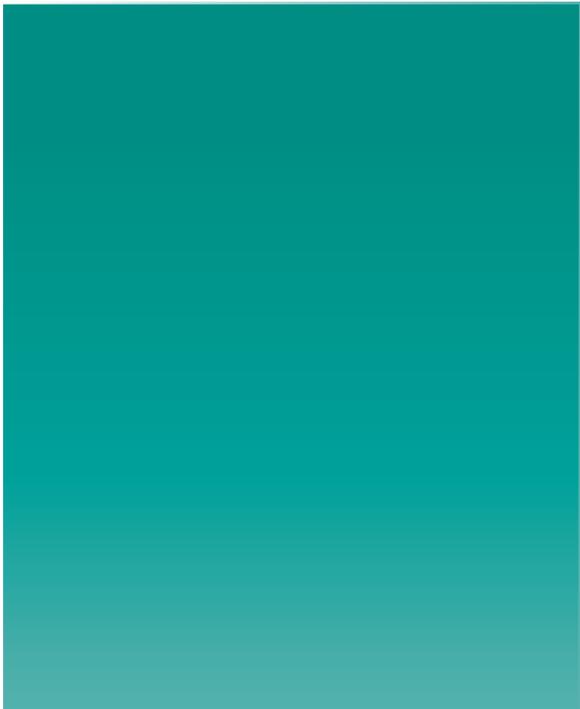


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# CONNECTIONS

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The EERI Oral History Series



**Roland L.  
Sharpe**

Stanley Scott  
*Interviewer*

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# CONNECTIONS

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The EERI Oral History Series

**Roland L. Sharpe**



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# CONNECTIONS

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The EERI Oral History Series

## Roland L. Sharpe

Stanley Scott, Interviewer



Earthquake Engineering Research Institute

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# The EERI Oral History Series

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

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

The EERI oral history program was initiated by Stanley Scott (1921–2002). The first nine volumes were published during his lifetime, and manuscripts and interview transcripts he left to EERI are resulting in the publication of other volumes for which he is being posthumously credited. In addition, the Oral History Committee is including further interviewees within the program's scope, following the Committee's charge to include subjects who: 1)

have made an outstanding career-long contribution to earthquake engineering, 2) have valuable first-person accounts to offer concerning the history of earthquake engineering, and 3) whose backgrounds, considering the series as a whole, appropriately span the various disciplines that are included in the field of earthquake engineering.

Scott's work, which he began in 1984, summed to hundreds of hours of taped interview sessions and thousands of pages of transcripts. Were it not for him, valuable facts and recollections would already have been lost.

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

Scott was assisted in formulating his oral history plans by Willa Baum, Director of the University of California at Berkeley Regional Oral History Office, a division of the Bancroft Library. An unfunded interview project on earthquake engineering and seismic safety was approved, and Scott was encouraged to proceed. Following his retirement from the university in 1989, Scott continued the oral history project. For a time, some expenses were paid 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 the 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, broad social aspects, and education, as well as interesting personal aspects of the subjects' lives.

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

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- Elide Pantoli
- Robert Reitherman
- Tony Shakal
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# Foreword

This oral history is the product of four interviews conducted by Stanley Scott (1921–2002) with Roland L. Sharpe (1923–2018) in 1989, 1991, and 1998. The manuscript at that point had not quite been finished, so we have made the necessary minor edits to produce this final version. Editorial review was also provided by EERI Oral History Committee members Loring A. Wyllie and Tony Shakal. Chris Rojahn and Bernadette Hadnagy filled in some details about Rol’s tenure at ATC. Sarah Nathe edited and indexed the manuscript. The page layout was accomplished by George Mattingly. Vida Samardžić, EERI Membership and Publications Coordinator, managed the production process.

Robert Reitherman  
Robert Hanson  
EERI Oral History Committee  
December 2020



# Personal Introduction

Roland Sharpe was an extraordinary contributor to the field of structural and earthquake engineering. Growing up in the Midwest, he was self-reliant and curious from an early age, and became interested in science and engineering by the time he was in high school. His college years were interrupted by the attack on Pearl Harbor in 1941, and that event inspired him to enlist in the Marine Corps. While on duty in the Pacific arena, he was recognized for his good judgment, creativity, and leadership skills (as happened again and again throughout his life), awarded a college scholarship (one of three recipients out of 20,000 marines), and transferred back to the Midwest to earn both a Bachelor's and a Master's degree in Civil Engineering from the University of Michigan. From there he pursued a wide variety of work options, settling finally on a 90-day job offer with John A. Blume & Associates in San Francisco that turned into a highly successful 23-year engagement that involved a variety of sophisticated engineering projects and culminated in his promotion to the position of Executive Vice President and General Manager.

Roland was highly skilled in identifying big picture problems and issues in earthquake and structural engineering and then formulating and managing engineering projects to address them. This was true throughout his career, from his work at John A. Blume Associates to his involvement in the early years of the Applied Technology Council (ATC). At the Blume firm he led a wide variety of highly visible major projects, including the siting and design of a 11,000-ft-long concrete tunnel at the Stanford Linear Accelerator Center (SLAC) facility located near Stanford University, and development of extensive nuclear reactor seismic design guidance for the Atomic Energy Commission (AEC). His work on SLAC involved numerous unusual challenges such as avoiding concrete shrinkage in that very long tunnel, designing the 10,000-ft-long building on top of it (the longest building in the world), and consideration of earth tides, as well as more standard problems like differential settlement. While at the Blume firm he also led projects in Puerto Rico and Iran and served as a consultant to several firms in West Germany that designed and built nuclear power plants. After he left the Blume firm, he went on to play a key role in establishing ATC, the Building Seismic Safety Council (BSSC), and eventually his own firm, Engineering Decision Analysis Company (EDAC), which focused on work for the nuclear power industry.

I first met Rol in the mid-1970s, soon after I joined EERI, which at the time had a relatively small membership of 100 or so. Early on, I learned about his engineering leadership and technical skills from colleagues who worked for the Blume firm, many of whom were on their way to becoming (or already were) stars in the earthquake and structural engineering practice and research community. However, I didn't get well acquainted with Rol until after I joined ATC as Executive Director in late-1981.

The impetus for the founding of ATC was the unexpected poor performance of engineered buildings, dams, and infrastructure during the MW6.5 San Fernando earthquake of February 9, 1971. The serious damage to engineered buildings in and around the San Fernando Valley, in particular, necessitated an immediate evaluation of the then-used building seismic design codes and practices. Up to that point, the Structural Engineers Association of California (SEAOC) and the practicing earthquake engineering community in the U.S. had been relying on volunteer committees to advance seismic design codes and practices, mainly through the updating and promulgation of SEAOC's Blue Book, *Recommended Lateral Force Requirements*, a product of the SEAOC Seismology Committee.

Given the significant unexpected damage of the San Fernando earthquake, the SEAOC Board of Directors decided that a radical new approach was needed to strengthen the technical basis for seismic design practices and codes. In pursuing options to achieve this goal, they appointed a three-person committee consisting of Roland Sharpe, John Wiggins, and Steve Johnston to advise on setting up a separate organization to speed the implementation of pertinent research. SEAOC also provided \$5,000 in seed money to establish the organization.

The result of their recommendation was the creation of the Applied Technology Council as a nonprofit organization in 1973. Rol Sharpe was the main creative force in the founding of ATC. He was also ATC's first Executive Director. His talents enabled him to play a pivotal role in establishing ATC's mission, its business model, and its project development process(es). The outsized role played by Rol is not surprising, given his knack for explaining

new concepts, identifying the individuals and groups needed to create a product that could readily be accepted by industry, and getting projects funded.

A main feature of the newly established ATC business model was that the private sector individuals involved in developing projects would be paid, as opposed to doing their work on a volunteer basis. A second important feature was that each project would be developed through an informal consensus approach similar to that used by SEAOC in the development of the Blue Book. Accordingly, each project would be led by a Project Technical Director (ideally the leading expert on the planet for the subject at hand), detailed developmental work would be conducted by a team of consulting engineers and other specialists drawn from practice, the research community, and often the regulatory community, and their work would be overviewed and guided by a senior-level blue-ribbon advisory panel. The business model and project development processes established and honed by Rol Sharpe have been in place ever since ATC's founding and are one of the reasons for ATC's contributions to the advancement of earthquake and structural engineering technology and practice over the last 47 years.

The engineering technology/methodology development projects undertaken by ATC under Rol's leadership in the mid- and late-1970s demonstrated the breadth of his technical capabilities, as well as his skill in negotiating and obtaining large government grants and contracts. Notable examples of early ATC project reports produced under his leadership include the ATC-3-06 report, *Tentative Provisions for the Development of Seismic Regulations for Buildings*, which was funded by the National Science Foundation (NSF) through the National Bureau of Standards (NBS). Developed by a multidisciplinary team of 85 nationally recognized experts in earthquake engineering and published in 1978, that report later served as the technical basis for the seismic provisions of the 1988 and subsequent editions of the *Uniform Building Code*.

Another notable large-scale project under Rol's leadership was the ATC-6 project report, *Seismic Design Guidelines for Highway Bridges*, which was funded by the Federal Highway Administration (FHWA). The development team consisted of 16 nationally recognized

experts that included consulting engineers, academics, and state and federal agency representatives from throughout the U.S. The report was published in 1981 and was adopted by the American Association of Highway and Transportation Officials (AASHTO) in 1991 as a standard specification for the seismic design of federally funded highway bridges.

Rol, and perhaps his colleagues in establishing ATC (Wiggins and Johnston), also created a project/report numbering system that had long-term impacts and benefits. The ATC-1 project, for example, was ATC's first project, and the ATC-3 project was its third. The number for a specific project (or report) could be extended with a hyphen to designate the project/report number for a subsequent related project (or report). For example, the ATC-3-06 report (described above) was the sixth complete version/draft of the original ATC-3 report. Another early example project/report series (also developed under Rol's leadership) was the ATC-4 series, which resulted in the U.S. Department of Housing and Urban Development (HUD)-funded ATC-4 report, *A Methodology for Seismic Design and Construction of Single-Family Dwellings*, published in 1976, and the subsequent companion ATC-4-1 report, *The Home Builders' Guide for Earthquake Design*, published in 1980 (also sponsored by HUD). While the numbering system seems trivial, the system was important because it made it extremely easy for ATC project participants and the profession at large to refer to specific ATC projects/reports (in a short-hand way), facilitated project management, and fostered ownership and pride by the persons involved in their development.

After Rol's management responsibilities at ATC ended in 1982, he continued to provide creative ideas and consulting services to it for several years that enabled other significant engineering contributions. In the early 1980s he recommended that expert opinion data be developed (in lieu of statistical data that didn't yet exist) to underpin the earthquake damage and loss estimation methodology that FEMA needed to assess the economic impacts of major California earthquakes on the western region and nation. This idea resulted in ATC's being awarded a major FEMA project to develop what became known as the ATC-13 Report, *Earthquake Damage Evaluation Data for California*, which was published in 1985. That report subsequently spawned an entire earthquake damage and loss estimation industry.

Rol also made other highly significant contributions to the structural and earthquake professions, including his role, along with Chuck Thiel of the National Science Foundation, in founding the Building Seismic Safety Council (BSSC) in the early 1980s. The founding purpose of BSSC was to conduct a consensus review of the above-mentioned ATC-3-06 report, *Tentative Provisions for the Development of Seismic Regulations for Buildings*, in a process involving all key stakeholders in the building design and construction industry. To this day, BSSC, which is an organizational unit within the National Institute of Building Sciences, is responsible for the update of FEMA's *NEHRP Recommended Seismic Provisions for New Buildings and Other Structures*, the resource document that is adopted into the International Building Code for the seismic design of buildings nationwide (the 2020 edition being the umpteenth generational offspring of ATC-3-06).

No doubt as a result of his work at the Blume firm, Rol also had an eye for the importance of international collaboration in structural and earthquake engineering. His interest in working with the Japanese engineering community, which had a lasting influence on ATC's international collaborations, was likely inspired by the decision by Japanese engineers in 1976 to use some of the ideas in the 1976 draft of the ATC-3 report in their seismic code. Several years later Rol proposed, with Masakau Ozaki of the Japan Structural Consultants Association (JSCA), that ATC and JSCA commence a series of U.S.-Japan Workshops on Improvement of Structural Design and Construction Practices. The first such workshop was held in Hawaii in 1984. Workshops between the two countries have been held every two to three years since then, and recently the concept was expanded to include New Zealand as a participating country. Interestingly, Rol continued to collaborate with members of the Japan Structural Consultants Association for many years and became one of the few Americans, if not the only one, to be invited to become a member of that organization—an honor that reflects JSCA's respect for his structural engineering talents and accomplishments.

Throughout his career, Rol was broadly recognized for his technical leadership skills, big-picture perspective on structural engineering issues, innovative problem-solving abilities, articulate messaging, kind manner, and management and negotiating acumen. His skills, matched with his easy-going demeanor, were extraordinary. From my perspective,

his role in the creation of the Applied Technology Council, and the establishment of its founding business model and project development processes, is especially noteworthy. He had a hand in the development of what are essentially the widely accepted methods, guidance documents, and standards now in widespread use nationwide for earthquake hazard mitigation: best practices for (1) the seismic design of new buildings and highway bridges, (2) the pre-earthquake seismic evaluation and retrofit of buildings and highway bridges, (3) the safety evaluation and repair of earthquake-damaged buildings, and (4) earthquake damage and loss estimation techniques for regional use. These accomplishments are enormous contributions to the wellbeing of humankind. Roland Sharpe's genius and foresight created ATC, and its influence on the protection of lives and property has been remarkable.

Christopher Rojahn, Director Emeritus  
Applied Technology Council  
August 2020

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# CONNECTIONS

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The EERI Oral History Series

**Roland L. Sharpe**



# Early Years Through College

*I was attending the University of Minnesota, while working 44 to 48 hours per week to cover my living expenses.*

**Scott:** Start by discussing your family history and your early years through college.

## **Scotland to America, Via Northern Ireland**

**Sharpe:** The Sharpe family originated in Scotland. They were Presbyterians, and in the early 1800s in Scotland were being severely persecuted. It was not just minor persecution, but very serious conflict. People fought over religious differences—Catholics, the Church of England, and Protestants. So in the 1820s the Sharpe clan moved to Northern Ireland, as they were afraid of being killed.

In Northern Ireland they found food and everything else very scarce. Then after about 15 years, having heard from some relatives about the land of food and gold and silver and riches—America—they finally scraped together enough money to take a boat from

Belfast to Liverpool. Then they left Liverpool in the early 1840s and arrived in New York about a month later.

Most of their relatives were in Iowa and Pennsylvania, so they first went to Pennsylvania. My grandfather and grandmother met in Pennsylvania in the 1850s and after a lengthy courtship were married. My maternal grandmother came from County Cork, Ireland and, like many Irish, had red hair. Over the next 30 years (1860–1890), she gave birth to eight children, one of whom was my father, Alfred. As they grew up, some of the children moved to New York, Chicago, Iowa, and some stayed in Pennsylvania. My father moved to Iowa along with two of his brothers and two of his sisters. One of the brothers went from Iowa to South Dakota where he became a judge and a rancher.

## My Parents, and My Father's Early Death

**Sharpe:** My mother, Ruth Carter, was born in Barnum, Iowa in June 1894. She finished eighth grade and then moved to Fort Dodge, Iowa, where she got a job in the Gates Dry Goods Department Store. My father finished two years at State College, Iowa Falls, and then became a cashier for a small Iowa railroad. He was then transferred to Fort Dodge where he met my mother. After several months of courtship, they were married on Christmas Eve 1910 in Des Moines, Iowa. Over the next 13 years, my mother had seven children, and I was the last. Two of the children died when they were infants.

I was born December 18, 1923 in Shakopee, Minnesota, a town about 30 miles southwest

of Minneapolis. My father was the railroad's district station agent in Shakopee. When I was about five years old my father died, and my mother was left with five children to raise. She could not afford to keep us together, so we were split up, the two older sisters staying with her because they could work and support themselves. The other three—my brother, my youngest sister and I—were farmed out to relatives. I moved to Fort Dodge, Iowa, where I lived for six years, and later to Webster City, Iowa, for two years.

## At the University of Minnesota

**Sharpe:** I moved to St. Paul, Minnesota when I was 13 and got a job carrying newspapers mornings, afternoons and Sundays. I was able to support myself and go to high school, and later to the University of Minnesota. My paper route was in a good neighborhood of large apartment building, and I was able to build it up to 350 customers. I cleared about \$125 per month, which was more than many adults earned. When I quit carrying papers after about four years, the company spit the route into two.

**Scott:** You got into science while you were in high school?

**Sharpe:** I took my first year of college chemistry while I was in high school. They had two tracks in high school: a science-engineering college track, and a business track for those not planning to study further after high school. Science-engineering required a lot of math and science courses—chemistry, physics, biology, etc. I became fascinated with chemistry, so the chemistry teacher allowed me to use his laboratory.

I was fortunate that the teacher gave me the run of the lab. He even managed to get extra chemicals when I wanted to do experiments for my first year of college chemistry after I enrolled at the University of Minnesota. I had fun performing experiments, and even caused a couple of small explosions. Fortunately I had the experiment covered with damp towels, so the glass did not fly all over the lab.

Actually I wanted to be a medical doctor, and the chemistry I had taken would fit in with pre-med. But I had taken two years of French

for my foreign language requirement, and when I was registering at the University of Minnesota they informed me that if I wanted to be in pre-med, I would have to take two years of German. I had already had a hard time with high school French, and thought, “No way would I take two years of college German!” So I decided to enroll in Chemical Engineering and Business Administration. After five years one would be granted a BS in Chemical Engineering and a Masters in Business Administration.



# World War II

*I heard President Roosevelt's declaration of war speech in the university's Student Union Building. After the Japanese attack on Pearl Harbor, we thought we had to do our part.*

**Sharpe:** On the morning of Pearl Harbor, December 7, 1941, I was ice skating with a number of friends including a “good friend,” the local Marine Corps recruiting sergeant. My best friend and I felt that we should sign up. Our “good friend” convinced us that we should not wait to be drafted, because you get treated “like dirt” if you are a draftee. Furthermore, if you joined the regular Marine Corps rather than the Reserves, you would be treated a lot better, so we signed up for a four-year hitch in the Marine Corps. I found out later the sergeant received \$5 a head for each guy he talked into joining the regulars.

I heard President Roosevelt's declaration of war speech in the university's Student Union Building. After the Japanese attack on Pearl Harbor, we thought we had to do our part. Almost everyone was very patriotic. On January 9, 1942, I enlisted in the Marine Corps as a regular and served four years and three days.

I finished the quarter at the university and then left for Marine training at San Diego. Only six-plus weeks after I joined the Marines, I was aboard ship heading for the South Pacific. We did not get any “shore leave” before boarding ship. As I wrote my mother at the time, “I don’t even know which end of a gun to shoot out of.” We only had minimum training, but troops were needed in the South Pacific. I spent the next nearly two years in the Pacific on various islands.

The Marines wanted officers, especially second lieutenants, but most of us would not take field commissions, because second lieutenants were prime targets. That is, if you went out on patrol, two scouts would go first and the second lieutenant would be the next. The Japanese were very aware of this arrangement, and so would let the first two go by and shoot the third one. Consequently there was a rapid turnover in second lieutenants, and few wanted such a job.

I had been in the South Pacific a little less than two years when a notice was posted one day announcing the V-12 Program. They were looking for Marines to send back to college in the United States. I was in British Samoa at the time in charge of the range-finder section for our Company. We had 155-millimeter World War I guns that had been stored on the parade ground at the Marine Corps Base in San Diego. The guns had solid brass shims as bearings, and solid rubber tires. The range-finder section spotted the targets, estimated the range to the target, and determined the settings—the trajectory and azimuth—for aiming the gun before it was fired.

One day in late fall, 1943, the Captain called

me in and said, “Sharpe, maybe you want to put in for this V-12 Program, because you have had some college. Why don’t you fill in this application and I will sign it?” I did not think any more about it, but a couple of weeks later the Captain called me in and said, “Sharpe, we want you to go and meet the Colonel.”

The Colonel had four to six people he was interviewing. You were asked to give a little of your background and state your objective in life. After the oral interview, the Colonel asked me to write my full name three times on a piece of paper. I was concerned, because I thought, “Oh boy, I cannot write my name three times identically.”

## The V-12 Program

**Sharpe:** Ten days later the Captain called me in again and said, “Sharpe, it was nice knowing you. You are to take the earliest available transportation back to the United States.” I was one of three selected out of about 20,000 Marines, and I was going to get paid to go to college, which I thought was tremendous.

A few days later I took a “yippee” boat to Pago Pago, a U.S. Naval Base in the South Pacific. “Yippee” was a nickname for small YP (patrol) boats, many of which were former fishing boats. They were used for sweeping mines and mostly operated within a few miles of shore. The ground swell waves that occur near shore made me seasick whenever we came close to an island.

As I say, all this was happening near the end of 1943. I waited in Pago Pago for about two weeks and finally was assigned to the USS Kit Carson, a Liberty ship. There were about 30

Marines and Navy personnel being transferred back to the states for various reasons. When we were about two days out, our ship broke down. The crew managed to get one boiler working, but we could only make four knots. It took over 30 days to reach San Francisco. We were too slow to be in a convoy, and kept kidding, “The Japanese would not waste a torpedo on this ship.”

After arriving in San Francisco, we were transferred to Treasure Island Naval Barracks, and then given one day of liberty. It was my first day of stateside liberty since I had joined the Marine Corps, when we had been taken out of “boot camp” and put aboard a ship without any liberty leave. We stayed at Treasure Island two days, and then boarded a train for San Diego.

In San Diego it was decided to “reorient” us, which included combat training. I could not understand that. Among other things, we had to go through the enfilade, where live bullets are fired about two feet above the ground, while you crawl through an obstacle course. If you panicked and jumped up, you might get shot, because the bullets were real. I never could find an explanation as to why they had us go through this course.

**Scott:** Describe the V-12 Program.

**Sharpe:** The V-12 program was mostly offered to people in the states and was not applied much for by people in the field. I felt very privileged because, as I mentioned, before the war I had been working 44 to 48 hours per week while going to the University of Minnesota. Now I was to go to school and have a free ride, so to speak. I could spend my time studying instead of working.

During all of 1944 and half of 1945 I attended different schools, because they kept transferring us around. For three months I was assigned to Gustavus Adolfus, a college close to Minneapolis. That made it nice because I could go home on weekends. It was only about 90 miles away, so I could take a bus. Then I was transferred to the University of Michigan and enrolled in electrical engineering and electronics because radar was coming along as a major development. People who could understand electricity and electronics were needed. I stayed at Michigan about 20 months.

**Scott:** I take it the V-12 part of your service was pretty good?

**Sharpe:** Yes, it really was. I thought V-12 life was great, because you had a nice place to stay, reasonably good food, and time to study. On weekends you could go out and have some fun. This was a real godsend for a person who had previously been working his way through college.

## The War Ends

**Sharpe:** The end of the war came in August, 1945, and we had a big celebration. Then we were transferred to the Camp Lejeune Marine Base in North Carolina.

**Scott:** They closed down the V-12 Program at the end of the war?

**Sharpe:** Yes. I still had four months to serve in the Marines because I was a regular. I had the necessary service points for discharge, but that did not make any difference, so I went to Camp Lejeune. We were put through combat training and then given an offer. If you would sign up for a Reserve Commission, you would

receive \$300 mustering-out pay, plus \$1,000 for uniform allowance, plus ten cents a mile to anywhere in the United States, which of course would be San Diego, about the farthest distance from Camp Lejeune. It totaled about \$1,600 in all.

I was so fed up with the military that I said, “No, thank you, I’ll take my \$300 mustering-out pay and be completely free.” Just a few years later I greeted some 90 of my friends who were coming through San Francisco on their way to Korea, because they had chosen to take Reserve Commissions. By then many were married and had finished college, and they were very upset about it all. I’m not saying I had more foresight—I was just fed up with the military. Four years was too much for me. I do not care for military life.

## Returning to the University of Michigan

**Sharpe:** I returned to the University of Michigan and met with the Associate Dean of Engineering. I asked him the quickest way to get a degree. He looked over my credits, and indicated that I could substitute naval history for geology, and a few other things like that. (I have since learned a considerable amount about geology, but not from college courses.)

I could get a degree in civil engineering more quickly than a degree in electrical engineering, because I would have to wait for some senior courses that were given only once a year and they had to be taken in sequence, which meant two-and-a-half years for electrical engineering, whereas it would be only about a year and a half for civil engineering. So I became a civil engineer.

I signed up for extra courses so that when I finished my Bachelor’s degree in 1947, I had almost enough credits for a Master’s degree. While finishing my MS, I worked part time at a local engineering firm. Then in 1949 the chairman of the Civil Engineering Department invited me to teach undergraduate courses.

One day in early summer 1950 the Chairman of the Civil Engineering Department called me into his office and said, “Sharpe, we want you to go some place else if you’re going to finish a PhD.” That’s just about the words he used, and my jaw fell. He said, “Oh, we want you to teach here, but you have received two degrees from Michigan, and we want you to get some cross-fertilization someplace else. We already have two people on the staff who have gotten three degrees here, and we think you should get some cross-fertilization so as to have a better perspective.” “Or,” he said, “you can go out and get five more years of practical experience.” So, I left the University of Michigan to get the practical experience, finishing my master’s in 1950.

## To California in Search of Employment

**Sharpe:** That is the reason I moved to California. My wife, Jane, whom I had married in late 1946, and I took an automobile trip through the Midwest and West. We stopped in Chicago, St. Louis, Kansas City, Denver, Salt Lake City, Sacramento, San Francisco, Portland, Seattle, and went back to St. Paul-Minneapolis looking at the job opportunities. We decided that San Francisco was the best.

**Scott:** You made a circular tour around much of the country?

**Sharpe:** Yes, we made a circle of about 15,000 miles, and did some sightseeing on the way including several areas in Canada. How did I end up choosing San Francisco? When I came to a city, I would have a letter of introduction to certain people.

**Scott:** These were letters of introduction from someone on the Michigan campus?

**Sharpe:** Yes, from Professor Robert Sherlock who knew a lot of engineers around the country. For example, when I reached the Bay Area, I had an introduction to the Chief Engineer at Bethlehem Steel in Alameda. He offered me a detailing job, a drafting job. That is the way you started out in the steel business, but I wasn't interested.

While I was talking to him, however, I asked, "Could you recommend six consulting engineering firms in San Francisco, that you consider to be the top professional firms?" He

gave me a list of six. Among these were Huber & Knapik, John Blume, Hall and Pregnoff, and Art Anderson (whose partner had been Chief Engineer for Design of the Bay Bridge).

I talked to each of the six firms. After discussing possible employment at each firm, I would ask, "Can you tell me in your opinion who would be the six top professional firms in the city?" Two or three names always floated to the top—everybody mentioned them. One of these was John Blume.

I talked with John, who said, "If you should be living here in San Francisco, I can give you a 90-day job, but can pay only \$350 a month, and I won't pay any moving expenses." It was typical policy not to pay moving expenses. So my wife and I drove back to Michigan with considerable sightseeing on the way, and packed all our belongings in a small wooden trailer. We headed for San Francisco in August 1950 pulling the trailer behind our 1949 Plymouth.



# Years with John Blume's Firm

*John Blume offered me a 90-day job because he did not have more work than that. I took the job and ended up spending 23 years there.*

**Sharpe:** After arriving in San Francisco, I went to talk to John Blume again. He said I would have to take a cut in pay from what I had been getting for teaching at Michigan, as academic experience doesn't really help you much in design. I still remember this. John Blume offered me a 90-day job because he did not have more work than that. I took the job and ended up spending 23 years there. I took the job with him and turned down other ones, including one from Anderson for more money. Incidentally Anderson had a heart attack about one year later and closed his firm. I thought John was a very good engineer, his office seemed to have a little more life in it, and the projects were a little more interesting.

## Wind Tunnel at Moffett Field

**Sharpe:** The first major job I worked on after I started at Blume was the NACA<sup>1</sup> Supersonic Unitary Wind Tunnel (SSUWT) at

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1. The National Advisory Committee for Aeronautics (NACA) was folded into the National Aeronautics and Space Administration when NASA was created in 1958.

Moffett Field in Mountain View. It was to be the largest of its kind, and is still one of the most important wind tunnels in the world. In a short time I became second-in-command of design for the project, and spent about three years on the job until the structure was completed. It was a one-of-a-kind project that you really enjoy working on.

The SSUWT has three branches, one is supersonic, one subsonic, and a transonic test section where models can be tested at ever-increasing speeds from subsonic through the sound barrier. The air speed through the Transonic Test Section could be maintained at or near the speed of sound while the model being tested is moved through different attitudes. Supersonic means speeds greater than the speed of sound, subsonic is less than the speed of sound, and transonic means the speed of sound. Transonic is needed to study the effects of air turbulence and other phenomena experienced as the model (plane) passes through the sound barrier.

The SSUWT looks like a huge figure eight in plan with an extra loop added on the side of one loop. The SSUWT is composed of several huge pressure vessels with a total length exceeding 900 feet. When we designed those huge shells we did not have any computer programs. The ASME Pressure Vessel Code requires that pressure vessels be tested before operating. We did not want to air-pressure test because a failure could cause a horrendous explosion. We couldn't water-test it because it had such huge volume (the largest diameter was 70 feet). It would have taken a tremendous structure to support all of the water required

to fill the SSUWT, and the weight of the water would impose greatly different stresses.

NACA decided to use a vacuum test on the structure. At the corners of each loop there were huge elliptical steel rings. The largest was on the order of 35 feet vertical dimension and 50 feet horizontal. We did not have much to go on for making an analysis of such an ellipse, except some rather old formulas. So we made analyses using a variety of assumptions. We determined that for many loading cases, the vacuum test would produce more critical stresses than would an air pressure test. Everything worked out, and the SSUWT is still operating.

The wind tunnel has four electric motors on a single shaft that are still the largest in the world. The four motors can generate 180,000 hp with a one-hour maximum rating of 215,000 hp. There is a dedicated electric power line for the SSUWT, but it is operated at full power only late at night because the power drain would dim most lights on the lower peninsula.

**Scott:** How do they use the electric motors?

**Sharpe:** The motors power big axial-flow compressors, which build up the air pressure and temperature. The air then travels through a large-diameter tunnel portion where the cross section is filled with cooling coils that cool the air to the required temperature. The air then travels through the tunnel to the specified test section. At the Supersonic Test Section the airflow is narrowed down to a very narrow orifice where it passes through at the speed of sound. Air cannot pass through an orifice at a speed higher than that of sound, but after the air goes through the orifice its

speed will increase depending on the shape of the downstream expansion of the tunnel cross section. The air can reach velocities in excess of 2000 mph, a real high-speed wind.

We had many problems to solve, including obtaining a mirror-like surface on the sides of the test section, which were 10 feet by 10 feet by 20 feet long. The mirror-like surface was needed to minimize build-up of boundary layer (dead air) at the sides of the tunnel due to friction. I saw them a couple of years ago, and the inner surfaces are just as shiny today as they were in the 1950s when they were finished.

We also did a transonic tunnel test section where speeds very close to the speed of sound from subsonic to supersonic could be maintained. This is a very important feature, because when supersonic aircraft take off they have to go from subsonic to supersonic, and back again when they land.

The SSUWT was scaled up from a small two-foot by two-foot cross section model tunnel, which had been used to test some of the wind speed theories, to the Supersonic Test Section, which is 10 feet by 10 feet in cross section. It was a very interesting project.

Later I worked on two other tunnels at NACA. They were having problems with a tunnel six feet in cross section, so I redesigned a portion of the structure. I also did the conceptual and preliminary design for expanding the 40 feet by 80 feet wind tunnel to an 80 feet by 120 feet test section, which is the large structure one can see from Highway 101. It was expanded in size and power so that a full-size Boeing 737 could be tested at speeds up to about 150 knots.

## Office Building for Bethlehem Steel

**Sharpe:** After designing a number of school buildings, in the mid-1950s I was assigned the design of a 15-story office building in San Francisco for the Bethlehem Steel Corporation. Located at the corner of Davis and California Streets, and now the Interstate Bank Building, the structure is of interest because the exterior columns do not have spandrel beams between them. The spandrel beams are located 30 inches inside from the column centerline. Three sides of each column are exposed for the full height of the building. A large floor girder connects the interior building framing to the exterior columns. There is a gap between each column and the window wall. Construction of the building was completed about 1957.

Here again, we did not have computer programs and had only the usual column formulas. This building had 15-story columns that needed to withstand earthquake loads, but did not have spandrel beams at each floor to provide lateral support. After a lot of research, and actually building some models, we were convinced, and Rube Binder, Chief Engineer at Bethlehem, was convinced, that we should use a torsion box girder member to connect the building structure to the exterior columns. The wide-flange girders were constructed with side plates to make a rectangular box section and thus provide a resisting moment to the columns at each floor when the building was subjected to seismic or wind forces.

We wanted to weld the side plates to the wide flange girders so as to make a stiff torsion box, but Rube had many reasons why welding would not be satisfactory. Rube never said it,

but I suspect the main reason for not using welding was that Bethlehem had hole-drilling and punching capacity at their Alameda plant and were familiar with fabrication using rivets or bolts.

**Scott:** If the connections were not welded, Bethlehem could in effect do part of the work in-house?

**Sharpe:** Yes. We ended up with a compromise, by using high-strength bolts. But I remember making some plastic models to convince Rube of the benefit of constructing torsional box girders, and showing how these would produce stable columns. I should probably comment that the welding we were planning would have been superior to those that failed in Northridge because the steel was more weldable than that currently produced, the welding rods were better, and the joint design would not have high concentrations of stress.

That was also an early building in which a story-drift device was installed. Rube was interested in the effects of earthquakes on buildings, as was the Blume office. We designed steel X-braces that extended from floor to floor at three elevations in the building. Each X-brace member had a stylus with a linkage attached at its lower end. As the story deflected, the brace member would move the linkage and the stylus would scratch markings on heavy paper mounted on a steel plate, thus recording the motion.

The drift recorder was designed on the premise that the diagonals on a square or rectangle will change length when the square is deformed in plane. One diagonal will lengthen and the other shorten. The linkage

with attached stylus would record any story drift on paper. The recorders were maintained while Rube was around. The present building owners may not be aware that there is a recording system installed.

**Scott:** How did you propose to use the information yielded by the recording system?

**Sharpe:** The drift “scratch” recordings were going to be used in conjunction with the recordings from a seismograph that was installed by Bill Cloud, who was in charge of the U.S. Coast and Geodetic Survey strong motion program in the western U.S.

**Scott:** So what you have described was in effect strong motion instrumentation of the building?

**Sharpe:** Yes, except that in those days we did not have much electronics. The seismograph had a roll of film, a mirror, and a light source. The instrument had to be completely enclosed so as not to expose the film, because the recording process shines a light on the film. Rube Binder persuaded Bethlehem to set aside a small room in which a small concrete pier was constructed where Bill Cloud installed the instrument. As I recall, Bethlehem also paid for the seismograph—something like \$3,000—which was a lot of money in those days.

We were going use the information from the X-bracing recorders and from the seismograph. We could only afford a few instruments to record the accelerations in the building. With 15 stories, we figured the building would deform in three significant modes. I can’t tell you exactly which floors were instrumented, but I think we put one on the third and one on the seventh floor. I’m not sure if we also put

one up near the roof, but I have a hunch we did. The locations were chosen so that we could pick out the mode shapes from a plot of the maximum deformations.

John Blume bought one of the first Sprengnether Portable Vibration Seismographs, a “black box” with which you could measure accelerations in a building. The record is produced on photo film, from which you could calculate the displacements. Once the building was under construction, we agreed with Rube Binder that Eric Elsesser, who then worked for Blume, would make measurements of the building as it was being constructed. Eric used the Sprengnether to measure the building periods of the bare steel frame, the periods two or three times as the contractor installed the steel floor decking, then as the concrete was placed on top of the steel decking, and then as the exterior cladding was installed on the building. I think these period measurements were the first made of a building as it was being constructed.

Later on, I believe Paul Jennings and others at Caltech made similar measurements on a building in southern California, and Jack Bouwkamp and others at UC Berkeley measured a building in the East Bay. They published their work, but we were actually a couple of years ahead of them.

This type of activity is one of many things that I enjoyed about working for John Blume. He had a very inquisitive mind and liked to do these kinds of innovative things. I also have an inquisitive mind, so on that score, he and I got along very well together. A paper was written on the measurement work and was published in the *Proceedings of the Second World*

*Conference on Earthquake Engineering*, held in 1960 in Tokyo and Kyoto. As far as I know, it was the first paper written about changes in the fundamental frequency of a building while it is being constructed. The authors are shown as John Blume and Ruben Binder; although Eric and I wrote the paper, as I recall we did not get mentioned.

## Measuring Periods of School Buildings

**Sharpe:** Jack Meehan became Research Director for the Division of the State Architect in the mid-1950s.<sup>2</sup> He and I got to talking and thought it sounded like a great idea to measure some periods on a number of school buildings. We worked out a contributory contract, whereby the State of California put up a few thousand dollars and Blume put up the rest in labor. We also worked with Bill Cloud of the Coast and Geodetic Survey. We selected 15 school buildings from throughout the state, ranging from one to three stories in height, and representing different types of construction. We measured the periods of vibration of each building.

**Scott:** Did you vibrate the buildings?

**Sharpe:** Yes. John had built a “shaker”

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2. Jack Meehan worked for John Blume's firm until 1949 when he left to work for the state agency administering the Field Act regulations covering public schools. See *John F. Meehan: Connections: The EERI Oral History Series*, Earthquake Engineering Research Institute, Oakland, CA, 2017. Other volumes in the *Connections* series that involve individuals who were with the Blume firm are on Joseph Nicoletti, Eric Elsesser, and John A. Blume.

during his college days at Stanford. I believe it was one of the first “shakers” built, and it was rather cumbersome, but we took it and shook some of the buildings. The shaker could put a lot of energy into a building, so we had to get releases signed, in case we caused any damage. We secured the shaker in place with wood blocking, etc.

We tried different schemes to vibrate some buildings. For example, there was a three-story concrete school building in Alameda that I had designed. We bumped it with a large truck to get it to vibrate. The problem with this method was there was no sharp demarcation at the end of the bump and thus the recording was not entirely free vibration, but we tried it anyway. We thought of using a “pull” test, by attaching a steel cable to the building, tying it to a big truck or tractor, and pulling on the cable to get some tension. Then the cable is cut with cable cutters, but we could not find a suitable place in the building to attach a cable.

For some other buildings, we simply relied on windy conditions to vibrate the building. We would have our equipment ready and then wait for a windy day. I still remember setting up the accelerometers. There was a big black cloth that we had to put over it. You had to get under the cloth and check to make sure everything was lined up. Bill Cloud did the first few, and then he had some of his other people come along and set up the rest of them. This was a cooperative effort.

I think these were the first efforts to measure actual building periods except for some that were done by John Blume in the mid-1930s when he worked for the U.S. Coast & Geodetic Survey. We prepared a very detailed report

(nearly 300 pages) covering all aspects of the study.<sup>3</sup>

There is an interesting sidelight on how we did that work, which dated back to just before digital computers became available. We wanted to do dynamic analyses on about ten of the buildings, but did not have access to any computers in this country. We had heard about an analog computer developed at the Dominion Physical Laboratory, Lower Hutt, New Zealand, so we contacted them and made an agreement.

We would send them data, they would process it for us and send it back, and we would let them use the data. We agreed to send full descriptions of the buildings, so they could do research studies with the data. The ten buildings we analyzed this way were some of the first that had any dynamic analysis. By using the analog computer, we were ahead of other people. The entire process is described in the report I mentioned above.

Another point of interest was the time required to process the data. It took a week or two to put the data together, two weeks to get it to New Zealand, two weeks to process the data on the analog computer, and two weeks to return the results. The total was somewhere between six and eight weeks. Nowadays, with high-speed digital computers and PCs, people complain if they have to wait a few hours.

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3. *A Structural Dynamic Investigation of 15 School Buildings Subjected to Simulated Earthquake Motion*, John A. Blume, Roland L. Sharpe, and Eric Elsesser, State of California Department of Public Works, Division of Architecture, Sacramento, CA, December 1960.

## South Terminal of the Airport

**Sharpe:** Two other buildings came along at about the same time. One was the South Terminal at the San Francisco airport. We did the structural engineering for the architect, Welton Beckett. The structure had some rather long spans. As I recall the main ticket lobby has a clear area that is 75 feet by 125 feet.

We had Dames & Moore make soil borings and prepare a soils report, and they recommended friction piles. In those days, soils engineering was still rather new so we drew our own profile through the site from the soil borings and plotted the underlying rock. I then went to Bill Moore and asked about point-bearing steel piles. The others were going to be steel friction piles.

We would extend the piles to the rock below. We only added, as I recall, an average of about ten feet additional length, but we reduced the number of piles by about one third. I used to kid Bill about this, but that's a long time ago. The airport south terminal building was finished in about 1961.

The Federal Office Building: 450 Golden Gate

**Sharpe:** In the late 1950s the Blume office was selected as the structural engineering firm for the 22-story Federal Office Building at 450 Golden Gate, here in San Francisco. There was a joint venture architectural group of four architects, including young Jack Warneke. Carl had started the firm in Oakland, and this was his son, Jack.

The building was to be the largest office building west of Chicago, 1,200,000 square feet. It had two basements and 22 stories above.

The top four floors had federal courtrooms. The courtrooms had high ceilings, generally extending two stories in height. If I remember correctly, each floor is about an acre and a half. Another interesting fact is that the broad face of the building is about 2 1/2 acres on a vertical plane. We did considerable study on the foundation for the building, which is just uphill from City Hall, and close to what was once a marsh area. We had Dames & Moore drill to something like 200 feet, and there was still sand at that depth. So we ended up with a large mat footing under the building. No piles, just a mat footing, because there is nice sand there with very good bearing capacity.

This was a fairly regular structure, but I made some very careful studies trying to get the maximum structure for the most efficient use of steel. I ended up with haunched girders in two directions. , We had a two-way frame, and I guess I came up with the idea of keeping the outside dimensions of the haunches and girders the same, and just varying the thickness of the flange plates. I believe we had about three or four sizes of girders in the whole building. After it was bid and Bethlehem got the job, I asked them about a few changes in girder depths. They said it was a good idea, because they could build about three jigs to make the haunched girders, and then they could just weld them up. It worked out very well.

**Scott:** Describe a haunched girder.

**Sharpe:** Normally a girder is the same depth its full length. But in order to increase the moment resistance at the ends, we designed a sloped lower flange, so the girder is deeper at the ends. I used to kid Bud Edwards, Rube Binder's number one assistant. As I recall, their

estimate was something like 18,100 tons. I asked Bud how long it took them to do the estimate, and he said so many man hours, about three weeks.

Well, I showed him our engineering estimate, which was only about 50 tons different from theirs. I told them that the next time they wanted an estimate, come to us. Of course, they took account of all the details, whereas we had made assumptions. We had estimated a little over 18,000 tons, and I think they were 50 tons more than we were. That building went together very well, once construction started.

It is also of interest the total construction cost of the structure was about \$26,000,000. GSA at the present time is spending in the order of \$80,000,000 to remove the asbestos, upgrade the HVAC systems, and do some modernization.

**Scott:** That is \$80 million for asbestos removal, HVAC upgrade and some modernization?

**Sharpe:** Right.

A Building at the Lawrence Livermore Laboratory

**Sharpe:** In 1957 or 1958, the Blume office was selected to design a building at what is now the Lawrence Livermore Laboratory (LLL). In an interesting arrangement, we went into a joint venture with architect Jack Warneke. But both firms could not be the project manager. It was one of the few instances where the structural engineer was the manager rather than the architect. While this caused a few problems as we went along, basically it worked out quite well.

**Scott:** Describe this unconventional arrangement a little more, and indicate why the structural engineers were given a larger role than usual.

**Sharpe:** Normally the structural engineer works for the architect. In this case we formed an equal-partners joint venture, with the understanding that the Blume firm would manage the project. I was the project manager. In fact, I think we were selected because the Blume firm had a good reputation at LLL, and as Jack Warneke wanted the architectural work, he sort of had to go along. Now, of course, he is a big famous architect, but this was back when he was starting out and was taking over his father's practice.

Then we got a very large building at LLL under a similar arrangement. It was for the mechanical engineering division. Interestingly, once that job was nearly completed, I was no longer able to go in and do the final check-out on one area of the building. This despite the fact that I had a "Q" clearance, a high security clearance. When I asked them why, they said, "Because you don't have a need to know." I then told them, "If so, I can't be responsible for what's installed in there." They said, "That is the way it is." I still remember that business about security.

**Scott:** You don't have a "need to know," although you were project manager for the job!

**Sharpe:** "Q" clearances are like that. There are probably only a few people at the Lab who have a "need to know" everything, and they would have the top clearance. Then others "need to know" a little bit more, and so on

down to some who “need to know” a lot less. That is the way it is.

### **First Quay Wall at Hunters Point**

**Sharpe:** Around the mid-1950s, I designed the first quay wall at Hunters' Point. It had four-foot-diameter steel cylinders that went down some 150 feet to bedrock, and the Navy approved the design concept. I checked my cylinder design and it looked correct, but the shell was only a half-inch thick and it did not look right. However, I kept going back to my calculations and everything seemed to check out. So I said, “OK, one-half inch.”

Then the Navy reviewed it and said, “That does not look right.” So John Blume had Joe Nicoletti go through my calcs, and at first he could not find anything, but finally he said, “You took the square root here and you should not have.” It should have been 3/4-inch in thickness. So the project turned out OK, but I still remember that you can be so sure you are right, but there can be a mistake and you do not see it. The computer programs are becoming so complex that a mistake could be made without realizing it. That really worries me.



# The Stanford Linear Accelerator

*The SLAC concrete tube was long enough so that the earth's curvature had to be taken into account.*

**Sharpe:** Because the Atomic Energy Commission (AEC) liked the work we did, we tended to get selected for some of the more interesting jobs. In 1959 they asked if we would do a feasibility study for constructing a two-mile-long linear accelerator on Stanford lands, or on other nearby areas. The study was to be done directly for the AEC in Washington, and they in turn were going to present it to the joint Congressional Committee on Atomic Energy as a report. We negotiated an agreement with the AEC.

Stanford had made a study of running twin tunnels under the foothills behind Stanford, more or less parallel to Junipero Serra Boulevard, and with one end at about Page Mill Road. They had submitted a proposal with a budget to the AEC. The AEC wanted an independent review of the proposal, and were concerned about the twin tunnels in the hills. The AEC people were concerned that Stanford could not come up with good prices, and also wondered if the location was even the best place to put the accelerator.

So they came to Blume. As the person who was handling the AEC work, I sat down with Joe Armstrong, head of the engineering

division at AEC, and worked out an agreement. We were to review the Stanford proposal and make a feasibility study to determine the best place to locate the facility. We studied some five sites. We looked at Moffett Field, which in those days the Navy was thinking of abandoning, although they still have not done that. Another site was roughly along where Highway 280 goes through Stanford land. A third was the original alignment that Stanford had proposed. A fourth site was over at the east end of the Dumbarton Bridge. The fifth site, and the one recommended, was the present location of the accelerator, which roughly parallels Sand Hill Road, just north of San Francisquito Creek.

One of the interesting things about the project related to the big difference in the annual rainfall at the two ends of the site. The rainfall at the west end was some 25–26 inches a year, but at the east end it was only about 16 inches. You don't encounter that kind of differential in many projects. But of course this site is close to the foothills, and as you go east from the foothills, the rainfall drops quite remarkably. The differential affects surface water drainage and other water problems that you have to worry about in project design

The site selection study was a very interesting job that had to be done to meet a Congressional deadline. That gave us only about eight weeks to complete the job, including the two weeks it took to write the report. We really pushed. We had a verbal contract for roughly \$100,000 to do the study. That was a lot of money in those days, but it was also a lot of work. I also had to get aerial photographs and the other necessary information. It took a while for AEC to process

the agreement, and we were almost finished with the work when we got the signed contract.

We recommended that the project be located along Sand Hill Road and came up with an estimated cost of \$114 million. Two gentlemen from AEC in Washington came to pick up the unbound report so they could deliver it to the committee the next day. Later they asked us to do a revised and more detailed study focusing on the selected site.

Congress appropriated the funds for the project, and a competition was held for selecting the architect-engineer-manager (A.E.M.) contractor who would design and manage the project. Fortunately Blume was able to get to form a joint venture with Guy Atkinson and Company, and with what at that time was Aetron Division of Aerojet. The joint venture applied for the A.E.M. work on the accelerator. We were up against Bechtel and Kaiser and a few others who were also bidding.

Surprisingly, we got the job, and I was appointed Technical Director in charge of all the design, including architectural, except for the accelerator proper and its ancillary equipment. It was a fascinating job that lasted five-and-one-half years. We did the design work on-site. All the construction was by competitive bid, but Atkinson provided construction project management. Aetron provided mechanical-electrical work, and Pereira and Luckman were the architects. Charles Luckman, I remember, used to be chairman of Lever Brothers.

**Scott:** I remember Charles Luckman. He was into a lot of things. About that same time, around 1960, Governor Pat Brown appointed

him as a member of the Governor's Commission on Metropolitan Area Problems, and I observed him in that capacity when I was a consultant to the commission for a couple of years.

**Sharpe:** We called him "Soapy" when he was not around. He had a good architect on-site, named Don Wilcox, who died shortly after the project was finished. Don got a bacterial infection in the lining of the heart and died while still a relatively young person, just 52.

## A Straight Line on a Curved Earth

**Sharpe:** An interesting aspect of the project was the need to build the tunnel in a straight line. That meant it could not be level. To fit it into the topography better and to better balance soil cut and fill, we decided to slant the straight line down toward the east. Instead of being tangent to the earth's surface, we dropped the east end 50 feet.

When we put out our drawings, the contractor did not see a straight line, but an upward curving line. The earth's surface is part of a globe. If you draw a straight line on a drawing, that straight line is level. Well, we were changing the level, but we wanted a straight line.

The SLAC concrete tube was long enough so that the earth's curvature had to be taken into account. In 11,000 feet it adds up to something like 3.7 feet off-center from a tangent to the earth's surface. Contractors could not believe that we were drawing a straight line, but thought we were showing a curve on the drawings. They had it laid out by elevation and

we were changing the elevations, but it turns out to be a straight line.

**Scott:** Did you have some hassles with the contractors over this question?

**Sharpe:** Well, at first they kind of ridiculed us, but after a while they realized what we were doing. We also provided the survey crew to lay out the work, because the earth's geoid changes as you go along the two miles. We were fortunate to get Captain John Brittan who had been Assistant Director of the U.S. Coast and Geodetic Survey. He had just retired and I hired him. We had him working at SLAC for about three-and-a-half years. He took care of all the geodetic measurements. For instance, he measured the changes in the earth's geoid.

**Scott:** For the record, note what the earth's geoid is.

**Sharpe:** The geoid relates to the gravity effect on a level bubble. In other words, as far as gravity is concerned, the earth is not a smooth sphere, as its surface goes up and down. The geoid is the surface within or around the earth that is everywhere normal or perpendicular to the direction of gravity and coincides with mean sea level in the oceans. If we hadn't corrected the level bubble in the two miles, we would have been off by 3/4 inch. Those were quite important corrections, so we had to adjust the level bubble as we went along the two miles.

We wanted to embed the research station into the lightly cemented sandstone at the east end. There were a number of reasons for this, one being safety. We did not want to point the beam at Hoover Tower about a mile away, or at residential areas nearby. By dropping

the east end down, we had a hundred feet of beautiful sandstone where the beam would go if something happened. By aiming into the ground, there was no chance of the beam ever going beyond the site. It would not do any real damage, but as a matter of practice you can't let an electronic beam get free. At least you do not want to.

**Scott:** You don't want to for psychological reasons as much as anything else?

**Sharpe:** Dr. Wolfgang H. Panofsky—we called him "Pief" as a nickname—was a very forward-looking individual who insisted on higher safety standards than the AEC required. SLAC is within even the more stringent standards that we have today.

**Scott:** Was Panofsky a Stanford faculty member?

**Sharpe:** Yes, he was a Stanford professor who became the Director of the building project and SLAC. A remarkable man, and one of the few genuine geniuses that we have in this country. I think at the time we had some 250 true geniuses in the US, and Pief was one of them.

At any rate, Panofsky was also worried about the impact on the environment, long before any environmental impact reports were being required. We spent a lot of time studying and discussing the potential impact of SLAC on the surrounding areas. There was a road up Skyline Boulevard in the hills to the west where we could go up and look at the length of the site. There was a bridge on this road where we would brown bag lunch and discuss the site. We envisioned what we should do and how best to treat the site.

I would like to give an example of the effectiveness of our efforts. A friend of mine who was chief geologist at Utah Mining and Construction, a big mineral company in San Francisco, lived in Sharon Heights, an area with very nice homes which is just to the north of Sand Hill Road, was complaining to me. "You guys are really going to make it horrible for us," he said. "You are going to be building this huge thing over there." I told him, "Look, we are doing our best, and we are going to try to make minimal impact on the landscape."

Then about two-and-a-half years later I was at a cocktail party with him, and he said, "When are you going to start construction?" I said, "Why don't you come over tomorrow or next week and I'll show you. We have it almost done." He responded, "That can't be, because I can't see it." He could see a few administrative buildings but really had not seen the accelerator itself or its ancillary structures.

**Scott:** The accelerator was mostly underground, wasn't it?

**Sharpe:** Yes, but it also has a metal building on the surface on top of it—the longest building in the world—10,000 feet long. It is longer than the gaseous diffusion plants at Paducah and Oak Ridge, because I've been in both of those, and they are only about a mile long.

## Designing the Concrete Tunnel

**Sharpe:** There were some other firsts in the project. For example, we were concerned about future cracking in the concrete, and I got about \$10,000 authorized so we could get the services of Professor Ray Davis from UC Berkeley, who was just about retired by then.

He was the expert in concrete. We asked him to come up with a low-shrink concrete, because we were going to place at least 100,000 cubic yards of concrete. We wanted to have a strong box for the accelerator proper. The concrete accelerator housing was going through varying ground conditions, and we wanted it to stay straight. Ray Davis came up with the recommendation that we use either granite rock or limestone, with a modified Type 2 low-heat cement.

We put concrete out to bid, and Kaiser Cement submitted a low bid using limestone. They were going to furnish limestone aggregate, which would have been beautiful, and then their management objected. As I recall, Kaiser turned around and paid about 75 cents a yard premium to use granite rock out of Watsonville. The granite rock aggregate was delivered to the site where all of the concrete was mixed.

The contractor built a concrete mixing plant on site, and eventually a total of about 150,000 cubic yards were placed on site, which is a lot of concrete. The accelerator housing was designed to be monolithic—no joints in it—although it was 11,000 feet long. In addition to the two-mile long accelerator—10,000 feet—there is an additional 1,000 feet for the beam switchyard, or 11,000 feet in all.

**Scott:** There were no joints in the concrete?

**Sharpe:** Although we cast it in 80-foot lengths, these sections were then epoxied together so it was all one piece. We put Adhesive Engineering—the epoxy company—in business. They were just a struggling company then. We liked the way they could glue

concrete together, so we had them come in and glue these 80-foot-length joints together.

**Scott:** So there are joints between the concrete segments, but they are glued together with epoxy, so it is almost as if it was solid concrete all the way?

**Sharpe:** Right. Also reinforcing steel goes through, except that the pieces of reinforcing cannot touch each other. There had to be a gap in between the pieces in order to avoid building up an electrical field in the reinforcing. So instead of lapping the bars by wiring the lapped ends of the reinforcing bars together, they were separated by about one inch. Adjacent bars could not be allowed to touch where the reinforcement goes across the joints.

We took concrete cores to check on how well the joints were glued together. It was amazing. Almost all of our cores came out showing the joint as strong as the adjacent concrete. I will discuss this more later. This was the first time that a concrete structure that long had been built without contraction-expansion joints.

We made a special study of the underground tunnel that was to be covered with 25 feet of earth. There was concern about the normal expansion and contraction of concrete. If the accelerator were shut down for any length of time, it would cool off, and then when they started it up, the water coils around the accelerator tube would warm up the air in the housing again to 113 degrees Fahrenheit. This would mean an eventual 40-plus degree temperature increase of the concrete. That kind of temperature rise in 10,000 feet would mean an expansion and lengthening of about 30 or 35 inches. But this would not work, because the

accelerator tube had been precisely tuned, and if the housing expanded, the tube would be stretched and get out of tune.

We made our own detailed study, and then we engaged Professor Jim Mitchell at UC Berkeley to make an independent study. He reported to the AEC and to us. Both his study and ours showed that with the amount of earth fill overlying the housing, friction between the fill and the concrete would be sufficient to keep the concrete from expanding.

**Scott:** The weight of the earth fill, and the amount of friction that would generate, would prevent the 10,000-foot structure from lengthening when it warmed up?

**Sharpe:** Yes, very interesting, the friction and the weight would keep it from expanding as the temperature goes up. It has worked in practice. So this is another interesting aspect of an unusual project. We had so many unusual things to deal with while we were designing the accelerator. For example, we also had to look into the effect of earth tides, which are analogous to water tides. Most people don't realize it, but the earth also moves like the ocean, in response to the moon, but at much lesser amplitude.

**Scott:** You are referring to the solid earth on land responding to the pull of the moon's gravity, the way the ocean does to produce tides?

**Sharpe:** Yes. John Brittan was out at the site with a theodolite early in the morning when the temperature is approximately at equilibrium, so we did not get temperature gradients or convection currents. Brittan conducted this study for about four to six months, checking to

see whether there were significant earth tides. They did not prove to be large enough to have an adverse impact on the accelerator.

Another tricky thing about the project was constructing the concrete housing within a tolerance of plus or minus one-half inch of a straight line. Those were pretty tough tolerances. In addition, we designed it so as to avoid differential movement from a straight line of more than one-sixteenth of an inch during any 90-day period. For a time this hurdle looked insurmountable.

In one area about 1,000 feet long, we surcharged the soil for about a year-and-a-half before the construction started. In other words, we built an earth load on top of the soil to weight it down. This loading compressed the underlying soil before we put a load on it with the construction. Dames and Moore predicted a certain amount of settlement that might occur afterwards, and fortunately they were high. The accelerator has not yet had to be realigned for differential settlement.

**Scott:** In other words, you planned to minimize settlement, but you still allowed for possible realignment?

**Sharpe:** Yes, but SLAC has not had to realign. Another question that came up during the design related to the San Andreas fault, which runs roughly at right angles to the linear accelerator at its west end. The generally accepted theory is that when there is no movement on a fault, stress builds up. There is deformation before the actual sudden displacement during an earthquake. That kind of horizontal deformation of the ground would put the accelerator out of alignment.

Parker Trask, who is dead now but was a professor of engineering geology at UC Berkeley, made a study estimating that in 20 years we would get something like 10 centimeters of offset from a straight line. Fortunately, this kind of displacement has never occurred, but Trask's estimate represented the state of knowledge at that time. SLAC has not had to realign the accelerator tube due to earth movement either horizontally or vertically.

One day early in the project, Joe Armstrong, head of the AEC Engineering Division, came rushing into my office saying, "I have a Congressional letter here and we must have an answer in 24 hours." The letter was from the Chairman of the Congressional Joint Committee on Atomic Energy. It said, "What's this we hear about you building the accelerator as a dowel through the San Andreas fault to keep it from moving?"

**Scott:** He must have been kidding!

**Sharpe:** Oh, no, he was serious. The west end of the accelerator is only about

three-quarters of a mile from the San Andreas fault, and somebody had written the committee chairman to say, "They're building this Stanford accelerator as a concrete dowel through the San Andreas fault. When the dowel breaks, you are going to have a ten-foot offset or some figure like that in the accelerator." This was important because if there were any truth in it, the project would be "down the tube." I had to spend the next day writing out a detailed answer.

**Scott:** You demonstrated that the structure would not actually cross the fault?

**Sharpe:** That's right. I had to give answers to quite a few questions. Joe Armstrong was there all the time. Anyway we went over the letter together, and took it over to Stanford.

They agreed, and then we sent it back to AEC and to the chairman of the joint committee by courier. In those days Congressional mail was answered in 24 hours. When a Senator or House member sent a letter asking for some information, you answered it immediately.



# Consulting in Puerto Rico

*They were probably the first two buildings designed using many of the concepts later included in the Blume-Newmark-Corning book, which was written after these projects were designed and constructed.*

**Sharpe:** I'd like to discuss some of the early earthquake design work we did in Puerto Rico.

Earthquakes do not occur as frequently in Puerto Rico as they do in California. There is, however, the Puerto Rico Deep located south of the island, and it has produced some very strong earthquakes over the years.

Even back in the days before we had computers, the Blume office had an excellent reputation for dynamics, and in about 1957 we were approached by Martinez and Costa in Puerto Rico, a small structural engineering office in San Juan. Sheraton Hotels had asked them to provide the structural design for a 24-story reinforced concrete hotel. They also had been asked to design the 20-story or 25-story *El Atlantico* apartment building, which was to be a tall blade-like structure, narrow and long. It was to be designed for both earthquake and hurricane forces.

**Scott:** When you were first called in, were they in the very beginning phases of thinking about the kind of design they wanted?

**Sharpe:** The design would have been in the concept development stage. They had the outlines of the building, and some preliminary sizes. They came in and just said “Will this work, and is this adequate, and what else is needed?” So they were in the early stages, which was good. It is very difficult to make changes if you only come in after the design has nearly been completed.

### Design Forerunner of the Blume-Newmark-Corning Book

**Sharpe:** I was assigned to work with Martinez and Costa. I spent several days in Puerto Rico going over the details they had in mind as far as concepts, and going over the kinds of forces and loadings that the structures might get. Then we brought back drawings and other data to develop rather crude mathematical models that we could use to do some pseudo-dynamic analysis. The 24-story Sheraton Hotel and the *El Atlantico* buildings were both built.

They were probably the first two buildings designed using many of the concepts later included in the Blume-Newmark-Corning book,<sup>4</sup> which was written after these projects were designed and constructed. These projects in Puerto Rico were done in the 1957, 1958, 1959 timespan, and the book was published in 1961.

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

Some of the problems we had to deal with were due to the geometry of the buildings and the space limitations. For example, we came up with bundled bars, a very large number of 14 reinforcing bars. We also came up with thoughts on confinement of the column steel.

**Scott:** These were groundbreaking designs at that time?

**Sharpe:** Yes. Bundling of bars had not really been used to any great extent. In fact, I am not sure it had been used before at all. When I refer to “bundling,” I mean something like putting three of the bars together, rather than spacing them apart. If you put them together they take up less space. There really had been no testing of this concept. Also we used transverse confinement, to prevent the bars from buckling and failing when exposed to very high axial loads.

**Scott:** Were these efforts in part attempts to get away from nonductile concrete problems?

**Sharpe:** Well, yes. They were efforts to provide enough steel, and you might also say they were early and rather crude attempts at providing ductility. At the time, we really were not thinking so much of providing ductility as just of using good design. Later, however, ideas like these evolved in the book, and in due course it became the ductility requirement. The analysis I did on the Sheraton Hotel was later written up, and some of the details that we designed were then shown in Chapter 7 of the Blume-Newmark-Corning book. I might mention that the book did not include any particular acknowledgment of such authorship of certain parts of the book, and of course I was not alone in this regard. On the *El Atlantico*

building, the tall blade-type structure that juts into the sky, we had to consider wind turbulence and a good deal of what might be called torsional flutter in the building.

**Scott:** You mentioned designing those buildings for both earthquake and wind forces. Say a little more about the magnitude of the seismic and wind forces you had in mind.

**Sharpe:** I believe we obtained the wind forces from the Weather Bureau. We got their best estimate of something like a 50-year hurricane, or maybe it was a 100-year return, something in that range. The wind speed was maybe 100/125 miles an hour. Then we also looked at flutter. Because the structure was a long vertical blade of a building, about 300 ft long, it could tend to flutter.

**Scott:** In high wind it could act somewhat like an airfoil?

**Sharpe:** That's right, it could act as an airfoil. To the best of our ability, using the data and formulas available at the time, we tried to cover all of the forces involved. Since then, the apartment building has come through some hurricanes. While I doubt that it has been exposed to the maximum hurricanes, it has been subjected to some very strong winds.

The earthquake forces we used were the best estimates that we could make of the probable earthquakes in Puerto Rico. It had not been mapped at that time, nor was it mapped until probably 20 or 25 years later. We looked at the earthquake history and the damages that had occurred in the past from very large earthquakes, the largest earthquakes that had occurred on the Puerto Rico Deep. Then we came up with a force level, and then reduced

this for what we now call "inherent ductility," and also made estimates of the predominant frequencies in the motion, so we could see what kind of amplification might occur in the structure. These were crude attempts by today's standards, but we were trying, and I think this was one of the first times that this approach had been applied.

**Scott:** I take it you had to design for both kinds of force, or was one of them clearly controlling?

**Sharpe:** We had to look at both earthquake and wind. I am sure wind largely controlled in the tall blade-like structure, certainly in the transverse direction. It was probably earthquake in the longitudinal direction, because the exposed area was relatively narrow. As to the Sheraton, probably earthquake controlled most of the time, although there may be some appendages sticking out where hurricane winds could have controlled.

They ran into some problems during construction of the Sheraton. The site was right on the beach, and when the contractor started to drill holes for deep piers or caissons, they ran into large solution cavities in the lightly cemented sandstone below the beach sand, where the water had dissolved out the calcium. This was an early case where they pumped in tremie concrete to fill the cavities.

**Scott:** What is "tremie" concrete?

**Sharpe:** It is a common method of depositing concrete under water. You put a canvas trunk or tube in the water down to the base and pump the concrete through it. When the concrete comes out at the bottom it displaces the water. This is done instead of dropping the

poured concrete into the water where it would mix up and get real weak. As far as I know the hotel has not had any problems since. When I ran into Dr. Martinez of Martinez and Costa in Washington, DC about six or eight years ago, he said that as far as he knew the Sheraton was still standing and had no problems. They had made a number of additions and modifications over the years, but it was still a good building.

You might ask why they used concrete. They could import reinforcing steel and also had a small rod mill where they could roll small diameter reinforcement bars. But they would have had to import all of the large structural steel, and it was just going to cost too much. Also, they made their own cement and they had the aggregate. So in the economy of Puerto Rico, it was going to cost less if they used concrete instead of structural steel.

This was back when the first Boeing 707 and DC-8 jet planes started flying. One time on a plane from New York to San Juan, they had some benches across the back part of the plane. If I remember correctly, people from Puerto Rico could fly to New York for something like \$20, and they would be seated on those benches.

**Scott:** They had some very low airfares that allowed plane travel between Puerto Rico and the mainland.

**Sharpe:** Yes, and of course there were also the full-fare passengers who had the seats and the cushions. This must have been about 1958, shortly after the first jet airliners were introduced.

# Work on Nuclear Power Plants

*During this period, 1968 to 1973, I believe I was involved in 18 or 19 nuclear power plants in the U.S.*

**Sharpe:** In this country the nuclear power industry got under way in the late 1950s. It soon became evident that the kinds of buildings and structures required for nuclear power would need much more careful analysis than normal buildings. Also the first digital computer came along about 1959, and computer development expanded rapidly over the next few years.

## **The Diablo Canyon Project**

**Sharpe:** From 1960 to 1966 I had been pretty much occupied with the Stanford Linear Accelerator, and worked mostly on-site. After that I came back to the Blume office as the Executive Vice President and General Manager. Pacific Gas & Electric's (PG&E) Diablo Canyon nuclear power project was one of the next major jobs I was involved with. Blume had also been involved in the Bodega Bay nuclear power plant project, which was finally shut down by the influence of environmentalists. Presumably, the

Diablo Canyon site had been agreed to not only by PG&E but also by the Sierra Club and others.

We had two or three engineering geologists on staff at the time, and they laid out a program for evaluating the site. It then became a question of developing the appropriate seismic criteria.

We came up with some preliminary criteria, and the AEC wanted more detail. So the geologists on the Blume staff, Fred Conwell and Roger Skjei, recommended a rather detailed program of trenching and seismic wave measurements.

Because of our limited geological resources at Blume, we retained Woodward-Clyde as a sub-consultant to help us. Woodward-Clyde recognized that Conwell and Skjei had suggested a very good, detailed approach to determining the seismic criteria for the site, and to evaluating other geologic constraints that should be considered. From then on they basically used the procedures proposed by Conwell and Skjei as a good engineering geology approach to assessing a site and evaluating its seismic hazards. As far as I know, however, there has never been any credit given to Blume, or to Roger Skjei or to Fred Conwell, for this work regarding the site studies.

**Scott:** The trenching would have been done to look for fault traces. You also mentioned measuring the seismic velocities. How was that done?

**Sharpe:** There were at least a couple of ways of doing that. One was to put out instruments that measure distances, and then set off charges, or on some occasions you could put

a steel plate on the ground and hit it with a sledgehammer. This would propagate waves out to the different locations of instruments. I believe this had been used on a smaller scale at other places, but to my knowledge that was the first time it was used for a nuclear power plant. Of course, Diablo Canyon was a site with potential for strong seismic motion, so it got a lot of attention and detailed assessment.

The people at PG&E were most cooperative in the setting up of the criteria and also later on during the analysis. Then they did most of the actual design, but we reviewed some of it. This may help clear up misconceptions on the part of some people that PG&E was trying to cut corners. In fact, at no time did they ever do that. As it turned out later when they had the re-hearings and the seismic criteria were increased, our having followed a very conservative approach on the initial analysis meant that only a relatively few changes were needed, mostly for piping restraints. We had used 1 percent damping, but with the higher forces, 2 percent damping was reasonable. Anyway, in most cases there were few if any changes. PG&E followed this conservative policy throughout on the whole design of the plant, and at no time did they try to “skinny things down” just to try to make the job cheaper. In my opinion, that is why the plant has had remarkable uptime, operating time.

In the analysis we came up with some firsts in a number of ways. We considered a lot of factors that had not been considered before on other plants. Referring back to the seismic criteria, I would like to note that we came up with a response spectrum that considered spectra from the 1957 Golden Gate record,

the Bakersfield-Kern County earthquake of 1952, and the 1940 El Centro earthquake. We developed very conservative envelope spectra, the Golden Gate record for the higher frequencies (or shorter periods), and Kern County record for lower frequencies (longer periods).

**Scott:** The 1957 record was of the Daly City earthquake?

**Sharpe:** Yes, but it is referred to as “Golden Gate 1957” because the record was taken at Golden Gate Park. The rupture was in the southwest corner of San Francisco, or possibly Daly City. I call it the 1957 San Francisco earthquake. Later on in the 1970s at the final hearings for Diablo Canyon’s operating license, they uncovered some new evidence of additional faults (the Hosgri fault) and raised the peak ground acceleration level.

## The Drawn-Out Licensing Process

**Sharpe:** From my prospective, despite the best efforts by the utility, and despite having excellent engineers, and despite the engineers trying to do the best they could, the construction and permit time for the project ran on and on and on. This was also true of a number of other nuclear plants in this country, because of the efforts of anti-nuclear groups. As I recall, the initial cost estimate was on the order of \$500 million for the two units at Diablo Canyon. And if I recall correctly, the newspaper said it cost over \$6 billion for the two units when completed. In other words it increased by a factor of 10 to 12 times the initial estimate. In large measure that was due to passage of time with lots of inflation and changes made. It was something like 16 years from the time of

initial application to the final granting of the operating license.

People have strong feelings on these matters, but with all due respect, I think the design was very safe and that it could have been licensed much earlier. There were red herrings in the press: “You found this mistake and that mistake.” While that may sound impressive in newspaper stories, technically I do not think there were many mistakes of any significance because they did an excellent job of quality assurance. There was a flap about a certain kind of piping in the two plants being analyzed as the same, rather than one right-hand and the other left-hand. The Blume office did all of the seismic analysis for piping systems on that plan. While I was there we did a very conservative analysis, and we discussed this with PG&E. They were very aware of what we were doing and said, “Let’s take the conservative course, to make sure.” Piping systems are of course critical.

Here is a little digression: In 1966 or 1967 the Blume firm got an IBM computer (model 1132), which we used for the piping analysis. We had to set up a special room and have it grounded to avoid all spurious signals. That computer was a big advance, and allowed us to set up the piping analysis programs and put them in at night, hoping to find the process finished in the morning, some 8, 10, or 12 hours later. While this was a very laborious process in comparison with current work, it was a big advance at that time. Later, when Control Data Corporation came up with their CDC computers set up especially for technical calculations, everybody switched to CDC because you could do

much more efficient calculations than on an IBM system.

**Scott:** Regarding the very drawn-out procedure for licensing, what were the central issues? Was design at least theoretically one of the principal issues? Or were there lots of others?

**Sharpe:** It was to stall it, or stop it altogether. Of course that is what they did on Bodega Bay.

**Scott:** People raised questions at several stages of the nuclear power plant review process, didn't they?

**Sharpe:** Yes. People raised new questions about a number of plants when they came back for an operating permit, perhaps some six or seven years after we had approved criteria for the design and construction stage. Except on Diablo Canyon, however, I don't know of any instances where major seismic criteria questions were raised, and that was because the additional fault was discovered during the geological research.

## Consulting for the AEC

**Sharpe:** About 1967 the Atomic Energy Commission (AEC) approached the Blume office and asked if we would be interested in being their seismic safety consultant to review nuclear power plants. After discussing this with John Blume, at first he said "No." The reason was that at the time we were doing most of the seismic analysis work for boiling water nuclear plants. In 1959, General Electric (GE) approached the Blume office for help with the seismic analysis of the boiling water reactors that they were producing. I cannot tell you how many plants there were in all, but I do

know that there were probably 15 or 20 plants on which the Blume office provided seismic analysis.

The AEC came back to us again, probably in 1968, and wanted me to be a consultant on reviewing nuclear power plants for their seismic safety, and reviewing the analysis and design. The other reviewer at the time was Professor Nathan Newmark at the University of Illinois.

After my going back to Washington and spending a day or two talking with the AEC people, we finally worked out an agreement.

We did not want to have any potential conflict of interest, so on any plant where there was a boiling water reactor—which would be GE's—we would not be contacted in any way by the AEC, because we were doing the analysis work for GE. GE would furnish the data we developed to the architect-engineer doing the balance of plant. So from the AEC side (the review side), we were not involved in any GE plants. We started in 1968, and I was the person that went to all the meetings and also conducted the review and had my people working for me. On occasion John would get involved on a question or two, but he generally was tied up with other things.

For the next five years, 1968 to 1973, we had a rather smooth-working review group at AEC. It was composed of the AEC project manager, who really did not participate too much technically, and there was Leonard Murphy of the U.S. Coast and Geodetic Survey, and later, Jerry Levine or Leonard. Jerry was Leonard's sidekick. He is now an Associate Director of USGS, into which Coast and Geodetic had

been absorbed, and there was Hank Waldron, who was a geologist from the U.S. Geological Survey. Each of us would do our homework on a particular plant, then we would meet for probably half-a-day to a day back in Bethesda, Maryland, where the AEC was headquartered, and review all the aspects of the plant.

This was in the seismic criteria stage. After we had reviewed what the applicant had submitted, we would agree on the seismic criteria, or what we thought it should be as a minimum. The three of us would make our best efforts, then we would sit and listen to the applicant. At most of these meetings the applicant would have maybe 80 to 100 people there, including engineers, management, vice presidents, etc. of the utility, and then the brass from the AEC, then everybody else, and all of their consultants. They would make presentations, and we would question them. We'd usually end up saying "Well, we don't think you have considered this factor enough, it should be more like this," on down the line.

## Sketching a Response Spectrum

**Sharpe:** I remember one classic case, probably in 1971 or 1972. I believe it was the Beaver Valley nuclear power plant. Bob Whitman from MIT was a consultant, and he presented a response spectrum that I did not think was adequate. They said, "Well if you don't think it is adequate, what would you accept as adequate?" That night I sat down with the response spectra from around ten earthquakes, looked at the site conditions at the plant, looked at the site conditions for those spectra, and sketched up a response spectrum. The next morning I showed it to the applicant

and AEC, and pointed out the basis for it. All I said was, "This is the type of spectrum." Well, the applicant took it, and the next thing I knew they had submitted it as part of their application. The AEC thought it was pretty good, so they gave it to Nate Newmark, and Nate issued it as a revised Newmark response spectrum. It is interesting that if you have a big name you get the credit, and if you don't have a name you don't get the credit.

**Scott:** So they in effect accepted and appropriated what you had done, almost on the back of an envelope?

**Sharpe:** Well, a lot of thinking had already gone into it. I had done a lot of study before. But I finally put something down that night because they had asked for something the following day. I should also say that this was a revised Newmark spectrum, because Newmark and Hall had come up with a spectrum that everybody thought was way too conservative. The amplification was very high. He had published it, but people would not use it because it seemed way too conservative. I can illustrate this with my hands as a response spectrum—this is the period (horizontal) and this is acceleration (vertical). Newmark's peak was much higher, and what I came in with was something about in here, significantly less acceleration.

A spectrum is a plot of the maximum response of a single-degree-of-freedom system with a particular damping. This idea was first developed by Maurice Biot and then refined by others. It is a plot of the maximum response, so you have acceleration on the vertical axis, and the horizontal axis would be either the fundamental period of the structure, or equivalently,

the inverse that is the fundamental frequency. You can do it either way.

**Scott:** You plot a curve?

**Sharpe:** Yes. A spectrum is a curve plotted on a graph. You plot it for different damping values like 1 percent, 2, 5, 7, 10, and even 20 percent.

**Scott:** The curve shows the maximum acceleration at different frequencies of vibration?

**Sharpe:** Yes, for a given ground motion, you input a response history, and then you determine the maximum response at each frequency. This calculation is all computerized now. This is a single-degree-of-freedom system, so it is a bit of a crude approximation as compared with a multi-degree-of-freedom system. But it is still indicative of what is going to happen. There have been a lot of improvements in analytical techniques so that you can make use of the response spectra. And of course nowadays people like to use response histories, originally called time histories, but time histories are not necessarily the answer either. They are only a partial answer.

**Scott:** Time history means the “history” of actual earthquake shaking?

**Sharpe:** Yes, or of artificially generated earthquakes.

**Scott:** By artificially generated, you mean simulation on a computer?

**Sharpe:** Yes, it is done on a computer. I guess it was Paul Jennings and George Housner who in the early 1970s came out with artificial time histories, and these are still used. People also

often use the actual records, depending on the site characteristics where the record was made.

These days they have very “precise” site-specific spectra. Frankly, however, I don’t see where they get any better results than we did by using a lot of good judgement and looking at other past records. Basically what is being done is to computerize the process, in some respects taking out the thinking process. I like the way we did it because you had to think your way through. That way, when I ended up with something, I had a good feeling about it.

**Scott:** You knew what lay behind your answer.

**Sharpe:** Right, and to me that is all-important. To my knowledge no computer program has judgement written into it yet, at least not for seismic analysis or seismic criteria. There may be some artificial intelligence programs that try to, but even there I am not sure about the judgement.

During this period, 1968 to 1973, I believe I was involved in 18 or 19 nuclear power plants in the U.S. There was also the Barnwell plant, which was to be a fuel reprocessing plant. When I started in 1968 the AEC really had no written criteria. Over the next couple of years I helped them with, and in fact drafted, much of the AEC design criteria. This covered from how you approached development of the seismic criteria to the analysis, the loading combinations, and the allowable stresses. It covered most of the details. While much of it has been revised since then, it has not changed materially.

**Scott:** What form did that take; was it published; and if so how?

**Sharpe:** It was published as a regulatory guide. In fact, on some of the regulatory guides I'd send in a draft, they would review it, and in a week or so I'd get a phone call. They'd say "Hey, how about this, and that, and the other?" We'd agree over the phone, and the next thing I'd know it would come out as a draft regulatory guide. Although there have been improvements and maybe a lot of complexities added, the basic concept is still there.

I doubt if the answers they get today are any better, or much better, than the answers we got then. I believe engineers who have used a lot of thought and judgement would say that the answers are probably not much different, although I am sure the computer artists will disagree with me. I feel that with the current "sophisticated" and complex methods of arriving at some of the criteria, you don't get a better answer than when we used good engineering judgement.

## Earthquake Records and the Regulatory Guide

**Sharpe:** In talking with the associate director of regulatory activities, Ed Case, we discussed coming up with more standardized criteria for nuclear power plants. I gave this some thought, talked with John, and we laid out a program to make a statistical analysis of about 33 earthquake records. We selected the records so they would have greater than 10 percent gravity as a peak ground acceleration, and they would hopefully have different ground conditions. At that time a number of the seismograph stations did not have soil data or a soil profile available to tell us what kind of material was underneath. There would be

guesses—somebody would say "Well, that's a rock station," or "That's a soil station." Not until a number of years later did Nuclear Regulatory Commission (NRC) contract with several geotechnical firms to determine soil profiles at the sites.

In looking back, I think we did the analysis for a very low price, and a lot of work was required. I had a young man from India, Jagat Dalal, who had done his graduate work at Stanford, and had gone back there to finish up his PhD. He used some of this material for his PhD thesis. We first made a very detailed analysis of each record. Then we combined all the records and came up with an average record or mean, then mean plus one sigma, mean plus two sigma, and mean plus three sigma. Making an arithmetical plot, I compared that with the one I had done on an envelope in a hotel room. Looking at the two, somebody would say there was collusion going on. I get a kick out of how many computer runs we made on the statistics, but the answers still came out about the same.

About the time we had started this work, Newmark heard about it. A couple of months later he also submitted a proposal to the AEC to make a similar study. He came out with his selection of records, and when he finished his analysis we each submitted our draft reports. AEC asked each of us to analyze and review the other's work, and then had a meeting in San Francisco. It included Nate Newmark, Bill Hall, and Bijan Mohraz (a grad student at Illinois who had done much of the work), John Blume, Jagat Dalal, and myself, plus Dave Lang, the program manager for AEC. Newmark liked to plot things on three-directional tripartite three-way log paper. I like to make

arithmetic plots because I can then visualize better what the changes are.

Newmark pretty much agreed with our results, except that he felt we should add a little in the high frequency range. Newmark had plotted his results and came out with a horizontal plot in the velocity range. If you plotted our results on tri-partite, you got a sloping line. This was a bit disconcerting to Newmark, because he had always proposed that you had straight lines in the acceleration portion, a straight line in the velocity, and a straight line in the displacement portion of the spectrum.

We got into a big discussion (I am going from memory), and I pointed out that in correcting his records, they did a statistical analysis for the acceleration range and had corrected the records for that. Then he made further correction for the velocity, and made a statistical analysis on the “corrected” record. Then he did the same for displacement. As a result, they came out with nice straight lines. We had done our homework and pointed out that statistically this was not correct procedure.

Instead, you should make your statistical analysis of the raw data, and then make your corrections. I am not that great as a statistician, but I do know a little bit about basic statistics. John just sat there and smiled, but of course he had agreed with what we came up with. We had done the work so he let us do the talking. Also it would not do for him to challenge Newmark, whereas young guys like me could do it and get away with it. I was a friend of Nate till he died and always had tremendous respect for him.

Anyway we finally agreed that basically the

Blume record should come out as the *AEC Regulatory Guide 1.60*, and it would be referred to as Blume’s and Newmark’s research. Newmark being an excellent salesman, he suggested that as there was going to be a national ASCE (American Society of Civil Engineers) meeting in San Francisco in April (of 1973), we should write a paper on the criteria, and submit it to be presented there.

Nate said he would be very happy to draft the paper. Well this was a bit clever, because he caught John a little short. You see, the name of the guy who drafts the paper comes first, so the paper came out Newmark, Blume, and Kapur. Because Dave Lang had a heart attack a short time later, another AEC representative (Kanwar Kapur) took over and got his name on it as an author, though really he had nothing to do with it.

**Scott:** What combination of people actually wrote the paper?

**Sharpe:** Bill Hall drafted it and sent it to me. I read it next, expanded it and redrafted it. Then I presented it to Blume, and he agreed. Then Bill presented it to Nate and he agreed. The submittal then went out as the Newmark, Blume, Kapur paper.

**Scott:** The work you and Bill Hall did was incorporated into the paper, which was published over their names?

**Sharpe:** Yes, it was published as an ASCE paper: “Design Response Spectra for Nuclear Power Plants,” by Newmark, Blume and Kapur. A few years ago I was talking to Bill Hall while looking up a reference. I said “Bill, didn’t we do something like this?” He said, “Yes, why don’t

you dig out the paper you and I wrote for Nate and John?” So Bill still remembered, too.

## A Damping Guideline

**Sharpe:** Around the time this work with Newmark was being done, I was talking to the AEC about how to determine damping to be used in different analyses for structures, piping, and so forth. Apparently Nate had not gotten into that, so at the same national ASCE meeting I made a presentation of a suggested damping guideline. I had been doing research on the subject, basic literature research and telephone research, talking to all the people that would have any material on damping. I had Gary Kost, one of my people, compile these data. We reviewed all the material, came up with recommendations, showed it to John, and he said, “Well, it looks fine to me.”

So I presented this tabulation at the ASCE meeting, and it was one of the few times that I one-upped Nate. We presented this and the background for it, and the AEC agreed. So then Nate said, “We’d like to look at it a little further.” He did, and made some changes in format on the damping. Then these two studies, the one on the response spectrum and the other on the damping, came out as a *Regulatory Guide 1.60* and *Regulatory Guide 1.61*. That was 1973, and they are still being used today.

## The Fast Flux Test Facility at Hanford

**Sharpe:** About 1968, I got involved in the Fast Flux Test Facility (FFTF), which was built at the Hanford Reservation, Hanford, Washington. The project had started earlier, and Bechtel was the AEC architect-engineer. I

believe the Westinghouse Hanford Company asked the Blume office to evaluate the potential site. This got into a lot of different things, from making a detailed geologic assessment of the site, to coming up with seismic criteria and a response spectrum. It also included some very detailed analyses of the structure, the piping, and some of the equipment inside. I don’t remember the exact dimensions of the structure. The AEC’s Division of Reactor Research and Development was building the facility, and the price was going up, so they decided to cut the diameter of the cylindrical structure. There was a big reduction in size.

The FFTF got to be a very fast-moving project, and I spent a fair amount of time up at Hanford and also met some of our people there. At one breakfast meeting I was talking to two people from the AEC, describing what more we needed in order to answer some of the questions that the regulatory people were asking. I outlined a program of geologic evaluation of the site. They said, “How much will it cost?” I did a little back-of-an-envelope figuring and came up with a figure of \$100,000. Before breakfast was over the AEC people said, “Go ahead, we will extend your contract.”

**Scott:** That was a very quick decision.

**Sharpe:** Yes, the point was that we were subject to audit, and this was only to pay for the labor and expenses. We did a very meticulous job, because a lot of questions were being raised.

**Scott:** When you say meticulous job, are you referring to the engineering or the geologic evaluation?

**Sharpe:** Well, we first did a very careful

job on the geologic evaluation. After we had quite a bit of this information, I came up with a response spectrum; I guess today you would call it a site-specific response spectrum. I did not have a computer program but did it on the basis of judgement, and looking at the geologic data and other material we had available and submitted it in a report. A week later I got a call from Dave Leeds, who was then with Dames and Moore. He was doing a seismic study for a site adjacent to Hanford, and said “Hey, Rol, I like the shape of your response spectrum, do you mind if I use it for my site?” I remember this because we now talk about these computer-generated site-specific response spectra and, with all due respect, I think they are not any better, and perhaps not even as good, as what we did in the older days.

**Scott:** Would you describe in lay terminology what you did in the geologic investigation, and also what kinds of judgmental factors were involved?

**Sharpe:** There were records of a lot of micro-earthquakes in the area. There was also a question of whether there were some active faults in the area. Andy Cunningham—an engineering geologist at Blume—along with Fred Conwell and I laid out a program of trenching across areas where there might be suspected faults. They also did a careful

geologic reconnaissance. They would go out and do the mapping and come up with excellent reports. As I recall, we found that there were no active faults in the area. We also came up with some explanations of the micro-earthquakes, and the ACRS (Advisory Committee on Reactor Safety) was satisfied with our work.

**Scott:** What about the micro-earthquakes?

**Sharpe:** The micro-earthquakes were very small ones, less than magnitude 2. There were a lot of those, which were picked up on recording instruments but not discernible to people. Anyway, the response spectrum was accepted by many in the area, and afterward the local power district built nuclear power plants across the Columbia River from Hanford.

Also this big outfit—which later went broke—was proposing to use the same response spectrum. Getting flack from certain groups, they retained Woodward-Clyde to do a very exhaustive study of the area. So almost ten years later in the late 1970s, Woodward-Clyde spent I believe something like \$2 million. They came up with about the same spectrum that we had come with earlier for much less expenditure, about the same answer that we had gotten.

# Other Blume Projects

*We set up the program with a very high priority with the Air Force to get B-58s to fly from their base in Oklahoma to Edwards Air Force Base in California, where we had the instrumentation set up.*

## **The Missile Bases**

**Sharpe:** About the same time, the first Titan missile bases and later the Atlas missile bases were started. John Blume and I were asked to be consultants. We needed to have top security and top secret clearances, yet they also wanted to get started on the job. To show how the government can do things when they want to: as I recall we got our clearances in a little over two weeks. That is unheard of. They told us they could have done it a little quicker, but we already had AEC “Q” clearances, which are top secret for the Atomic Energy Commission. They said, getting the records from AEC delayed the process. Fortunately we were U.S. citizens and have always lived here, and so we were a little easier to check out.

**Scott:** Where was the work?

**Sharpe:** The Titan was going to be located in Colorado, and I believe the Atlas bases were also going to be somewhere in Colorado. They were going to be constructed in other parts of the country. The consulting was really more on the response to dynamic forces rather than on earthquakes.

We met most of the time at TRW in Los Angeles. The architect-engineer was a joint venture of DMJM (Daniel, Mann, Johnson & Mendenhall) and three other firms. I think Mendenhall was the engineer, and the other three were architects. They joint ventured with Silas Mason, Mason-Hanger, Rust Engineering and one other group. The Air Force had a Senior Consultants Advisory Panel chaired by Nate Newmark. There were a number of other consultants to the architect-engineer. When we had meetings with the Senior Consultants Panel, we could only address them by means of written notes.

**Scott:** You mean you are sitting there in the room with them, but you could not talk to them directly?

**Sharpe:** That's right. So when we'd ask a question or want to make a comment, we had to write it down and hand it to the colonel, and then he'd take it up to them. Rather ridiculous.

**Scott:** Why did they use such a procedure? Why was it so highly bureaucratic?

**Sharpe:** Some of it was the bureaucratic Defense Department. We had lunch with Nate Newmark. At that time we were just beginning to get acquainted with him. We had a chance to discuss things with him at lunch and get things solved, and then go back to the meetings. We had to come up with analytical solutions and

loadings. They wanted to design for very high overpressures—almost a direct hit (but not quite) by an atomic bomb. That was very high overpressure. Overpressure is the air pressure wave that would travel across the surface. Another type of loading is generated by the explosion of the atomic bomb that causes ground motions that would impact the silo.

**Scott:** With an atomic bomb actually hitting nearby?

**Sharpe:** Yes, very close nearby. I think the loadings may still be classified. It involved tremendous loads, and we did some rather innovative work. Our analytical approach at the time was rather simplified and crude by today's standards; now there are some very fancy computer programs to do the same thing. We looked at the passage of traveling waves going across through the ground and affecting the structure, the amplification with depth and the effect of soil materials and things like that. Also, the effect of the flexibility or deformability of the silos. This was all below grade.

**Scott:** These were to be underground reinforced concrete silos?

**Sharpe:** Yes, they were huge. We came up with the best solutions we could, with somewhat simplified analyses. We did not have computers in those days, in the late 1950s. As far as I know, neither John Blume nor I were ever mentioned in any reports. Nothing was ever public; we were not allowed to write about it. We had some interesting times and had a chance to do some innovative thinking on a crash basis.

**Scott:** It is interesting that it probably has

never really been written up in a publicly available way.

**Sharpe:** I doubt if it has. I think they are still in the form of old classified documentation.

**Scott:** And yet some of that work would probably have been really fascinating and a good source for articles that could have fallen into the technical literature.

**Sharpe:** Much of the early work in dynamics was classified, and I think it still is, although a lot of it has now been developed for the public use. We had more classified documents that we looked at, based on nuclear testing and things like that, that we also used to give us some kind of a check on what John and I were coming up with.

**Scott:** You used classified material. This is a whole literature kind of hidden away from the general public or the main bulk of the profession. They cannot have access to it.

**Sharpe:** Not only do you have to have clearance, but also you have to have a “need to know.”

**Scott:** “A need to know” That means to use that information you have to be under contract for a relevant project of the Defense Department?

**Sharpe:** Yes.

## The Sonic Boom Program

**Sharpe:** I’d say the Blume office, John being the brilliant engineer he was, and with the reputation he had, got into some really interesting, really fantastic research and consulting. The sonic boom program ran from about 1965

through 1968. The U.S. government was planning on building a supersonic transport, and the big question was the impact of sonic booms on structures. The Federal Aviation Administration (FAA) came to John because of John’s reputation in dynamics. John set up a program of experimentation, and then I came back from SLAC about that time and took over as Project Manager. The testing program was conducted at Edwards Air Force Base in California, where later on they landed space vehicles.

**Scott:** Can you explain a little about sonic booms?

**Sharpe:** The ambient air pressure at sea level is about 2,100 pounds per square foot, or 14.7 pounds per square inch. As a sonic boom passes over, the pressure wave might hit say as high as 7 or 8 pounds per square foot, abruptly changing the air pressure from 2,100 to 2,108 psf, which is a very small percentage increase as the pressure wave goes across. Yet despite the small increase, we know that sonic booms can crack windows, and there have been reports of cracking plaster or sheetrock in buildings. So the FAA was charged to work with the Air Force. Later the Air Force Office of Sonic Boom Research took over from the FAA.

It was a fascinating program. Of course we needed some empirical data, which meant we had to have airplanes fly at supersonic speeds, and we also needed to have structures to instrument. In searching for the availability of hardware at the time, we found a total of something like 35 pressure gages and velocity transducers that might be available for us to use to measure the sonic boom pressures.

There were only 35 such instruments in the whole country, and the two companies that had them were Lockheed and Boeing. For us to get these instruments, we had to sign on Boeing and Lockheed as subcontractors to the Blume office. We could then brag that while we were a 40- or 50-person engineering office, our total manpower including subcontractors was something over 300,000.

We also subcontracted with AeroJet's Aetron Division for instrumentation specialists to install and monitor the system and then to monitor the recordings. At the time we had rather sophisticated recording instrumentation, 14 channels on 1-inch wide tape on 12 inch reels. We installed the instruments on the structures and also wanted to get some free field recordings from instruments located out away from any building, bushes, or trees so you'd get the unadulterated pressure wave.

The basic purpose was to determine the effect on houses, because a very high percentage of structures that would be subjected to booms would be such homes. On a rush-rush basis we built two houses adjacent to Edwards AFB. We designed them and got a contractor near there to build them. As I recall, from design to completion of the houses it was something like six weeks. These were lovely homes, painted, complete with HVAC and everything else. Then it took another week to install instruments. We put accelerometers on the glass windows and sliding doors, and different types of strain gages on the partitions and other areas where we thought there might be cracks in the structures. We had these all connected into the tape recorders, so we could record what we called the "signature of the sonic boom," really

the acceleration, or the air pressure v. time, and the response of the panes to this loading.

Sonic booms from an F-104 fighter last about 0.08 to 0.1 second, so we had to pick up the signature in a very short time. With a B-58 the signature was something in the range of 0.2 second and for the YF-12 it was something in between. The YF-12 was a spy plane like the ones they recently decommissioned. We set up a high-priority program with the Air Force to get B-58s to fly to Edwards Air Force Base, where we had the instrumentation set up, from their base in Oklahoma. They refueled in the air, and then flew what we called a "race-track course," because we wanted them to fly a straight level course roughly parallel to the runway at Edwards Air Force Base.

**Scott:** You wanted them to come in at a certain supersonic speed?

**Sharpe:** Yes. Generally their operating speed would be 800 or 1,000 miles an hour (although some of the planes we used exceeded 3,000 mph). We would have the pilots on voice radio, and then start our machines and our tape recorders running. We ran the tapes at something like 30 inches per second, the reason being we wanted to get the sonic boom signature spread out far enough on the tape so we could readily read it and hopefully filter out any ambient background noise from the small change in air pressure. We would get the pilots on voice, and an Air Force colonel gave them directions. We had the voices on our radio speakers so we could all listen. When we got everything running, the colonel would have the pilots tell us their heading. We also had the radar tracking officer at the base plotting the track of these planes.

The course of the plane was like a racetrack, because we wanted the plane to be in steady flight as it passed over on the straight-away segments. At operating speed the plane would make a 180-degree turn, come back parallel, make another 180-degree turn, and then make another pass over the site. It was a rather large race-track, something like 200 miles long by 100 miles wide.

We also had F-104s and YF-12s. We had a high-priority rating so that any time any of these planes took off from Edward Air Force Base they had to run a sonic boom run for us.

This also applied to the XB-70, the B-2 supersonic bomber prototype. There were two of them there. The first one was limited in speed, because it had micro pinholes in the wing gas tanks and if it flew faster than about 1,200 miles an hour enough vacuum would be set up over the wings so that some of the fuel would start leaking out through the pinholes. That model was limited in speed and also could not fly for very long. The second model corrected these deficiencies and could fly at higher speeds.

We would have a briefing at the Air Force offices where they would tell us what planes were going to fly that day. I remember we'd ask the YF-12 people, "When you fly this track, what will be your heading, altitude, and speed?" I remember there was a real trim-looking major who would always say, "Well, excuse me, if you need to know, let me go find out what I can tell you." So he'd go out of the room and come back in about ten minutes, and say, "Well, I cannot tell you, that's classified." What we did not tell him is that we always got the radar track plots, and so we knew how fast

and how high the plane was flying. We never let him in on the secret.

## The 2XB-70 Crash

**Sharpe:** Once, the model 2XB-70 was going to fly the next day. The colonel found out about it and said, "Are you going to make this a sonic boom run?" They said, "No." So he said, "Well, we have our priority." So he insisted that he talk to the two-star general at the base. The general said, "I am sorry, I cannot do anything." So the colonel went up to the Chief of Staff of the Air Force, and then got the Secretary of the Air Force out of bed at 2 a.m. back in Washington and said, "Hey, my orders are this, and our priorities are this, and so we have to have this flight make a sonic boom run." The reason for his concern was that the plane could take off and land only four times on a set of tires. A set of tires cost \$150,000. Also there was a 600-man ground support crew required to take care of these two airplanes. When one of the planes flew, it could not take off again for about a week or ten days, because of all of the maintenance and everything else that had to be done. So we needed the sonic boom run.

**Scott:** So it was very important to piggyback the sonic boomer on every flight you could?

**Sharpe:** Yes, and we had the priority so we could demand that.

**Scott:** And the colonel didn't want to let that priority slip, which is why he went all the way to Washington?

**Sharpe:** Yes. So the Air Force Secretary said "okay." This was at 2 o'clock in the morning, and I think the flight took off at about 7 o'clock in the morning. The plane took off and made

the sonic boom run. We still had them on voice, and all of a sudden we heard “May Day, May Day!” Something went wrong and the plane crashed. One of the two pilots got out all right. The other XB-70 model of the plane is in the Wright-Patterson Air Force Base museum.

## Estimating Window Pane Damage

**Sharpe:** In order to obtain a data base for predicting probable damage to windows and other building elements, we asked the commanding general of the air force base, via the colonel, to distribute a form to every occupant, every shop, and every hangar on the base. We estimated there were about 100,000 glass panes in all the buildings on Edwards AFB. We asked the occupants to inspect their facility each time we flew sonic boomers—we didn’t do it every day, because we didn’t have the planes available. When we did fly, we would notify the people and ask them to inspect afterwards within a few hours and report any cracked panes. Then we would send somebody over to investigate.

This was of interest because it gave us a statistically significant base of 100,000 window panes. As I mentioned, when a sonic boom is being laid down, it normally ends up as a two and a half or three-pound overpressure shock, or even five pounds per square foot pressure rise. That is not enough to overstress a glass pane. It is really the outliers, in other words, that might break. It may be the glass is weak or had a scratch in it or a defect of some type. Sometimes it may have been too tight in the frame, and there could be a rough edge or something that initiates and propagates the crack. As far as I know, this is still the only

statistically significant work that has been done on the effect of sonic boom pressures on glass and on structures.

The reason for this research is that if a supersonic transport flew from San Francisco to New York at 50,000 feet altitude, a normal flight would lay down about a swath of sonic booms 50 miles wide. The plane would not want to go much higher than 50,000 ft because they could get into radiation problems. In fact this was a concern of the YF pilots, so they had to have some insulation shielding in their flight suits to protect against radiation, because at 90,000 feet there is quite a bit of radiation from space getting through the atmosphere.

Flight paths could be selected to minimize the exposure, but still several million panes would be exposed in crossing the country once. We estimated the total number of glass panes in the U.S. at that time, as something like four billion. So even though you have only a very small rate of cracking, you could still get several hundred panes cracked in any given exposure. This would cause problems and there would be a lot of lawsuits. People would all of a sudden notice cracks in the glass panes and want damages, or they would see a crack in a gypboard wall or other element and want damages.

While we were flying the program, the planes would lay down sonic booms all the way around the “race track.” So we publicized in the newspapers that anybody noticing any cracking from sonic booms should call our number. Then we would send an engineer to inspect. We got to be quite expert in examining cracks and determining whether they were new or old. You’d be amazed at how many

cracks people would call in, and then we would find dirt or dust or cobwebs in the cracks.

After we finished the program, we ended up with something like 50 boxes of 1-inch tape reels, but the usable record on each tape reel would probably be 1 percent of the length. In other words, we could take the 50 boxes and probably condense them into one box. I tried to get the Air Force to pay for us to use the computers at Lockheed in Sunnyvale—they were the only ones around here then that were capable of editing the tapes—to take off just the relevant portion, because we had so much blank space on the tapes.

**Scott:** Blank space during the time when you were waiting for the actual boom to occur?

**Sharpe:** Yes, and we would run the tape for a little bit before the boom occurred and a little bit afterwards. At the speed we were running it, we used an awful lot of tape. We never could get the editing okayed, however. The last I heard these 50 boxes with 12 12-inch reels per box were in storage in Alexandria or someplace in Virginia. For anybody to use the data, they would have to run a lot of tape to get to the actual boom signatures.

Then came the question, can we get some data from a populated area? The Air Force asked if we would lay out a program of measurements for flying B-58s from Indianapolis, Indiana to Louisville, Kentucky. We wanted to get a better handle on how many cracked window panes there might be. As I recall this track is about 150 or 200 miles long, and we laid out an instrumentation program with quite a few instruments in a carefully planned pattern on the ground. These would be transducers

to pick up the air pressure (boom signatures), and also instruments to pick up response of structures and glass panes. It was a rather ambitious program so the Air Force asked the National Academy of Sciences to appoint a committee to review it. Stanford Research Institute (SRI) was also involved, but not in the structural part. SRI, being a big research outfit, didn't really like a small firm like Blume being the expert in air pressure shocks and similar phenomena. We had a big meeting at Lockheed in Palo Alto and SRI pretty much set the agenda. We gave our report on the Edwards AFB testing. SRI had their big guns there; there were just two of us, myself and a young PhD from the Blume office who came with me.

All of a sudden out of the clear blue SRI proposed a Midwest testing program (I cannot remember the exact details now), but I turned to Jacques Proulx, the engineer with me, and said "My God, Jacques, I didn't bring anything on that, what have you got?" Well he dug in his brief case and came up with something, and we had about ten minutes to put it together. SRI really blind-sided me. I am sure they did it deliberately. It was amazing that Jacques and I in ten minutes could put it together. I made some notes, and I got up and talked. This was on a Friday, and the Air Force colonel called me at home on Saturday morning. He said, "Rol, I want to tell you that I think you made a hell of a fine presentation. We decided to use your plan instead of SRI's. "In fact," he said, "I want you to come back to Washington, and give a presentation to Dr. Nick Golovin and to the full National Academy of Sciences Committee." Nick was the Deputy Director of the Office of Science and Technology, the

scientific advisor to the president. So we went to Washington and made our presentation, and they agreed with our proposal. They thought it was very sound statistically, very well laid out. Jacques was a good man, and I had a few ideas to contribute too. Dr. Golovin phoned me a week or so later to discuss the Edwards AFB report and he said he thought the statistical work done on the glass panes was superb. Everything was set to go ahead, and then President Lyndon Johnson decided to cancel the SST program.

### **Military Protocol and My Tight Schedule**

**Sharpe:** As part of the SST work, every four weeks I had to go back to a meeting with the Air Force on the SST programs. We met in the Executive Office Building, which is the building right next to the White House. I had to meet them at 8:00 a.m., and because of the military protocol, we quit at 5:00 p.m. We always had a fixed agenda, and when it came to my part, I had 15 minutes to get up and give my status report on what we were doing. Then there were usually about five minutes, and not more than ten minutes for questions before the session ended at 5:00.

The major general was a real sharp guy who had gone back to school and gotten a PhD in physics. He was a very nice guy, but boy! he ran a strict schedule! When I tried to get the general to shorten my time so I ended at 4:45, he said, "Sorry, we meet 8 to 5."

**Scott:** He wouldn't budge? Did that create a particular problem for you?

**Sharpe:** Yes, it did. The last non-stop flight

from Dulles Airport to San Francisco left at 5:55 p.m. So I would dash out of the Executive Office Building, run across the street, get my rental car out of the garage, hit the George Washington Parkway and then the Dulles road. I would be almost flying, usually about 90 or 95 mph, skid in about ten minutes to 6:00, throw the car keys to the rental car people, and dash onto the plane. I had to take a TWA flight because it left ten minutes later than United. I did that for about a year-and-a-half, making a dozen or more trips to Washington just for those SST meetings.

### **A Blume Office in Tehran**

**Sharpe:** In the late 1960s the Blume office had up to 100-plus people, including several Iranian engineers. One of these was a really bright thinker, whose wife was from Sacramento. He said, "You know, Iran is starting to awaken, and we have no structural engineering offices there; I think we should open an office in Tehran." So he got two Iranian engineers to come over from Iran. One of them had just retired as the Director of the Iranian National Railways, and the other was a construction contractor.

We talked about it and worked out an agreement. By Iranian law, the company had to be owned 51 percent by Iranians. In 1970 we opened a structural engineering office in Tehran, called John A. Blume and Associate Engineers, Iran. I first went there in the fall of 1970. I had been in Germany for a week, and then flew to Tehran. When I got there the partners had all the news media present. I still have copies of the English-language newspapers, "Famous American Engineer opens office

in Tehran.” We had an office directly across the street from the entrance to the U.S. Embassy on Takte Jhamshid. That was a main drag, and was given another name after Ayatollah Ruhollah Khomeini took over in the Islamic Revolution of 1979.

I went to Tehran about every three months, and each time I would have lunch with the U.S. economic attaché, so I got fairly familiar with the U.S. Embassy. It was a beautiful sprawling structure with nice gardens around it. I believe we also helped set up the soils engineering business there in Tehran. Dames and Moore was just opening an office in Tehran, and I think we gave them their first contract because we needed some geotechnical help. It was the first of several contracts we gave them to do soils work for buildings we were designing.

In about 1972 we were told that if we could help the government of Iran get a large loan, we could get the contracts to design two big military bases. This was my first experience with this kind of thing, but I talked to Wells Fargo Bank and their International Division, and they were interested. This was the first western loan to the country of Iran, for \$24.5 million and for seven years. I didn’t know the ways of the financial types, so we agreed to pay Wells Fargo’s expenses to go to Tehran. Instead, it should have been the other way around. You usually get a 1 percent finder’s fee. One percent of \$24 million is a quarter of a million dollars!

The loan ended up being handled out of the London office of Wells Fargo. There was an executive vice president there who was rather arrogant, and who tried often to give me a hard time. I remember one phone conversation

when he said, “Do you know who you are talking to?” and I said, “Oh?” He said, “Well, I am executive vice president of Wells Fargo International in the London office.” And I said “Well, how do you do—I am executive vice president and general manager of John A. Blume and Associates Engineers in San Francisco.”

To make a long story short, the Iranians got the loan, which was the first loan made by a U.S. bank to the Iranian government. I assume Wells Fargo got paid back, although I cannot tell you for sure. Anyway we got the jobs. Later on we arranged another loan. So you see only a short while ago Third World countries and U.S. banks did not really know each other.

The Tehran office built up to around 25 people and did a lot of work. We sent one man over from the Blume office, Siavash Adib-Zadeh. Siavash had worked for British Petroleum, which pretty much ran the Iranian Oil Company. He moved back to Tehran to live, and was in charge of the office.

During one of my trips I spent about two hours with the Deputy Minister of the Interior in Iran, proposing that we do some research with the government to see how they could reinforce many of the unreinforced residential structures that had been built around the Iranian countryside. They are built almost like an igloo, which you might think would be pretty strong.

The trouble is that lacking much wood, the hut-like structures are made mostly of mud, so a number of them fall in every earthquake. The Iranian government almost decided to give us a big contract for something like

\$75,000 to make a study of this problem and see what we could come up with. The aim was to retrofit the structures by spending not more than \$300 each. Well, this project never came off, and they have had damage in several earthquakes since then.

Another project we were involved with was to build a resort for the Shah close to Bandar Abbas, which is down close to the Indian Ocean. We did the architectural, structural and the whole works on that project.

A good friend of one of the Iranian partners came from a family that went back to 800 A.D. They had been one of the largest landowners in Iran, but had turned over most of their lands to the government in exchange for government bonds. The Shah was the only ruler that could ever complete land redistribution. You hear a lot of bad things about the Shah but he was trying to help his people. Granted, the Pahlavi Foundation (owned by the Shah) generally would get a 10 percent cut on things like the Hilton Hotel, the Sheraton, and other foreign-owned facilities.

This landowner had turned all of his land over to the government and received government bonds in exchange. The land was then distributed to the peasants in one-hectare plots, about 2.2 acres. He had about 7,000 acres left near the city of Rascht and proposed to build a new modern city with a major university and turn it over to the government. He would donate the land, and the government could build the buildings. But he would do the planning, and his friends would do the construction and so on. It was a good deal for the government, so we went up to look at the area.

We were up on the mountainside, and I asked him, "How much land did you own?" He said, "Well, look around 360 degrees. You see all those green valleys out there, all those rivers? That was all my family's land." I said, "Well, you must be bothered by this." He said, "At first I was, but now I am not. As the landowner I was responsible for every need of all the people who lived on my land, for housing, clothing, food, medicine, retirement, you name it. I was responsible for everything. When you have a lot of land, you have a lot of people and it is a lot of responsibility. I tried to keep track of all this but it can become very difficult with minimum communications, so I am really relieved." Then I asked him a rather impertinent question, "Do you expect ever to get paid for the bonds?" He said, "Well..."

**Sharpe:** The landowner and our firm hired some architects to lay out a conceptual master plan for the new city. We were going to call it Farah City after the Empress Farah. We had an appointment with the Shah, and went to his office and waited and waited. Then finally his Secretary said he would meet with us instead because the Shah and the Empress were getting ready for a trip to Canada. The Secretary was an older gentleman and a pretty powerful individual. He was the number one person next to the Shah. We explained the whole plan and showed him the drawings and everything. He thought we might get favorable consideration.

As part of this process, the secretary to the Secretary was Madame Hoveyda, sister of the Prime Minister, Mr. Hoveyda. If you recall, Hoveyda was shot when Khomeini took over. During the 45 minutes to an hour or so when we were waiting to see the Shah's

secretary, Mrs. Hoveyda made me Turkish coffee, only she said it was not Turkish coffee, it was Persian coffee. She said, “There is a difference,” but I thought it was the same. You know, the coffee is poured into a little cup. When I finished, she put the saucer on top and turned the cup over, and then told my fortune with the grounds that leaked down. Well, when things went downhill with the Shah, we never did go ahead with the city. That was about 1973 or 1974.

**Scott:** What was the sequence?

**Sharpe:** We opened the office in 1970 and built up a pretty good engineering staff of 25 to 30 people. In late 1977, the partners called me from Tehran and arranged for a meeting with me in Palo Alto a couple of weeks later. I had left Blume, as I can describe later, but the Iranians would always call me for advice. At the meeting they told me they wanted to close up the office because “work was getting harder to get, fees were harder to collect, etc.” I told them they were the controlling entity and thus could go ahead. I wondered about their reasons but they were knowledgeable people.

The Shah was deposed some months later. Shortly afterward I met with the ex-partners when they were in Palo Alto and I asked if they had closed the Tehran office in anticipation of the Shah being ousted. They nodded and said the Shah had made a few fatal mistakes. First, he let Khomeini return from exile in France. Also, the Bazaar Merchants controlled about 80 percent of the commerce in Iran, and the Shah had been encouraging foreign firms to establish plants or facilities in Iran. The Bazaar Merchants became worried that foreigners would take over much of the business in Iran.

Khomeini and his people approached the Bazaar Merchants and said “Come with us and we will take care of you.” Once the Shah lost support of the business community, the end was in sight. The partners recognized the situation, closed the office, sold much of their other property, transferred the money to the U.S., and then moved to California. I have always wondered how our CIA let Khomeini’s people take over all of the military weapons and latest fighter planes with state-of-art weaponry and instruction books, when ordinary people could see what was happening and made appropriate plans for exiting.

## Developing a Piping Analysis Program

**Sharpe:** Around 1971, while I was still at Blume, I felt that we should come up with a piping analysis program that would be applicable to nuclear power plants and that would enable us to make checks at different steps in the analysis. I contacted Professor Graham Powell at UC Berkeley who had developed PIPEDYN, probably the first dynamic analysis program for piping systems. We purchased the PIPEDYN program, which was then used as a basis for developing a more comprehensive piping analysis program with the acronym PIPESD.

This was the first real piping analysis program that did the analysis and answered just about all the questions. The final printout could be used directly in the formal reports required for licensing approval. I’ll say this, I had some real bright guys and they put many different requirements together into a complex computer program that could be readily used

by piping analysts. Control Data Corporation, CDC, cooperated, giving us three hours of free computer time at \$1,000 per hour—a lot in those days—with the understanding that we would license the finished program to them. So the Blume Company then licensed the PIPESD program to CDC on a royalty basis. With PIPESD, Control Data cornered about 80

percent of the piping analysis computer time used in the world. Almost all piping analyses for nuclear power plants were being done using PIPESD. PIPESD is still a popular program, and in its heyday, the Blume firm was receiving between \$25,000 and \$30,000 royalty income per month from the program.

# Blume Merger with URS and My Departure

*I knew that to expand the company, we needed to keep good people. Over a number of months I developed a plan to have some of us gradually step aside into a senior consultant's role and let the younger guys move up in the firm.*

**Sharpe:** In 1957 Blume promoted four of us to be associates in the firm, and we were able to buy a small percentage of the company stock. I was at SLAC from 1961 to 1966. Later, in 1966 I was asked by the other associates to be the chief negotiator to see if we could work out a stock buy-sell agreement with Blume. I had an attorney assisting me. Every time I thought we had an agreement worked out, John would change his mind. Finally I told John, "I guess it is hopeless." In short, although we had stock, we could not work out an arrangement to put a value on the stock or for a buy-sell agreement.

Then the URS corporation approached John in 1970, and after considerable negotiations, he decided to sell. He called us in and said, "Ok, you guys are stockholders, you should vote on it." I voted "No," and the others voted "Yes." John said to me, "Well, you are a very minority shareholder, now that you have made your point you might as well vote "Yes," so we have it unanimous." So I did.

We merged with URS in 1971. At the time John was the President at Blume, and I was Executive Vice President and General Manager, running the office. URS realized that they did not want top people in Blume to leave after they merged, so they wanted John and me to sign five-year employment contracts. I believe Joe Nicoletti was to sign a one-year contract.

At any rate, I had to sign an employment contract. They had wanted a five year contract, but my attorney said, "You are a goddamn fool. Look, don't sign anything. Employment agreements are one-sided, they are all for the benefit of the employer." But I finally got pressured into signing a two-year employment contract for a fixed salary. Well, the years 1971, 1972, 1973 and thereabouts were years of high inflation during which I was getting a fixed salary.

Meanwhile, because of the inflation, other people in the firm were getting raises every four to six months. Their compensation was going up at a rate of 7 or 8 or 10 percent a year. I went to John and said, "How about me?" He said, "Well, you signed a contract, you get a fixed salary." I then went to URS, and they said, "You signed a contract, you get a fixed salary." I said, "Well, now that really bums me, I am giving everybody else increases but not

myself." When my contract was up at the end of the two years, they said, "We would like to work out a deal with you." I replied, "I am not sure I want to work out a deal with you people, because you certainly stuck it to me over the past two years."

Meanwhile, almost two years after the merger, I started working on a reorganization plan for the Blume office. I did that mainly because I wanted to set up the office on a more democratic basis. I had been with John for 23 years and was running the office. As the firm was then organized, I was the Executive Vice President and General Manager, Joe Nicoletti was the Senior Vice President, and Don Teixeira and Jim Keith were Vice Presidents. We were all in our early- or mid-50s.

I recommended that we make some rather drastic changes in organizational structure over several years so that many of the bright young guys we had working there would feel that they had a future in the company. The Blume office was considered the top post-grad office in the western United States, the best place to come and work for two or three years after graduation or early in one's career. Consequently, we had our pick of people and had a lot of bright engineers who had come to work at the firm. But when those younger ones could not see themselves being able to move up in the firm, they would decide to leave.

I knew that to expand the company, we needed to keep good people. Over a number of months I developed a plan to have some of us gradually step aside into a senior consultant's role and let the younger guys move up in the firm. I tried to convince John of the merits of my plan, but he seemed to see this as something being done to

push him out the door. I really was not trying to do that. I wanted John to be Chairman, but also wanted to provide for a more orderly progression so the younger people could see the benefits of staying with the firm.

This is how you make a company long-lived. You have to bring the young people up to take over. My plan was that after a time people like Nicoletti and Teixeira and I would step aside to become senior consultants, and the younger guys would be running the firm. We would have continued to work full-time, but would not have had the operational and administrative responsibilities.

In 1973, some three or four months after my two-year contract had expired, I passed my proposed reorganization plan on to John, and he turned it down. He did not like my plan to let the younger people step up into management and have the senior people move aside.

**Scott:** He was reluctant to have authority delegated?

**Sharpe:** Yes. I used to have big arguments with him.

**Scott:** What did you do when he turned your reorganization plan down?

**Sharpe:** I told John that I would go to the Rome for the Sixth World Conference on Earthquake Engineering being held in the summer of 1973 and then on to Tehran because my quarterly trip there was due, and I would be there a couple of weeks. Then I would take a couple of days' vacation, and be back at the office in about 30 days. I told him, "Meanwhile you can think it over. If you don't want to reorganize the firm, I think I should leave."

URS called and tried to convince me to not be hasty. I told them "You people brushed me off for the last two years, and treated me that way even though I was responsible for up to 115 people. You did not feel I merited an increase in salary, and I told you then that a time would come. That's why I refuse to work out a contract with you people again, because it is a one-way street." They said, "Well, don't worry about it, we will talk to John. You enjoy your vacation, but don't plan on quitting."

### **Trip to Europe: Rome and Frankfurt**

**Sharpe:** My wife Jane and I took off for Rome and Tehran. We went to Rome and thoroughly enjoyed the conference. While there, some Germans whom I had worked with in 1970, before I had traveled to Tehran, came to see me. In 1970 I had been invited to Germany for a week to talk to their engineers about dynamic analysis of nuclear power plants, developing seismic criteria, and the whole ball of wax. I took Gary Kost with me from the Blume office, and between the two of us, we lectured for four and a half days. These people were from Siemen's Kraftwerk Union and AEG, the two German firms who were manufacturing and building nuclear power plants. This was when the Germans were first looking at nuclear power plants and wanted someone to talk to them about seismic problems.

I had never heard anything more from the Germans since 1970. But in Rome at the World Conference a couple of the Germans came up to me and said, "Hey, we liked the presentation you made to us in 1970. Would you come up to

Frankfurt and talk to our management about what you told us two-and-a-half years ago?" I said, "Well, I plan to be on vacation." They said, "We only need you for a day." So I finally agreed to cut our vacation in Italy short by two days and fly up to Frankfurt.

I started to talk to the Germans at 9:00 o'clock in the morning and after an hour-and-a-half break for lunch, I finished at 7:30 that night. This was all impromptu. I'd like to get a transcript of my talk. I didn't have any notes or anything else with me, but talked for a full day to several of their managers and directors. This was all done from the top of the head, but I think I gave them a good presentation. Anyway, the Germans were very impressed, and they all took copious notes.

## On To Tehran, and Back to San Francisco

**Sharpe:** After the meeting in Germany, Jane and I went on to Iran for my quarterly visit to John A. Blume Associates Engineers, Iran. I still had business to do in Tehran, but Jane was worried about the children, [Douglas, Deborah, and Sheryl] so she left Iran a few days early and came home.

Then, the evening before I was to return home, I went to dinner with the manager and his wife and had my passport and airplane tickets with me. I left them at the restaurant, and it was a frantic process to get a hold of the restaurant after it had closed to catch my flight the next morning. I ended up being driven in a jeep out to the Swissair plane, but it had closed its doors and wouldn't re-open them, so I ended up with a trip taking 39 hours. In Saudi Arabia the guards would search everything, including

your body. I later discovered they had stolen a few items.

The next day after I got back, I went in to see John. He said, "I am sorry but I just cannot accept the reorganization plan." I said "Well, John we might as well shake hands and part friends." He didn't believe that I was going. I had made up my mind I was either going to break the mold or not. So I said, "Why don't we set the date for me to leave a month from now?" This was about the first of August. "That will give you time to work out things."

I got more phone calls in the next couple of days, "Why did you get yourself in the corner?" I said, "I did not get myself in a corner. I made a perfectly good plan, but John was not willing to go along with it, and I said the hell with it. I was also disturbed by the way you URS people handled my contract."

The URS chairman of the board took me to breakfast and said, "Look, if you would want to be president of our office in Seattle, or the office in Denver, we will make you president of any one of these you want." I said, "Well, I appreciate it, but I said I don't care to work for you people." He had a hard time with that. He was an accounting type, a financial type. There is a difference between that and being a professional engineer. The president of URS also came, and everybody was twisting my arm, "Look, if you don't like John, we will set you up as president of any of our other subsidiaries." I said, "I am not interested." They could not understand that.

I must say that both in working for Blume, and in my later career, I have been fortunate to be associated with a bunch of bright people.

John Blume was one of the best engineers and one of the smartest ones I have ever run into. I learned a lot from him. He was also one of the best businessmen, and one of the best salesmen. The problem with his businessman side was that he was not willing to give credit to some of the people who were helping him build the business. But I have no complaints. During my career, I have been fortunate in associating with the right people at the right time, and I still have tremendous respect for John.



# The Applied Technology Council

*The SEAOC board appointed a three-person committee, Steve Johnston, John Wiggins, and me, to advise on setting up a separate organization to speed up the implementation of research. That turned out to be ATC.*

**Scott:** When you left Blume, did you have a pretty good idea of what you would do next?

**Sharpe:** No, I was just going to be a consultant, so I set myself up as a one-person consulting office. Very soon, however, I got involved in two enterprises. One was managing the Applied Technology Council (ATC). The other was setting up an engineering firm, Engineering Decision Analysis Corporation, that we gave the acronym EDAC. I will discuss ATC first.

**Sharpe:** I was involved in ATC from its very beginning. I had been talking with Chuck Thiel, then of the National Science Foundation (NSF), about getting together a large group of engineers and researchers, plus some architects and building officials. The

object was to develop nationally applicable provisions for seismic design. In 1970–71 an ad hoc committee of SEAOC explored the idea of an organization to help speed up the transfer of research information into practice. It concluded that we should take a new approach to the codes, and that they should be rewritten.

The SEAOC board appointed a three-person committee—Steve Johnston, John Wiggins, and me—to advise on setting up a separate organization to speed up the implementation of research. That turned out to be ATC. We had a number of meetings and came up with a proposal that SEAOC set up an organization that should be separate from SEAOC and its normal committees. We hoped that this organization would be able to obtain funding from NSF and/or other governmental agencies, which would enable us to expedite the SEAOC committee process of turning the results of research into something useful for the practice of engineering. Something in the form of the building code, design guidelines, or something else.

**Scott:** This was the very beginning of ATC?

**Sharpe:** Yes, this was the genesis of ATC. Our three-person committee made its recommendations, and the SEAOC Board adopted them in late 1971, agreeing to put up \$5,000 as seed money. In those days \$5,000 was a lot of money. We did not have any official organization then, so Bill Giles, Executive Secretary of SEAONC, was handling the administrative work. A lawyer was retained to write bylaws and incorporate ATC. The SEAOC Board appointed a Board of Directors, and a few months later, the National Bureau of Standards (NBS), with funding from NSF, announced

the first workshop on Building Practices for Disaster Mitigation.

SEAOC was asked to arrange for several background papers, and SEAOC gave the request to the SEAOC Seismology Committee and ATC to respond. The first project was largely coordinated by the Seismology Committee, of which I was chairman at the time. ATC received funding for preparing background papers and for travel expenses for the authors to present the papers in Boulder. Later on ATC named these papers ATC-1. Clarkson Pinkham prepared a paper on “Procedures and Criteria for Earthquake-Resistant Design,” Neville Donovan wrote a paper on “Earthquake Hazards for Buildings,” and I wrote a paper co-authored by Gary Kost and Jim Lord on “Behavior of Structural Systems under Dynamic Loads.” We had a very useful conference in Boulder, and you might say this was the beginning of a new era in code development. The papers were published in the workshop proceedings.<sup>5</sup>

Then another project came along, which became ATC-2. It was largely put together by Bill Giles, with assistance from me and a number of others. It was funded by the Bureau of Standards, but with National Science Foundation money that had been transferred to NBS. The purpose of ATC-2 was to evaluate the design of existing buildings, and then redesign them to (1) an elastic threshold, and (2) a collapse threshold. This was one of the

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5. *Building Practices for Disaster Mitigation*, edited by Richard Wright, Samuel Kramer, and Charles Culver, Building Science Series 46, U.S. Department of Commerce, National Bureau of Standards, February 1973.

first major attempts to apply these procedures for building design. The project got started and was going along, but by the summer of 1973 it was starting to stutter and to go way over budget.

In 1971 and 1972 Chuck Thiel and I talked about the idea of developing seismic guidelines that could be used for all of the U.S. In the spring of 1973, I helped rough out a project budget for ATC to conduct such a project. This became what was later known as ATC-3.

## Managing ATC for a Decade

**Sharpe:** In September 1973, at about the time I left Blume and was planning to be a sole consultant, I was talking to Steve Johnson about ATC. He said that they needed somebody to straighten it out. After some discussion, I agreed to head it up. So I ended up managing ATC. I left Blume right after Labor Day, 1973, and took over ATC in October or November. It turned out they did not have any money at the time, so I worked for five months without pay. But I got ATC going on track.

My first job was to get ATC-2 squared away, and my second job was to prepare a detailed proposal for ATC-3 and then negotiate a contract for it. I worked on both projects at about the same time. I brought ATC-2 back into shape and on line; it had been very scattered and screwed up and had been without a manager. I took over the project and got it organized so we could send out drafts of the various parts of the document for review. Then I found that if we spent all of the money that had been committed to our subcontractors, we would exceed the budget. I renegotiated most

of the subcontracts so we would have enough money to cover the project.

I talked to each of the subcontractors and said, “You know, we have only this much money, can we adjust your fee? In fact, since ATC is a non-profit organization, perhaps you could take it off as a charitable contribution.” I will say that all of the more than ten firms cooperated, and I was able to work out a balanced budget. We finally got ATC-2 printed, and it still serves as a reference document, about an inch-and-one-quarter thick. NBS and NSF were quite pleased with the ATC-2 document.

## ATC-3

**Sharpe:** Meanwhile we went ahead with ATC-3. Chuck Thiel and I talked over the amount of effort it might require, and I submitted a budget of \$750,000 for a project taking about three years to complete. At the time it seemed like a big proposal, but in retrospect it was a bargain. NSF transferred the money to NBS, and we agreed on the first half of the contract at about \$375,000.

ATC-3 was to be different from the other code-related efforts, in that nominal payments were to be made to each committee member, in addition to travel expenses being paid to come to the meetings. While a budget of \$750,000 sounded like a lot, there were 85 people and we had 50 meetings or more over the next few years. I must have made 150 phone calls to get those 85 on board. The travel expenses got to be considerable. In fact, a large part of the budget went to expenses. The committee members got paid an average of only about 50 cents an hour for their time spent—maybe a little more, but not much more. They spent a

lot of time on the project, and I take my hat off to them for this.

After the budget was approved, the next job was getting a steering committee for ATC-3 put together. We got the cooperation of the major figures in seismic design in this country—it was really something. The list of people was like a “Who’s Who” in seismic engineering in the United States. The group included Nate Newmark, Bob Whitman, Henry Degenkolb, John Blume, and George Housner. I believe I had a total of 10 or 12 on the steering committee, which was to provide overall guidance in organizing the project, deciding on its scope, and providing suggestions on who should be invited to sit on the various committees.

**Scott:** The 85 participants were organized into the steering committee and the 14 task committees?

**Sharpe:** Yes, we set up an 11-person Executive Panel to review, monitor, and advise the project. The Task Group Coordinating Committee plus the 14 task committees were responsible to the Executive Panel. Chairmen were appointed for each of the task committees. We had a number of kick-off meetings to make sure that everybody was on the right track. With the exception of one or two people, all of the prima donnas cooperated. In fact, even the one or two who gave us little problems when they could not get their way later toned down, changed their minds, and continued to work on the project.

We would have meetings in Los Angeles and San Francisco. In San Francisco, we met at the Clift Hotel. Their room rate was about \$40 per night then; now it must be \$400. They would

give us a nice conference room, and we would set it up in a square. I remember at one meeting counting 44 people around the table. You could look around the table and see a “Who’s Who” in earthquake engineering present. Nate Newmark and I served as co-chairmen, or rather he and I took turns as chairman during the meeting. That was done to sort of give it a little more authority.

We came up with ideas, and there was really a lot of original thinking and input from a lot of people. They were very cooperative. Sometimes we had to make some pretty tight deadlines, and the participants got only a nominal compensation for their time. With 85 people over a four-year period, you can figure it out. Not a lot per person, and yet they put in a lot of time.

ATC-3 required a lot of effort from all 85 participants as we worked over the next four to five years to put the document together. We came up with several internal drafts, and finally in 1976 we got to the final draft that we were willing to send out for review to some 40 different professional organizations like ACI, PCA, ASCE, ASME, ASHRAE, AISI, and ICBO. We gave them about two or three months to submit comments. I received something like 800 pages of comments. In setting up ATC-3, we had made a point of not having any with material interests involved in writing the provisions, but made sure that they would have a chance to comment.

**Scott:** So they took advantage of that opportunity to comment.

**Sharpe:** Yes, they did, and by and large they made good comments. I had the job of

sorting out these comments and transmitting them to each of the fourteen committees, which considered them and came back with responses. We incorporated the ones that we thought were appropriate.

This was late in 1976, and there was a big rush to get it out before the Sixth World Conference on Earthquake Engineering in 1977. We sent out a final draft for a final review by all of the members. I remember things being rather hectic. It was around Christmas, and I had received funding to go to the conference in India in January 1977. As I recall I was going to leave a few days after New Year, so there was a lot of work to make sure it was edited and the final copy sent out for final review. I still consider ATC-3 a landmark document. The Japanese used the 1976 draft of ATC-3 as the basis for their code. While the Japanese code is rearranged differently, basically they used many of the ATC ideas.

**Scott:** Say more about what ATC-3 was trying to accomplish.

**Sharpe:** The big reason behind ATC-3 was to expedite the transfer of research findings into code provisions. There was a lag between research being completed and the results finally being put into a building code. We had this utopian dream: we thought if we put an efficient organization together and involved the best brains, the researchers and the top practitioners, we could short cut the process.

We wanted the product to be really up to date. We were trying to shorten the time lag, the time it takes to go from research into code, to less than five years. It didn't work out that way, but that was the dream. I think if we had not

had all of these somewhat peripheral things, worrying about whatever the professional and trade organizations thought and so forth, we could have done it quicker. But we are a democracy so you cannot do that.

One basic idea behind ATC was to provide the organization some funding, primarily for expenses and in addition, we hoped, to pay people to provide time. We could see a lot of research results coming out of Berkeley and other institutions, such as Illinois, Michigan, and Caltech. These research findings needed to be put into a form that could be used either in building codes, or in documents to help engineers understand the latest technology.

Back in the late 1960s and early 1970s we had estimated that it took a minimum of ten years for the transition from research lab to the building code. Because a number of other things intervened, however, ATC-3 really did not get into code documents until in the 1980s, so it had still taken about ten years from its beginning.

On the other hand, ATC-3 was the genesis of the provisions used in FEMA's design guidelines published as the *NEHRP Seismic Design Provisions*, and the Building Officials and Code Administrators building code and the Southern Building Code Congress International both adopted appendices containing the NEHRP<sup>6</sup> provisions, which were in large

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6. FEMA (1985, 1988, 1991, 1994, 1997, 2000, 2003, 2009 and 2015). *National Earthquake Hazards Reduction Program Recommended Seismic Provisions for New Buildings and Other Structures* (the *NEHRP Seismic Provisions*), Washington, DC. The 2020 edition of the *NEHRP Seismic Provisions* will be the tenth edition.

part the ATC-3 provisions. There have been some changes made by NEHRP, many of them good, but in my opinion some are not so good. I am not completely happy with the process. I can talk about that process and the Building Seismic Safety Council later.

**Scott:** Were you then in effect laying groundwork for what could become a national seismic code or working on something of a national Blue Book?

**Sharpe:** We did not want simply to modify and expand the Blue Book, which was a historic document when it came out, just like ATC-3 was. Because ATC-3 was going to be a nationally applicable document, one of our concerns was to come up with more realistic ground motion guidelines.

**Scott:** Would you explain that?

**Sharpe:** In the Blue Book we depended on damping, overstrength, and the resistance of a lot of nonstructural items to give a building enough strength. That is, in drafting the Blue Book, we implicitly consider these factors and thus use lower seismic forces for design. If we used those low force factors for design in ATC-3, that might mislead people in other parts of the country. They might not realize what the actual earthquake motions in a structure might be.

**Scott:** So one of your main reasons for wanting more realistic ground motion guidelines was concern that people in other parts of the country might not understand this thinking that underlies the Blue Book?

**Sharpe:** Yes.

**Scott:** If you were explaining ATC-3 to a

layperson with a policy orientation, how would you describe what was it trying to accomplish?

**Sharpe:** The major goal was to establish a fresh approach, and to get the results incorporated into the building code in a short time. We had building code people in our group because we wanted to get the building code promulgators' perspective, so when we came up with the document it would have a better chance of being accepted.

We had a lot of meetings and discussions about the code philosophy. I would compile an agenda, after having talked to the people beforehand. Then when we met, the Executive Panel would go through the agenda items. There was a tremendous amount of interaction and input from all the participants. It was interesting that on the practitioner side we had some very strong competitors there. Blume and Degenkolb, for example, were competitors in business, but when it came to this ATC-3 effort, they worked together. Newmark was somewhat of a competitor of John Blume, yet they worked together. I got to know all of these people very well.

**Sharpe:** In looking back at the problems we confronted, you see many things that seemed big at the time, but in retrospect were really not such big problems. I had the problem of getting the Design and Analysis Committees to work together. Henry Degenkolb, Chairman of Design, did not have much use for the analytical types, the dynamic analysis guys, and Anil Chopra, Chairman of Dynamic Analysis, was equally strongminded.

I had to get the two coordinated, so Nate and I would arrange a meeting when Nate was going

to be in town. I would call Henry and then call Anil, asking each of them, “Could you meet with Nate and me on a particular date when Nate is going to be in San Francisco?” I would not tell either of them that the other one was coming, and then they’d each walk into the room. Nate was pretty good at “patting people on the butt” so to speak, and we got them to work together.

These are the little things that happen. We had some other ego problems, but that’s to be expected. Elmer Botsai, an architect, now over at the University of Hawaii, at one of the early meetings stood up and said, “Well, this is the way to do it, and if you don’t do it this way, I don’t want to participate.” Henry said, “Well, cool down, if you don’t want to play, it is nice of you to attend the meeting.”

## ATC Projects: Housing Guidelines for HUD

**Sharpe:** By this time word about ATC was getting around. HUD (the Department of Housing and Urban Development) contacted us and we arranged to meet. They asked if we could help in developing some guidelines for housing. A bunch of houses had been damaged in the San Fernando earthquake in 1971, and the HUD people thought it would be good to have some earthquake guidelines for the design of housing. I prepared a proposal and submitted it to HUD. After receiving a contract, we advertised the project, asked for proposals, and had a selection committee to review the proposals.

The proposals were reviewed and judged on the basis of qualifications, not on the basis of money and the lowest bidder. We selected a

subcontractor to work on this, Ralph Goers, a structural engineer from southern California. Ralph put a lot of work into the effort, but he dictated his document, which caused a problem. I think the draft he finally submitted was close to 1,200 double-spaced pages. I was a little overwhelmed by its volume. Ralph had compiled a lot of material, but some of it was a little verbose.

About that time I had managed to get Roger Scholl as a part-time Technical Director when he was working with Blume, and assigned him the job of cutting the over-long draft back to some semblance of a readable report. Over the next few months, Roger had a lot of conversations with Ralph, and finally got the draft down to something like a 550-page, single-spaced document. That represented a reduction in length of about one-third.

## Seismic Design for Highway Bridges (ATC-6)

**Sharpe:** Then I met Jim Cooper of the Federal Highway Administration, who wondered about ATC setting up a group to come up with a seismic design standard for highway bridges. We submitted a proposal, and it was approved. We then set up an advisory committee and got several subcontractors. Our advisory committee on that project was nationwide. Most of the advisory committee members were also members of AASHTO (American Association of State Highway and Transportation Officials). We had state highway officials from New York, Oklahoma, Idaho, and a number of other states. They were all very cooperative and put in a lot of effort. We came up with ATC-6, *Seismic Design Guidelines for Highway Bridges*, which took

a couple of years to complete. Once we came up with a final document, we made a presentation to an AASHTO regional meeting. Their comments were incorporated and the final document was submitted to AASHTO, where it was adopted.

### Additional ATC Projects and a Change in Management

**Sharpe:** Also there were a number of other projects along the way, and it became evident that ATC was a lot more than a one-person operation. I offered Roger Scholl the Technical Director position, but he was working part-time with URS Blume and did not want to leave there. I told him we needed someone full-time, and then I persuaded Ron Mayes, who was at UC Berkeley, to come aboard as Technical Director. In about 1979, Ron was promoted to Executive Director and I assumed the title of Managing Director.

During those early years, we also conducted the ATC-7, ATC-8, ATC-9, and ATC-10 projects. Along about 1980 or 1981, Ron Mayes indicated that he probably would not stay with ATC too many more months, because he wanted to start his own business. We then went through a search for an Executive Director and had quite a few interviews. We ended up with Chris Rojahn as the first choice, so Chris was appointed Executive Director and I retained the position of Managing Director. I think Chris started about 1981. Then in late 1982 I discussed with the board the fact that I could not continue to spend much time with ATC because I still had my duties with EDAC, the company that I had started. So I stepped aside

to become a senior consultant to ATC, and Chris headed it up.

**Scott:** For about ten years you were pretty much in charge of ATC—from its 1973 initiation to 1982. It was a series of rather intensive committee operations. How would you characterize the experience?

**Sharpe:** The various committee members who were appointed for ATC projects were always very cooperative. When I asked them to give me extra support by studying or coming up with ideas or reviewing things that I had proposed; they always were very timely. Structural engineers, I think, are one of the few professions where people volunteer to devote an awful lot of free time. It is amazing if you look at all of our seismic codes: they have been written by volunteers. For ATC-3 they came up with new ideas, and while there was some payment, they still put in a lot of volunteer time.

**Scott:** So the compensation was pretty nominal for the amount and quality of their time, much below the market value?

**Sharpe:** If they got paid \$5 or \$10 an hour on the average for their time, they would be doing well.

**Scott:** That's pretty nominal.

### Inauguration of the U.S.-Japan Seismic Design Workshops

**Editors' note:** *The Scott interviews with Sharpe ended before they touched on another of Sharpe's significant involvements: in 1981, Sharpe was among those who, with Japanese colleagues, established a series of workshops among U.S. designers (ATC) and*

*Japanese designers (Japan Structural Consultants Association [JSCA]). Sharpe participated in 13 of these workshops, which are now held every two to three years. In several instances, the workshops have included representatives from other countries; in 2016, the workshop series was formally expanded to include New Zealand. The first U.S.-Japan-New Zealand workshop was held in Queenstown, New Zealand in November, 2018, under the sponsorship of ATC, JSCA, the New Zealand Centre of Research Excellence (QuakeCoRE), and the New Zealand Society for Earthquake Engineering (NZSEE).*

*The following is abstracted from a review by Sharpe presented at the 2014 joint U.S.-Japan Workshop on Improvement of Structural Design and Construction Practices in Hawaii.*

**Sharpe:** In 1981, the U.S. National Science Foundation asked three structural engineers -Henry Degenkolb (San Francisco), Roy Johnston (Los Angeles), and me to observe and review the construction and testing of a full-size six-story reinforced concrete structure being designed and built by U.S. and Japanese researchers in Tsukuba as part of the Joint Technical Coordination Committee. The structure was to be tested at the Tsukuba laboratory.

During meetings to discuss the program I had the opportunity to meet professor Masikazu Ozaki. I believe at this time he was deputy director of the Building Research Institute (BRI) in Tsukuba. He had reviewed and commented on a final draft of the ATC-3 document. Ozaki and I decided a joint meeting of U.S. and Japanese representatives to discuss seismic design and construction issues would be helpful to both countries. I then contacted the National Science Foundation and Dr. Jack

Scalzi, an NSF program manager, recognized the merits of such a meeting and provided a \$15k grant to help with expenses.

I arranged the first meeting, which was held in Honolulu on March 13–15, 1984. If I remember correctly, there were about 35 U.S. and Japanese attendees who were leading engineers and researchers. They enjoyed meeting and talking with each other.

After discussions at coffee breaks and lunches, it was unanimously agreed we should hold another meeting in about two years in San Francisco. This is how we established the two-year intervals between meetings. I believe the U.S.-Japan workshops have been very beneficial to everyone involved. The papers presented and the insights shared about major issues have led to many beneficial changes in research and practice.



# The Building Seismic Safety Council

*Chuck Thiel and I were the two who pretty much put BSSC together, after negotiations with the National Bureau of Standards and a few others.*

**Sharpe:** The Building Seismic Safety Council (BSSC) provides the consensus process by which contributions and suggested code improvements from ATC and others are reviewed for inclusion in the National Earthquake Hazards Reduction Program *Seismic Provisions*. Most of the BSSC activities are carried out under FEMA's auspices. BSSC is a branch of the National Institute of Building Sciences (NIBS).

You could say I was one of the founders of the Building Seismic Safety Council back in 1980 to 1982 or thereabouts. Chuck Thiel and I were the two who pretty much put BSSC together, after negotiations with the National Bureau of Standards and a few others. The National Bureau of Standards (NBS) had proposed that the ATC-3 provisions be reviewed in detail before being recommended for adoption.

This proposal upset some of us here in California who had put a lot of effort into ATC-3, but there was not much we could do about it. There was deep concern that, in coming up with a national building code, there would not be enough input from the practitioners. I was talking with Chuck Thiel, who at that time was with the Office of Science and Technology in the Executive Office of the President. Chuck and I organized a meeting of NBS, ASCE, SEAOC, and ATC.

We discussed what organization might be able to undertake this review. We were concerned that if a government agency took the effort under its wings, it would become part of the national building code, and the expertise that went into it would not keep having its say. We saw that SEAOC could not do it because SEAOC is too parochial, being only Californian. We considered EERI, but it only had a few hundred members and had not developed into a really national organization. It was still largely a California organization, although it was just then embarking on an expansion of the membership. Also, such a review would have been really difficult for EERI, which was not set up to do this kind of project. ASCE offered to do it, but ASCE is a rather large group, with 100,000 members or thereabouts, and we did not see it as an organization that could efficiently handle what needed to be done. Too many people would be involved and we were afraid that a big bureaucracy would be set up.

Then Chuck and I came up with the idea of a separate organization. The National Bureau of Standards also had its own proposal, which it appeared the bureau was going to pursue. Chuck and I had some very long discussions,

and finally about 10:00 o'clock one night I called Dick Wright. I believe I called him at home. I said, "Dick, we propose to set up a group that NBS would have input into, and we might call the Building Seismic Safety Council (BSSC). It would be charged with reviewing ATC-3 and handling that effort, and Chuck believes that we could get some funding via FEMA." Well, Dick agreed. I am not sure whether it was a reluctant agreement or not, but at any rate he agreed to our proposal.

### **Significance of NIBS's Umbrella Role**

**Sharpe:** NIBS said, "We will draft the articles of incorporation and bylaws." Also, since BSSC was not yet a going concern and did not have an administrative organization, NIBS offered to provide the administrative services. Looking back afterward, I realized that I missed the significance of this arrangement, and I think Chuck missed it too. As it turned out, although BSSC is still called independent, it became basically a subsidiary of NIBS.

**Scott:** For the lay reader, say a word or two about NIBS.

**Sharpe:** NIBS had been set up as an interface between government and industry with the goal of coming up with standards and guidelines for the buildings industry. Congress funded it for the first five years at decreasing levels of funding. I believe it started with about \$1.5 million per year, and then decreased to zero over about five years. At the end of the five years, it was up to NIBS to find the funding for its activities.

Well, I did not realize, and I guess Chuck did not either, that while providing BSSC's administrative services, NIBS could then charge an overhead on every BSSC contract. Down the road it reached a point where, NIBS being short of money, it used BSSC funds for NIBS business and did not pay the BSSC subcontractors. This almost became a scandal. It was finally worked out for NIBS to repay the money, but BSSC would have a separate bank account, and there would be two signers on all BSSC checks. It just goes to show how things can work out.

**Scott:** Did the NIBS umbrella role have policy implications, in addition to the fiscal-budgetary implications you must mentioned? Did the NIBS role influence the directions that BSSC has taken in its work?

**Sharpe:** At first it did not, but later I think it has had an effect. So yes, I believe NIBS very definitively has had some influence on the policy, although BSSC does have its own Board of Directors. I also think NIBS was interested in handling the administrative function so they could get some overhead money to keep their organization going.

## The Organization of BSSC

**Scott:** Describe how BSSC was organized.

**Sharpe:** We set up a Board of Directors for BSSC, and invited the concrete, timber, steel, and the masonry industries to appoint members. We also needed one member from the public, and NIBS appointed that member, and SEAOC, EERI, ASCE, and one or two other organizations became members.

We set up two mandatory provisions: (1) the

chairman had to be non-aligned, and could not be from one of the materials interests, and (2) BSSC had a "sunset" arrangement under which it would expire after four years. Chuck and I thought if it did not succeed in four years maybe it should be rethought, or killed. But that did not work out, as the sunset provision was later rescinded. I suggested that Bill Moore be the first chairman, and Bill accepted. William W. Moore is a founding partner in the geotechnical engineering firm Dames & Moore. Bill was an excellent chairman. Bill had also been the first president of the ATC board at my urging.

The BSSC process is called a consensus process, but it is primarily a consensus process involving only the members of BSSC. Many of these are representatives of materials industries interests. They of course all have their own ideas as to what is the best way to write a code. Generally they each tend to favor their particular material.

Although BSSC has done some good work, I am concerned, and I think others are also, about too much control by materials interests, whereas those who are most familiar with seismic problems here in California have limited influence. The trade organizations have the money to develop justification and substantiation for code changes. In contrast, the practicing engineers find it difficult to counter some industry proposals, because they must do their homework as a freebie.

There are some 55 to 60 members in BSSC, and most of them are not technical people. They can be swayed by arguments put up by the materials interests. So some good things have come out of BSSC and some that are

not-so-good. I am the Chairman of the Design Subcommittee and am also on what they call the PUC, the Project Update Committee, an overall committee that makes recommendations to the board.

They have taken ATC-3, which was an excellent document that had inputs from the best brains in the country, and made a number of changes. Many of the changes have been accommodations to views from other areas of the country. But I think other changes have been made to favor certain materials a bit, or to be less restrictive on those materials. So you can see that, while BSSC has its good points, it also may have some bad points, or maybe I should say not-so-good points.

On the other hand, the *NEHRP Provisions* are moving ahead. Although in the original ATC-3 we kept telling people, “We are not developing a national building code,” it begins to look [1998] as if eventually we will have made an attempt at a national building code. I have mixed feelings on this.

**Scott:** Explain your mixed feelings a bit, and your comments about the materials representatives.

**Sharpe:** The final product of the code is the design and construction of a building or other structure. A building code is pretty much a guideline, a mandatory guideline, and is not a design manual. The design and performance of a building may vary, depending on the experience of the designer. The design will also vary depending on whether it is designed by an engineer who practices in the East, the Midwest, or the West. The performance of the buildings would also vary.

For example, over the years California engineers have learned a number of practices that they use for detailing. They have developed ways of looking at a structure overall, and an understanding of how a building will perform, that has come from years of designing for seismic forces. We do not have this same level of design experience throughout the country. So if you have a national building code, in order to have roughly equal performance of structures throughout the country, you would need to train all the designers to deal with earthquake forces. There are also a lot of other local differences, such as in the use of certain building materials. Thus they prefer a lot of brick in the East. Brick can be designed to resist earthquake forces, but it takes time and care to build properly, and the masonry contractors need the necessary knowhow.

**Scott:** In other parts of the country, architects and masonry contractors have been doing it their way for a couple of hundred years.

**Sharpe:** Yes. In California it took quite a while for the industry to understand what was required for earthquakes. There was a major drop in masonry construction here, because of the seismic provisions. I am not sure that brick has ever come all the way back.

At least back in those early days of BSSC, brick masonry buildings were not engineered in the eastern U.S. They could build a five- or six-story brick masonry building, with the design drawn up by the architect and no structural engineer would be involved. So when we talked about non-engineered buildings, we had to be careful, because in the East we could be including some rather large structures.

**Scott:** So the very definition of a “non-engineering building” can be substantially different in different parts of the country?

**Sharpe:** Yes. In California, drafters and what are termed building designers are restricted here in the size of building they can design without being licensed in civil or structural engineering or in architecture. Most houses are done that non-engineered way.

### Comparative Tests of Building Designs

**Sharpe:** For a number of years I continued to be involved with BSSC via ATC. We came up with a program to have comparative test designs made of buildings throughout the country, using the ATC-3 provisions. I managed that program, which involved something like 47 or 48 buildings. I helped select the contractors, negotiated the contracts, and reviewed the reports and coordinated all the activities.

We used actual building designs. In Memphis, for example, we asked two firms to design a concrete building of roughly a certain size and shape, according to the local code, redesign it to meet the ATC-3 provisions, and then tell us what the difference in cost would be and what other problems they had. We had this kind of thing done in Seattle, Los Angeles, Phoenix, San Louis, Memphis, Charleston, New York City, Boston and Chicago. I believe (there may have been one or two others). The program took about two or three years to accomplish.

I do remember one comment by a New York engineer who said; “I don’t see the need for designing for earthquakes in this area because

we don’t have earthquakes.” But he also said, “If we had these kinds of provisions in our code, our building designs would be much more reliable because of all the detail, and all the things we would be required to look at in the design.” New York did adopt seismic requirement in about 1996 or 1997.

**Scott:** The New York engineer said their designs would be more reliable if buildings were designed under a seismic code like ATC-3? He presumably meant they would be better buildings for non-seismic purposes, as well as for resisting earthquakes? Do you also think their buildings would be better for non-earthquake purposes?

**Sharpe:** Yes, because the engineers would be looking more carefully at all the design details.

I think BSSC is now pretty much a captive organization of FEMA. I think it gets almost all of its contracts from FEMA, and it does what FEMA wants done. While that is probably working out, there is not always much input from practitioners. Although SEAOC and EERI have representatives on the BSSC Board of Directors, practitioner viewpoints are sometimes a bit diluted in BSSC.

**Scott:** How about the influence of the California engineers and those in the western states? They are still the ones who are more knowledgeable about seismic design problems.

**Sharpe:** That influence is only nominal in BSSC.

**Scott:** What about California practitioner representation in ATC?

**Sharpe:** When ATC started out, SEAOC provided all of the members. ATC now has a

more national membership, and I believe that SEAOC, ATC's founder, has only a majority of the board members. A more national membership was something I pushed for, being convinced that if ATC was going to be successful in the long run, there had to be at least a perception that it was a national organization.

**Scott:** Is California's influence in ATC still considerable?

**Sharpe:** Yes. And ATC's other members are also practicing engineers or researchers.

**Scott:** That is really fundamentally different from BSSC.

## The Uniform Building Code in a National Perspective

**Sharpe:** The International Conference of Building Officials (ICBO) publishes the Uniform Building Code (UBC), which has been used in most areas west of the Mississippi where there are building codes, as well as in a few places east of the Mississippi. Early in the 1990s, ICBO, which had always adopted SEAOC's seismic provisions, asked SEAOC to start looking at things in a more national perspective. You might say they almost threatened SEAOC at the EERI meeting in Salt Lake City in the early 1990s. ICBO approached SEAOC with the request, "Look, you have got to come up with a nationally applicable code, rather than just one for California."

Then the *NEHRP Seismic Provisions* were adopted as appendices to their codes by the Building Officials Conference of America (BOCA), and also by the Southern Building Code Congress International (SBCCI). That caused some problems with ICBO and the

Uniform Building Code (UBC). There were fears that the influence of ICBO and UBC might be supplanted by the *NEHRP Seismic Provisions*.

**Scott:** So ICBO wanted SEAOC to move in the direction of a national code?

**Sharpe:** Yes, SEAOC was being urged to come up with a more nationally applicable code, although SEAOC does not claim national expertise. SEAOC does have the best expertise in seismic design, but of course is more familiar with California. Other engineers in the country tend to feel, "Well, these California guys think they know it all, and we think they don't." Nevertheless California engineers do know more than the other engineers about seismic safety and about how buildings perform in earthquakes. The California engineers have actually been involved in seismic work since back in the 1930s. Also SEAOC got formally involved in the late 1950s, and came out with the first Blue Book in 1959. It has the experience.

## A National Building Code?

**Scott:** So in moving toward a nationally applicable code, there probably needs to be continuing pressure to consider the California vantage point and earthquake experience. Otherwise, isn't there a danger of a future watering-down of the seismic codes?

**Sharpe:** Well, there might be a partial watering down, but maybe not a serious one, although the resulting code may or may not be suitable for California conditions. The Blue Book is still looked at as the earthquake code document, and has been transferred to

the Uniform Building Code almost verbatim. If the UBC loses its influence, then SEAOC would also lose its influence exercised via the Blue Book. I am not sure what the answer will be, but in any event, the different code organizations have finally agreed to publish one code, called the International Building Code, in 2000.

When we were doing ATC-3, we maintained adamantly to everybody that what we were doing would not be a national building code. Then BOCA and Southern Building Code Congress adopted the *NEHRP Seismic Provisions* as appendices. The President signed an Executive Order directing NIST (the National Institute of Standards and Technology, formerly NBS) to come up with seismic design standards for all federal buildings, and the federal government is by far the largest building and land owner in the U.S.

Anyway, all of these actions were tending to push towards a national building code. To sum up, the picture has changed rather drastically in the last 10–15 years. It has gone from disclaiming the idea of a national building code, to being on the verge of having one. [This was in the 1991 interview.<sup>7</sup>]

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7. The 2000 edition of the International Building Code, formed from the unification of the organizations promulgating the Uniform Building Code, the National Building Code, and the Standard Building Code, adopted the 1997 *National Earthquake Hazards Reduction Program Recommended Provisions for Seismic Regulations for New Buildings and Other Structures* in toto, with some re-formatting, to comprise its seismic regulations. The *NEHRP Provisions* are funded and published by FEMA and updated by the Building Seismic Safety Council at three-year intervals.



# The Engineering Decision Analysis Company

*EDAC was a good company, but it had too many prima donnas, including myself.*

**Sharpe:** As I said, a couple of other engineers and I formed a new engineering consulting company, The Engineering Decision Analysis Company (EDAC), at the same time that I got involved in managing ATC.

About three weeks after I left Blume I got a call from Frankfurt. Kraftwerk Union (KWU) said they would like to have me submit a proposal on some work for them. I said I could not, because before I left Blume I had submitted such a proposal on behalf of Blume. They said that if I was not involved, Blume would not get the contract. They sent me a roll of half-size drawings about 15 or 20 inches in diameter. So I got some help from a couple of engineer friends and submitted a proposal. KWU also asked whether I had a business. I said, "Sure I do." So that's how I started EDAC.

**Scott:** Say something more about the formation of EDAC.

**Sharpe:** The acronym EDAC stood for Engineering Decision

Analysis Company. At first we called it Dynamic Analysis Systems, DAS. But das is a common German word and we decided that was not a good choice, so we came up with EDAC.

To make a long story short, about a year later we got this large contract from Kraftwerk Union. Eventually we had to set up an office in Frankfurt.

**Scott:** Describe how you set up EDAC, and why you did it the way you did.

**Sharpe:** When I left Blume I had not intended to set up another company. but then I was approached by the Germans, and submitted the proposal to them. A number of the younger bright people had started leaving Blume, because they thought if Sharpe left, the future must not be there. A couple of them, including Gary Kost, came to me and said they heard I had submitted this proposal. If I got the contract, or if I wanted to start a company, they wished to join me.

It became obvious that we should start a company, so I discussed it with Jack Benjamin. Jack was a professor at Stanford who had retired. Shortly after he retired his wife died, and Jack had become sort of a recluse. Haresh Shah was a good friend, and we were worried about Jack. So we finally got hold of him, and got him to work for EERI as project director on the “Learning From Earthquakes” project.

Then we started talking about forming a company, so Haresh, Jack Benjamin, Gary Kost, and I formed the company. I had been very much disturbed in my earlier career by the fact that John was the majority owner, and the way he would act. So I set some rules. We’ll

go in equal, we will invest the same amount of money each, and we can put this company together. But I would like to set it out that within ten years nobody will own more than 5 percent of the company, because I would like to have the ownership spread amongst the employees. That kind of an arrangement [majority ownership] is very agreeable when the stock is not worth very much, but when a company becomes successful, then people don’t want to let go of their ownership.

After about a year, Haresh Shah came to us and said he could be involved in a conflict. John Blume was going to donate money for the Blume Earthquake Engineering Center at Stanford, and Haresh was going to be the Director of the Center, so he thought that would be a conflict. We reached an agreement with Haresh, and I think we paid him three times what he had invested. It was not a lot, but he still got a good return on his funds.

Then there were just three of us, each owning one-third. I started working on a plan to divest ourselves of the stock. Well, it is very interesting how you have great difficulty trying to get people to sell stock. They were all worried about it being worth more at the time than it really was. Or maybe they were right about the value of the stock.

## Helping the Germans with Their Nuclear Criteria

**Sharpe:** I helped the German nuclear regulatory people set up most of their seismic criteria. I am not sure about their criteria now, but at least through about 1984 the German nuclear power plant criteria were a great deal similar to what we had here in the U.S. I made

some upgrades on a few things. They flew me to Hanover and I made a six-hour presentation to a group of nuclear regulatory people. Each German state has its own regulatory group. I made my presentation in English. The Germans had the paper that I wrote up in English translated into German.

## A State-of-the Art Firm

**Sharpe:** I felt that the strength of the company was going to be the people in it. That is the reason we came up with the name EDAC. Jack Benjamin wanted to make it Sharpe, Benjamin, Kost & Shah, or however he wanted to arrange the names. I told him that I thought there was too much of a personality cult at Blume, and it would be better if we did not have any personal names in the company name. Hopefully the company could go on ad infinitum.

I did not realize till later that Jack was very disturbed by the choice of EDAC as a name, because he wanted to have his name in a company. So in 1979 Jack split off to start his own company as Jack Benjamin & Associates and took several people with him.

We had good people at EDAC. I had a good reputation, and because I had also been a consultant for the AEC for five years reviewing nuclear power plants, I knew what the nuclear power business required. We started writing letters to people and offering our services. The first big contract we got was with Kraftwerk Union, but then we started getting other contracts from different companies here in the U.S., and we built up to 40-plus people, all high-tech and with excellent reputations.

EDAC was a good company, but we had five or six prima donnas, including myself. A couple of them split off in 1979. There is always a problem with prima donnas; they don't think they are getting enough credit. That's a hard one to know how to handle. If I were to go back to school, I think I would also take some psychology. Anyway, EDAC started doing a lot of work.

We learned a number of things. For example, we had the Germans pay us in dollars rather than in Deutsch Marks, because of the fluctuating market. I know at times we could have made money by getting Deutsch Marks, and at other times we would have lost. But we wanted a common denominator so we knew where we were, and most of our costs were in dollars. We had a very good relationship with the Germans, and then about 1980 to 1982, the nuclear business fell off all over the world. I believe no new nuclear plant has been sold in the U.S. since 1979.

We had some excellent people, bright and good thinkers. I also think we were one of the few firms that was really state-of-the-art. We were doing analyses that were about ten years ahead of their time. We considered soils effects (soil-structure interaction), site geology, time of passage of seismic waves through a structure, aircraft impact on a reactor building, explosions, and other esoteric phenomena.

**Scott:** As you built up EDAC, how did you keep the company state-of-the-art?

**Sharpe:** Well, because we were specialists we managed to get enough fee so that we could have people research some of the problems that came up. And then from overhead we would

buy the latest computer programs as soon as they came out of the University of California or wherever. Cal is now the best place for programs, and there were a couple of others. Then we would take the programs apart, we would fully understand them, and then put them back together and make any changes we had to do to make them a production program. We did a lot of that.

## An Innovative Pipe-Snubber Program

**Sharpe:** In 1982 at EDAC we were discussing the need for a program to reduce the number of snubbers on piping systems in nuclear plants, because they were such a heavy maintenance item, on the average costing about \$1,500 to \$2,000 per year per snubber.

**Scott:** What are snubbers?

**Sharpe:** Snubbers act similarly to shock absorbers in a car. The snubbers allow the piping system to grow and move slowly with temperature and pressure. If the piping system is subjected to sudden movement like in an earthquake, the snubbers lock up momentarily and keep the piping from moving, which could cause high stresses. The earlier snubbers were pneumatic or hydraulic and would leak. Then the snubber industry came up with mechanical-type snubbers, and all the tests showed that they were the best answer. There are now many mechanical-type snubbers.

A major problem with snubbers is that there are a lot of cycles of movement in piping systems, because the temperatures and pressures cycle up and down, and so the snubbers work back and forth, and at something like 4,000

cycles, they tend to hang up. There might be 20 or 30 cycles a day up to maybe a hundred cycles of temperature or pressure a day. That's why they were having all the costly maintenance problems. The piping systems have to be allowed to grow and shrink. You cannot make all of the supports fixed points. Many supports have to be a type that will move to allow this slow motion, but would stiffen up when they are subjected to a sudden earthquake force.

**Scott:** What is the order of magnitude of the maintenance costs?

**Sharpe:** I believe for example the two units at Diablo Canyon have about 750 snubbers, which means a cost of from \$10,000,000 to \$15,000,000 per year, just for their maintenance. We felt there were too many snubbers on most piping systems in most plants. We thought that by using the latest theories, and better analysis with better models and so forth, we could come up with something that would help. So some of my bright people and I sat down, and they said we could develop a completely new program for about \$50,000. We actually spent at least \$250,000 to develop this program, just in salary time and computer time, not including overhead. We came up with two programs that still are state-of-the-art.

One of the programs was "Opt Pipe," which determined optimum location and number of snubbers for piping systems. We used the latest NRC regulatory requirements, the latest theories, and developed an entirely new approach for piping analysis. It took a lot of effort to work out all of the bugs. But we got the program to work, and then got it approved by the NRC. We beat out Bechtel and Sargent Lundy, and other companies that also were

trying to develop computer programs for reducing the required number of snubbers on piping systems.

We were invited to participate in a research/study program with Commonwealth Edison in Illinois, being one of four firms selected. We were the only firm among the four that was able to complete the analysis requirements of the program. The others said there was something wrong with our program. Then Commonwealth Edison retained an independent reviewer. We did not want to show him all the details of the program, but he made the checks that he could, and then we would run the same problem. Finally he said, "What they have done is right." We were deleting as high as 90 percent of the snubbers on some piping systems, so if you had 750 snubbers, you could drop off almost 700 of them.

**Scott:** So your program was basically to figure out how many snubbers were needed and where they needed to be?

**Sharpe:** Yes, to determine how many snubbers were needed for each piping system to resist the specified earthquake forces.

## Success Led to EDAC's Sale

**Sharpe:** The pipe snubber program's success led to the sale of EDAC.

**Scott:** How did that happen?

**Sharpe:** While we had the program, we could not exploit it because the nuclear power plant operators wanted the analyses and redesign of the replacement for the snubbers done on an expedited basis. They did not want to shut down a plant for any longer than the

time required to complete other shutdown maintenance. So you had to do all this work in about five months—maybe \$3,000,000 worth of work. That's a lot of work.

The issue came to a head a few months after we did our snubber study for Commonwealth Edison, when they planned to ask for proposals to have the piping systems in two of their plants reanalyzed and modifications designed to remove redundant snubbers. To do the work on an expedited schedule, so the modification work could be performed during plant shut down for maintenance, I would have had to hire about 35 or 40 people just for the analyses.

That would be a big risk, because I would also need to hire a few experienced supervisors to direct the new people. Then Nutech approached us about a joint venture on the proposed contracts. They had a large staff with experience in designing piping modifications. So we negotiated. We wanted to exercise technical control of the analyses because we had developed and understood the Opt Pipe program in detail. But Nutech also wanted control, so we came to an impasse.

After a few days, Nutech made a proposal to EDAC to buy most of its assets, and hire one or two key EDAC personnel. The main item that Nutech needed was the Opt Pipe Program, and also another state-of-the-art program we had developed for nonlinear analysis of piping systems called NonPipe. After some quite intensive negotiations over the next few weeks, we arrived at a purchase agreement. Nutech was very interested in buying because, within a month after completing the purchase, they got two contracts of about \$3,000,000 apiece.

The main factor in all of this action was that EDAC could not handle such large contracts on the required expedited schedule. That is why I agreed to sell: I did not think we could handle the work. In late 1987 EDAC was sold to two companies. The nuclear portion was sold to Nutech in San Jose. The building design/analysis portion was sold to Forell/Elsesser in San Francisco.

Anyway we sold EDAC, where we had done a lot of interesting state-of-the-art work. I have formally been an independent consultant since October 1987, although I had already started being a consultant before that, because EDAC did not require all of my time. All of those other bright people went on to greener pastures; some started their own businesses; others got high management positions with various firms.

# Epilogue

Roland Leonard Sharpe died on March 15, 2018, at the age of 94. As noted in the Foreword, the last interview between Sharpe and Stanley Scott took place in 1998, two decades earlier. Stanley Scott died in 2002 and he never finished the final interview he had planned with Sharpe. As with some other manuscripts that were left unfinished but are substantial enough to be included in the EERI Oral History Series, this volume has been edited for publication after both the interviewee and interviewer were deceased. The 2017 John F. Meehan volume is an example, and others are planned for the future to bring valuable historical resources to light.

During that last part of Sharpe's life, he continued his consulting work, and among the honors he was awarded are these: Earthquake Engineering Research Institute Honorary Member, American Society of Civil Engineers Distinguished Member, Structural Engineers Association of California Honorary College of Fellows, Structural Engineers Association of Northern California Honorary Member, Japan Structural Consultants Association Honorary Member, Applied Technology Council Award of Excellence for Extraordinary Achievement in Seismic Design of New Buildings.





# Photographs of Roland Sharpe



*Sharpe's senior photo in the 1941 yearbook from  
Mechanic Arts High School, St. Paul, Minnesota.  
He was a member of the Physics Club.*



*Third Joint Technical Coordination Committee for the U.S.-Japan Large-Scale Testing Program, funded by the National Science Foundation and the Japanese Ministry of Construction, in 1983.*

*Left to right, front row*

*Gene Corley [Portland Cement Association], Japanese name [Director, Building Research Institute], Bob Hanson [U of Michigan], Hajime Umemura [U of Tokyo], Jack Scalzi [National Science Foundation], Joe Penzien [UC Berkeley], Roy Johnston [Brandow and Johnston, LA], Tsuneo Okada [U of Tokyo], Roland Sharpe [ATC and EDAC]*

*Left to right, second row*

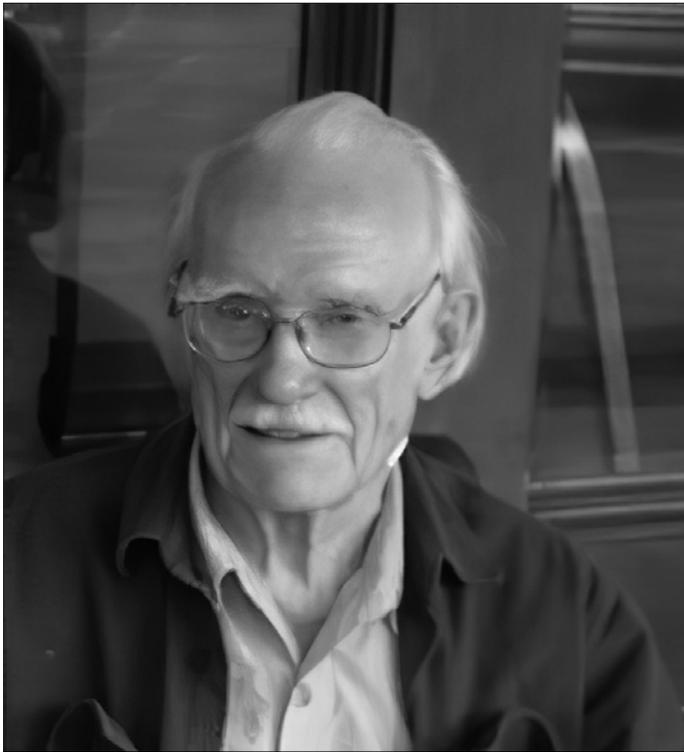
*Makoto Watabe [BRI, Tsukuba], Helmut Krawinkler [Stanford U], Steve Mahin [UC Berkeley], Henry Degenkolb [Degenkolb Engineers, SF], Paul Jennings [Caltech], Hiro Aoyama [U of Tokyo], Le-Wu Lu [Lehigh U], Mete Sozen [U of Illinois], Ben Kato [U of Tokyo]*

*Left to right, third row*

*K. Takanashi [Kyoto U], Japanese colleague [structural engineer], Japanese colleague, Jim Jirsa [U of Texas], Japanese colleague*



*Sharpe and his wife, Jane, at a conference in Maui in 2012.*



*Roland Sharpe in 2014.*



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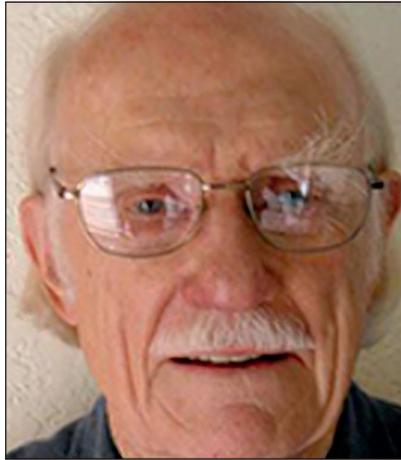
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**T**HIS VOLUME in the EERI Oral History Series presents the life and career of Roland (Rol) Sharpe (1923–2018).

After serving in the U.S. Marine Corps in World War II, Sharpe completed his bachelor's and master's degrees in civil engineering at the University of Michigan.

His first job was with the firm of John A. Blume in San Francisco, where he worked for 23 years. The firm was doing innovative projects, and Sharpe had a chance to work on several unique ones: the large wind tunnel at Moffett Field in Santa Clara County, California; a 15-story building in San Francisco with an unusual column design; field experiments with the dynamic properties of school buildings; and the Stanford Linear Accelerator (SLAC), a concrete tunnel over a mile long that had to take the curvature of the earth into account because of its precision.

Beginning in the late 1950s, he took a lead role in the Blume firm for the seismic analysis of nearly 20 nuclear power plants. His other projects included sonic boom impact testing for the U.S. Air Force, and establishing a Blume satellite office in Tehran, Iran, that worked on major infrastructure projects until the Islamic Revolution of 1979.

He was the first executive director of the Applied Technology Council and project manager for the very influential ATC-3 project that has formed the basis for seismic codes since its publication in 1978. Subsequently, he was one of the founders of the Building Seismic Safety Council.

