Center, University of California, Berkeley, November 1972.

28. *SMIS—Symbolic Matrix Interpretive System*, UCB/SESM Report No. 73/3. University of California, Berkeley, April 1973.

29. *SAP IV—A Structural Analysis Program for Static and Dynamic Response of Linear Systems* (with K. J. Bathe and F. E. Peterson), UCB/EERC Report No. 73/11, Earthquake

Engineering Research Center, University of California, Berkeley, June 1973.

30. *Static and Dynamic Geometric and Material Nonlinear Analysis* (with K. J. Bathe and H. Ozdemir), UCB/SESM Report No. 74/4, Structural Engineering Laboratory, University of California, Berkeley, February 1974.

31. *NONSAP—A Structural Analysis Program for Static and Dynamic Response of Nonlinear Systems* (with K. J. Bathe and R. H. Iding), UCB/SESM Report No. 74/3. Structural Engineering Laboratory, University of California, Berkeley, February 1974.

32. *Finite Element Formulations for Large Deformation Dynamic Analysis* (with K. J. Bathe and E. Ramm), UCB/SESM Report No. 73/14, Structural Engineering Laboratory, University of California, Berkeley, September 1973.

33. *Three-Dimensional Analysis of Building Systems (Extended Version)* (with J. P. Hollings and H. H. Dovey), UCB/EERC Report No. 75/13, Earthquake Engineering Research Center, University of California, Berkeley, 1975.

34. *Finite Element Analysis of Nonlinear Heat Transfer Problems* (with R. M. Polivka), UCB/SESM Report No. 76/2, University of California, Berkeley, June 1976.

35. *CAL-Computer Analysis Language for the Static and Dynamic Analysis of Structural Systems*, UCB/SESM Report No. 77/2, University of California, Berkeley, 1977.

36. *Three to Nine Node Isoparametric Planar or Axisymmetric Finite Element* (with J. P. Hollings), UCB/SESM Report No. 78/3, University of California, Berkeley, December 1977.

37. *Dynamic Behavior of a Pedestal-Base Multistory Building,* UCB/EERC Report No. 78/13, Earthquake Engineering Research Center, University of California, Berkeley, July 1978.

38. *CAL 78 User Information Manual*, UCB/SESM Report No. 79/1, University of California, Berkeley, 1979.

39. *Numerical Methods for Dynamic Substructure Analysis* (with J. Dickens), UCB/EERC Report No. 80/20, Earthquake Engineering Research Center, University of California, Berkeley, June 1980.

40. *The Design of Steel Energy Absorbing Restrainers and Their Incorporation into Nuclear Power Plants for Enhanced Safety, Volume 2: The Development of Analyses for Reactor System Piping* (with others). UCB/EERC Report No. 79/08, Earthquake Engineering Research Center, University of California, Berkeley, February 1979.

41. *Numerical Methods for Dynamic Substructure Analysis* (with J.M. Dickens), UCB/EERC Report No. 80/20, Earthquake Engineering Research Center, University of California, Berkeley, June 1980.

42. *New Approach for the Dynamic Analysis of Large Structural Systems*, UCB/EERC Report No. 82/04, Earthquake Engineering Research Center, University of California, May 1982.

43. "STOCAL—User Information Manual"

(with M.R. Button and A. Der Kiureghian), Report No. UCB/SESM-81-02.

44. *New Approach for the Dynamic Analysis of Large Structural Systems*, UCB/EERC Report No. 82-04, Earthquake Engineering Research Center, University of California, May 1982.

45. *Numerical Techniques for the Evaluation of Soil-Structure Interaction Effects in the Time Domain* (with E. Bayo), Report No. UCB/EERC-83-04, February 1983.

46. *New Approaches in the Structural Analysis of Building Systems* (with H. Saffarini), Report No. UCB/SESM-83-08, June 1983.

47. *Dynamic Properties of a Thirty-Story Condominium Tower Building* (with R. Stephens and N. Stander), Earthquake Engineering Research Center Report No. UCB/EERC-85/03, University of California, Berkeley, April 1985.

48. *The Use of Load Dependent Vectors for Dynamic Analysis* (with P. Leger and R. Clough), Earthquake Engineering Research Center Report No. UCB/EERC-86/04, University of California, Berkeley, March 1986.

49. *CAL-86 Computer Assisted Learning of Structural Analysis and the CAL/SAP Development System*, Structural Engineering and Structural Mechanics, Report No. UCB/SESM-86/05. August 1986.

50. *Comparison of Iterative Methods for Adaptive Mesh Refinement in Finite Element Analysis* (with K. J. Joo), Report No. UCB/SEMM-86/14, 1986.

51. *A Triangular Thin Shell Element for the Linear Analysis of Stiffened Composite Shells*, NPS 69-88-003, Naval Postgraduate School, Monterey, CA, 1973.

52. *CAL–91: Computer Assisted Learning of the Static and Dynamic Analysis of Structural Analysis of Structural Systems*, Report No. UCB/SEMM-91/01, January 1991.

3. Non-archival Publications or Conference Proceedings

1. Discussion: "Solution of Eigenvalue Problems by the Sturn Sequence Method" by K. K. Gupta (with K. J. Bathe), *International Journal for Numerical Methods in Engineering*, March 1973.

2. "Finite Element Analysis on Microcomputers," *Proceedings, ASMC Winter Annual Meeting,*, Boston, MA, November. 13-18, 1983.

3. "CAL-80: An Education and Development Environment for Engineering" (with M. Hoit), *American Society for Engineering Education Proceedings, 1984 Annual Conference*, Salt Lake City, Utah, June 24-28, 1984.

4. "Structural Analysis on Microcomputers" (ed. by B. A. Schrefler, R. W. Lewis, and S. A. Odorizzi), *Proceedings, 1st International Conference on Engineering Software for Microcomputers*, Venice, Italy, Pineridge Press, 1984: 3-18.

5. "Some Thick Shell Test Problems" (with R. L. Taylor), *AIAA/ASME/AHS 26th Structures, Structural Dynamics and Materials Conference*, Orlando, FL, April 15, 1985.

6. "Structural FE Analysis to Suit the User and Computer," *ASME, Computers in*

Mechanical Engineering, Vol. 3, No. 4, January 1985: 22-28.

7. "TABS 77: A Program for Three Dimensional Static and Dynamic Analysis of Multistory Buildings," *Structural Mechanics Software, Series* Vol. 2, The University Press of Virginia.

8. "Expert SAP—A Computer Program for Adaptive Mesh Refinement in Finite Element Analysis," *Reliability of Methods for Engineering Analysis* (ed. by K. J. Bathe and D. R. Owen), Pine Ridge Press, Swansea, U.K., July 1986.

9. "Finite Element Analysis on Computers with Multiple Processors," *Supercomputers in Engineering Structures* (ed. by P. Melli & C. A. Brebbia), Computational Mechanics Publications, Springer-Verlag, 1988.

C. Books

1. *Numerical Methods in Finite Element Analysis* (with K. J. Bathe), Prentice-Hall, Inc., Englewood Cliffs, New Jersey, 1976.

2. *Static and Dynamic Analysis of Structures: A Physical Approach with Emphasis on Earthquake Engineering*, Computers and Structures, Inc. Walnut Creek, CA, first edition 2000, fourth edition 2010.

Edward L. Wilson Photographs

Ed's parents James (Jim) C. Wilson and Josephine (Jo) Wilson. They were married in 1916.

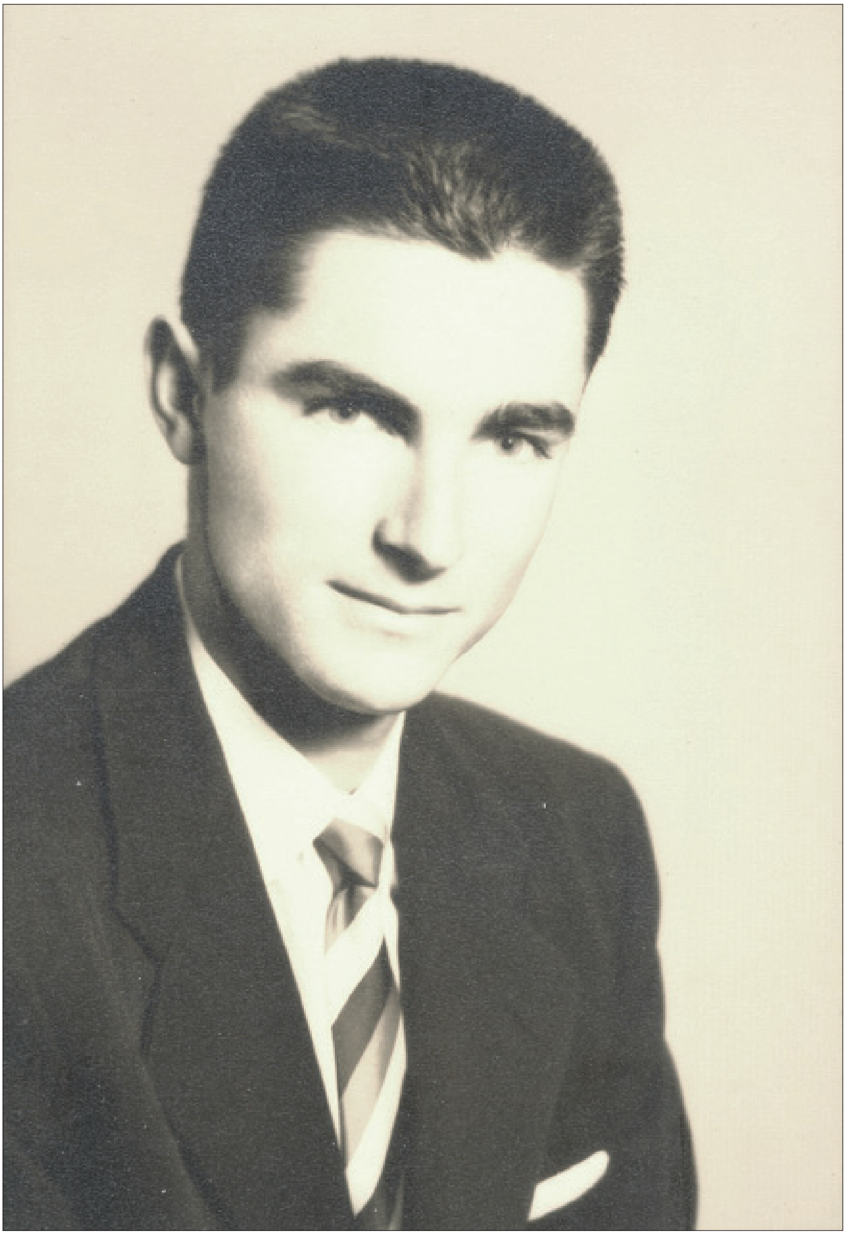
Jim first traveled to Humboldt County in 1910 to work on the construction of Fernbridge across the mouth of the Eel River. At that time, Jo lived on a dairy ranch less than a mile upstream from the construction site. That is why Ed calls Fernbridge "The Love Bridge."

Fernbridge was completed in 1911, 18 months after the start of construction.

Fernbridge is a 1,320-foot-long bridge comprising multiple arches to carry a highway across the Eel River. It was designed by John Leonard. It is still the longest poured-concrete bridge in operation in the world. During the past 105 years, this structure has been battered by a large number of floods and earthquakes and has survived with a minimum of maintenance. In addition, the concrete surface indicates very little deterioration due to the extreme salt water environment. In 1987, Fernbridge was added to the National Register of Historic Places.

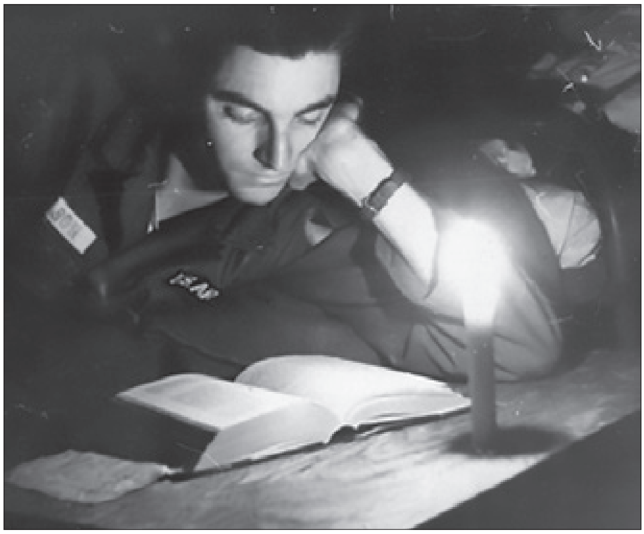
Left to right: Bill, Ed's older brother; Jo, Ed's mother; Ed; and sister Blanche Josephine (BJ) at the Seaview ranch in 1941.

The caption in this 1954 Los Angeles Times newspaper clipping reads: "Robert Gordon Sproul, president of the University of California, holds tape of finish of 880 in Westwood meet. Ed Wilson wins race in 1:54.0, meet record."



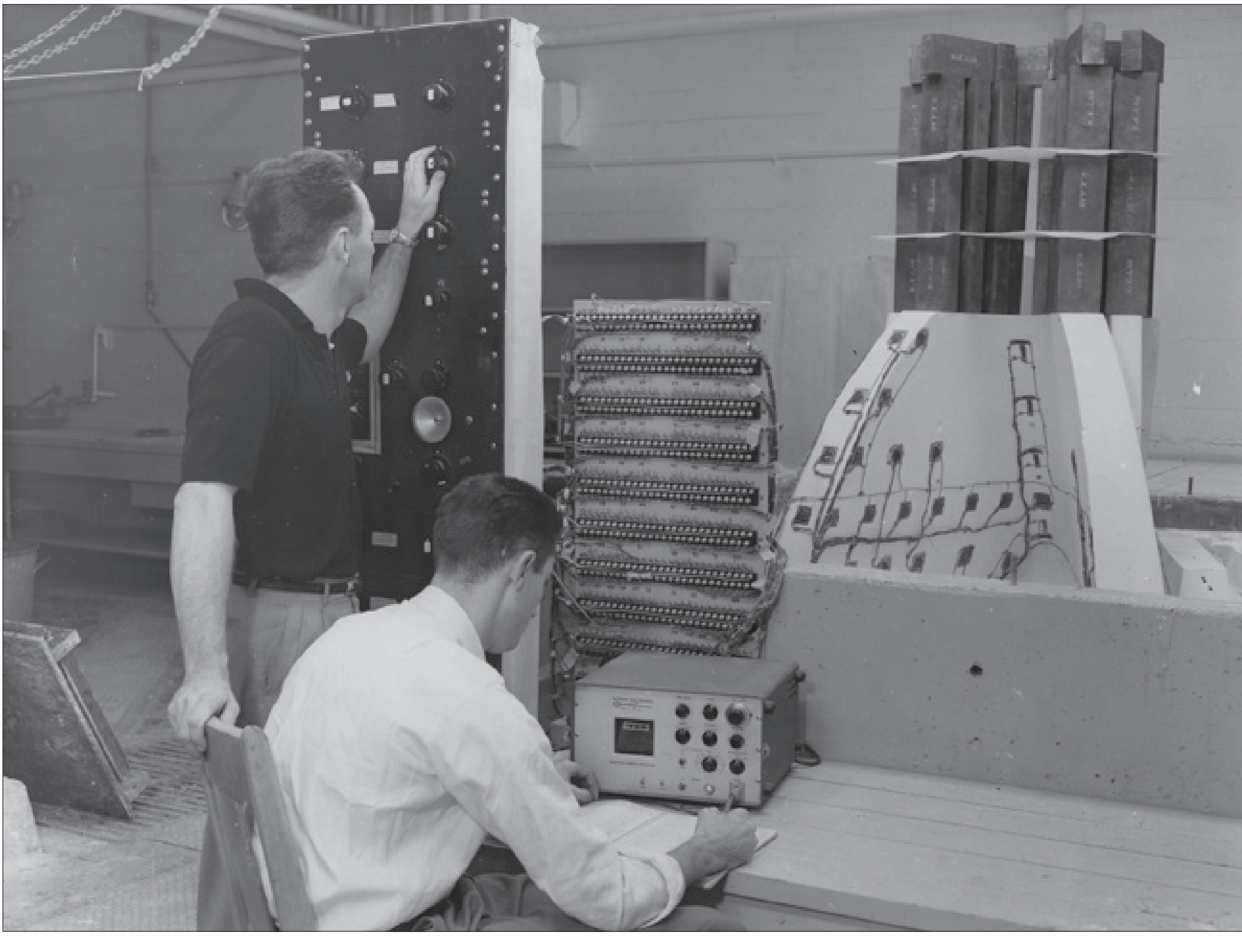
Ed upon graduation as an undergraduate from the University of California at Berkeley, age 23.

Ed in the U.S. Army in Korea, 1956.

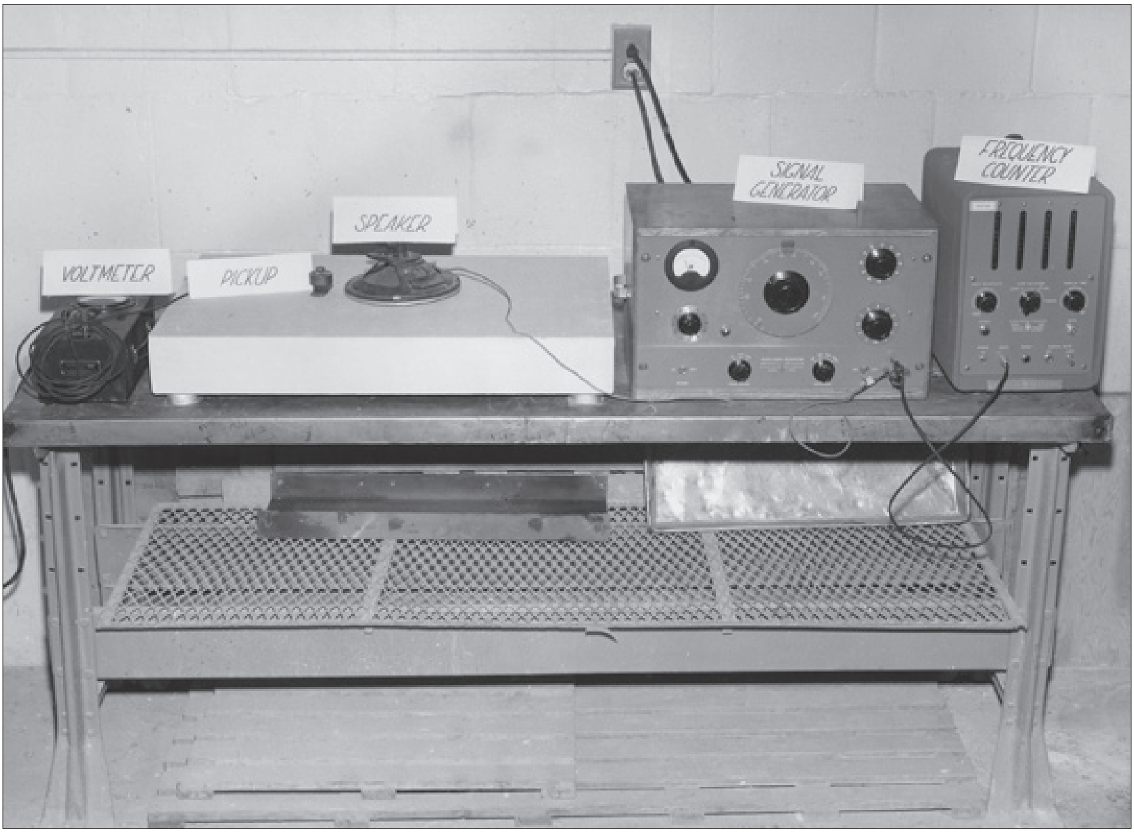


Ed reading by candlelight in the Army while serving in Korea.

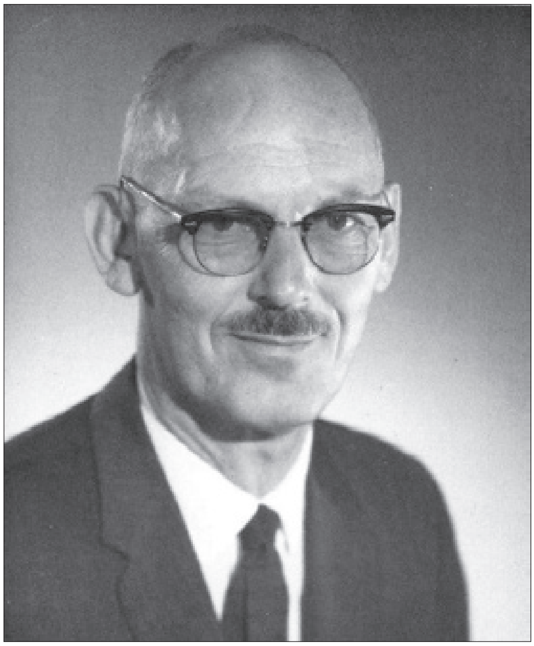
The team at Berkeley on the experimental aspects of the proposed arch-buttress Oroville Dam, 1957. Left to right, front row: Yuko Yoshikawa, Gene Croy, Jerome Raffael, Ed Wilson; back row: Y. Katsura, A. Bulow, and V. K. Sondhi.



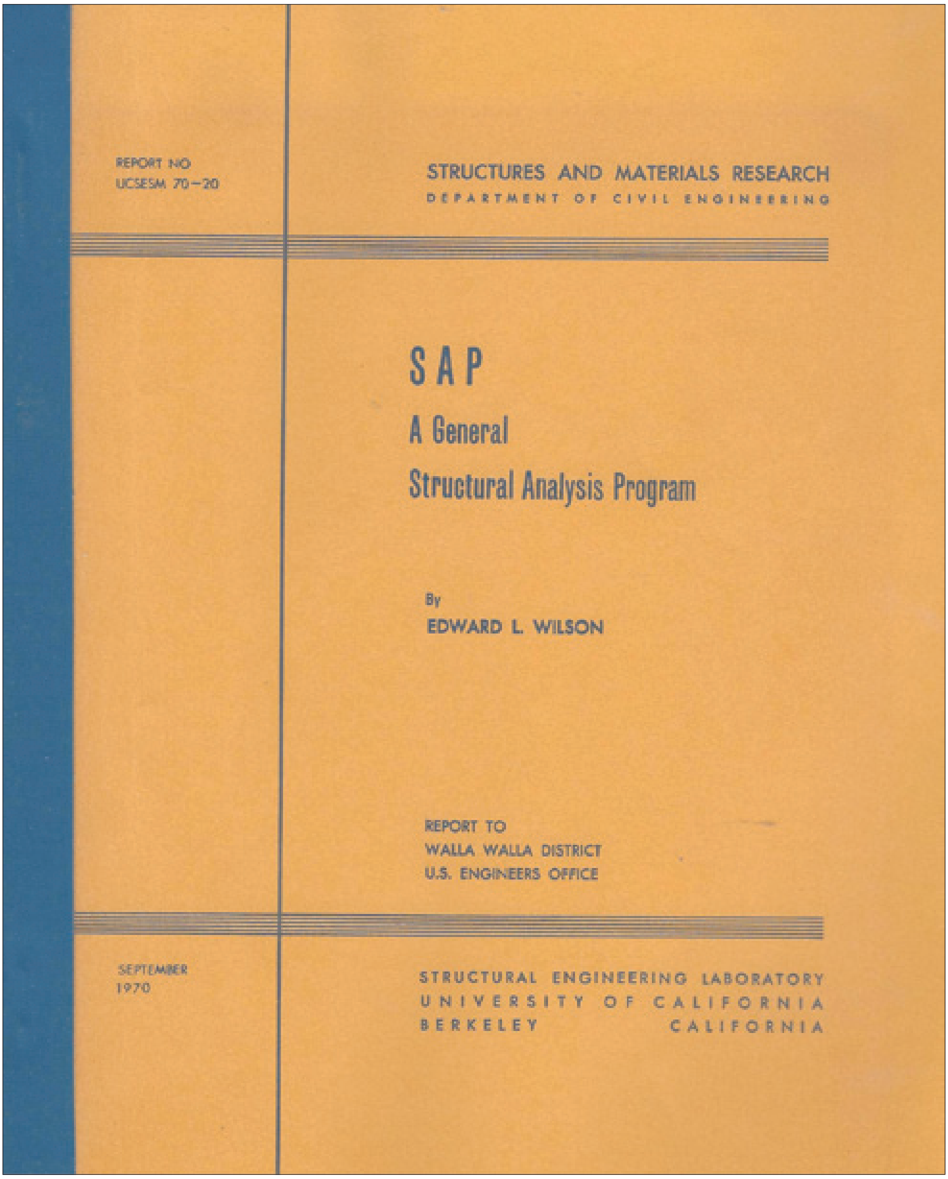
Ed Wilson (right) determining gravity load stresses on the structural model of an arch design for Oroville Dam.



To precisely measure the stiffness properties of the foundation blocks in the Oroville Dam experiments, Ed Wilson devised a resonant test set-up that derived the stiffness from the carefully measured response of a block when it was vibrated by an audio speaker.



Professor Howard Eberhart, U.C. Berkeley, was a great teacher. He hired Ed while he was in Korea to work on the Oroville Dam Project. Later, he encouraged Ed to get his doctoral degree. In 1972, when he was Chairman of the Civil Engineering Department, he recommended Ed be appointed as Chairman of the SESM group.



The original report on the SAP computer program was published in 1970.

Ed stated in the foreword of the report: "The slang name SAP was selected to remind the user that this program, like all computer programs, lacks intelligence. It is the responsibility of the engineer to idealize the structure correctly and to assume responsibility for the results."

Ed's license plate in 1980 indicated how proud he was of his new SAP program.

The larger computer shown at the rear is an IBM P75 portable, made in 1991, the year Ed retired from teaching. It was the most expensive personal computer he ever purchased at $15,000. However, over the next four years his productivity increased significantly because he could easily take it with him on vacation to Hawaii and his fishing trips to their mountain home.

The portable laptop PC shown in front of P75 was purchased in 2013 for less than $1,000. However, compared to the P75 it is 1,000 times faster, has 1,000 times more storage, and users can watch movies in color and communicate with people all over the world.

Left to right: Ed Wilson, Joe Penzien, Ashraf Habillulah, Alex Scordelis, and Graham Powell.

Diane and Ed Wilson, 2014.

Left to right, at the 2014 CSI party at San Francisco City Hall:

Doug Clough (Ray Clough's son); Diane Wilson; Ashraf Habibullah; Linda Clough (Doug Clough's wife); and Ed Wilson.

Ashraf Habibullah and Ed Wilson at Ashraf's Computers and Structures, Inc. office in Walnut Creek, California, 2015.

Ed and Diane Wilson at the graduation of their daughter Terri (Teresa) in 1984, with their son, Mike.

Diane and Ed Wilson at their second home in Callahan, California, Memorial Day 2015.

Ed Wilson with his son Mike, after seeing a U.C. Berkeley basketball game, 2015.

Ed Wilson on the deck of his home in El Cerrito, California, 2015.

The house was new when he moved into it in 1966. Therefore, Ed has spent nearly 50 years landscaping and maintaining a large yard overlooking Wildcat Canyon. This has included building decks, fences, and major remodeling projects. He has finally succeeded in becoming a good carpenter, which was his goal in life when he was 18 years old.

Appendix

Ray W. Clough

[This appendix consists of the incomplete oral history that Stanley Scott developed from interviews he had with Ray Clough in October of 1993 and July of 1994.]

Personal Introduction

Ray Clough's Oral History was incomplete when he and Stan Scott left off working on it in 1994. Therefore, I believe it is necessary to add a few comments to give the reader a more complete insight into the life and family of this great American engineer and scientist. He is one of the very few faculty members at Berkeley to be elected to both the National Academy of Engineering and the National Academy of Science. Also, he received the National Medal of Science in 1994.

In January 1953, at the age of 21, I transferred from Sacramento Community College to U.C. Berkeley. Ray Clough was my advisor for my junior and senior years. I only took one course, Analysis of Aircraft Structures, from Ray. It was a very enjoyable course. Ray was always well-prepared and very calm. He covered a significant amount of material, and the homework problems were always interesting and satisfying to work. I was fascinated that the same basic equations we used to design bridges could be used to design airplanes. Partly as a result of taking Ray's course, I changed my major to structures and decided to go to graduate school and learn more about structural engineering and structural mechanics. That changed the course of my career.

As an undergraduate student, I had one other significant encounter with Ray at the annual student-faculty tag-football game. I found myself lined up opposite Ray at the line of scrimmage. He had taken off his glasses and had a very determined look on his face. At the snap of the ball, I found myself on the ground and Ray was on his way to tag the quarterback. On the next play, I decided to play defense back where my running ability might allow me to avoid contact with this reincarnation of superman. Several years later, I learned of Ray's mountain climbing, skiing, cycling, and many other athletic skills.

When I returned to U.C. Berkeley to start work on my master's degree in 1957 after two years in the army, the topic Ray suggested was the Finite Element Analysis of plane stress structures. For the next six years, I worked part-time with Ray to obtain my MS and DEng degrees as I have indicated in my oral history.

Those graduate student years I spent at Berkeley between Ray's sabbatical leaves were the most productive years of my life. That was when I participated in the birth of the Finite Element Method. Also, Ray and I developed new methods for the linear and nonlinear earthquake analysis of tall buildings and nuclear reactors. When I was approached by EERI concerning my oral history, I accepted the invitation under the condition that Ray's incomplete oral history would be published in the same volume. I was very pleased EERI agreed to do so.

One reason Clough went back to earthquake engineering, after he was so notable for his original finite element work, was that the university wanted him to develop the department's earthquake engineering capability. He never forgot that part of his charge when he was hired: he was supposed to build up Berkeley's capability in that area, and he certainly did. That's one reason he went from analytical work to experimental work, once the Earthquake Engineering Research Center and the shaking table were established.

He and his wife Shirley were married in 1942, and they recently [2015] had their 73rd anniversary. Then Clough went into the military. Ray and his wife have retired to Oregon now. He retired from the faculty in July of 1987.

After reading both Ray's incomplete manuscript and the manuscript of mine, I concluded Ray's role as a husband and father had not been covered in those interview sessions back in the 1990s. For approximately 10 years, 1966 to 1976, we lived on the same street as the Clough family, Leneve Place in El Cerrito. During that period, my wife Diane and his wife Shirley become life-long friends. Also, I became friends with Ray's son Douglas (born in 1947 when Ray was a graduate student at MIT) and his daughters Allison (born 1951) and Meredith (born 1953). Doug received a PhD in civil engineering at Berkeley. I contacted Doug and asked him to comment on his early memories of his father and the travels of the Clough family. Doug replied with a fascinating document that is too large to publish here. Therefore, I will just quote, or summarize, selected topics from Doug's document.

"Ray's professional and family life were not totally separate. Mom's talent for interior design and her skills in the kitchen made their home always an inviting venue through the years. He, a genial host, and she, an ever-welcoming hostess, plainly enjoyed having people over. We children grew up with the cheerful hubbub of graduate students and colleagues and their families at our house, celebrating holidays and other special occasions.

Seen in its entirety, the list of academic and professional friends they entertained could well serve as an international who's-who of structural engineering. Friendships they formed during the years of our childhood are cherished to this day.

And, of course, Ray's family accompanied him on his sabbatical leaves, first to the Norwegian Institute of Technology, NTH, in Trondheim, in 1956–1957, and to Churchill College, Cambridge, England, in 1963–1964. These were wonderful times for all of us, filled with new sights and sounds, and exposing us children to foreign cultures, languages, and school environments. Away from his usual routine, Dad had more time during these years to spend with us, and we were delighted.

For my sisters and me, Trondheim was a fabulous playground, with unparalleled scenery, friendly and energetic playmates, and changes of season unlike any we'd experienced before. For Dad, it was all of this, plus engaging in productive research, the laying down of professional foundations, and the cementing of friendships to last a lifetime. For Mom, I must acknowledge, our enjoyment of Norway's wonders came at the expense of her extra toil, clothing and feeding us in this unfamiliar and challenging environment—though I know she will exclaim that we, and the enduring friendships she formed, made it well worth the effort!

Mom's folks joined us for our departure from Norway. We packed the belongings we'd shipped to Norway from home, plus memorabilia acquired during our stay, and delivered the several large trunks to a shipping agent at the harbor. As planned, Dad piled our personal luggage onto the roof rack, cinched down his tarpaulin cover, and ushered us all, three children and four adults, into the car for a magnificent, three-month European tour. Exploring the spectacular Norwegian fjord country, hiking in the Alps, swimming on the Riviera, visiting Paris, London, and Amsterdam, and then along with our car by ocean liner across the Atlantic to New York City. Mom's folks made their own way back to Seattle, while Dad drove us home in the small station wagon we had purchased when we arrived in Trondheim.

Accustomed now to the scale of European geography, I was impressed for the first time with the vastness of the North American Continent and the spectacular beauty of the United States. For a 10-year-old boy the 1956–1957 journey to and from Norway was one of the most unforgettable experiences of my life.

On our return to California, our family summer and weekend trips to Northern

California, Oregon, and Washington continued. Camping, skiing, and hiking were an important part of our lives. In the summer of 1965, the family lived in a tent while we built a four-bedroom house in Pinebrook near the Ebbetts Pass Highway. Ray designed the house and, with the help the family and Mom's father, produced a wonderful home in the mountains. This mountain home was used extensively by the Clough family and friends until Ray and Shirley moved to Sunriver Resort, Oregon in 1991. In 2008, they moved 20 miles north to a large two-bedroom apartment in Bend, Oregon.

I believe the impression made on him as a member of the Ptarmigan Club while an undergraduate at the University of Washington was profound, as if the ridges, peaks, and glaciers, the high places, represented for him what life itself offered. After that remarkable success as a teenager, he set his sights on greater objectives, climbed ever higher, and delighted in his friendships with his students and colleagues, like you, Ed, who joined him in the pursuit.

Last week on March 15, 2016, I had a very enjoyable dinner with Doug and Linda Clough, and we talked of our many experiences with Ray and Shirley. I asked Doug if Ray ever let him win a game when he was a teenager. He replied "No, he played every game to win." I then asked if Shirley ever complained while on their strenuous hiking or skiing outings. He replied "No, she was an active participant."

—Edward L. Wilson
March 31, 2016

Postscript: I received word from Ray's son, Doug, that Ray's wife Shirley passed away on April 17, 2016, and more recently that Ray died on October 8, 2016, at Mount Bachelor Village in Bend, Oregon.

Edward Wilson
October 11, 2016

Appendix

Chapter 1: Early life in the Pacific Northwest

Even for somebody as ignorant about the world as I was then as a seventeen-year-old, being in Germany in 1937—seeing the Nazis marching around—was "interesting."

Clough: I was born in Seattle, Washington, July 23, 1920, and was one of four children. I had an older brother and sister, and a younger sister. My father was a food chemist for the National Canners Association and was responsible for the quality of the canned salmon pack coming down from Alaska. While he was employed at the National Canners Association he took graduate studies at the University of Washington, and eventually obtained his PhD in food chemistry. He was well respected in his profession. He made a good salary, so our family was not much affected by the Depression years.

Scott: What were your parents' names and where did they come from?

Clough: My father's name was Ray W. Clough. For a time, I went by the name Ray W. Clough, Jr., then dropped the Jr. after he died.

He was from Vermont. He moved out to Seattle after finishing his college degree and began working with the National Canners Association. My mother's name was Mildred Nelson. She was of Scandinavian stock from North Dakota.

My Brother's Influence and the Boy Scouts and the Outdoors

Clough: I also want to mention my brother's influence. Ralph is four years older than me, and in some respects he led the way and I sort of followed along on a somewhat similar path.

Scott: Did that influence start when you were quite young?

Clough: Yes, from the beginning, when I was a baby and he was four. He was the leader whom I followed in neighborhood activities and things like that. For example, he led me into the Boy Scout movement. He became an Eagle Scout, but I never managed to get beyond Life Scout. He helped create in me a very strong interest in hiking, backpacking, skiing, and all of those kinds of activities because he pulled me along.

One major difference between us was that he was very good at foreign languages, and I turned out to be terrible in them. His interest in foreign languages was a major factor in his career. He became interested in the Far East in 1933 when he went to the International Boy Scout Jamboree in Australia. On that trip, he went by way of Canton, China and other Far East locations.

In about 1936, he spent his junior year as an exchange student from the University of Washington at Lingnan University in Canton. After completing his bachelor's degree at the University of Washington, he went to the Fletcher School of Law and Diplomacy at Tufts University in the Boston area, and then took the examination for the State Department's diplomatic service. He spent his whole career with the diplomatic service and spent all of it in the Far East after his initial assignment in Honduras, Central America. He spent a lot of time in China and was there during the Japanese war against China. He ended up getting kicked out of China—along with all the other Americans after the communists won the civil war—and going to Taiwan.

Scott: That would have been in 1949?

Clough: Yes. In 1947 and 1948, he was going to language school, and then went on down to Hong Kong, and finally ended up in Taiwan. His language ability was very useful, in his career, and he did well in it. He retired from the diplomatic service in 1971 at age fifty-five, and has made his home in Arlington, Virginia. He continues to be busy, writing books, giving lectures, and he most recently has taken up active work with the Johns Hopkins School of International Studies, in Washington, where he lectures one semester each year. He seems to enjoy the academic career. When I reached retirement age from the University of California, I said, "No more of that!" But my brother jumped into teaching activity with great relish.

The International Scout Jamboree, 1937

Clough: The next point at which I followed my brother was the International Scout Jamboree in 1937, which was held in Holland. Since my father had sent my brother on to Australia in 1933, he felt he owed me a similar trip when the next Jamboree came around. So I was one of the Seattle contingents that went to the 1937 Jamboree in Holland.

Scott: How old were you at the time?

Clough: I was sixteen when we left Seattle and turned seventeen during the trip. While the Jamboree was in Holland, we traveled over England, Belgium, France, and Germany. Even for somebody as ignorant about the world as I was then as a seventeen-year-old, being in Germany in 1937—seeing the Nazis marching around—was *interesting.*

Scott: You got a pretty good idea what was going on?

Clough: Yes, we could see it—we could not fail to see it! We were very aware of the militaristic program. Hitler was great on the color and pageantry of the marching groups. He liked the huge displays—any time he could put a few people out on display, he would put three hundred. That impressed me a lot, and I was happy to get out of Germany without having to spend any more time there.

Scott: Yes. He put on those big Nuremberg Nazi party conferences, for example.

Clough: Yes, that kind of thing. Very impressive pageantry. As I said, however, I was glad to get out of Germany. The part of that European trip that I really enjoyed was about ten days in Switzerland. We did nothing significant in mountain climbing, but we were getting at the edges of the important climbing. For example, up to the hut level on the Matterhorn, where the peak climbers spend the night. The guide who was our Boy Scout leader said he did not want to go any farther. But it was interesting to get that far.

When we got back to Seattle after about three months, I remember the Scout leader who managed our program made some sort of comment that it was nice we had that trip, because he doubted that any of us would ever see Europe again. Well, what has impressed me since is how frequently I have been to Europe. Also I would guess that a very large percentage of the boys in our group have been in Europe on occasion since then.

I remember how big an expedition it was to get to Europe. In those days, you didn't just get on a plane and fly there. We took the train across the United States, got on a ship in New York, sailed across to England, sailed across the Channel to get to the Continent, and traveled around by train on the Continent. Then we reversed the process when coming back. It took a lot of time and effort to make a trip like that. In contrast, I'll be going to Norway in October, 1994, and it is only a matter of about 14 hours flying from San Francisco to Hamburg. After that we will move a bit slower because I want to visit with many people on the way. But in terms of travel convenience, it is now a different world altogether.

Anyway, that International Scout Jamboree introducing me to those foreign countries was very significant. I have had a lot of interest in contacts with the foreign earthquake engineering groups since I am in the earthquake business, and I am sure that is what started it. I could see how interesting it was to meet people from different countries and try to interrelate with them in whatever way was appropriate.

Appendix

Chapter 2: Mountain Climbing and the Ptarmigan Traverse

We were out a total of 10 to 12 days, and during the process we climbed about six peaks that had not been climbed before.

Clough: I mentioned mountain climbing before. It was an outgrowth of the Boy Scouts. My brother, being four years older, was the patrol leader as an Assistant Scoutmaster, whereas I was just following along. Anyway, he and I switched from the Boy Scouts to what was called Rover Scouts in those days, or the senior scouting program. In the senior scouts, we were able to influence the emphasis of our activities toward mountain-climbing. We had what was called the George Vancouver Rover Crew, one of several Rover crews in Seattle. Ours was dedicated to hiking and outdoor activities.

The next step was to convert our George Vancouver Rover Crew into a mountain climbing club. It became the Ptarmigan Climbing

Club, named after little mountain birds found in the Pacific Northwest and other mountainous regions. We began using our time out-of-doors on mountain-climbing expeditions. As time went by, we began developing a small reputation for doing things differently. The big mountain-climbing group was the Seattle Mountaineers, which even in those days had several hundred members. We did not like the idea of being one among several hundred, and we wanted a small group of no more than 25 to 30; then, we could do anything we wanted and would not need to get approval from anybody else.

We organized our own hikes and did whatever seemed appropriate. To climb the more difficult peaks more safely, it became more and more technical, using ropes and pitons for belay purposes. We did a lot of rock climbing, using pitons to get attachments to the rocks, and threading the climbing ropes through the carabiners in the pitons to provide safety. These are not direct climbing aids, but are strictly for safety. In principle, if you have good friends with you, you can fall without significant damage. We learned how to be pretty good in managing the technique of climbing.

Part of it was on rock climbing, but a large part of it was on snow and ice. In the state of Washington, we have a whole succession of volcanic peaks—Mt. Baker, Glacier Peak, Mt. Rainier, Mt. Saint Helens (which used to be a good mountain to climb), Mt. Adams, and down as far as Mt. Hood. That was as far as our interests went. We began climbing all those major peaks and learning the techniques of using ice axes and chopping steps. I did not climb all the major peaks, but I climbed Rainier twice, and Baker, Glacier, and Mt. Saint Helens a couple of times—I think that is all the major peaks I climbed myself, although Ptarmigan club members in various combination climbed all of them many times. It was a question of who was available on particular days. We spent a lot of weekends on that sort of thing.

The only specific trip I want to mention is the one for which our Ptarmigan club became famous—called the Ptarmigan Traverse—which is still well-known among the Northwest mountaineering group. Four of us planned this trip in a part of the Northern Cascades that had hardly ever been explored. We started at Suiattle Pass and traveled north along the crest of several mountain peaks to the Cascade Pass and then returned by the shortest path available (approximately 25 miles) to Suiattle Pass. We were out a total of 10 to 12 days, and during the process we climbed about six peaks that had not been climbed before. One of our group had a Model-A Ford that we had parked at Suiattle Pass until we returned. When we got back, we loaded the Ford and away we went.

I remember planning very carefully to keep down the weight we would carry. Our plan was a pound of food per man per day, and that is not much. It was basically dehydrated food that could be cooked up with water, so that it was more than a pound of food to consume when you got it prepared. Our objective was for each one of us to carry about 12 pounds of food, plus a sleeping bag, a tent—which we never used but wanted for emergencies—the ropes, ice axes, and all those kinds of things. The packs were pretty heavy and it was a major effort, but it was something that we could do easily because we were young and strong. The food, however, was only enough for a bare existence. We got hungrier and hungrier as we went along, and toward the end, we talked mostly about all the food we would eat when we got back.

Scott: That was an austere diet to be on, along with all that very strenuous effort.

Clough: Oh, we lost a lot of weight. Nothing serious, but we had no spare fat when we finished.

Scott: The whole thing sounds like quite an expedition!

Clough: It was a great expedition. The Ptarmigan Traverse was recorded in the "Seattle Mountaineers" annual bulletin of about 1965. The Seattle Mountaineers published a bulletin once a year in those days in which they included descriptions of expeditions that members had made. It actually took place in 1938, but of course we had no publicity at the time—we just made the trip. We had left our names on the succession of peaks, however, indicating which ones were the first ascents and which were not. As the years went by, people began recognizing that a fairly significant trip had been made back there in the late 1930s. So one of the Mountaineers decided to write it up, and was able to contact one member of our group who still lived in Seattle. It made a nice little 12- to 15-page description of the trip.

The Ptarmigan Traverse is well known among the Seattle mountaineering community. If you now ask somebody in the Seattle Mountaineers or in the Recreational Equipment Cooperative group about the Ptarmigan Traverse, they would immediately know about it. Some 15 or 20 years ago, I gave a major lecture on earthquake engineering at the University of Washington, a nighttime event with 300 to 400 in the auditorium. Billy J. Hartz, my first PhD student, who had gone to teach at the University of Washington and is now semi-emeritus from the university, introduced me by describing me as one of the members of the four-man Ptarmigan Traverse team, commenting that while most in the audience knew my reputation as an engineer, very few of them would be aware that I was also "famous as a mountain climber." I appreciated that introduction and enjoyed it. I am still interested in the mountains and like to go up high, but nothing spectacular any more.

Appendix

Chapter 3: Early Education and the University of Washington

All the way through high school I had this vision that I was going to be a forester.

Elementary and High School

Scott: Could you talk a little more about your earlier years and your experience in elementary school and high school?

Clough: I remember grammar school. I went to Fairview grade school in Seattle. It was a first-to-sixth grade school, and one in which I had no real academic interest. It was just something that kids did. Then I went from grade school to John Marshall Junior High School, a typical three-year junior high school. I still essentially had no interest in learning as such; it was just something you did when you were at that age. I did not look on it as getting me anywhere in particular. It was something you did as part of growing up. I finished junior high school before developing any interest in the academic side of things.

In high school, however, I began to recognize that I could get good grades if I wanted to. I took a typical kind of pre-science or pre-mathematics high school program. It did not emphasize English, history, and subjects like that, but rather mathematics, chemistry, and subjects needed to go into science later.

Scott: By then, you had developed some of those interests or leanings toward science?

Clough: I could recognize that I did not want to follow my brother in languages. He was excellent in language, history, and subjects like that, but had no interest in mathematics or science, whereas I found all my interest lay there. I was not a mathematical genius, but of course in high school we only went through advanced algebra. I took physics and chemistry. I did not particularly like chemistry, but I went through it. Then I graduated from high school in the class of 1938, and started at the University of Washington in the fall of 1938.

At The University of Washington

Clough: When I got to the University of Washington, I began to have a different attitude toward education. At first, however, I was not really clear as to what I wanted to do. All the way through high school I had this vision that I was going to be a forester. I took some botany in high school, but did not enjoy it. Still, all through high school I keep visualizing myself as a forest ranger.

Scott: I suppose you thought of forestry partly because you liked the outdoors?

Clough: Yes, but when I entered the University of Washington, I did not really know what I wanted to do. Although my original interest was in forestry, one of the four members of the Ptarmigan Traverse who was a couple of years older and ahead of me at the University had gone into forestry. He had been at the university for two years when we did the traverse. I remember on that long expedition, sitting around the campfire with him asking me: "Do you really want to go into forestry? Do you really want to sit in a cabin in the woods all your life, with mostly animals to talk to?" What he really emphasized was that it could be a boring kind of existence. He sold me on that idea, because I would end up as he said. I had already taken courses in botany and realized I did not like that aspect of it. So he was the one who converted me out of forestry.

When I got to the University of Washington, I had to decide something, and my first thought was to follow my dad and go into chemistry. So I signed up in chemistry and was there for one quarter. I disliked it intensely, and the only "C" I ever got in my academic career was in that freshman chemistry class. I could see from the beginning it was not for me, even though my dad did well as a chemist, and liked being in a chem lab and doing all of those things. I switched out of it at the end of my first quarter.

Majoring in Civil Engineering

Clough: That was when I got to thinking that engineering might be what I wanted, specifically civil engineering. From that point I really never had any doubts about engineering, particularly civil engineering, partly because I thought I could do the outdoor things that I liked. During my career, however, I did not do a lot of surveying and other outdoor things that civil engineers often do, although I did some.

As soon as I got into civil engineering, I felt comfortable with the math and the physics that were part of the program. The engineering courses at Washington in those days were very good. They had what they called “general engineering” that they gave freshman engineers. Even as freshmen we were already doing what you could recognize as engineering.

A Trainee at Bonneville Power

Clough: With that background, by the time I had finished my junior year, I took a U.S. government civil service examination, and applied specifically for a student engineering trainee position, an appointment you could get after having finished three years. In the summer of 1941, after my junior year, I was appointed to work for the Bonneville Power Administration in Portland. There were many different kinds of student trainees, and I do not recall requesting Bonneville Power. I think it just happened, but it had significant effect on my engineering interests.

This was strictly a three-month summer trainee program. I think all of us who were in the program had finished the junior year. The salary was not much, but it was a professional kind of activity. That is when I began doing the outdoor activity I had visualized when I decided I wanted to be a civil engineer. I was out with survey parties most of that summer. I enjoyed it and have felt ever since that it was an excellent way to spend the summer.

Scott: So the work was interesting?

Clough: Yes, it was, and I enjoyed being part of the Bonneville Power Administration. Bonneville was and still is unique in the United States as a federal program providing power to a specific region. It has been compared with TVA, but it is not very closely parallel. Bonneville was quite new at the time and was doing significant engineering, taking advantage of the power generated as new dams were built along the Columbia River. Bonneville Dam is just upstream from Portland, and I believe was the first of the Columbia River dams producing power. From Bonneville, they strung lines to wherever they were going to distribute the power. I was in the transmission group the summer I worked for Bonneville.

Scott: Where were you located?

Clough: I started in Portland, in the office where they decided where the transmission lines were going to go. After about three weeks in the office, they sent me into the field to work on transmission surveys. First I was sent to Spokane and worked on part of the lines in that area. I worked out of Spokane part of the summer and also worked out of the mountains east of Ellensburg.

Part of the summer I worked on the construction of a little substation close to Seattle. Except for the time near Seattle, I was always working on transmission line surveys. The first step was to decide where the lines would be located, and the second was to specify exactly where the towers would go along the lines. We were primarily on the tower survey. When around Seattle, I was primarily working on construction of one of the power substations. I was just doing typical inspection work for the construction of the reinforced concrete building. All in all, however, it was a very valuable kind of experience. It gave me contact with some engineering in the real world, after having seen it from the academic side for three years.

In the summer of 1941, I began thinking about graduate school. I expected to graduate in June of 1942 and applied to the Massachusetts Institute of Technology, hoping to get into their graduate program to start in the fall of 1942. I was accepted at MIT and got a small scholarship, but of course a very significant event occurred at the end of 1941 that changed my plans.

Appendix

Chapter 4: The War Comes

We had not been told exactly where we were going, of course, but the rumor was that we were part of the group being readied for the invasion of Japan.

Clough: After Pearl Harbor, I checked with my draft board about my status if I went to MIT. They told me that as long as I was finishing my undergraduate degree, they would not threaten me with being drafted, but said I would become a real target if I decided to go to graduate school in the fall of 1942. That convinced me to defer my graduate studies and to look toward the Boeing Airplane Company.

In June of 1942, I took a job with Boeing engineering Seattle. It was essentially trivial engineering work. That job pretty much guaranteed my deferment throughout the war. I stayed with it until about September of 1942, but by then I'd started thinking that it was a menial kind of job and a pretty dumb way to spend the war. It was not interesting engineering. So, in September of 1942, I saw some advertisements about becoming a weather officer, so I went down and signed up to become a cadet in meteorology in the Air Force. That also guaranteed that I would not get drafted.

Enlisting in the Air Force

Clough: I then enlisted in the Air Force aviation cadet program as a weather officer. In those days, the Air Force was part of the Army, not yet having become a separate branch. I chose that because I could pass the physical examination. Due to my poor eyesight, I could not pass the physical for the Naval See Bee (construction battalion) program, or any of the other regular officer programs. About then I decided to get married, which I did at the end of October 1942, prior to reporting for active duty. I had met my wife, Shirley Potter, while we were attending the University of Washington.

My wife and I took the train down to Pasadena for training at Caltech, and I reported there about the end of November. I was immediately issued a uniform and they got me to acting like an aviation cadet in a basic training program. The housing for the Caltech group was in the Constance Hotel, which is in Pasadena, about a mile from Caltech. As an aviation cadet, I lived in the Constance Hotel, and my wife found an apartment close to Caltech that we could afford. I spent six nights a week at the Constance Hotel, and had Saturday nights free to stay at the apartment with my wife. She got a job at Caltech, which was short of any kind of help. She was able to help support us and we survived as I was getting only $60 per month, which would not even pay the apartment rent. Also we purchased a used car.

A Master's Degree in Meteorology at Caltech

At Caltech I got my master's degree in meteorology in September of 1943. Then they kept me on at Caltech as an instructor, which I continued until June of 1944. At that point, in principle, I was supposed to become an active weather forecaster, somewhere. But I never felt very comfortable as a weather officer, and started immediately applying to become an engineering officer. Eventually I was transferred into the aviation engineering program. Aviation engineering was like the Corps of Engineers, but under the Air Force. While I was stationed in the States, Shirley was able to live off base during all of my assignments.

Shipping Out to the Pacific Theater of the War

Clough: I went overseas and was working as an airfield construction officer during the last part of the war. At the end of July 1945, my men and I were on a troop ship going from Hawaii to join the war in the Pacific. Not long after we left Hawaii in the convoy, the Hiroshima bomb was dropped during the first week of August 1945. That was great news for all of us on the ship. We had not been told exactly where we were going, of course, but the rumor was that we were part of the group being readied for the invasion of Japan. Airfield construction would obviously be an important part of that.

So the atomic bomb sounded like a great idea to me. We were still proceeding in that same direction, toward Japan, when the Nagasaki bomb was dropped. Then everything sort of came to a halt. The whole convoy was sent to Ulithi Atoll, a typical Pacific atoll that made an ideal harbor, having a ring of reefs with openings through which they could bring the bigger ships. We were fairly well protected from the elements there, and it was safe from submarines. We just sat there at Ulithi Atoll for something over two weeks.

Scott: They in effect parked you there because the war and military situation was obviously changing very rapidly.

Clough: Yes. After Nagasaki, the Japanese made signs that they would like to surrender and get the whole thing over with.

We proceeded to Okinawa, where we arrived on September 2, about when the peace document was signed in Tokyo. Our landing at Okinawa was straightforward. It was necessary to get all our equipment off the ship. We all had to go ashore in landing craft because it was the only facility they had to get ashore. But it was routine, since nobody was opposing our landing. We were brought ashore and directed to the area where we were going to set up our own camp. We were there from September 1945 to May 1946.

Scott: Is this when you received orders to go to China?

In China

Clough: Yes. I suddenly got orders from General MacArthur's office in Japan to report to Mr. Ralph Clough in Tientsin, China. I had been assigned to report to my brother because he had been asked to assist General George Marshall in getting U.S. equipment in China back in order again. He said he would be glad to do that, but he needed engineering assistance in order to fully assess what was needed. The idea was to take surplus equipment and supplies to give to our allies in China. Anyway, Ralph had the good idea, since I was on Okinawa doing nothing, that I might as well go over to China and help him with his new job. It was a great assignment for me.

Once I met my brother, we started doing what we were supposed to do. We first surveyed the facilities in Shanghai where we were, and then went on down to Canton and looked over those facilities. Then we went to Hong Kong. There were lots of other facilities that needed inspection, and I was enjoying myself, but I knew that by then I had accumulated enough points to get back home again, and that I would not get shipped home out of China. After about three weeks, I told my brother: "Thanks, but that is about enough." I had to rejoin what was left of my unit to get orders to be sent home. It was interesting, however, to have had a visit to China in 1946, long before Nixon made his "ping pong visit."

Scott: You were there three years before the final take over by the communists under Mao in 1949.

Clough: Yes. My brother was able to get assigned to the Beijing language school after he finished the stint for General Marshall, and he was able to get his wife sent to Beijing to join him. He spent about two years in intensive language studies, working for the U.S. State Department. Part of the problem was that for the most part, he had studied Cantonese Chinese. But he really had to polish up his Mandarin, which was what he was doing in Beijing. After two years of that, the Chinese communists had begun closing in and pushing all of the non-communist people ahead of them back to Shanghai, and finally back to Hong Kong.

So after three weeks on that assignment with my brother, I went back to Okinawa. Then I got out of the service about the end of May or maybe the beginning of June in 1946.

Appendix

Chapter 5: Graduate Study at MIT

In the Department of Aeronautical Engineering, I took a course in the dynamics of aircraft structures from Professor Bisplinghoff, which was probably one of the most influential courses at any time in my college training. That was really what got me pointed toward dynamics in engineering.

MIT ScD 1946–1949

Clough: By early June 1946, I was out of uniform, and somewhere along the line, I reapplied to MIT, reminding them that I had already been admitted to the graduate program back in 1941. I did not need the fellowship because I would be supported by the GI Bill program. Shirley and I went to MIT in the fall of 1946. I finished in June of 1949. At MIT, I had the standard things in civil engineering to get my master's in structural engineering.

In addition, however, I also had opportunities at MIT that I had not had before. In the Department of Aeronautical Engineering, I took a course in the dynamics of aircraft structures from Professor Bisplinghoff, which was probably one of the most influential courses at any time in my college training. That was really what got me pointed toward dynamics in engineering. I enjoyed that course, and it taught me a lot. I also took a course in mechanical vibrations from Professor Hartog, one of the great names in the field of vibrations.

Doctoral Thesis on the Buckling of Arches

Clough: My ScD thesis was on the buckling of arches using both analytical and experimental methods. When I was about to get my doctor's degree, I began writing to various places about the possibility of employment. I wrote probably half-a-dozen places and got two job offers that had considerable interest. One was from Caltech and the other from Berkeley. I had already been at Caltech and knew quite a bit about Pasadena and the area. Also, while teaching meteorology there, I had taken a non-credit course in structural engineering, so I knew some of the faculty in civil engineering. I went to my boss at MIT, Charles Norris, and said, "I got these two interesting job offers, both starting as beginning assistant professor and at essentially the same salary. Which one do you think I should take?" He had no hesitation: "Go with the University of California at Berkeley. Don't go to a private university." For him, the fact of state support of the educational program was the dominant factor. He had been at a private institution at MIT, and the whole time he was there had suffered from the shortage of money. I thought if he was that enthusiastic about the state institution and Berkeley, I'd go with his recommendation. I never regretted that decision.

I wrote the people at Berkeley to accept their job offer and tell them more about my background. When they found out that I had taken the dynamics of structures, they said, "He's going to be our specialist in the earthquake field." When I showed up, they told me what I was going to be doing.

Appendix

Chapter 6: Joining the Faculty at U.C. Berkeley

I was not optimistic about the prospects of getting financial support for earthquake engineering research in those days. Nothing was being funded.

Hired to be Berkeley's Earthquake Engineering Specialist

Scott: You were to be the earthquake engineering specialist at Berkeley?

Clough: Yes. As a matter of fact, before I came here, they had hired a man from the Naval Research Lab as a visiting professor. His name was Henri Marcus, and he was a Frenchman hired by the U.S. government after the war when they were picking up scientists who were available in the European countries. He was a first-rate person in the field of dynamics. Not primarily structural dynamics, but he knew something about it. He had a significant reputation in France in civil and structural mechanics. The U.S. had brought him over to work in the Naval Research Laboratory in Washington, DC. Berkeley had hired him to be the first person to teach structural dynamics here at Berkeley.

Structural Dynamics Course

Clough: I had come in the fall of 1949, but he had already been contacted and was going to be teaching the course, so they asked me to be his assistant. He came out and taught in the spring of 1950. I worked closely with him as he was giving lectures. I took notes, wrote up problems, graded the answers, and did the other things to help Marcus. That work put me in contact with structural dynamics from the teaching side. It was very useful to have that introduction to the teaching of structural dynamics, but one of my first decisions when I taught my own course was that it would definitely not be the same one he taught.

In my second full year at Berkeley, I was given the assignment of developing a course on structural dynamics. I integrated some of the material from the Marcus course in my lectures; however, I followed much more closely the dynamics of an aircraft structures course I had taken at MIT. That was really the beginning of the course called dynamics of structures taught continuously at Berkeley since then.

Scott: That has been the basic course in earthquake engineering, I presume?

Clough: Yes, the basic course. The versions have varied some, depending on whether we were on the quarter system or the semester system. It was always the course where students were introduced to what is different when you get into a dynamic situation, as contrasted to the traditional static structural analysis.

Scott: Would you say a word or two more about how you shifted from what Marcus was teaching, and about what you built on from the course you had at MIT in the dynamics of aircraft structures?

Clough: Actually, the difference between aircraft structures and civil structures is immaterial as far as the teaching of structural dynamics in concerned. The logic of the presentation at MIT was excellent. The course was given by Professor Bisplingoff, a very able teacher and an outstanding aircraft structures engineer. He knew how to present the material, whereas I do not think Marcus had ever had any teaching experience.

The material Marcus used was good, but the presentation was such that the students could not understand the material. I took notes and developed problem assignments that the students could work. It was hard even for me. The course lacked the logic and proper pedagogical approach.

The Beginnings of Dynamics in Structural Engineering

Clough: That basic course in earthquake engineering was important. It was the beginning of dynamics in structural engineering at Berkeley, and it began to develop the field of structural dynamics here. It also began to create some interest among the structural engineers in San Francisco.

Scott: Previously, in the 1930s, through World War II and up to about 1950, earthquake studies at Berkeley would have been principally in geology and seismology, especially the work of Perry Byerly?

Clough: Yes, it would have been in Perry Byerly's group, and looked at from the point of view of geophysics and seismology. For structural engineers back then, the solution to the earthquake problem was simply 10 percent of the weight applied horizontally. It was strictly static, and nothing about the dynamics of structures was even mentioned. It was inadequate. That was basically why the I-880 double-decked structure (the Cypress Freeway) collapsed in the Loma Prieta earthquake in 1989. The engineers did not have the concepts of the dynamics of the responding systems. The transportation department that built the freeway thought 10 percent of gravity horizontal force requirement was sufficient.

The Extension Course for Practitioners

Clough: I think in the second year I was here, San Francisco engineers began requesting a University Extension course in structural dynamics. Probably in 1951, I began teaching in Extension the same course I was giving for the regular graduate students of engineering. That proved to be an excellent thing to have done, because it brought me in contact with active workers on the practical side of structural engineering in this area. People like Henry Degenkolb, John Blume, John Rinne, and others who were really big in structural engineering in those days. They sat in on my Extension class.

Our Research Program at Berkeley

Clough: The other important thing was the research program. I was not optimistic about the prospects of getting financial support for earthquake engineering research in those days. Nothing was being funded. I just kept my eyes open for anything that involved structural dynamics. It turned out that a succession of summer jobs helped me along the way. The first two summers, 1950 and 1951, were on a San Leandro Creek bridge project. The next two, 1952 and 1953, were at Boeing in Seattle. And the next three after that, 1954, 1955, and 1956, were on a torpedo net project for the Navy. I'll discuss each of these, although not in precise chronological order.

The San Leandro Creek Bridge, 1950–1951

Clough: The first job that related to structural dynamics was through the Institute of Transportation and Traffic Engineering, which Harmer Davis directed. Somehow he got the job of field measurements for the San Leandro Creek Bridge. They were in the process of developing what used to be called State Highway No. 17, the Nimitz Freeway, along the east side of San Francisco Bay. One of the early structures on that was the San Leandro Creek Bridge. Davis had already more or less contracted for doing a field measurement program on that structure as it was built, and I was in a good position to begin doing the work. Charles Scheffey, who preceded me on the Berkeley campus, was in civil engineering structures and was interested in experimental methods and structural engineering in general. He was active there on the civil engineering staff for close to 20 years, and then retired to take a job with the Federal Highway Administration, in charge of their research program. He still lives in Washington, DC. Scheffey and I became the investigators on the San Leandro Bridge project. In the summers of 1950 and 1951, as the bridge was built, we went down and put in all the field instrumentation, including strain gauges, accelerometers, and recording systems.

The highway people's interest had nothing to do with earthquakes. They were only concerned about vertical dynamics, not horizontal, and simply wanted to know what happened when the heavy trucks came bounding along the freeway and over the bridge. They were concerned with the vertical effects of the loads, and that is basically what we instrumented. I don't think we put anything on the columns. The instrumentation was strictly on the girders, measuring the frequency of the vibrations as they were generated.

Nevertheless, the job gave me support to work in structural dynamics and into the business of field measurements. It was a good job, and we spent two summers working on it. The first summer we got the instrumentation up, and the second summer we did lots of tests with trucks. This was before the route was totally opened, but the bridge itself was available, and we had trucks moving over boards on the pavement, or something else that would make an impact.

Testing Torpedo Nets for the Navy, 1954–1956

Clough: A second job involving structural dynamics came up when the Navy was interested in having tests done on the strength of torpedo nets. In the early 1950s, the Navy had a net depot over at Tiburon, where I think they still have some sort of small facility. They wanted some measurements to show what happens when torpedoes hit the nets. This was a very interesting thing to work on. Again, Scheffey and I agreed to do the job, financed by a research grant handled through whatever they called the research group here at the Richmond Field Station at that time. That job lasted through the summers of 1954, 1955, and 1956.

Scott: Describe that project and the work you did.

Clough: The Navy was quite interested and willing to stretch out nets and string cables at appropriate locations and would arrange to have a boat that had a torpedo-launching device on deck. They would shoot the torpedo off from the deck. The torpedo would land in the water and go charging off into the torpedo net. Our job was to measure what happened.

Scott: Were the nets instrumented?

Clough: The nets were not instrumented themselves, but the supporting cables were. We put in instrumented links to measure the dynamic forces caused when a torpedo hit the net. Our research grant was to make these measurements and do analytical studies to see how well we could predict what happened to the stresses in the cable when the torpedo hit. That work in itself was very valuable to us, because it was something we could only do with digital computers, and that job gave us access to really good computer capability. The equipment we had at U.C. Berkeley was limited—the University did not have really good computers, but only small card-programmable calculators. Through the Navy, we got access to the computers at the Naval Ordinance Test Station (NOTS) at China Lake, California. Those computers were as good as were available anywhere. So on that project, we learned a lot about the computation of dynamic response under rather severe impact conditions, as well as about dynamic instrumentation. In short, that job gave us capabilities in structural dynamics that, although not relating directly to the earthquake problem, were extremely valuable as we got into earthquake-related field measurement.

The Boeing Summer Faculty Program, 1952, 1953

Clough: In 1952 and 1953, between the San Leandro Creek job and the torpedo net work, I spent two summers at Boeing. In 1952, the Boeing Aircraft Company announced what they called a "Summer Faculty Program." Under that program, the firm would hire younger faculty people and give them summer jobs when they were away from their regular teaching responsibilities.

Scott: Was that open to young faculty members anywhere in the nation, or did they draw mostly from the western United States?

Clough: They were from anywhere, although I don't know how selective they were. I applied, got an appointment, and went there for the summers of 1952 and 1953 (June–September). It was a remarkable program, and very, very important to my academic career. I was interested because I found that I could work at Boeing with their structural dynamics unit. It was a real opportunity to work with people who were experienced in structural dynamics. As I mentioned before, it does not matter whether it is airplanes, buildings, bridges, or what, it is all structural mechanics in the dynamic mode. It was important partly because it gave me experience in dynamics. We will discuss this work in more detail under the chapter on the development of the finite element method.

Secretary of the Earthquake Engineering Research Institute

Clough: I also want to talk about the Earthquake Engineering Research Institute (EERI). You probably already know something about the origins of that.

Scott: Yes, from interviewing John Blume and others.

Clough: John Blume was one of the originators of EERI, probably as much as anybody. John Rinne was another. Henry Degenkolb was active. EERI started out as the Advisory Committee on Engineering Seismology (ACES), a committee of the U.S. Coast and Geodetic Survey, to give advice on developing and putting out strong motion instruments. The committee members later decided that working under the format of a Coast and Geodetic Survey committee was not suitable for their interests, and they started up EERI. I could never understand why they called the new association a research institute, because it had no means of doing research.

Anyway, EERI was activated in 1949, and when I came to Berkeley in the fall of 1949, the University of California was represented on EERI by Harmer Davis, who at that time was a professor of soil mechanics at Berkeley. He had been a member of the advisory committee and was part of the group that became EERI. By that time, however, Harmer Davis had become director of the recently formed Institute of Transportation and Traffic Engineering. When I showed up at Berkeley, he said, "I'm not really in earthquake engineering anymore and think you would be a much better person to represent the University on EERI." He made the suggestion and it was approved by EERI. I think I attended my first meeting of EERI in the spring of 1950, as a real neophyte. I hardly knew my way around.

In the early days, EERI was an extremely limited group. Everybody who was there had some function or reason for being there. So it was important from my point of view to be recommended for that post. Of course, once I was there, the first thing they did was make me secretary. I think I functioned as EERI secretary for the first five years that I was with them. That was a very valuable experience because it put me in contact with the right people in the earthquake engineering community.

Scott: Yes, teaching the University Extension course and being secretary of EERI would have been almost ideal ways to make contacts with the top structural engineers.

Penzien Joins the Berkeley Faculty

Clough: I should have mentioned another important development in the summer of 1953. Joe Penzien was working at an aircraft company in Texas. He wrote me saying he was tired of working in the aircraft industry and wondered about an opportunity for somebody of his background here at Berkeley. I had known Joe at MIT, where he had been one-and-a-half or two years behind me. He had graduated from the University of Washington, the same as I did. I had not known him at Washington, but because he was a Washington graduate I tracked him down at MIT. I was a teaching assistant there by the time Joe came. Joe's interest struck me as the greatest of opportunities—raising the possibility of getting a second person into structural dynamics at Berkeley. In fact, I had already recognized the difficulties that were coming up because there was no one else at Berkeley who could teach the course in structural dynamics. In that circumstance, I could not go on sabbatical leave, so I was very happy to get the letter from Joe.

In those good old days, we were able to get him appointed very quickly, so I immediately had a back-up teacher, and would not be forced to forego sabbatical leave. His coming did much more than that, of course, as it broadened our capabilities tremendously. Initially we had the idea that Joe would focus on wind engineering, and I would continue to be Berkeley's earthquake specialist in structural engineering. Very quickly, however, we realized that there was much more activity in earthquake engineering than in wind engineering. So there was no problem in finding plenty of room for both of us in the field of earthquake engineering. Having a second person on the staff in this field was a tremendous step forward.

Clough: We designated Joe as the person to deal with the probabilistic side of earthquake engineering. All I had ever done was strictly deterministic. Joe later took a leave of absence, went back to MIT, and studied the probabilistic, random vibrations approach. From that point forward, I said I would have nothing to do with the probabilistic approach—that is

Joe's. That was true even of our writing the structural dynamics book together.[73]

Earthquakes 1957 to 1967

Clough: In my thinking, a key to the development is the earthquakes that occurred in the 1950s and 1960s. The Mexico City earthquake occurred in 1957, but I was then already on my sabbatical leave in Norway and not available to go down and visit the damage, so Joe Penzien was the person from structural engineering in Berkeley who went to Mexico City.

The Chilean earthquake (1960) and the Agadir earthquake (1960) were important events to earthquake engineering worldwide, and I went to both of those. Mentioning the 1960 Chile trip brings up another name that we should not forget, that of Karl Steinbrugge. He was with the U.C. Berkeley Department of Architecture and was an important figure in the Earthquake Engineering Research Institute. He was very important in the observations of earthquake damage. Karl was the leader of the EERI team that went to Chile after the 1960 earthquake, and he and I were the two team members who went from U.C. Berkeley.

The Agadir earthquake occurred later in 1960, and I went there on a visit sponsored by the American Iron and Steel Institute. A Pittsburgh man from AISI was designated as the leader of the team, and Rube Binder, a significant person in EERI in those days, was a team member. We wrote up what I think was a pretty good report on the Agadir earthquake.[74]

The next major earthquake I can think of was the 1964 Alaska earthquake, but I was again on sabbatical leave overseas, and Joe Penzien went. By 1964, there were probably half-a-dozen who went from Berkeley to survey the damage. By that time, there was a big interest on the part of geotechnical engineers, as well as structural engineers.

The earthquake in Niigata, Japan also occurred in 1964, and, of course, I was still overseas on my sabbatical. There was very active participation by Berkeley as well as EERI in the observations on that earthquake. That earthquake got Harry Seed very actively involved in studying the liquefaction phenomenon.

The next big earthquake that people visited from all over was in 1967, in Caracas, Venezuela. I was not involved in the observations made immediately afterward, but did go there a few months later and gave some earthquake engineering lectures in the national university. I do not know who all went down, but am sure that Karl Steinbrugge must have.

Scott: Yes, and Henry Degenkolb. I know they went, and several other practicing engineers from California.

Clough: You know, a good way to track the development of our Berkeley group would be to look at the reports on the earthquake investigations made after the successive earthquakes

73 Ray W. Clough and Joseph Penzien, *Dynamics of Structures*, New York: McGraw-Hill, lst ed., 1975, 2nd ed, 1993.

74 Ray W. Clough et al., *The Agadir, Morocco Earthquake, February 29, 1960*, American Iron and Steel Institute, 1962.

to see who participated. That would give a pretty comprehensive view of participation by Berkeley people. It would provide very good clues as to who came next in the additions to the earthquake engineering group at Berkeley. As for myself, however, after about 1967 or thereabouts, I no longer went on the post-earthquake visits, because there were other people who were much more involved. I am sure Vit Bertero was on board at Berkeley well before that, and he was an extremely good one to participate in earthquake visits.

Appendix

Chapter 7: Development of the Finite Element Method

Although the Boeing people did not invent the name "finite element method," in most people's thinking, that work represented the beginning of the finite element method.

Working at Boeing

Clough: Working at Boeing during the summers of 1952 and 1953 was the beginning of my research on which later would be called the "finite element method." Boeing was particularly interested in the dynamics of delta-wing airplanes, those with triangular shaped wings. In vibrations of ordinary airplanes, the wing can be treated as a beam. Such a wing has more or less the same dimensions from the beginning to the end, and can be treated by ordinary beam theory. But the delta wing changes width dramatically as you move from the beginning of the wing to the end, and this cannot be approximated by beam theory. Jon Turner, the head of Boeing's structural dynamics unit, with whom I worked, was a really sharp person and an ideal man for me to interact with at that time. He considered the big problem to be the fact that they could not treat the delta wing as a series of short beam segments. He wanted to treat it by two-dimensional elasticity instead of by any approximation of beam theory.

Scott: So the basic problem was developing new ways of analyzing dynamically what happens in a delta wing?

Clough: Yes. It was for dynamics, of course, because this was the structural dynamics unit. The stresses developed were never really a matter of concern in that group at that time I was there. What they were trying to do was predict vibration and "flutter" properties, because that is what gave them trouble in their supersonic airplanes. The vibration and aerodynamic instability produced in flight tore up several airplanes and they crashed. This was a significant problem for many military aircraft from factories all over the world. They wanted to avoid the conditions that they called flutter, which could lead to rupture and the breaking up of the wing and the plane.

Dividing a Delta Wing into Plate Elements

Clough: Turner thought we should somehow divide the delta-wing panel into little plate elements of appropriate shapes, and then assemble the plate elements to develop the complete structural system. First of all, you would try to understand the properties of the individual elements. We worked with two types, rectangular and triangular. If the stiffness properties of each of the individual elements could be established, then all the little elements could be assembled into a stiffness matrix for the entire structure. The system they had been developing at Boeing was called the "direct stiffness assembly method." The concept was well understood, but determining the properties of the individual elements was the place where they were having difficulty. That is what Jon Turner asked me to start working on in the summer of 1952. I spent the summer reading literature that might relate to that subject, and trying different ideas. Anyway, the techniques I used that first summer were methods developed by other people that simply did not do the job.

What we did have available at Boeing was static deflection measurements in models of delta-wing structures. Our purpose was to develop an analytical procedure capable of predicting the deflection in the structures. The approach—again, figuratively speaking—was to cut it up into little pieces, and then assemble these to get the total stiffness. Once you get the wing stiffness, figuring the vibration properties is straightforward mathematics.

I spent the whole summer working on different ideas, and comparing the results we got from actual measured results. The answers we got were obviously not very good. Then the first summer ended and I had to go back to Berkeley in September 1952. I told my supervisor, Turner, that I hoped to come back the following summer and work on it again, because I thought by the next year we could solve the problem.

He agreed, and did a lot of thinking on the problem himself during the winter of

1952–1953. He basically created the concept that we ultimately developed. I won't go into the details of the method, but it was based on a virtual work analysis of the triangular or rectangular plates subjected to appropriate loads. By making very simple assumptions, you could get a pretty good approximation of the stiffness properties of those little plate elements. With a triangular plate, the assumptions were as simple as uniform stress in the X direction, uniform stress in the Y direction, and uniform shear stress. Those three assumptions led to numbers for the three coefficients we were trying to calculate.

That is what we thought would do the job. We tested it with all of the experiments for which information was available, and came up with pretty good results. Jon's group at Boeing continued putting these results into the structural dynamics capabilities and began predicting vibration frequencies based on the coefficients we were getting from the individual elements. That led to a paper presented at a conference in January 1954, and published as "Stiffness and Deflection Analysis of Complex, Structures."[75] Many think of that paper as the beginning of the finite element method, although the Boeing people used the terminology "direct stiffness method." That was proper terminology in one sense, of course, but did not address the characteristics of the individual elements. If you calculate the stiffness of each individual piece, you can add them together to get the stiffness of the complete structure. Although the Boeing people did not invent the name "finite element method," in most people's thinking, that work represented the beginning of the finite element method.

Still, it did not completely address the problem, because it only dealt with stiffness and deflection analysis. It did not come to grips with what most engineers are interested in, namely the stresses developed in the structure. To all of us working on that at Boeing, it was quite clear that the stresses were available in what we were doing, but the internal turf politics at Boeing were such that the structural dynamics unit was not permitted to talk about stress analysis, because there was another unit called the stress analysis unit. So we just did not talk about or deal with stress analysis. We would calculate deflections and vibrations, but nothing about stresses.

Fulbright Fellowship to Norway, 1956–1957

Clough: I have accounted for my first seven summers after coming to Berkeley: two on the bridge project, two at Boeing, and three on the torpedo net project. After that, in 1956–1957, a Fulbright fellowship got me to Norway, to the Ship Research Institute of Norway in Trondheim. I applied for that because they were interested in ship vibrations, and I was eager to get into anything that looked like structural dynamics. The year in Norway gave me almost full free time. I worked with the Ship Research Institute a little bit. I was on a sabbatical from Berkeley, and the Norwegian institute also treated me as on a sabbatical. They were doing ship vibrations work, and I did go on one cruise from Trondheim to Bergen and back, when we were measuring vibrations of a new ship.

75 M. J. Turner, R. W. Clough, H. C. Martin, and L. T. Topp, *Journal of Aeronautical Science*, Vol.23, No. 9, pp. 805-823, September 1956.

While that year was valuable in giving me contact with people interested in the practical side of structural dynamics, the most important thing was that it gave me time to dig into the literature that led up to the finite element method. It gave me time to begin development of the finite element stress analysis procedures, but my work was extremely rudimentary because in Trondheim in those days they did not have any computers. But I could work on the fundamental concepts. My 1960 paper was a direct outgrowth of the 1956–1957 year in Trondheim, plus getting back to Berkeley and working with the IBM computers that were then becoming available.

That is when I wrote the paper in which the name "finite element" was coined.[76] So there was a period there when a certain group of people recognized the stress capability of the method, but simply did not do anything about it. That 1960 paper introduced the concept of the finite element method as a technique for stress analysis.

Scott: I found your retrospective 1979 paper given at a conference in Norway.[77]

Clough: Yes. I did not pursue the history much at that time, but did in the 1991 paper for a Duke University conference.[78]

In retrospect, I believe the finite element paper presented at a conference in Lisbon, Portugal in 1962 had a more significant impact on the engineering profession. It was co-authored with Ed Wilson.[79] The paper illustrated the superior power of the very simple Finite Element Method in the analysis of Norfork Dam in Virginia, which had a vertical crack. In addition, the paper was also published in the International RILEM Bulletin, which had a very large circulation. Therefore, within the next several years, funding for finite element research was pouring into the Department of Civil Engineering at U.C. Berkeley. Also, international students and visiting scholars came to the campus to work in the new area called "Computational Mechanics."

Ed Wilson was my student fairly early and did his thesis on finite element studies. He joined our staff here not too many years after he got his degree. He told me today that he is deliberately putting together some of these things. Ed has been putting together a more recent

76 Ray W. Clough, "The Finite Element Method in Plane Stress Analysis," *Proceedings of the ASCE 2nd Conference on Electronic Computatio*n, Pittsburgh, PA, September 1960.

77 Ray W. Clough, "The Finite Element Method After Twenty-five Years: A Personal View," in *Engineering Applications of the Finite Element Method*, vol. 1, papers presented at the International Conference on Application of the Finite Element Method, May 1-11, 1979, Hovik, Norway, also published in *Computers and Structures,* vol. 12, no. 4, 1980, pp. 361-370.

78 Ray W. Clough, "Original Formulation of the Finite Element Method," *Finite Elements in Analysis and Design*, vol. 7, pp. 899-101, 1991.

79 Ray W. Clough and Edward L. Wilson, "Stress Analysis of a Gravity Dam by the Finite Element Method," *Symposium on the Use of Computers in Civil Engineering*, Lisbon Portugal, No. 29, October 1962. Also, published in RILEM Bulletin, No. 10, June 1993.

collection of papers now to document the early history.[80]

I had a fine library of early finite element material that I gave away to Jurgen Bathe, a former student of Ed's, who now teaches at MIT. Bathe was one of the good early students conducting finite element research here at Berkeley.

This is all prehistory as far as earthquake engineering here is concerned. But I think it was extremely important to the Berkeley's structural mechanics program.

80 Edward L. Wilson, "Automation of the Finite Element Method—A Personal Historical View," *Finite Elements in Analysis and Design*, vol. 13, pp. 91-104, 1993.

Appendix

Chapter 8: Berkeley's Field Testing Program

I could not get any support for anything called earthquake engineering. I could only get financial support when I was talking about the finite element method.

Hiring Jack Bouwkamp

Clough: In 1957, during my return trip from sabbatical leave in Norway in 1956–57, I interviewed someone then in Holland, who had written the University of California about a possible teaching job. It was Jack Bouwkamp, who had been doing work on field measurements of structural problems and was interested in coming to California. I was asked to go by his home to interview him during my planned travel from Norway, through Holland, and back to the United States. I met with him and gave a very strong recommendation, having concluded that he was the kind of person we really could use at Berkeley, as a field-measurement person more than anything else.

Meager Support for Earthquake Engineering

Scott: Up to that time, the mid-1950s, there still was not much research in structural dynamics going on. And even less in the dynamics of earthquake engineering. You mentioned the summer projects on the bridge and the torpedo nets. You had to pick funding where you could. You had not been directly involved in any earthquake engineering research?

Clough: That is right. During the period between my first sabbatical in Norway and my second sabbatical in Cambridge (1957–1963) I was mixing my time between the finite element method and some consulting work related to earthquake engineering. I was putting in more time on finite elements than earthquake engineering. I could not get any support for anything called earthquake engineering. I could only get financial support when I was talking about the finite element method. Consequently, my doctoral students all drifted into finite element work. I did not have contacts that led to support for what we would call earthquake engineering.

None of the research support I had gotten up to that time related to earthquake engineering, per se. In fact, even when the 1957 Mexico City earthquake struck, I was in Norway on sabbatical, and so could not go to Mexico. Joe went down and observed the damage from that earthquake. Joe had already been in contact with the professional engineers in San Francisco who were interested in earthquake engineering. Because of those contacts, and the Mexico City earthquake, he quickly got into the practical side of designing so as to avoid earthquake damage.

I cannot remember when the first thesis in the earthquake engineering field was done with me. Joe began getting doctoral people in that field, but right on up into the 1970s, all of my doctoral thesis students were working on the finite element. They were working on the digital computer formulation of structural analysis. In only a few cases did I get actively involved in earthquake engineering research. Almost all of the work I was involved in was on the finite element, and computer-oriented.

One of the early activities of the group here at Berkeley, before the creation of EERC, the Earthquake Engineering Research Center, was the field measurement program. EERI had acted as developer of a set of four rotating-mass shaking machines in the mid-sixties. That was a very constructive result of EERI efforts in the early days, although it was also unusual, because EERI was simply not in a position to do that kind of thing on a regular basis.

The shakers were designed and built under a grant from the California Department of Architecture and Construction. The machines were to be used for measuring the vibration properties of buildings. There was considerable interest in measurements of unusual kinds of buildings, not just tall multistory buildings. EERI formed a small committee consisting of Don Hudson of Caltech, who was chairman, and about four others, including me. John Rinne was probably one of them. The committee drew up the specifications for the shaking machines, and the detailed mechanical design was done at Caltech. Don Hudson managed to have them built in southern California under an arrangement made through Caltech. The four machines then became available for assignment by EERI, but that did not prove to be a useful concept because EERI did not do that kind of thing. Actually, one of the first tests using those machines was done by Caltech faculty on an earth-fill dam. But at that time, very few Caltech people were interested in doing field work of that kind.

Thus it was more convenient and appropriate to have the machines located up here at Berkeley. So the four machines were brought up here to Berkeley and made available for testing on specific jobs of interest. Projects were handled through the University's research arm, usually funded by the companies that had designed and built the building being tested. At a later date, two of the machines were assigned to UCLA, the other two being left here. We collaborated with UCLA on that work, for example shifting our two down there temporarily if they were needed, or conversely shifting those two up here.

Jack Bouwkamp was the Berkeley person who took the machines out into the field and managed the projects. One of the early test jobs was on the Trans-America Tower with its pyramid structure. On building projects, usually Jack and I collaborated in writing up the reports. We usually compared analytical results against measured results. Jack did half-a-dozen or more buildings and at least two or three dams. I worked with him in doing the backup computer analyses. We almost always had a student doing a thesis in connection with the work.

Scott: Who funded these activities? You did mention that building owners usually paid for vibration measurements of their own structures.

Clough: Yes, the owners and designers had the work done on buildings. Particularly in the case of unusual designs, such as the Trans-America Building, responsible owners and engineers felt it was important to know how their buildings responded. The vibration studies were made during construction and after completion. We were almost always able to get permission to get into the frame during construction, and thus see what happened as the exterior cladding was added. We could first get the bare steel frame results, and then as the cladding and windows and things went in, we could see the changes. A single job usually extended over a period of several months.

Most of the buildings studied were unusual, but one of the first buildings I worked on was one of the medical center buildings at UCLA, which was a regular, straightforward, multistory, steel building frame. It was one of our opportunities to make a comparison of vibration measurement with computer analyses results.

Scott: The field measurement program preceded the formation of EERC?

Clough: Yes.

Funding Following the 1964 Alaska Earthquake

Clough: We began to get funding from NSF for studies of the energy absorption characteristics of structures in earthquakes. We had a whole series of NSF-funded projects going on under the generic title "energy absorption." We began getting this even before EERC was created. An example is our 1965 New Zealand paper on the nonlinear response of a steel frame building to an earthquake.[81] The funding for the development of the nonlinear computer program was from a structural engineering consulting firm in 1963 that had a blast analysis contract with the federal government. We could do the calculations, but did not know anything about how to describe the damage mechanisms—the loss of strength and stiffness during an earthquake.

Scott: Was the improvement in funding for earthquake engineering due largely to reaction to the March 1964 Alaskan earthquake?

Clough: Yes, I presume. But here again I was away when that earthquake hit. In 1963–1964, I took my second sabbatical and went to Cambridge University in England. I was there at the time of the Alaskan earthquake, and so was not available to look into the damage there, whereas of course Joe did and was actively involved from that side. That earthquake convinced both of us of the need for experimental test facilities to evaluate what happens in structures when shaken by a strong earthquake.

81 Ray W. Clough, K. L. Benuska, and Edward L. Wilson, "Inelastic Earthquake Response of Tall Buildings," *Proceedings, Third World Conference on Earthquake Engineering*, Auckland and Wellington, New Zealand, January 1965.

Appendix

Chapter 9: Creation of the Earthquake Engineering Research Center

One of the first opportunities for significant earthquake engineering research funding came after the San Fernando earthquake in February 1971.

Scott: Who was involved when you got the idea of setting up a center at U.C. Berkeley? You and Joe Penzien were key people, clearly.

Clough: As I mentioned, Jack Bouwkamp was the leader on the field experiment side of a whole series of vibration studies, I was a background person and collaborator on that program. It was Joe Penzien, Jack, and I who initiated the effort to establish a center at Berkeley. I think Joe was really the person who first had the dream of establishing an earthquake engineering research center. He battled the proposal through all the University committees to get it approved and was the first director of EERC.

Scott: I presume that all three of you contributed quite a bit to the effort. But I guess Penzien was the main sparkplug.

Clough: Yes. He was the lead man in the interaction with the University. Jack and I were dealing with specific projects and very much interested in the idea of having a center. Joe was the one who carried the ball.

Scott: You got the idea of a center a little while after the Alaskan earthquake of 1964?

Clough: Yes, after that. The idea of setting up an earthquake engineering research center at Berkeley resulted from a conversation Jack Bouwkamp, Joe Penzien, and I had at the Third World Conference on Earthquake Engineering in New Zealand, held in 1965. Our Earthquake Engineering Research Center, EERC, was created and began initial operations in January 1968, when development work on the shaking table also started. Joe had been involved in experimental work here before we got the shaking table built. He had tested models of beams and things like that. He was studying energy absorption in what amounted to extended damping. I was still primarily on the analytical side until we got the shaking table built and in operation. I was fully involved with students in the finite element work. Anyway, after the 1964 earthquake and the 1965 New Zealand conference it was very clear that we had to get more information, but I still had to complete several doctoral theses that were dedicated to the analytical side. I was, however, committed to working with the shaking table here when that became available.

Designing and Funding the Shake Table

Clough: The first report produced by EERC was the shaking-table feasibility study, which was funded by the California Department of Architecture and Construction.[82] Jack and Joe spent a lot of time on mechanical design of the 20-by-20-foot square shaking table.

By then we had developed the analytical tools quite completely, and I knew how to do the work with computers. The main thing we did not know was what happens to the properties of a structural system when it begins to be damaged. As long as it remained in the linear elastic range, we had excellent tools for calculating behavior, but when talking about damage mechanisms during earthquakes, we were just guessing as to the behavior patterns of the elements. That led me to realize that we had gone far enough on the purely analytical side, and that we needed to do experiments to measure what goes on in the damage mechanisms. We needed some way of following the analytical response up to the initial damage level and beyond.

After the shaking table was built and ready to operate, I was supposed to take over as the operator of the shaking table for the first

82 Joseph Penzien, J. G. Bouwkamp, R. W. Clough, and Dixon Rea, *Feasibility Study Large-Scale Earthquake Simulator Facility*, College of Engineering, University of California, Berkeley, September 1967. This study included a very large (100-by-100-foot) table, with three translational degrees of freedom, requiring 52 hydraulic actuators generating 25,000 horsepower. Only the study's 20-by-20-foot table proposal was funded.

series of tests, so I worked on developing the models that were going to be tested. But the table was dedicated in 1972, when I went to Norway on my third sabbatical. (I always seemed to be separated from the program at critical moments!) Due to sabbatical scheduling, I had to leave about the time the shaking table was ready to be used. We learned that Dr. Makoto Watabe of Japan was interested in coming to Berkeley. So Dr. Watabe was hired as a research engineer to run the table during the testing of the first models. He is now [1994] head of research at Shimuzu Construction, one of Japan's top design and construction firms. Shimizu is one of about five huge Japanese construction firms, each of which have experimental facilities that make ours, in the United States, look quite anemic. Our work compares favorably with theirs, but our facilities do not.

First Significant Support: After the San Fernando Earthquake

Clough: One of the first opportunities for significant earthquake engineering research funding came after the San Fernando earthquake in February 1971. By then, we had the shaking table at EERC nearly ready to operate. Immediately after the earthquake, Joe Penzien and I went down to observe the damage on the highway structures. Joe subsequently got a research grant from the state highway department to study that damage in more detail, both analytically and in shaking table studies.

Being EERC Director

Clough: I was the director of EERC, which started immediately after I came back from the sabbatical in 1973. I was director for about three-and-one-half years. As director, I tried to do everything. But I also found that all my interests were on the experimental side, making the shaking table go, getting funding for shaking-table programs. So I frankly acknowledge that I was neglecting everything else that needed to be done at EERC.

Scott: At that juncture, in order to make the shaking table operate, did you almost have to focus your energies on dealing with it?

Clough: I felt that was true, but the shaking table was only part of what needed to be done. It was clear to me that there was more work than one person could do, acting as both EERC director and manager of the shaking table programs. I quit as director, having gotten Joe to agree to come back as director, with me as assistant director and dealing with the shaking table and the rotating mass shakers.

Significance of EERC as of 1994

Clough: EERC had a very significant effect on earthquake engineering in the United States. It has played a very important role, although it did have its props knocked out from under it with the development and funding of the National Center for Earthquake Engineering at Buffalo, New York in 1986.

Scott: Despite that setback, do you think it is pulling itself back into shape?

Clough: In my opinion, although Buffalo has the name of national center, I do not think they really function as the national center. I don't think they can, really, because they are not recognized as being where the action is. They do have funding, of course—which EERC has lacked since that happened—and they get a lot of people to come there from Japan and elsewhere, but the real earthquake research action is not at Buffalo. Also I think that with the aftermath of the 1989 Loma Prieta earthquake, our funding situation at EERC has reversed itself.

We at Berkeley have always shared happily with Caltech the role of heading up earthquake engineering research, and we have been happy with Stanford's participation, and UCLA's, and now UC San Diego's. All of those California contributions have been good. There is a consortium of California universities in earthquake engineering, with the acronym "CUREE," although it has not had as dramatic a leadership role as some thought it might.

I am, however, very satisfied that EERC has played and is playing a very important role in earthquake engineering development in the U.S. as a whole. The things that led to the creation of EERC have been important to earthquake engineering in the United States and worldwide. I think EERC continues to hold that importance in worldwide recognition. Groups in the field worldwide would like to maintain a relationship with Berkeley.

Appendix

Doctoral Students of Ray W. Clough

Graduation Year	Last Name	First Name
1954	Lee	Seng-Lip
1956	Hartz	Billy J.
1961	Adini	Ari
1962	Tocher	James L.
1963	Laursen	Harold
1963	Wilson	Edward L.
1964	Jenschke	Victor A.
1965	Cherem,	S.
1965	Goudreau	G.
1966	Johnson	C. Philip
1966	Chopra	Anil K.
1967	Carr	Athol
1967	Felippa	Carlos
1968	Cantin	Gilles
1968	Johnson	Philip C.

1969	Kavanagh	Kenneth
1970	Greste	Ojars (or Ojois)
1970	Pawsey	Steuart (or Stewart, Stuart)
1970	Yeh	C-H.
1971	Bergan	Pal G.
1972	Fonder	Ghislain
1973	Pinkney	Robert Bruce
1973	Reimer	Richard
1975	Hidalgo	Pedro
1976	Mojtahedi (or Morjtehedi)	Soheil
1976	Tang	David
1977	Huckelbridge	Arthur
1978	Chen	Jony Shy-Wen
1978	Niwa	Akira
1979	Ghanaat	Yusof (or Yousef)
1981	Blondet-Saavedra	J. Marcial
1981	Oliva (or Olivia)	Michael G.
1982	Yang	M.-S.
1982	Kuo	James S-H.
1983	Croteau	Paul

Appendix

Publications of Ray W. Clough

Journals

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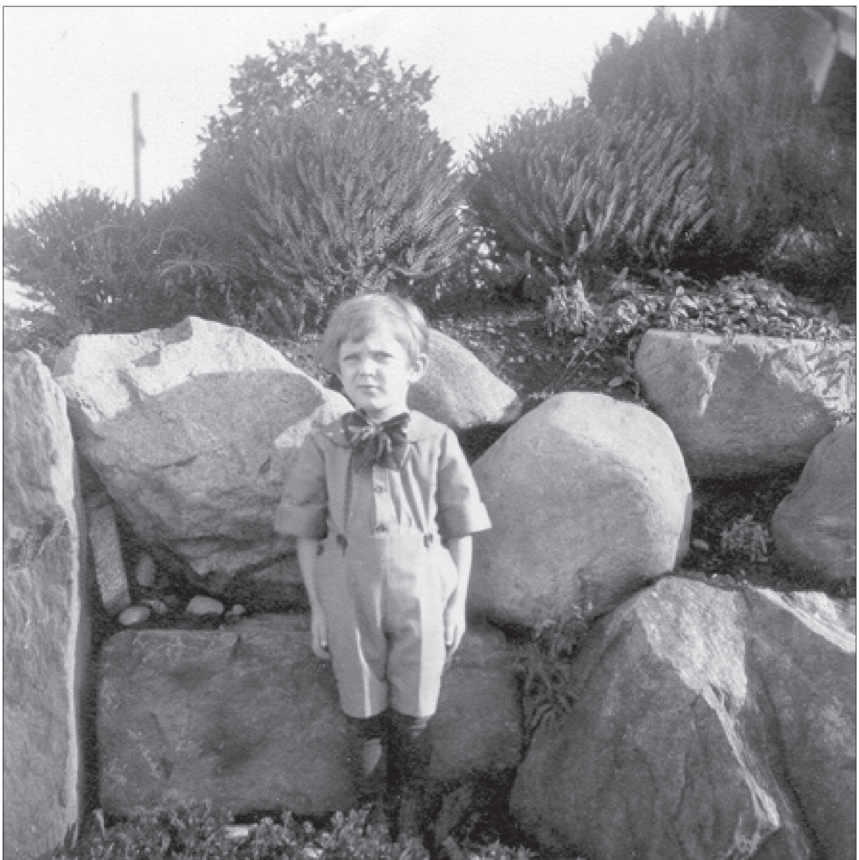
Ray W. Clough Photographs



Ray Clough, age 5 in 1925, in front of his family's home in Seattle, Washington.

Ray as a lieutenant in World War II in his office in Pasadena, California in 1944.

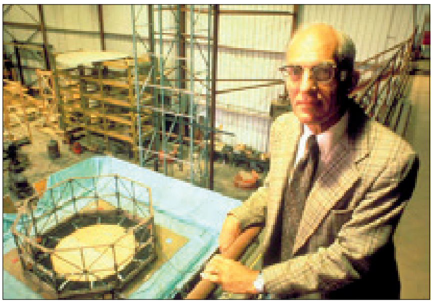
A camping trip in Yosemite in 1955. Left to right: daughters Allison and Meredith and son Doug.

*Ray Clough at the Lorenzas Arenas Market,
Concepcion, Chile, 1960 Chile Earthquake.*

Steinbrugge Collection, U.C. Berkeley NISEE-PEER Library

Joseph Penzien with Ray Clough at the Sixth World Conference on Earthquake Engineering in 1977 in India.

Masonry specimen ready for shake-table testing. Clough headed this mid-1970s project, along with graduate student Ron Mayes. Other graduate students working on the project were Polat Gulkan, Shy Chen, R. Hendrickson, George Manos, and Yutaro Omote.



Ray Clough on the mezzanine level of the earthquake simulator laboratory (shake table) building at the U.C. Berkeley Richmond Field Station, with a test set-up for a storage tank test underway in 1981.

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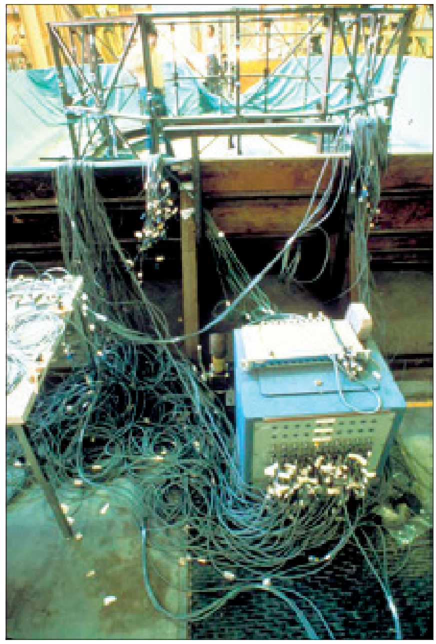
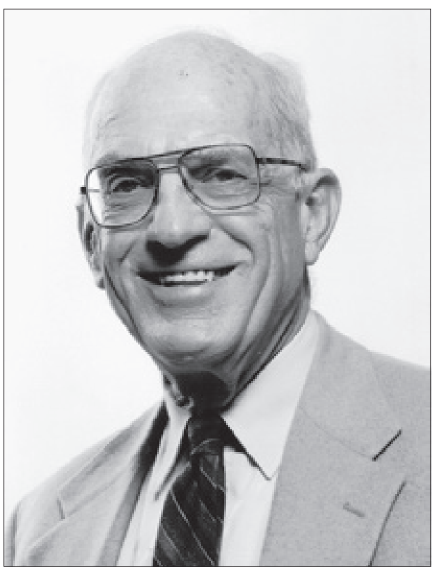


Data acquisition cables for the 1981 water tank shake table testing.

Ray and his wife, Shirley, in Alaska, 1993.

1994 in Venice, left to right: Ray Clough, Shirley Clough, Diane Wilson, and Ed Wilson.

Clough receiving the National Medal of Science from President Bill Clinton in 1994.



Ray W. Clough

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This volume in the EERI Oral History Series deals with the lives of two prominent individuals in earthquake engineering, Edward (Ed) Wilson, and Ray W. Clough, whose careers had a large influence on each other.

Prominent in the literature of earthquake engineering are the computer-related accomplishments of Ed Wilson, including the computerized development of the Finite Element Method and creation of structural analysis programs such as SAP and ETABS. Less well known are his origins on a dairy ranch in northern California, milking the cows, helping with the carpentry work, and climbing the family windmill to pump water when there was no breeze. He recalls that "the only engineers I ever knew about growing up were driving trains," and he took his first course in structural engineering at a junior college because it sounded "close to being a carpenter." At the University of California at Berkeley, his analytical skills began to shine, but he also developed innovative experimental methods in the structural laboratory. In addition to his teaching, research, and consulting work in the earthquake engineering field, Professor Wilson relates in this oral history his experiences in the aerospace field, in which he worked before joining the Berkeley faculty. Included in Wilson's oral history is a chapter capturing a discussion with Ashraf Habibullah relating the early years of Computers and Structures, Inc. and how their collaboration resulted in engineering software being used by thousands of engineers worldwide.

Wilson's mentor and PhD advisor, Ray W. Clough, is the subject of the second section of this volume, which contains the incomplete Clough oral history produced by Stanley Scott from interviews in 1993 and 1994. Clough is famous for helping to develop—and name—the Finite Element Method (and like Wilson, worked on that method while in the aerospace industry). Also similar to Wilson's youth, Clough's early years included the strenuous outdoor life, in his case mountain climbing. Hired in 1949 specifically to develop an earthquake engineering program at Berkeley, Professor Clough recalls that "I was not optimistic about the prospects of getting financial support for earthquake engineering research in those days. Nothing was being funded." While known for his analytical accomplishments, Clough's role as Co-Director of the Earthquake Engineering Research Center at Berkeley was to direct the shake table operation and research, an experimental role he felt complemented the use of increasingly complex analytical methods.

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