Regional Oral History Office
The Bancroft Library

University of California
Berkeley, California

University History Series

Robert L. Wiegel

COASTAL ENGINEERING: RESEARCH, CONSULTING, AND TEACHING, 1946-1997

With Introductions by
Rodney J. Sobey
and
Orville Magoon

An Interview Conducted by
Eleanor Swent
in 1997

Underwritten by
The U.S. Army Corps of Engineers

Since 1954 the Regional Oral History Office has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the Nation. Oral history is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is indexed, bound with photographs and illustrative materials, and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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Oakland childhood, UC Berkeley, 1940-1943; ROTC experience, Army Ordnance officer, England, France; graduate studies in coastal and ocean engineering at UCB: wave and beach research, tsunamis, risk analysis, turbulent motion; thoughts on synergy of sharing ideas, value of publication, advising and mentoring graduate students; UC Berkeley College of Engineering teaching and governance, 1957-1987: acting dean, 1972-1973, work with State Coordinating Council for Higher Education, State Technical Services Program from 1965 to 1968, and technology transfer; consulting and public service: Namibian diamond project, Hilo tsunami protection, Alaska beach nourishment, Nile Delta, Papua New Guinea port construction, Sines (Portugal) breakwater, Strait of Hormuz, Mica Dam, oil platforms in North Sea and Gulf of Mexico, pipelines in Patagonia and Alaska, Manfredonia (Italy) breakwater; contribution to California Advisory Commission on Marine and Coastal Resources, Office of Naval Research Natural Hazards Review Board, Coastal Engineering Research Board.

Introductions by Rodney J. Sobey, Professor of Civil and Environmental Engineering, UC Berkeley; Orville Magoon, Vice Chairman, Coastal Engineering Research Council of the American Society of Civil Engineers, and President, Coastal Zone Foundation.

Interviewed 1997 by Eleanor Swent for University History series. The Regional Oral History Office, The Bancroft Library, University of California, Berkeley.

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PREFACE

When President Robert Gordon Sproul proposed that the Regents of the University of California establish a Regional Oral History Office, he was eager to have the office document both the University's history and its impact on the state. The Regents established the office in 1954, "to tape record the memoirs of persons who have contributed significantly to the history of California and the West," thus embracing President Sproul's vision and expanding its scope.

Administratively, the new program at Berkeley was placed within the library, but the budget line was direct to the Office of the President. An Academic Senate committee served as executive. In the four decades that have followed, the program has grown in scope and personnel, and the office has taken its place as a division of The Bancroft Library, the University's manuscript and rare books library. The essential purpose of the Regional Oral History Office, however, remains the same: to document the movers and shakers of California and the West, and to give special attention to those who have strong and continuing links to the University of California.

The Regional Oral History Office at Berkeley is the oldest oral history program within the University system, and the University History Series is the Regional Oral History Office's longest established and most diverse series of memoirs. This series documents the institutional history of the University, through memoirs with leading professors and administrators. At the same time, by tracing the contributions of graduates, faculty members, officers, and staff to a broad array of economic, social, and political institutions, it provides a record of the impact of the University on the wider community of state and nation.

The oral history approach captures the flavor of incidents, events, and personalities and provides details that formal records cannot reach. For faculty, staff, and alumni, these memoirs serve as reminders of the work of predecessors and foster a sense of responsibility toward those who will join the University in years to come. Thus, they bind together University participants from many of eras and specialties, reminding them of interests in common. For those who are interviewed, the memoirs present a chance to express perceptions about the University, its role and lasting influences, and to offer their own legacy of memories to the University itself.

The University History Series over the years has enjoyed financial support from a variety of sources. These include alumni groups and individuals, campus departments, administrative units, and special groups as well as grants and private gifts. For instance, the Women's Faculty Club supported a series on the club and its members in order to preserve insights into the role of women on campus. The Alumni Association supported a number of interviews, including those with Ida Sproul, wife of the President, and athletic coaches Clint Evans and Brutus Hamilton.

Their own academic units, often supplemented with contributions from colleagues, have contributed for memoirs with Dean Ewald T. Grether, Business Administration; Professor Garff Wilson, Public Ceremonies; Deans Morrough P. O'Brien and John Whinnery, Engineering; and Dean Milton Stern, UC Extension. The Office of the Berkeley Chancellor has supported oral history memoirs with Chancellors Edward W. Strong and Albert H. Bowker.

To illustrate the University/community connection, many memoirs of important University figures have in turn inspired, enriched, or grown out of broader series documenting a variety of significant California issues. For example, the Water Resources Center-sponsored interviews of Professors Percy H. McGaughey, Sidney T. Harding, and Wilfred Langelier have led to an ongoing series of oral histories on California water issues. The California Wine Industry Series originated with an interview of University enologist William V. Cruess and now has grown to a fifty-nine-interview series of California's premier winemakers. California Democratic Committeewoman Elinor Heller was interviewed in a series on California Women Political Leaders, with support from the National Endowment for the Humanities; her oral history was expanded to include an extensive discussion of her years as a Regent of the University through interviews funded by her family's gift to The Bancroft Library.

To further the documentation of the University's impact on state and nation, Berkeley's Class of 1931, as their class gift on the occasion of their fiftieth anniversary, endowed an oral history series titled "The University of California, Source of Community Leaders." The series reflects President Sproul's vision by recording the contributions of the University's alumni, faculty members and administrators. The first oral history focused on President Sproul himself. Interviews with thirty-four key individuals dealt with his career from student years in the early 1900s through his term as the University's eleventh President, from 1930-1958.

Gifts such as these allow the Regional Oral History Office to continue to document the life of the University and its link with its community. Through these oral history interviews, the University keeps its own history alive, along with the flavor of irreplaceable personal memories, experiences, and perceptions. A full list of completed memoirs and those in process in the series is included following the index of this volume.

September 1994
Regional Oral History Office
University of California
Berkeley, California

Harriet Nathan, Series Director
University History Series

Willa K. Baum, Division Head
Regional Oral History Office

INTRODUCTION by Rodney J. Sobey

Robert Wiegel's international reputation as a pioneering researcher and prime mover in coastal engineering has been well established since the 1960s. It would have been very easy for him to live off this reputation for the balance of his career. But that is not Robert Wiegel.

He has always taken his commitments to his family, to his profession, and to his university very completely and very seriously. Much of this is public record. More still is compellingly documented in this oral history. Parts are known only to those privileged to know him well.

Robert Wiegel's interests are catholic in the extreme, and belie the popular image of an engineering professor. He has always been concerned with the entire spectrum of engineering--research, instruction, investigation, environmental interaction, design, construction, and communication.

Bob always recognized the importance of the written word. Beginning in the 1950s, he developed and personally maintained a library, the "Wiegel Ocean Engineering Archive." It is housed in the Water Resources Center Archives on the Berkeley campus with the status of a reserve collection. It is a unique collection with a very Robert Wiegel personality. It has a wealth of historical documents from the early years of coastal engineering, together with numerous extracts from the quality press and the scientific press on aspects of coastal and ocean engineering. It also includes a lot of the gray literature, unpublished reports with limited distribution lists.

Bob has always been an avid consumer of quality newspapers, from all over the country. His particular interest is in the reporting of aspects of coastal engineering and natural hazards, but his concerns have never been restricted to just coastal engineering. He has been an equally avid reader of the scientific press. He is very well read on the "pre-history" of coastal engineering, with particular interest in Mediterranean harbors in biblical and Roman times.

His legacy to his students is considerable and largely untold. To his students, he was always patient and encouraging. He was always available and his open door has been legendary in O'Brien Hall. It has continued to this day, a decade after his "official" retirement. His teaching was committed and enthusiastic. He has continued to contribute to coastal engineering graduate classes, giving today's students a glimpse of his magic. The place of a focused discussion in the entire spectrum of engineering is always a secondary theme in his classes. He communicates much of this through his stories and through commentaries drawn from the current press. During his active teaching, his passion for the written word and the value of library research were central themes in his graduate teaching. His insistence on written term papers on a focused topic was and still is unusual in engineering. He recognized writing and library research skills as perennial weaknesses of engineering education.

His legacy to close academic colleagues is an issue to which I can personally relate. I came to Berkeley because of Robert Wiegel. His international reputation was established and intimidating when I first met him. Robert Wiegel as a mentor and a colleague has been a joy. He has set an example of academic commitment that I have always viewed as the goal. Students were always his first concern. Canceling class or office hours was never an option. His busy schedule of national academic and professional committee assignments was always scheduled around his classes. Overnight "red-eye" flights to and from the East Coast were a standard practice. His insatiable reading has been a continuing source of inspiration, support, and encouragement. Despite a ballooning literature, he has remained exceptionally well informed. A week does not pass without at least one item of "compulsory reading" in my campus mail box from Bob Wiegel. He has been a consistent source of calm rational advice, encouragement, and moral support. He has always been there for me. Thank you, Bob.

In my travels, it has always been a pleasure to be identified as a colleague of Bob Wiegel. The genuine admiration and affection for the man who has gone before me has been a continuing source of pleasure. Apart from his expertise in coastal engineering, the message I hear every time is of Robert Wiegel, the gentleman.

I would like to acknowledge my co-conspirators in making this oral history happen. The planning was initiated together with Linda Vida (Librarian, Water Resources Center Archives, Berkeley). Financial support was provided through Robert Whalin (Director, Waterways Experiment Station, Vicksburg). Steven Hughes guided the project through the Vicksburg bureaucracy. Mabel Iwamoto and Orville Magoon assisted with background information. The interviews were planned, completed, and edited by Eleanor Swent (Regional Oral History Office, Berkeley) with considerable assistance from Bob Wiegel.

Rodney J. Sobey
Professor of Civil and Environmental Engineering, University of California, Berkeley

September, 1997
Berkeley, California

INTRODUCTION by Orville Magoon

Bob Wiegel epitomizes inspiration, professionalism, technical insight, encouragement, and the highest degree of colleague relations and loyalty. Bob's oral history is a testimony to these and many more facets illuminating his brilliant career. Unquestionably, Bob Wiegel has had an enormous impact on my life.

In 1951, while I was a graduate student at Stanford, I found the proceedings of the First Conference on Coastal Engineering, held in Long Beach, California. After ordering my own volume from the Council on Waves Research at UC Berkeley, I went personally to the Berkeley campus to discuss my interests in the coast. When I arrived at O'Brien Hall, I walked to the appointed room to find Bob Wiegel, Joe W. Johnson and Dean M. P. O'Brien, a constellation of mind and inspiration I shall never forget. After I explained that I was having trouble understanding breakwaters, Bob not only encouraged my questions and prompted my study of concrete armor units, but he soon asked me to lecture on the same subject to one of his classes at the university. Bob always answered a question with a challenge of his own.

Perhaps the most difficult coastal project I have ever tackled was the repair to the seaward heads of the Humboldt jetties. In my now annual visits and lectures for Bob at Berkeley, I discuss the concepts that formulated the triumph of coastal engineering over the forces of the ocean waves at those jetties. Those concepts, which became the foundation of my philosophy on coastal engineering, were woven successfully from the threads of inspiration engendered by Bob's academic encouragement at that first meeting back in 1951.

From the start, Bob taught me with questions that made me determined to find my own answers. In this way, he encouraged a professional attitude of personal responsibility that provided a solid ground for my own career. I had to discover for myself how to solve the problems we confront in an ever-changing coastline defining an ever-changing world. Because of Bob, I was well equipped for the challenges that would confront me in the years ahead.

Today, I am deeply grateful, not only for Bob's professional leadership, time, and encouragement, but, above all, for the friendship of a great man.

Orville Magoon, Vice Chairman
Coastal Engineering Research Council of the American Society of Civil Engineers
and President, Coastal Zone Foundation

Middletown, California
December 1997

INTERVIEW HISTORY--Robert L. Wiegel

Robert Wiegel's pioneering contributions to coastal engineering, his five decades of world-recognized research in coastal wave and beach processes and earthquake-generated tsunamis, and the practical application of his work for government and industry gave impetus to his oral history. His textbook *Oceanographical Engineering* established the discipline. His role in fostering research, mentoring several generations of students in civil engineering, and serving the College of Engineering at Berkeley and the University of California at large, as well as the national and international professional community, made him an ideal candidate for an oral history memoir.

I was asked in the fall of 1996 to work with Professor Wiegel to document his career in a field only slightly related to my experience as a research interviewer of mining engineers. Fortuitously, I had just returned from a holiday at Cape May, New Jersey, where I spent many hours walking and driving along the waterfront and observing its historic works of coastal engineering, although at the time those words were not yet in my vocabulary. I did not feel totally unqualified to interview Professor Wiegel, and any trepidation I felt diminished when I first spoke with him by telephone; as a first-rate teacher, he is very willing to impart information to an eager listener. In this first conversation, we established an additional basis for rapport: my husband served in the amphibious forces of the U.S. Navy, and much of Wiegel's research was generated by that branch of the service; at that point we became Bob and Lee.

As preparation, I conducted informational interviews with his Civil Engineering Department colleagues Professor Rodney Sobey and Mabel Iwamoto at the University of California at Berkeley, and Orville Magoon, past president of the American Shore and Beach Preservation Association, in Middletown, California. Apart from the useful information acquired about his work, I gained a strong sense of the very deep affection and admiration which Wiegel's associates have for him. I was also supplied, by Wiegel and others, with abundant references and materials on the history of the Beach Erosion Board, the Coastal Engineering Research Council, and the various organizations which have evolved since protecting the fashionable beaches of Cape May first became a governmental priority in the nineteenth century.

We held a planning session in his office on the fourth floor of O'Brien Hall on 2 December, 1996, and then we recessed while he and his wife spent their customary vacation in Hawaii. Seven interviews were conducted in the same office on 8 January; 4, 12, and 19 February; and 10, 12, and 31 March, 1997.

The Wiegel office is crowded with well-organized reference materials and up-to-date equipment. When I arrived, Bob was usually working at his new 100-megabyte computer. At our first meeting, he explained his physical limitations: he is totally deaf in one ear as a result of surgery for an acoustic neuroma. The consequent nerve damage has also moderately impaired his speech. He preferred to be interviewed in the morning before he tired. He presented this information cheerfully; in the same practical way, he showed me where the materials relevant to the interviews were stored in his drawer, so I would know, "just in case something happens." His memory seems unimpaired and his travel schedule is still one that might daunt someone much younger. He prepared carefully for each interview, with notes and references at hand. He returned telephone messages promptly and kept interruptions to a minimum.

While the tapes were being transcribed, the Wiegels went off to England and Paris. The transcript was sent to him on 1 July and was returned less than two weeks later, meticulously reviewed, with corrections and additions for accuracy and clarity but no substantial changes. After these corrections were made, Bob requested a second review which resulted in only a few very minor corrections. He selected a number of photos to accompany the text. I prepared a skeleton index which he enlarged.

Thanks are due to Professor Rodney Sobey for suggesting this oral history and arranging for the funding which made it possible. He and Orville Magoon also contributed introductions. Mabel Iwamoto, administrative assistant for thirty-seven years with the Sanitary, Environmental, Coastal, and Hydraulic Engineering Division of the Civil Engineering Department, indicated to me how much Bob Wiegel cared for those he worked with and for his students. Linda Vida-Sunnen of the Water Resources Center Archives also made me feel that it was an uncommon privilege to work with Bob Wiegel, and indeed I came to agree. As an editor of professional publications with many years of experience, he understands the detailed process of putting together a document. This awareness, together with his personal kindness, facilitated to an exceptional degree both the interviewing and the editing.

Researchers in the history of coastal engineering may also want to consult the oral history of Morrough P. O'Brien, *Dean of the College of Engineering, Pioneer in Coastal Engineering, and Consultant to General Electric*, Regional Oral History Office, University of California, Berkeley, 1988. For more information on the history of the University of California, consult the University History series list which is included in the appendix of this volume.

The tapes of the Wiegel interviews are deposited in The Bancroft Library and are available for study. The Regional Oral History Office was established in 1954 to record the lives of persons who have contributed significantly to the history of California and the West. The office is a division of The Bancroft Library and is under the direction of Willa K. Baum.

Eleanor Swent, Research Interviewer/Editor
Regional Oral History Office

October, 1997
University of California, Berkeley

Regional Oral History Office
Room 486 The Bancroft Library

University of California
Berkeley, California 94720

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Robert Louis Wiegel

Date of birth 17 October 1922 Birthplace San Francisco, CA, USA

Father's full name Louis Henry Wiegel

Occupation Engineer Birthplace Irwin, Westmorland County, PA

Mother's full name Antoinette (nettie) Lavern Decker

Occupation Housewife Birthplace Kelsey (near), El Doerado County, CA

Your spouse Anne Pearce

Occupation Birthplace Ebbw Vale, U.K.

Your children John Mansfield Carol Elizabeth Diana Louise

Where did you grow up? Oakland, California

Present community Berkeley, California

Education B.S., mechanical engineering, 1943; M.S. in mechanical engineering, 1949
University of California, Berkeley, CA

Occupation(s) Professor of Civil Engineering, Univ. of California, Berkeley, CA
Consultant in coastal and offshore engineering

Areas of expertise Coastal and ocean engineering (mostly civil engineering)

Other interests or activities Extensive participation on professional society activities

Organizations in which you are active American Society of Civil Engineering, American Shore & Beach Preservation Association, Engineering Committee on Oceanic Resources, National Research Council committees

I EARLY YEARS, 1922 TO 1940

[Interview 1: January 8, 1997] ##[1]

The Wiegel Family

Swent: We'll begin at the beginning and you can tell where and when you were born, and we'll talk a little bit about your family background.

Wiegel: I was born in San Francisco in 1922 on the seventeenth of October, and about two weeks later, the family moved to Oakland, California. I was raised in Oakland; went through the Oakland school system, all the way through, and then the University of California.

Swent: How long had your family been in California?

Wiegel: My father came out in the early 1900s from a small town [Irwin] near Pittsburgh, Pennsylvania, to go to Stanford. In those days, Stanford was not expensive.

Swent: It was free at one time, wasn't it?

Wiegel: Yes.

Swent: But you had to be a very good student to get in.

Wiegel: He had to work, and then school, work and school.

Swent: All the way from Pittsburgh?

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[1]## This symbol indicates that a tape or a segment of a tape has begun or ended. A guide to the tapes follows the transcript.

Wiegel: Yes. He started in a college up in Pennsylvania: Bucknell College. He worked for the railroads for a couple of years as a surveyor both on the Northern Pacific Railroad up in North Dakota, and then the Western Pacific Railroad in Nevada and across the Sierra Nevada. I guess in his first year he had had surveying in college. So that was about the turn of the century. I know he started Stanford in 1902, and was in the class of 1906.

My mother was born in California, in Kelsey, which is near Placerville in the gold country. Her mother had come out from Ohio with her family via the Isthmus of Panama in 1868. So on my mother's side, we've been in California since 1868.

The Decker/Carothers Family, in California Since 1868

Swent: Was there a train across the Isthmus by then?

Wiegel: I believe so. I don't know. All I know is that the family records said she came out via the Isthmus in 1868.

Swent: That was a heroic thing to do.

Wiegel: They lived in San Jose, farmed, but then moved up to the gold country.

Swent: What did they do there?

Wiegel: I'm not completely sure. I know her brother owned one-third of one of the gold mines and mills in the hills up there between 1879 and 1883. It was the Rising Sun Mine.

Swent: They had wonderful names for those old mines, didn't they?

Wiegel: Yes. This information is lost.

Swent: No, it's not lost, we're preserving it. What was your mother's family name?

Wiegel: My mother's name was Decker and her mother is Sarah Elizabeth Carothers. Her father was William Decker and he was born in Wisconsin Territory in 1845. He moved to California, we believe, in 1868.

Swent: The same time as the mother.

Wiegel: But we've no idea how he got to California. We were long-time Californians.

Swent: Yes, indeed; lots of strong people there.

Wiegel: My mother lived in San Francisco at the time of the 1906 earthquake. They lived in a tent for two weeks in Golden Gate Park in San Francisco.

Swent: How old was she at that time?

Wiegel: My mother was born in 1885.

Swent: So she was about twenty years old, twenty-one--would have a very clear memory of it. She was not married yet?

Wiegel: No. She got married in 1907. I think it's of interest that both my grandfathers were in the Union army in the Civil War.

Swent: From California?

Wiegel: No. My grandfather on my mother's side was from Wisconsin, and my grandfather on my father's side was from Pennsylvania. My grandfather on my father's side is John Wiegel, and he lived until he was ninety-nine. The interesting thing is he attended the Seventy-Fifth Encampment at Gettysburg.

Swent: Oh, my. What a powerful experience that must have been.

Wiegel: I know. We have a photograph of him up at the Seventy-Fifth Encampment.

Swent: Did you know him?

Wiegel: I met him twice. My mother's father died before I was born; just a year before I was born. But my father's grandfather I did meet on a couple of occasions.

Swent: Did he talk to you about--

Wiegel: Oh yes. The Civil War? Yes. He didn't remember much by the time he saw me, of course, because he was an old man; a very old man.

Swent: But that had been important in his life.

Wiegel: Yes, it was.

Swent: Of course it was. How did your parents meet each other?

Wiegel: One of my mother's cousins, Bob Hudson, was an engineering student (also, class of 1906) at Stanford, and that's how they met. Incidentally, he was co-founder of the firm, Hunter and Hudson, in San Francisco; an engineering firm which is still in existence, I believe. I don't know for sure.

Father, Stanford 1906, and the Ocean Shore Railroad

Swent: What kind of engineering did your father--

Wiegel: My father started--I think it was mechanical and civil--but then at the end, he went into law. When the earthquake hit, I think--I don't know--there were jobs available. He was up in Oregon, and we don't know exactly what--but from the little records, I suspect he was working on a railroad up there. But when he come back to California--

Swent: So he was actually not here at the time of the earthquake?

Wiegel: I think he was at Stanford, I'm not sure.

Swent: Yes, spring of 1906, he would probably have been still at Stanford.

Wiegel: We assume he was working on railroads, but then he came back and went to work for a company called the Ocean Shore Railroad, which was putting a railroad from San Francisco south along the coast to Half Moon Bay and then it was to continue all the way to Santa Cruz, but never did. Construction was started in September 1905. One of the real problems was that the right-of-way was across what is known now, and was known then as the Devil's Slide. It was as difficult for the railroad as it is for the highways. [laughs]

Swent: It hasn't changed. That is south of Pacifica--

Wiegel: He was the engineer in charge of the construction.

Swent: Was this railroad a private enterprise?

Wiegel: Yes, those things were all private.

Swent: Subscription, I suppose?

Wiegel: I don't know the names, but they were big San Francisco people; the money people. They called themselves capitalists. They were honest capitalists. One was J. Downey Harvey, who became president of Ocean Shore Railway.

Swent: Yes, they accumulated capital--

Wiegel: --and they invested it. But they also developed property, and of course the reason for putting the railroad in was so they could sell property in the Half Moon Bay area and all the way down. The fact is several little towns were started by those people: Half Moon Bay, El Granada[1], Princeton, Moss Beach. There were several other ones; all the ones south of Pacifica were really started by those people. For several years, my father was also resident manager of the property development.

Swent: As well as the railroad?

Wiegel: That was finished by then, as far as they were going to go. The first train ran to El Granada on 21 June 1908. They decided not to continue south of--I don't know if they ever went south of Half Moon Bay; I can't tell you.

Swent: But it did go as far as Half Moon Bay?

Wiegel: Yes. It went as far south as Tunitas Glen, where work stopped in December 1908. The railroad had been constructed from Santa Cruz north to Swanton, but my father did not work on this section.

Swent: Over Devil's Slide? Or through, I guess.

Wiegel: Yes. On it? [laughter]

Swent: Did it have tunnels?

Wiegel: I think there were a few tunnels.

Swent: Were there?

Wiegel: Well, if they were, they were little, tiny short ones. Nothing like the one they're proposing for the highway now. It would have been maybe a hundred yards long or so, through San Pedro Point. There was a saddle cut on Montara Mountain.

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[1]Incidentally, El Granada was originally named just "Granada," the name in the tract map and on the rairoad station. The U.S. Post Office changed it for some reason in January 1909. -R.W.

Swent: I wonder if there are any remnants of that roadbed?

Wiegel: I believe there are. Highway 1 inherited parts of the roadbed. I've read the track was removed in 1920.

Swent: Very interesting.

Wiegel: So the next stage in this: he started to buy up some property. They owned a fair amount of property there; they also owned a half interest in a farm in the San Jose area.

Swent: This was through his wife's family, perhaps?

Wiegel: I'm not sure. I really can't tell you; I don't know. The farm, I suspect, was joint with his wife's family--I'm not positive. He died in 1940, so I was just--

Swent: You were a youngster.

Wiegel: I had just graduated from high school and was going to start Cal.

Swent: You said your parents were married in 1907, soon after he left Stanford. Did they live in San Francisco?

Wiegel: They first lived in a little town: San Juan Bautista.

Swent: Lovely place.

Wiegel: But that was just for a short time, and then they moved to San Francisco. Actually, they had a home, they lived--my brother and older two sisters, when they were young, were raised in El Granada, because they had a home down there and my father would commute to San Francisco. Then they moved to San Francisco.

Swent: He commuted by train?

Wiegel: At the start, but it never really worked well. [laughter] That train was a financial failure. One reason was, I think, because of the cost of maintaining the road beds and rails in the area. Also because of relatively poor service, buyers of property were letting it go.

Swent: You have two brothers and a sister?

Wiegel: Three sisters and one brother. My brother was the oldest; he was born in 1908, and then my eldest sister was born in 1911 and a second sister in 1912, and I was born in 1922, and my youngest sister was born in 1925.

Swent: Almost two families.

Wiegel: Almost two families, yes.

Swent: Were there other children in between who died?

Wiegel: No. In the previous generation, of course, children died. My mother's brother died when he was a young man, and another one of her brothers died when he was an infant. That was what life was like back in those days. But by the time my family came along, they had decent medical--

Swent: I was wondering about the flu. The Spanish flu came in that period.

Wiegel: No. I think my mother's brother died of scarlet fever, or something like that. Which, again, was not uncommon in those days.

Swent: No, it wasn't at all. Why did your family move from San Francisco to Oakland?

Wiegel: My guess is to get more sun.

Swent: Where was their home in San Francisco?

Wiegel: They had several homes out near what they call the panhandle of Golden Gate Park, between there and Pacific Heights, in that area.

Swent: Where there was fog?

Wiegel: Yes. Of course, they had the ferryboat system, so when they moved to Oakland, it was easy to take what they called the commute train which then went from Piedmont Avenue to the ferryboat. It was not a bad commute.

Swent: I think it was easier then than now.

Wiegel: I remember when the San Francisco Bay Bridge opened up, it was marvelous there for a while. It was very easy. Then they'd catch the commute train at Piedmont Avenue and go straight to downtown San Francisco. So that worked really well.

Swent: Where was your home in Oakland?

Wiegel: It was very near the Oakland Rose Garden; what is now the Rose Garden. Not too far from Lake Merritt.

Swent: Lovely area.

Wiegel: Very nice area.

Good Schools in Oakland

Swent: What schools did you go to?

Wiegel: I went to Lakeview, the grammar school, then Westlake Junior High School, and then Oakland High School and then to Cal. All five of us went to Cal, which is what we called it then. We didn't call it UC Berkeley or Berkeley.

Swent: No, it was just Cal.

Wiegel: We called it Cal. My brother graduated in 1929 in Economics, and then from Boalt Hall in 1932, and then passed the State Bar exam.

Swent: What was your brother's name?

Wiegel: John Mason--he went by his middle name of Mason. He was a lawyer, but he also started a law publishing firm, which published The Weekly Law Digest, and he was editor and publisher of it for his whole professional life. Then my two older sisters both graduated in 1933 from Cal.

Swent: Were they twins?

Wiegel: No. My elder sister, Marion, had pneumonia, and had to drop out a semester, and in those days, you graduated once a year, so she graduated the following year because she had to make up a semester's work. Then my youngest sister, who started Cal during World War II, didn't finish. She became an airline stewardess instead, and then met a young graduate of Annapolis, George Dickey, and got married. She died years ago of cancer.

Swent: And your other sisters you said were teachers?

Wiegel: No, not teachers. They're both still living. One's married name is Morris, and the other's married name is Christopher.

Swent: Do you have any recollections of your school that were particularly important that you would like to mention? Teachers who were influential?

Wiegel: You mean grade school?

Swent: Any school. Were there outstanding teachers that influenced you?

Wiegel: We had very good teachers, very good. We had good science courses.

Swent: When did your science courses begin?

Wiegel: We had something in junior high school; it wasn't just biology. In high school I had the chemistry and the physics and all the mathematics courses. I did have very good teachers in chemistry, physics, mathematics, but I also had an excellent teacher in English that drummed into us how to write.

Swent: I gather you think that was pretty important?

Wiegel: I think that is very important. There's been an oral history of Morrough P. O'Brien,[1] and I worked for him directly here for quite a few years, and I remember him once saying after he had been listening to a student trying to present his research--because he had read what the student had written--and I remember him saying once, "If you can't write clearly, you can't think clearly." I think this connection of thinking, writing, speaking, reading, is absolutely vital. As you know, reading the newspapers, we don't have as much emphasis on doing this as there used to be.

Swent: No.

Wiegel: There were no excuses; you did it. My mother taught me to read before I started school. I can still remember that. But there was no nonsense. You studied.

Swent: Yes.

Wiegel: And I've always enjoyed studying. So I can remember having good teachers, and very supportive teachers.

Swent: What about your science training in high school? Were there laboratory courses as well as lectures?

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[1] Morrough P. O'Brien, Dean of the College of Engineering, Pioneer in Coastal Engineering, and Consultant to General Electric, Regional Oral History Office, University of California, Berkeley, 1988.

Wiegel: Yes. Chemistry certainly was laboratory. There wasn't much in biology; I think it was mostly lecture. But chemistry and physics were both laboratory. But also I took machine shop and wood shop.

Swent: Were these required?

Wiegel: No, I had the time so I took them and I'm glad I did because I've done most of my own repairs ever since.

Swent: That's helpful.

Wiegel: I think we had a good school system; a well-rounded school system.

Swent: Where did your interest in the ocean and coasts begin? Was that an earlier track or not?

Wiegel: We had a summer home at Capitola which is on Monterey Bay about five miles southeast of Santa Cruz, and my mother owned the house--after my father died, of course--for quite a few years so even when our children--and we have three children--were small, we would go down there and spend a couple of weeks at the ocean and so forth. But also when I was in high school and junior high, we used to go up to Yosemite and camp. That's why I got interested in geology.

I was interested in geology, and I was going to become a geologist when I came to the university because my middle sister worked for Shell Oil Company and at that time there were jobs in oil companies, and I liked geology, and oil companies used geologists so I thought, "I'll become a geologist." But during the summer, I met a person who was a Cal student in mechanical engineering, and he said, "If you want a job, there are more jobs in mechanical engineering in the oil company." So when I came, I wrote down I wanted to be a mechanical engineer, and I was. I didn't even know what mechanical engineering was. So it was just by happenstance, and I got my degree in mechanical engineering, and after the war a master's degree.

Swent: Let's not jump up to that quite yet.

Wiegel: We won't jump into how I got into the ocean.

Swent: Your father died when you were very young.

Wiegel: I was eighteen, maybe seventeen, but it was the summer just before I started Cal. He died of a heart attack in Yosemite.

A Summer in Hawaii, 1930

Swent: That's too bad. How did the Depression affect your family?

Wiegel: Definitely. We had owned a fair amount of property, and most of it had to be sold off during the Depression. I do remember we went to Hawaii in 1930 and spent the summer in Hawaii.

Swent: That was a bit unusual, wasn't it?

Wiegel: It took you five days to go over by boat and five days to get back, and we had rented a cottage on Waikiki Beach, and I remember my brother running into a record that showed it cost twenty-seven dollars a month.

Swent: And you stayed there for the summer?

Wiegel: For the summer, and that's where I learned to swim. I was seven years old.

Swent: Was your father working there at that time?

Wiegel: No, he wasn't. He went over with us, and came back to California after a few weeks. When I say we were over there--my mother, my three sisters and myself. My older brother was not there because he was, at that time, he was a cadet for the summer for the Dollar Steam Ship Company, and he had an opportunity, so he was sailing around the world at that time.

Swent: Wonderful. What ships did you take over to Hawaii?

Wiegel: I think it was called the Matsonia.

Swent: The Matson Line. That must have been a wonderful time.

Wiegel: I've loved Hawaii ever since.

Swent: And you still spend time there?

Wiegel: I think I've been out more than fifty times.

Swent: But that was the first time, in 1930?

Wiegel: Yes.

Swent: Had your mother been there before?

Wiegel: No.

Swent: That was a new experience for all of you.

Wiegel: My mother had some relative that had moved out there. She was a school teacher, I believe. Again, this is pretty hazy.

Swent: Perhaps it influenced you?

Wiegel: It could have, but I've loved it ever since.

Swent: Of course you have. We've mentioned the Depression, but it didn't devastate your family, I gather?

Wiegel: No. I know it was severe because I know my father had to sell quite a bit of property at that time. Bits and pieces and things of that sort. I think that this is true: that anyone raised during the Depression time became quite frugal and learned to save.

Swent: It was a formative experience.

Wiegel: To this day I've never, ever, bought anything on the installment plan with the exception of a home. Even an automobile--I saved the money ahead of time. I'm sure that was the result of the influence of the Depression and how it affected people.

Swent: It was a watershed.

Wiegel: It was a watershed, and nobody had anything. There was no problem because you couldn't compare anything; nobody had any money.

##

Swent: Did your father have a car?

Wiegel: Yes.

Swent: What kind of car did he drive?

Wiegel: The ones I can remember were--on two occasions he bought a Nash, but before I was born--and I know this from a photograph--I think it was back in 1918, he bought a car, and it was an open car. I think it was called a Vielie. All I know is there was this photograph, and it was an open car, and I can remember my brother saying that my father bought the car, and the driving lesson consisted of the salesman getting in the car with him, driving around the block, and that was it. It's your car; you're a driver. He was living in El Granada at that time, and he drove it down over--at that time it was a mountain road, dirt road, from Redwood City over the mountain and down. I'm surprised that I had a father. [laughter]

Swent: When you lived in Oakland and he commuted to San Francisco, how did he get to the train?

Wiegel: I think he drove to the train and parked it. The train then went to the ferryboat, which crossed the bay. That's my remembrance.

Swent: Did your mother drive?

Wiegel: No, she never drove. I understood she started to drive once, and had a bad experience and decided she didn't want to.

Swent: That was rather typical.

Wiegel: Our home in Oakland was only two blocks from a very nice shopping area which is called Grand Avenue, so there was never any problem. Of course, in the early days, when I was a boy, I can remember a truck would come along with the person that sold vegetables and eggs--eggs were delivered, milk was delivered, ice was delivered. But this person sold from this truck all your vegetables.

Swent: What kind of person was it? Was he Italian or Chinese? Do you remember?

Wiegel: Yes. I remember calling him Tony, so--

Swent: A number of those vegetable people were Italian.

Wiegel: Of course in El Granada, the farmers there were all Italian, and of course they were very wealthy. My father worked closely with Dianda, who was a close friend of Giannini, and he was the big artichoke farmer there. The farmers were almost all Italian and very successful. I always say I grew up with an Italian stomach.

Swent: That's good.

Wiegel: I still like Italian food.

Swent: Do you have any recollection of the Long Beach earthquake?

Wiegel: No.

Swent: It wasn't even noticed here, I suppose?

Wiegel: No. The reason I'm familiar with it is because it was a landmark in the state of California. It's what triggered our earthquake codes, the first codes, and also the structural engineer license that didn't exist before then. When I got interested in earthquake engineering, then I became very familiar with the Long Beach earthquake.

Swent: But when it happened--

Wiegel: But nothing personal.

Swent: What about the general strike on the waterfront in San Francisco?

Wiegel: All I can remember was that it existed. I can remember all the headlines in the newspaper. This is the one in San Francisco on the waterfront.

Swent: There was one also in Oakland.

Wiegel: The one I'm familiar with--I think is the general strike--was the waterfront one in San Francisco.

Swent: That was San Francisco in '34, I think.

Wiegel: Remember I was twelve, and when you're twelve you don't pay that much attention.

Swent: What kind of clothes did you wear in school?

Wiegel: When I was in high school, it was just slacks. It wasn't jeans.

Swent: Not jeans, no.

Wiegel: Slacks and what we call sports shirt now, and a sweater.

Swent: What kind of shoes?

Wiegel: I don't know. [laughs] A pair of shoes. They certainly were not--tennis shoes we wore during the summertime. I don't remember wearing tennis shoes to school.

Swent: I don't think so.

Wiegel: During the summer, I think we wore tennis shoes. Of course, when I went to Yosemite and so forth, we always wore boots up there.

Swent: Did your mother have household help to do the washing or cleaning?

Wiegel: When I was very young, yes. But that ceased. I can only remember Mrs. Kraevish--I can remember her name--but that must have only been for the first couple of years.

Swent: So your mother did the washing and the cooking?

Wiegel: Yes.

Swent: Maybe sent laundry out? As a boy, maybe you didn't even know that this has happened.

Wiegel: I know we had a washing machine downstairs because I remember the clothes.

Swent: But in those days the boys didn't mess with that, I guess.

Wiegel: I don't think so.

Swent: Times have changed a lot.

Wiegel: Times have changed. That's rather hazy, but I know we had a washing machine and a clothes drying line.

Swent: Did you help at all with any household things?

Wiegel: Gardening; I've always liked gardening. I thoroughly enjoyed it. We had a nice garden, and I always liked it. No, I studied.

Swent: That was your job.

Wiegel: That was my job.

Swent: I noticed in one of the other oral histories--I think you wrote an introduction--I think it was in that--there was talk about the importance of good math background early, a good foundation in math. I gather that you feel you got that in your schooling.

Wiegel: I remember when I started engineering--and later on, people say the same--it is so important to have a good background in math and physics and chemistry. Then you were flexible. I got into mechanical engineering not even knowing what it was. Later on, I could understand: if you had this good background, you can shift. This is what permitted me to get into the physical oceanography and so forth--was a decent background in those three subjects.

Swent: What was the level of math, let's say in high school? Did you get calculus?

Wiegel: No, no one got calculus then in high school. We went through--

Swent: Algebra?

Wiegel: Trigonometry, solid geometry--we didn't get into spherical trigonometry--I don't think I got spherical trigonometry until I got to college. Then that's where I got calculus and--

Swent: And chemistry and physics--at that time they were separate courses. A lot of blanks in the atomic chart in chemistry.

Wiegel: Oh, yes. When I took physics in college, it was just the start of nuclear physics. Engineering students didn't have any of the nuclear physics.

Swent: What did you have in high school physics?

Wiegel: Just mechanics and electrical circuits, maybe a little bit of electromagnetic radiation--a little bit of that. It wouldn't have been too much.

Swent: Did you get good grades in school?

Wiegel: Yes. I was always a good student and a hard worker.

II ENGINEERING STUDENT, UNIVERSITY OF CALIFORNIA, FROM 1940

Before World War II

Swent: Going to Cal--what sort of admission application procedures were there?

Wiegel: I think it was based on your high school grades. I think I signed up, and then I remember having to take a Subject A exam, which I passed because of that very good teacher I had in high school in English that really drummed it in. I can remember her saying, "Write simple sentences. If you write complex sentences you'll trip yourself up." When you took the Subject A exam, and part of it was the written part--I kept it simple and I passed it. I still remember that.

Swent: So this exempted you from--

Wiegel: It exempted me from taking whatever they call that--Subject A, I guess they call it. Maybe they still do.

Swent: Perhaps they do.

Wiegel: I've always read a lot too. Again, everyone in the family read. Books, magazines, everything. I think that's very important.

Swent: Yes. Did you live at home when you came to Cal?

Wiegel: I lived at home. There were three friends going to Cal that lived close to each other, so we would take turns driving.

Swent: Again, what kind of car did you drive?

Wiegel: It was my brother's--I guess I drove once a week, I think that is what it was, and one of the others drove twice a week. It was a Nash.

Swent: Your family car, or was it your own?

Wiegel: Oh no, in those days almost no person that age had a car. When I was at Cal there were a couple of students that had cars, but that's all. Just a couple. Nobody else had cars.

Swent: You could borrow the family car for a day?

Wiegel: Yes. There was an arrangement. Coming by bus took a long time. I did it a few times, but I would have to walk like ten blocks to catch a bus.

Swent: And you could park here?

Wiegel: Yes. There was an old dirt lot; I guess it's about where the new business school is, and that was just a big open dirt lot there. You just parked; there were no fees or anything like that. Almost nobody had a car, so there was no difficulty. I remember--I think this is kind of important--it was in high school--you talk about peers; you read it all the time--peer pressure and so forth. There were five or six of us that were good friends, and after school in high school, we very often played basketball--not teams or anything, I don't know what you'd call it, but we'd just break up--three on a side--and play. Two of them became medical doctors, one clinical professor at Stanford, one a clinical professor at UC San Francisco, one got a ph.D. in physics, and the other got a ph.D. in chemistry, and another became a dentist. So it was kind of interesting.

My guess is that we were about the only ones that did this sort of thing.

Swent: Did so well.

Wiegel: I guess it was because we were all into chemistry and into physics and that must be it because those were small classes. Not many students really took them; looking back I realize that that was probably the connection.

Swent: These were high school friends?

Wiegel: Those were in high school; we all went to Cal.

Swent: Would you like to give their names?

Wiegel: One was Charles Miles. He became a clinical professor at Stanford; I remember him well because his brother, who was a little older, John, went to Caltech. He is a retired professor at

UC San Diego and was vice chancellor for a while. Smith was the other one who was a clinical professor at San Francisco--Ed Smith. Doug Pingree became the dentist. There was another one, Mort Sarver, who became an optometrist and a clinical professor--or whatever they call them at Berkeley in optometry. He did a lot of work on the contact lens sort of thing. Bill Eustis, got his Ph.D. in chemistry under Professor Calvin. His brother became a professor of French literature here at Cal. I guess what it was: these were academically oriented families or something.

Swent: You all helped each other?

Wiegel: I think so. A couple of those names I haven't thought of in a long time.

Swent: So when you started Cal--

Wiegel: 1940.

Swent: You graduated from high school in '40--

Wiegel: --and then started Cal in fall. My father died that summer. Then in 1941, of course, was Pearl Harbor.

Swent: Your first year and a half was before the war then?

Wiegel: The first year was before the war.

Swent: Were there intimations of the war--that you were sensitive about it?

Wiegel: Not that I can remember. In those days we all took ROTC for the first two years. It was mandatory for men. Every male had to take ROTC for the first two years.

Swent: How did you feel about that?

Wiegel: I didn't feel one way or another; you had to take it; you took it. At that time. Then, when the war came, I could see the advantage of becoming an officer and so I asked to get into the upper division ROTC; you needed a minimum of B grades or something like that. In those days, a B was a good grade; you had to have a minimum of a B average. So I applied, and because I was in mechanical engineering they accepted me in army ordinance.

Swent: You had the two years of basic ROTC and then you applied for--

Wiegel: Then you applied, and I was accepted. I'm not completely sure, because things got a little hazy as to how the military treated you. Remember it was a war, people were being drafted; you had to go to the draft board. When I started upper division--that would have been my junior year--we were cadets and into the military. In my military service [record] it says that the starting date of service is something in the middle of 1942. This was not good planning on my part--it just was very fortunate.

I and a number of others wanted to be sure we got through and got our degrees, so we went during the summer. At some stage, we were actually put into uniform and moved into one of the fraternity houses--it served as a barracks, or whatever you want to call it--and went through another summer. I started Cal in September or the end of August in 1940, and I graduated in engineering in early October 1943. That was hard.

Swent: So you accelerated your college?

Wiegel: Yes. In engineering you had to take quite a few more courses than what you had to in L&S--that is, Letters and Science. We had to take many more units. We had to satisfy all our engineering requirements with the ROTC in addition, so you did nothing but study, period. You didn't do anything else except study. It was no pleasure, but it's just one of those things. Like people say, "There's a war on," so you did it.

ROTC and Accelerated Studies in Wartime

Swent: Can you be more specific as to what changes the war brought to you as a student on the campus?

Wiegel: You mean when I was a student?

Swent: Yes, before Pearl Harbor and the bomb.

Wiegel: In the first place, your friends all started to disappear into the military! It was only if you were like myself, that is, in upper division ROTC and in engineering where the military wanted you to get that degree because they wanted engineers. So those of us that were left--again, we just worked. There was all the difference in the world. There were no social activities. You look at these movies at what people did in college; there's not any resemblance whatsoever to my remembrance of going to college. You worked all the time. But I always appreciated ROTC because I think it was vital; I could see it even then.

Many of the previous ROTC people had been called in--young officers--prior to Pearl Harbor. Obviously, there was a lot of planning going on because my cousin was pulled in; a couple of other ones were pulled in. My brother--well, he didn't go in immediately. He wasn't in upper division ROTC in the late 1920s. After the war started, he became a member of the Judge Advocate General's department, which is the legal section of the army. There was just a day-and-night difference. The student population amongst the males just dropped way down. Is that what you asked?

Swent: Yes; I was wondering what changes the war brought to your studies.

Wiegel: Just a day-and-night difference.

Swent: Classes were much smaller?

Wiegel: We went right straight through. I'll say this: because some of the professors--they knew what pressure we were under, this going continuously--some of them, especially Professor Everett Howe--I remember him. He was so kind to those of us still here, knowing the circumstances that we were studying under and working under. But then some of our professors like--when I was a senior I had a course from O'Brien in fluid mechanics and he was gone most of the time. Later on, after I came back and got to know him, I realized what he was doing: all this amphibious and undersea warfare research type of thing, so there was good reason for him to be gone.

Swent: But you didn't know this at that time?

Wiegel: No, we hadn't a clue. It was all classified. Then our last year--I think this is kind of interesting--when we graduated--this small group--several of them disappeared. The rest of us were going to officers candidate schools. At that time we didn't get commissioned at graduation because they didn't have the time to have the summer camp and things like that. So you didn't get commissioned directly; you were sent to officer's candidate school directly, and then you got your commission. But several of the people disappeared, and we didn't know what had happened to them. I think it was three or four, and they were ones that were in what we called metallurgy. After the war we found out they had all been sent down to Los Alamos. At that time all we knew is one day they were gone.

Swent: Very secret.

Wiegel: So that was after the war and in hindsight was kind of interesting.

Swent: Do you have any recollection of the Port Chicago explosion?

Wiegel: That there were sirens for hours.

Swent: Did you feel it here?

Wiegel: No; but the sirens--I guess there were a lot of casualties.

Swent: Were you informed at all about it?

Wiegel: Only that it had occurred. I would have been interested, so obviously there couldn't have been much.

Swent: It must have been kept rather quiet.

Wiegel: It must have been, but I do remember the sirens going.

Swent: There were black-outs of course?

Wiegel: Yes, there were black-outs. I don't think it bothered us. There was gas rationing, which meant that things became very difficult, but on the other hand, when we were pulled on active duty and housed by the military on campus, there was no problem. We didn't have to worry about commuting or anything, you were just here.

Swent: You were no longer commuting then?

Wiegel: No.

Swent: This began--

Wiegel: Living with the military in one of the fraternities that they took over.

Swent: This is when you began the upper division ROTC?

Wiegel: Not immediately. Sometime in between--I can't remember. We were all pulled in and sent down to the Presidio at Monterey to process us. But we were only there for about two days and brought back up here and moved into one of the fraternities. The fraternities--there wasn't anybody there so the houses were available and the military leased them. We ate there; they had cooks that prepared the food, so it was a regular military type of living there, which was lucky.

Swent: And lots of studying.

Wiegel: And lots of studying.

Mechanical Engineering Professors Boelter, O'Brien, Folsom

Swent: Who were your teachers that you especially remember?

Wiegel: Well, Boelter. He became the first dean of engineering at UCLA after the war, and he was in heat transfer. Then, of course O'Brien at Cal. Even though I didn't see much of him, he was impressive and I liked the subject. And Folsom; Richard Folsom. They were all three professors in mechanical engineering. And Howe, who I mentioned earlier. But those are the four that I remember. I remember Professor Buck in mathematics.

##

Swent: You were just saying that your professor Folsom became very--

Wiegel: He was my advisor when I was a junior and senior. In those days, you had to have a major area within mechanical engineering, and mine was fluid mechanics and heat transfer, and I took several courses from Folsom. I took one course in pumping machinery, but then I did what was known as ME 199--that was the research available for undergraduates and I did that with Professor Folsom. Then when I came back after the war, when I was interested in going to graduate school, I went to see him because he had been my advisor. I was still on active duty. I was a first lieutenant and you accumulated leave because you couldn't take it during the war. But anyway, he's the one that then got me started after the war.

Swent: Let's go back a bit.

Wiegel: But that was the importance.

Swent: Was it a major decision to concentrate on fluid mechanics? Did you have to actually declare this?

Wiegel: Yes. I think what it was--during the junior year, that was the course that interested me the most. Also, it was important in the oil industry. So then in the senior year, I concentrated on it.

Swent: What was the status of that discipline at that time?

Wiegel: Not very advanced. [laughs] But fortunately, I had three people who, in hindsight, I can see were developing these fields, and so I was fortunate that I had three very interesting people.

Swent: These were?

Wiegel: Boelter, O'Brien, and Folsom. It was them, obviously, that got my interest aroused in fluid mechanics and heat transfer. Incidentally, that's the area that our present chancellor, Chang-Lin Tien--his field was fluid mechanics and heat transfer.

Senior Laboratory: Terribly Difficult in One Semester

Swent: Yes. What sorts of things did you study at that time? What sorts of things did you do your research or your laboratory work in at that time in fluid mechanics?

Wiegel: There wasn't any particular thing in fluid mechanics. At that time they had a senior mechanical engineering laboratory and every student had to take it, and in the semester system it was four units plus four units, and then three units plus three units of lectures that went with it. But because of those of us going through, we had to lump it altogether in one semester. The professors said they didn't want to do it; it would kill us. When we finished we said, "You were right."

Twice a week we would do experimental work that would last all afternoon or in the evening, and we'd have to write it up and get the reports in and come back within a couple of days for the second experiment of the week.

Swent: What sorts of experiments were you doing?

Wiegel: All types: mechanical, internal combustion engines, steam engines, wind tunnels, pumps, all kinds of mechanical equipment, air compressors; you do experiments with all of them. Study the theory, do the experiments, compare results with theory and so forth. There wasn't anything specifically hard in the laboratory, but it was a terribly difficult thing to do because of this time constraint. But when you finished, you really thought you knew something about engineering because you could really sit down, read something, and do something. Of course they knew that. This was designed by people that knew what they were doing even though it almost killed us.

We learned a lot. That was more general in mechanical engineering. We had to write these reports, and you had a senior course that you had to write something up and make an oral presentation, and it was graded both from the written and the oral standpoint. That, I thought, was very useful too.

Swent: Yes, to learn to present your ideas.

Wiegel: To learn to present your ideas, because for the rest of your life --of course, I've been in teaching, but even those who are not in teaching are going to have to present their ideas to the boss or the owner, or somebody. You're always having to present ideas.

Swent: Was there a graduation ceremony and all that sort of thing?

Wiegel: These presentations were both in mechanical engineering, and electrical engineering, and maybe in civil--I don't know. But I do know they were in electrical and mechanical because they were given by the chair of the department.

Swent: It was a serious matter.

Wiegel: In mechanical engineering, it was O'Brien who, at that time, was not yet dean; he became dean the following year. Because of having to take the ROTC at certain times, I wasn't able to take it from him, so I took it from the person who was the chair of electrical engineering--I remember that. But it didn't make any difference because these people were broad, and you could choose your subjects and so forth. There is one other thing I should say: because of the ROTC thing, which had to be fit in, there was one senior course that I had to take in civil engineering that I couldn't go to any lectures because it was given at exactly the same time. So I got permission of the professor, who was Harmer Davis, and I should give him credit; Harmer Davis said, "Okay."

So I did the homework, and then the military would allow me to take the mid-term. So I wasn't able to go to any lecture, but Harmer Davis was fortunately an understanding person. So I passed the course; it sure wasn't with the highest grade, but I passed it without being able to go to any lectures.

Swent: But you did the work?

Wiegel: I did all the work, yes. But it wasn't easy; that interval. I don't look back upon it with fondness; going to college.

Swent: Lots of fun and games. No.

Wiegel: Graduation. Yes, there was a graduation, and about the only people in engineering who attended it were those in ROTC because we were ordered to do it and march to it because the rest of them were all--there weren't many. As soon as the last exam was taken, they would be leaving. Nobody would be around, you see. They either got a job or they were going into the military or something like that. So about the only ones that attended the graduation were those of us in uniform. So I don't remember much more about it than the fact that we marched to the graduation exercise in the football stadium.

Swent: There must have still been women students.

Wiegel: Yes, but in mechanical engineering--of course the graduation ceremony was for the whole university at that time. I can't remember anything about it except for the fact that we were ordered to attend and we marched up, took our seats, and that was it. Yes, there were women.

Swent: You were too busy even to notice the girls.

Wiegel: We might have noticed them but--[laughter]

Swent: [laughter]--didn't have time to do much about it.

Wiegel: You didn't have much time, no. Of course, in engineering--women just didn't go into engineering in those days; that's all there is to it. They would be--like my sisters were political science majors. English, of course, was the most popular major. I guess it still is probably. Now, of course, we get women whose fathers were engineers. It's just a whole new world now because they are encouraged to go into these subjects. Especially the environmental aspects got quite popular.

III ARMY ORDNANCE OFFICER IN ENGLAND AND FRANCE

Motor Vehicle Repair and Leadership Training

Swent: So then after you graduated, did you go into active service?

Wiegel: Immediately within one day, or two days or something like that, we went by train back to the officer's candidate school.

Swent: Where?

Wiegel: In my case, this was ordnance at Aberdeen Proving Grounds in Maryland.

Swent: What about that?

Wiegel: Well, it was very rigorous, but I remember that compared with my last year at college, I thought I was on vacation almost. You're up at 5:30 in the morning, and went to class, class, class, and you studied until 10:00 at night--this was easy! You had to be on, but compared with that last year in engineering at Cal, it was very simple and well done. They were trying to pick up on the leadership aspects: administration, how you get people to do something. No place in engineering--or, as far as I know, anything else on campus--were you ever exposed to how to get somebody to do something. So it was the leadership thing that was interesting.

Then, after you got your second lieutenant's bar, they sent you to specialty school, and because I was in mechanical engineering, I went to the one on motor vehicles. Some would go into armaments; they pushed me into motor vehicles because I'd had a course in internal combustion engines. Then I went overseas to England.

Swent: Where was the motor vehicle duty?

Wiegel: The same place.

Swent: At Aberdeen? And then you were shipped over to England?

Wiegel: Shipped over to England.

Swent: How did you get there?

Wiegel: By ship. It was one of those--we had our own bunks. Some of them there were three people in each bunk. In other words, each person got eight hours. It was that bad. But I was on a smaller boat, and I can't remember too much details about it, but I know each had his own bunk.

Swent: When was this?

Wiegel: It was '44.

Swent: It was still a pretty dangerous trip across.

Wiegel: Oh, yes, it was all convoys.

Swent: There were a lot of submarines still.

Wiegel: Oh yes, it was all convoys. I know we landed at Liverpool, I remember that. Then, from Liverpool, we went into what they called a replacement depot and then different units would say, We're short a person, and they would match your specialty. I think I was there for two or three weeks, and I was shipped into an ordnance battalion which was in England.

Ordnance Service near Cheltenham and in Paris

Swent: Where?

Wiegel: Near Cheltenham in Gloucestershire. Cheltenham is about halfway between Oxford and Birmingham. It's in what they call the Cotswolds Country. It's beautiful; Cotswolds is a beautiful country. I was in an ordnance battalion, and it was completely rebuilding engines and power plants and things like that. It was a shop that was set up by General Motors to completely redo vehicles because they got worn out--with two and half ton trucks and things like that--the wear on them was tremendous. So that's what we did.

Swent: Was there bombing in that area?

Wiegel: Not while I was there. There had been. My wife--I met her at that time. We didn't get married until after the war, but I did meet her at that time. She's from Cheltenham. She can remember that. Incidentally, she had two brothers, both who were in the R.A.F. One was a pilot, and he was killed in October of '44; his plane went down. Her other brother was a navigator. They weren't together. But her other brother survived and became a teacher; he taught English for years.

So then, about Christmas of '44, our unit went over to France, and it was a pretty cold winter. That was a very cold winter over there, just like this year is a cold winter in Europe. We ended up with another big factory outside of Paris. I was in heavy ordnance. I ran into my brother, because my brother was an officer in the Judge Advocate General's Office, and he was stationed there. Not in the factory. His group was stationed in Paris. So we would see each other very often on Sunday; Sunday we had off.

Swent: I'm trying to think of when Paris was liberated.

Wiegel: He was there. Not when it was liberated, but he was there when Churchill came through. They moved in very shortly after the troops moved through. He can remember seeing Mr. Churchill marching up the Champs Elysée, very impressive and moving. I forget when that was.

Swent: That was before you got there?

Wiegel: I didn't see it, I know that. Oh yes, he was over before I was.

Swent: What was your wife's name?

Wiegel: Anne Pearce. She was born in Wales, in Ebbw Vale. But she was raised in Gloucester and then Cheltenham.

Swent: How did you meet?

Wiegel: At a dance. The military had a dance. Her father was manager of the local bus company and both her brothers were in the R.A.F. Everyone in England was involved. There was no alternative in England. Most people were here too, but not as much. There, everyone was.

Swent: What was your experience in Paris?

Wiegel: I never liked Paris until many years later. There was nothing wrong.

Swent: But you weren't seeing the glamor.

Wiegel: No, they had been through war; everything was in bad repair. It was beautiful. I must be careful in describing it. Museums weren't open, restaurants--none of the things that people associate with Paris existed except the scenery. I can still remember the cathedral, and I can remember going out to Orleans Cathedral but the stained glass had all been removed to put it in safety, so you didn't see what you see today. But still it was impressive; the buildings were certainly impressive.

Swent: In Christmas of '44 you were there?

Wiegel: Yes, that's when we moved over; in Christmas of '44.

Swent: How long were you there?

Wiegel: In the summer--

Swent: The war ended there in May.

Wiegel: The war ended there in May. Of course I can remember that very clearly.

Swent: What are your memories of that?

Wiegel: Just that everybody was shouting and running around--it's France. Obviously they were absolutely thrilled that it was over. That's about all I can remember. But as I hadn't been in the military that long, I volunteered to serve in the Pacific theater. First they asked for volunteers; then they used the army system to say, "You volunteered." First they ask, and there were several of us that decided to volunteer to serve in the Pacific theater. We were sent down to Marseilles to ship out. We were on a troop ship heading for the Philippines, because by that time, the military had secured the Philippines and they were building up for the invasion of Okinawa. We were out for a couple of days when there was this flash about the atomic bomb going off, and then a few days later there was a flash that the war was over.

Swent: Where were you?

Wiegel: I don't know. We were in the middle of the Atlantic Ocean someplace.

Swent: I'm trying to think how you were going from Marseilles.

Wiegel: It would have gone through the Panama Canal.

Swent: Panama?

Wiegel: It wasn't going the other way--we weren't going down around Africa. We weren't going through the Suez.

Swent: Through Gibraltar.

Wiegel: Yes, I can remember Gibraltar. So when you said, "Where were you," we were someplace in the Atlantic. They got orders to move to the Atlantic coast and unload us. So I was back in the States, and I had another year to serve. I didn't know how long I had to serve; as it turned out, it was another year. So, first, because I came from California, they just gave me orders to go home and then to--

Swent: Where did you disembark?

Wiegel: It's just down below Washington, D.C., in Virginia. Hampton Roads, I think it was. Then, on a train, I got back.

Swent: Were you married yet?

Wiegel: No.

Swent: So you took the train all the way across the country?

Wiegel: Yes. I don't know whether it was a train--I remember arriving out here. It might have been air on a stand-by basis. It must have been air on a stand-by basis because it was too quick. Then they put me in an army camp that had a whole bunch of German P.O.W.'s [Prisoners of War] in the Stockton area--all prisoners of war that were all working with sugar beets and in the potato fields, harvesting them. I was there for a few months and then they shipped me back to Fort Dix, New Jersey, which was a big base.

Swent: What were you doing with the P.O.W.'s?

Wiegel: Army officer--somebody had to be in charge--we had soldiers as guards for them. They worked on the farms; they were farm laborers.

Swent: You were a supervisor?

Wiegel: I had seven hundred in my group--

Swent: Nothing to do with your training?

Wiegel: Absolutely nothing except that you were a lieutenant and a lieutenant was supposed to work through a couple of sergeants, and it works. It does work. Then I was shipped back to New Jersey--Fort Dix--and there I got back into ordnance again. I had the motor vehicles on Fort Dix. Then I got sick and ended up in a recuperation hospital in Florida for a month. It was hepatitis. Every time you turned around in the army you get an injection, and they were pretty sure--because there were several cases--they thought it must have--in those days nobody knew much about it, so the treatment was just--you took it easy, no fatty food or anything like that.

After the recuperation hospital, I received my separation from service. I had a couple of months of accrued leave, so I was still on active duty, but detached until you served out the length of time that you had coming to you from accrued leave. But they just said, goodbye, and that was it. I came back here, and that's when I went to visit Professor Folsom here on campus to see about going to graduate school.

Swent: You came back to live with your mother?

Wiegel: Yes; came back home.

Swent: And came over here to talk to Folsom?

Wiegel: So that would have been June; I know I started to work on June 20, 1946. That was the interesting thing too, because I was on terminal leave, but I got a job at the university--it was a strange thing, the military said, goodbye. They gave me a check up until the end of my leave.

Swent: You were free to do whatever?

Wiegel: Whatever I wanted to do. At that time, everybody wanted to come back to college. Most of them wanted to finish. I had gotten my Bachelor's degree, but I wanted to go to graduate school, and that's why I saw my former advisor, Professor Folsom.

IV RETURN TO CAL, 1946: RESEARCH IN COASTAL AND OCEAN ENGINEERING

Full-time Research Employee

Swent: Why did you want to go on to graduate school?

Wiegel: I don't really know; I just decided I wanted to go to graduate school. I actually was thinking about what nowadays we call the M.B.A.--they didn't call it that. But it was called the School of Commerce, and I was actually thinking of doing that, but my sister--the middle sister, Wilma, who was a secretary at Shell Oil--and her boss was the head of manufacturing at Shell; that is all of the refineries. At that time it was Monroe Spaght.

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Wiegel: He said it would be better to get an M.S. in mechanical engineering. He had a Ph.D. in chemistry from Stanford. He was from Eureka. He eventually became managing director of the entire worldwide Shell Oil complex in London. So I got good advice from the person who obviously knew. [laughter]

Swent: Yes, indeed.

Wiegel: It was just one of those fortunate things. So when I came on campus, I came to see my advisor in mechanical engineering, and he said, "What are you doing?"

And I said, "I'm on terminal leave."

He said, "Would you like a job for two weeks?"

I said, "Yes." It was a classified job on the Bikini test, and I've been here ever since. I started the 20th of June, 1946, and I'm still here on campus in 1997. I was hired fifty years ago last summer.

Swent: Last June. And it wasn't anything you intended, it just--

Wiegel: Nothing was planned. I cannot tell you why I decided I wanted to go to graduate school; I have no idea.

Swent: But it turned out to be fortunate.

Wiegel: I'm very pleased that I did. It's been a very interesting and pleasant life.

Swent: And Folsom was still here, and had been one of your favorite teachers?

Wiegel: He was a tough person.

Swent: Was he?

Wiegel: Yes. But he left a little later, and went to [University of] Michigan as the head of engineering research, and then he became president of Renselaer.

Swent: Renselaer Polytechnic?

Wiegel: Yes, Renselaer Poly; he was president of it. Incidentally, the library there is named after him now, I understand. He died last year. I kept up with him all these years. We've been good friends all these years.

The Bikini Test: Would Operation Crossroads Cause a Tsunami?

Swent: So the Bikini job--I think we have time; would you like to talk about that research project? Your first one.

Wiegel: It is interesting. Several of the people involved in the Bikini Test from Berkeley and from Scripps Institute of Oceanography--which is a part of the University--were on Oahu as a part of the staging for the tests because it took months to get everything down there and installed.

On April 1, 1946, there was an earthquake up in the Aleutians, which--now we know--triggered a massive underwater landslide--triggered what we call a tsunami, popularly called tidal waves, and it caused a lot of damage and loss of life in the Hawaiian Islands. Remember this was in the Aleutians that it was triggered. The tsunami waves also caused damage along the north coast of California. They were worried that a nuclear explosion might well trigger an underwater landslide on the ocean side of the atoll which would generate a tsunami, which might cause devastation.

Let me back up: about ten years ago, I was having lunch in southern California with Roger Revelle. Roger was largely responsible for the start of UC San Diego. Roger was also the person that most people give credit for as thinking about the problem with carbon dioxide and the greenhouse effect. He's the one that is generally given credit for that. But he was a navy commander during the war, and he was the contact with the people--and this is an area we'll get into much detail later--at Scripps and Berkeley on the amphibious research, but he also headed up the oceanographic segment of the science thing for the Bikini test.

He was talking with O'Brien at Bikini, and was worried about this. O'Brien said, "I'll send a cablegram back and we'll get a test done, because I know that we can do something in the laboratory." That's when I happened to walk into Folsom's office. He had just got this cablegram from Bikini, and O'Brien was the dean of engineering here at the time, but he was at Bikini for the test. I walked in the door, and I did the job--

Swent: So this was a rush job?

Wiegel: I had two weeks to do the whole thing: figure out what to do, set it up, run the experiments, analyze the results, say whether it looked like it was going to be dangerous or not dangerous.

Swent: What a responsibility!

Wiegel: For reasons that it took years to find out, what I did was right. [laughter]

Swent: Fortunate.

Wiegel: But O'Brien knew it probably would be, because he had the background to realize it.

Swent: So you had to know all the structure of the atoll and so on, didn't you?

Wiegel: No, nobody knew very much of anything about that. I just made an equivalent of something like fractured rock, and put it in an unstable situation, triggered it so it went down--it was in the water--it moved under the water, and it generated water waves as it went out, and I could measure them and then scale up to the prototype. Of course, now it's very clear, but it was not at all clear in those days, that it wouldn't be very important, because you had to have something with much larger length than the water depth in order to generate waves that would be of ocean-wide importance; it would only have local importance.

Swent: So you decided it was okay?

Wiegel: I said, "It scales up small." They decided it was okay. [laughter]

Swent: I see.

Wiegel: So it was just a complete accident that I was employed at Cal. (I was, however, a reader for one semester when an undergraduate.) I don't know why--they had me on the payroll--they had other work to do, so I just did it. In other words, it was just for two weeks, but once I was on the payroll, there were other things. It was all classified in those days. It was all very interesting. Because of this, I didn't go back to school as such. They had a deal that a university employee could take a course for credit each term if he or she was accepted, and I had been accepted. So I worked full-time and then took a course, and then another course, and then another course.

So I never came back as a student as such, I simply took one course at a time. Then at the end, I was sent away before the last semester ended, but I finished my Master's. That's why I didn't get a Ph.D., because I was sent by Dean O'Brien to do something else for the Marine Corps, and I spent two years. By then, I wasn't even interested in any more advanced work.

Swent: What sorts of experiments--after the Bikini one then--were you doing research?

Wiegel: I did field measurements, and measured waves, worked on the design of wave recorders. I was also loaned for an interval to Professor Harold Johnson, who was doing work in high speed aerodynamics of what we call rockets now--in the atmosphere on the heating and I worked with him. I did some of the first tests and calculations on that. But then I went back into oceanography, but I did do some of the earliest work on the aerodynamic heating of rockets--re-entry vehicles--whatever you want to call them.

In those days we were worried about getting them out because it wasn't anything to do with manned flight; it was straight military applications. So that's how I did it, and eventually I did get a Master's degree, but then I was sent to head up this research project on amphibious oceanography.

Swent: That was down the road a ways.

Wiegel: That was in December '48. Anne came over to visit in 1948, and we got married.

Swent: But you started here in '46?

Wiegel: In two years, I was working full-time, but I managed to take enough courses to get my master's degree. I've always been a hard worker.

Swent: Were you instructing students as well?

Wiegel: Not then.

Swent: You were just doing research?

Wiegel: I had no intention to go into teaching; never entered my mind.

Swent: Oh, really?

Wiegel: No. That all came by complete accident at a later date.

Swent: You were doing research and experiments?

Wiegel: And a lot of field work. A certain amount of field work. Then, when I went down to Camp Pendleton with the Marine Corps, it became all work in the field, which is very important because--this was the real thing.

Swent: That's the real thing.

Wiegel: That's one part of the real world, yes. That was very important, because that has always given me a different way of looking at research, both laboratory and theoretical. I'm very pleased that it went that way.

Swent: We might expand just a little bit more on Bikini. This was research that was done by the government and the university in collaboration?

Wiegel: Operation Crossroads. It was the big nuclear device in Bikini Lagoon, and they had many, many naval ships and craft: LSTs, LVTs, aircraft carriers. They were in the lagoon, and they exploded the device and studied what happened to them. We were interested in the size of the waves; the base surge that generated waves that moved out, and how it impacted the ships, and then how it overran land.

It's a huge amount of water, and it moves out. It turns out mathematically that it's in many ways similar to the generation of tsunamis, so there was a classified segment of stuff that always went with this kind of thing for quite a few years.

Swent: I was interested too in the collaboration between the university and--was it the army or the navy?

Wiegel: That was navy. The Bureau of Ships at that time--the university's connection was with the Navy Bureau of Ships. That was the nuclear device test, Operation Crossroads.

Swent: Because I think this was sort of the beginning of such collaborations, wasn't it?

Wiegel: It started during the war.

Swent: Before the war, universities didn't do military research, did they?

Wiegel: MIT [Massachusetts Institute of Technology] might have. Somebody must have been working on the radar and stuff. Somebody must have been doing that; my guess is that it was MIT. Berkeley--it was during the war, and it was before this; this goes back to the amphibious work and the generation of waves by winds; the prediction of surf; how high will it be in a landing?

Swent: --for planning invasions.

Wiegel: --for invasion, and what will be the effect on landing craft? The effect on landing craft--a good deal of that was done at Berkeley. We had a little model towing tank here at Berkeley. But also, they were out measuring waves--measuring surf, surveying profiles through the surf zone--because nobody knew anything about this. Landing craft would come in, and suddenly would hit bottom and would be inundated by--if you turn around, and look at the wall, you'll see two photographs: at the top is a Marine Corps amphibious tractor that has been waterproofed. That's an amphibious tractor coming in through the surf. Down at the bottom, you see what they call a DUKW, which was a two-and-a-half-ton truck by General Motors--but a boat, also.

By amphibious, it can work in the water or on land, and can move between the two. That's the sort of stuff that was being done at Berkeley--how were these things behaving and why, and I spent quite a while working on this. Berkeley had--there were only four schools of naval architecture and marine engineering in the United States, and one of them was at Berkeley, one at MIT, one at Michigan, and a private one on Long Island. So we had a little bit of background here at Berkeley in the naval architecture stuff.

Swent: So when were you working on this?

Wiegel: That was--

Swent: When you were an undergraduate?

Wiegel: No, that's when people like O'Brien and Folsom--

Swent: But these were developed--

Wiegel: This was afterwards. I worked on them after the war.

Swent: Because they were used in the war.

Wiegel: Oh, yes. The Marine Corps wanted to know much more about how they behaved.

Swent: They were used, but they didn't know what some of their characteristics were, and how to improve them.

Wiegel: They lost a lot of people; and they were killed not by the enemy gun fire--there were a lot of people killed by enemy gun fire--but a lot of people were killed by straight accident of these operations, turning over in the surf and were drowned.

Swent: By what kind of accidents?

Wiegel: Just accidents; capsized, and they would be trapped and drowned. That's what the Marine Corps wanted to find out after the war.

Swent: Why that was happening.

Wiegel: Why it was happening, how they could design better techniques, better equipment. This then got into the development of helicopters; maybe the best thing is not to go through the surf, but to fly over the top. That's a whole other phase; that's after the war. During the war, some people at Berkeley like O'Brien and Folsom and Joe Johnson (who was working on underwater explosions) were working on--and another name, John Isaacs--he was older, came back to school and went through in three years in civil engineering--but he's the one that developed most of these techniques for measuring things in the actual surf. He had been a commercial fisherman as a part of his younger career.

But that was the war stuff. After the war, the Marine Corps wanted to find out much more about it. But that we'll get into later, because ultimately I was the project engineer writing a seventeen-hundred-page manual on amphibious oceanography for the Pentagon. We've kind of gone ahead now, this was done after the field work for the Marine Corps, after we had returned to campus. I never intended to teach; it never entered my mind. It came about at a later date.

Swent: Serendipity.

Wiegel: So I was doing all this between '46 and '48, and that's when Anne came over to visit my mother. Then we got married--about a week later, Dean O'Brien sent me out to head up the project for the Marine Corps. That's when the semester system--you finished lectures before Christmas, but then came back after Christmas for examinations. I was sent before Christmas--I know I never took a written examination in the last course; I think the professor must have just examined me orally or something. It's hazy. Anyhow, I finished; I got my master's degree.

Swent: This might be a good place for us to stop, although I do have more tape if there's anymore.

Wiegel: This seems to be a reasonable place to stop.

Swent: Let's just talk about your children for a few minutes.

Wiegel: Yes. We have three children, and they all graduated from Cal.

Swent: So that's two generations of 100 percent Golden Bears.

Wiegel: Right. I want to be sure that's in there.

Swent: Let's get your children's names.

Wiegel: The oldest is John Mansfield; he graduated--I can't remember the dates. He's just forty-one this year; he graduated in chemical engineering. My eldest daughter, Carol, graduated in forestry, and she's thirty-eight this year. My youngest daughter, Diana, graduated in economics, and she's thirty-five. Then later on, she went and got an M.B.A. at UCLA [University of California at Los Angeles]. Anyhow, all three were Cal grads.

Swent: That's wonderful. John Wiegel: he would be the third?

Wiegel: No, because there were different middle names. Our son is John Mansfield, after my wife's older brother.

Swent: John was your grandfather's name also, and your father's.

Wiegel: No, my brother's name is John, but he was always known by his middle name, Mason.

Swent: So you've continued that tradition? And he's also an engineer?

Wiegel: Chemical engineer.

Swent: That's wonderful. I think perhaps that's a good place for us to stop. You're going off for three weeks in Hawaii.

Wiegel: Maui first, Kauai for a week, and then the Kona coast of Hawaii.

Swent: And you've done this many times?

Wiegel: Yes.

Swent: It's a tradition.

Wiegel: It's a tradition; we've done it--I won't say every year since I've retired, because I was recuperating from open-heart surgery two years ago. Didn't go any place then.

[Interview 2: February 4, 1997] ##

Swent: We're beginning the second interview, and you're recalling being at Berkeley beginning your research work. Perhaps you would like to talk about the Richmond Field Station.

Wiegel: Well, the Richmond Field Station didn't come into existence until several years after the war. I think it's best to talk about it at the appropriate time, and start in what I did when I first came here.

Swent: You were a graduate student at that time.

Wiegel: I came here to apply for graduate school, and was offered a job to do for two weeks--a rush job, which I mentioned earlier, and then I was asked to do something else, and then something else. I became a graduate student, but not in a normal sense. I was a full-time research employee, and at that time--maybe still--a university employee can take one course at a time as long as he or she meets all the criteria, and that's what I did. So even though the G.I. Bill [Servicemen's Readjustment Act, 1944] was in effect, I made almost no use of it whatsoever because I was a full-time employee, and I believe the tuition at that time was only something like twenty-seven dollars a semester. So it was not really important. So I was working full-time.

Swent: In reading Morrough O'Brien's oral history--when he first started out, there was only one student in the graduate school, and then I believe there was a huge burgeoning after World War II, wasn't there?

Wiegel: When I started to do the graduate studies--and it would have been in the fall of 1946--there were not many graduate students at that time because they had this big bulge--a tremendous demand of returning veterans to finish their undergraduate degrees, or, because of the G.I. Bill, start with their undergraduate degrees. So there were very few graduate students.

Swent: Still?

Wiegel: Yes.

Swent: Do you recall how many there were?

Wiegel: I can't remember. The college would have all those statistics. There were very few, that's all I can remember. I was a graduate student in mechanical engineering, and I believe I knew all of the other graduate students in mechanical engineering. I think there were only a few dozen; it's something that small.

Wave and Beach Research for the Bureau of Ships

Wiegel: I joined, then, this group that were doing the wave and beach observations: the study of landing craft performance for the United States Navy Bureau of Ships. This was a continuation of the war-time activities that was done in support of the amphibious landings in both the Pacific, North Africa, and across the channel into France. We were doing field work, so I would go out, and we were doing surveys of beaches and using the DUKWs, which is an amphibious truck. It was called WOBS (Wave Observations, Bureau of Ships); then the WAVES project.

Swent: Where were you going?

Wiegel: Well, the group had been doing studies in California, Oregon, Washington. The first one I worked on was surveys at Half Moon Bay in California. Then the university was asked to observe a major rehearsal at Camp Pendleton of the amphibious landings--it was a rehearsal or practice by the Navy Pacific Amphibious Command and the Marine Corps--and so I participated in that and went down to Camp Pendleton with a group of engineers and others from Berkeley.

We observed the observations and made measurements--more as an operations research activity. Some of the photographs that you see on the wall of these landing ships and things of that sort were taken of these rehearsals.

Swent: You are an expert photographer. When did this begin? When did you start taking pictures?

Wiegel: I'm not an expert photographer. I learned that if you take a lot of photographs, occasionally you get a good photograph. [laughter] There's a big difference.

Swent: I've seen some very nice photographs that you took.

Wiegel: That's because I took many, many photographs and occasionally got some very nice photos.

Swent: There were some of Half Moon Bay that were in one of the publications that you had taken.

Wiegel: Yes. Some of these you see on the wall I took; some, other people took. Willard Bascom (he was known as Bill then) took some of them, John Isaacs took some of them, Don Patrick--many people took different things. We had a very large collection of photographs which are still around. I'm afraid they'll get lost one of these days. Those things happen.

Security Clearance

Swent: We'll have to talk about your archive later. Maybe you'll keep them there. Did you have to get clearance for these things?

Wiegel: I had clearance. Yes, it was a classified contract. We all had clearance.

Swent: You went through a clearance procedure?

Wiegel: Yes. I don't remember what the procedures were at that time, but certainly we all had clearance.

Swent: The reason I'm asking this particularly is because in John Whinnery's oral history,[1] the question was asked about the furor over the loyalty oath later, and he said it didn't affect these people because you had all clearance and gone through that anyway. It was a matter routine to--

Wiegel: We never even thought about it. Many of us had been in the military. We understood the necessity for confidential work for military sort of things. What we were doing eventually became generally useful worldwide for civil works, but we were doing it for military operations at that time. So every one of us were cleared. It was a routine check-up; I don't remember the details.

Swent: Were you able to discuss what you were doing, for instance? Did you tell your wife where you were going?

Wiegel: Yes. There are different levels of clearance. The fact that we were working on things was not classified. The details of some of some of these things were classified. But within, I would say, a couple of years at the most, they declassified most of them. It's only later on when I started to do some work on the preliminary studies of the Polaris missile launchings from submarines that it was very high classification. That you couldn't even talk about at that time. You couldn't even say you were doing anything. But that was a very special thing.

Beach Trafficability, Liquefaction, Surf Forecasting

Swent: We'll have to mention that in more detail later. At this time, the landing craft and the DUKWs--you were observing the waves?

Wiegel: Waves--how they affected beaches, how the landing craft operated when they were coming in through the surf, how they operated on the beach face, how the vehicles behaved as they were coming off the landing craft and moving up over sand. In fact, it's rather

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[1] John R. Whinnery, Researcher and Educator in Electromagnetics, Microwaves, and Optoelectronics, 1935-1995; Dean of the College of Engineering, UC Berkeley, 1959-1963, Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 1996.

interesting: historically, one portion of it developed into what is known as beach trafficability. Too often a vehicle would unload from the landing craft and get stuck in the sand--couldn't move--and they'd just clog up your beaches. After studying it for a while, it became evident that the problem was largely what we now call liquefaction.

Harry Seed and Others: The Synergy of Sharing Ideas at the University

Wiegel: In the early 1950s, we had a special contract with the navy--not part of the broad studies--but a special contract to look into this beach trafficability. Professor Horonjeff was the principal investigator, and I was a member, and a young assistant professor of civil engineering by the name of Harry Seed was on it. Harry Seed went on to develop most of the theory which is used worldwide on liquefaction, which is a major problem of some earthquakes.

Swent: Now this gets into geology and seismology.

Wiegel: It gets into geology, seismology. I could get into that later, but Harry Seed is important because he eventually received the National Medal of Science for the work that he did in soil mechanics, earthquake loading, and especially liquefaction. It stemmed from these studies he made for this military problem. Now we can go back because that happened at a later date, but I wanted to be sure we get that in.

I think I'd like to say in general that looking back, I've really been fortunate with the remarkable type of people that I have worked with, both with faculty, other engineers, and then the high quality students that we've always had. This all works together. This is synergy. You don't do these things by yourself; everybody talks to each other, you share ideas, you talk back and forth, and sometimes, you're never completely sure where something came from because each of you adds a little bit and piece and modify it and so forth. That's one of the reasons this university became great; there was this large group of people that were very, very good. Harry Seed, of course, being one of them.

Swent: That's the beauty of the academy, isn't it?

Wiegel: Yes. Of course, the problem is now we're getting to where many of them are deceased. Both Seed and Horonjeff died some years ago. Folsom, O'Brien, and Isaacs are also dead now.

Swent: I thought that they laid out metal screening on the beaches?

Wiegel: In some places, but it didn't work very well. They bent down and wound up and it didn't work very well.

Swent: No. It was impractical.

Wiegel: The other one I worked with at that time on these things in addition to O'Brien--of course O'Brien was dean so I'd see him very, very rarely--but it was Professor Joe Johnson; John Isaacs--who shortly afterward left and went down to Scripps Institution of Oceanography, which is a part of UC San Diego. Well, then there wasn't a UC San Diego; it was a part of the university. He spent the whole rest of his professional career at Scripps. Then Willard Bascom went on to do a lot of the early work on the deep sea drilling project, which is a very, very important scientific advancement.

Swent: You mentioned at that time this was under contract with the Bureau of Ships--

Wiegel: Bureau of Ships, that's correct.

Swent: So it was entirely a military--

Wiegel: It was all military at that time. It's just that almost all of the things that we learned turned out to be very useful for civilian applications and still are. Part of it was the study of how waves are generated by winds blowing over the ocean, how they're generated by ships moving across water, how they're generated by underwater explosions, how they're generated by underwater earthquakes and landslides and all of these things. So how waves are generated and how you can predict them was and is important for the military--to be able to predict changes in wave conditions and not just the waves in the deep ocean, but the much more complicated as they come onto shore and form breakers--what we call surf.

So it's surf forecasting that was an important part of it, and the characteristics of these waves: how they're generated, how to forecast them, and what are their characteristics. It became evident that one had to deal with statistical representations. So this got us into probabilistic theories and things of that sort.

Swent: And this is fluid mechanics?

Wiegel: It's applied to fluid mechanics, correct. We were fortunate at Berkeley--and it was a new professor who was an older person, Jerzy Neyman--one of those unbelievable Hungarians, I believe. He had formed what is now known as the statistics department in mathematics; it was separate. So one of his graduate students, Robert Putz, worked with us on these problems to really get into making use of the advanced work that was done during World War II in communication theory, and to use that background for the statistics of waves. So again, it was one of these fortunate things that these people were here. We could all talk to each other and work together. It was a wonderful time. Just like Professor Whinnery talked about in his field; what a good time it was to talk to everybody in related areas.

Swent: So you were actually measuring or observing the waves--

Wiegel: Measuring them. I did some work on the design of wave instruments under Professor Folsom and John Isaacs. The person that did most of this was another young person--he was just out of the navy--Frank Snodgrass--who was doing a Master's degree in electrical engineering. He was the one who developed the advanced wave recorders and analysis equipment. But also working in conjunction with him was Neyman's graduate student, Robert Putz. So it was not just measuring waves; it was the analysis.

We did much of that here at Berkeley, which then of course was taken over; other people did it and everybody adds to it both here and--a lot of this was still being done in the UK in England by that group there. We knew each other; we would write back and forth and share all of our ideas. There wasn't competition for publication in those days. You shared ideas. You were trying to solve a problem.

Swent: And this was a very new field, wasn't it?

Wiegel: Yes, it was brand new in many ways. In other ways, it's a very, very old field. Read the Bible and St. Paul--

Swent: Shipwrecks.

Wiegel: --and his travels in the Mediterranean, and you'll recognize many of these problems: trying to land a ship on shore under bad sea conditions. So it's a very old problem, but a very new way of trying to study it. [laughter] It's still fascinating. We did then, you see, field work--field measurements, and we worked in the laboratory.

College Avenue Pool a Hydraulic Model Basin

Swent: I wanted to ask about the College Avenue pool. Was that part of this?

Wiegel: Yes. The College Avenue pool--when we did field work, we worked in the hydraulic laboratory and we did theoretical studies as well, and the College Avenue pool was originally a swimming pool on the extension of College Avenue, which doesn't exist any more because that was then a part of campus--that's gone. College Avenue--you know where it stops now--well, it continued. It jogged up and continued on to what is now part of campus.

There was a swimming pool there, but sometime during the 1930s--the Hearst swimming pool was built for women, and then there was one down in the men's gymnasium, so there was this earlier pool left. Fortunately nobody had bulldozed it; I guess there was no money to do anything like that, and so it was turned into a hydraulic model basin. Also, a small towing tank had been built there. This was during the war. I had nothing to do with that; it was prior to my coming.

Swent: A towing tank?

Wiegel: A towing tank, to tow ship models. So that's where some of the early studies of amphibious vehicles were made for the navy--being developed by a company then known as Food Machinery Corporation. They were developing these amphibious vehicles; these tractors, one type of amphibious vehicles. They were studying the performance of various models in the towing tank which was right adjacent to College Avenue, next to the pool, and then some three-dimensional studies were made in the College Avenue pool.

Swent: How valid was this?

Wiegel: Well, many of these things were valid. Others had scale effects, but it was quite a while before we found out about scale effects, quantitatively. It wasn't until we had a full department of naval architecture and marine engineering at Berkeley, which was developed after World War II. The lead of that was a retired navy commodore, which is one star, and he was a naval architect--Henry Schade, called "Packy". Then they built a big towing tank because they had just obtained Richmond field station at that time--I don't remember the date--but that's jumping ahead.

Swent: Were you working at the College Avenue pool at all?

Wiegel: Just a little bit.

Field Work and Theory; Engineering and Science

Swent: Mostly you were doing the actual field work?

Wiegel: Field, and things of that sort. Correct. So we did field studies, hydraulic laboratory work, and theory. We did research, but we also did practice. We were trying to develop better ways for these things to perform. So it was a combination. This is very important, I think--that I had this opportunity to spend so much time doing field work, because during my whole career I've gone out in the field almost every opportunity that came to me. To see things as they actually are and work with full-scale things because many of the things in the laboratory do have what we call scale effects and I was able to observe some of these things. That's very important in engineering. Very important.

I think I'd like right here to mention--because both O'Brien and Whinnery had mentioned about what similarities and what differences there are between engineering and science. Like John Whinnery said, "It's really a spectrum and at the two tails of the spectrum, they're very different. In other words, the real scientist scientists at one end, and a type of engineer that knows almost no science at the other. But most engineers and scientists are in the middle of the spectrum where you do both. You can't separate them."

Swent: There's a good deal of overlap.

Wiegel: Tremendous overlap. One of the things about engineering is that we so often have to deal with uncertainties. Somebody wants something done, and we know approximately what's going on, but there's all kinds of variabilities. So we're always dealing with these uncertainties, and we're trying to estimate: can I build something that will do something that is safe, and I'm not going to lose people and they're not going to be killed, it's not going to fall down. But what kind of safety factors? I think this is one of the main things in engineering, and of course a lot of scientists do this same thing; they're interested in the same thing.

But another factor in engineering is the economics. If something costs too much, somebody's going to say, "Hey, wait, I don't have that kind of money. It's all very nice, but I can't afford it." So this is a difference in many cases, but on the other hand, many scientists have the same restraints. They'd like to perform a test, and somebody says, "Hey, wait, that costs too much money." So this is why I say there's many, many overlaps.

There are two types of design in engineering: functional and structural. This is important--how do you design something functionally, so that it can perform its function. As an example, the design and the layout of a harbor and the protection of the docks from waves so ships can be loaded and unloaded.

Improving Risk Analysis and Probabilistic Models

Swent: Then you mentioned the probability--probabilistic I think was the word you used--

Wiegel: Nowadays there are two terms that are used: risk analysis and probabilistic models. These are two pieces of the same thing.

Swent: Now is this new wording for an old thing, or is this really kind of a new way of looking at things?

Wiegel: It's an old, old thing, but it's much more quantitative now. In other words, when I say "we," I mean people that have this ability--I'm using the very general term "we"--to be able to have really quantitative estimates--what they call distribution functions of these occurrences. Like the distribution functions of floods, or the distribution function of wave heights, or the distribution functions of earthquakes. In other words, the probability of occurrence of these things.

Then in the risk analysis, what you're doing, you're applying this to some other thing to see how this wave height--and the uncertainties in the wave force equation--what kind of a loading it's going to cause on a structure--what load will a higher wave height exert on a structure. Then you come up with an analysis of the loading on the structure and the risk that it might fail. Under wave loading or earthquake loading, or wind loading. So the probabilistic information is in there, but then the risk analysis, in my opinion, is combining the probability of occurrence of different things with how it will affect a structure or an operation. So they're old, but we're doing much better now quantitatively and presumably will continue to improve our quantitative ability.

Now to get back onto a schedule. In late December 1948: O'Brien asked me if I would go down to the US Marine Corps base at Camp Pendleton in southern California and take over as the project engineer on a new study that the university had contracted with the Office of Naval Research to do on amphibious oceanography. This was to work with Marine Corps personnel and equipment and run tests through the surf under different conditions and at different areas, and again, find out what was happening.

Swent: Had you gotten your master's by now?

Wiegel: No.

Swent: You were still plugging along.

Wiegel: Let me explain: At that time, we were on the semester system, but the semester continued after the Christmas break. I was sent down to Camp Pendleton, so--I don't know--I had this one last course I was taking and whether or not the professor gave me on oral exam, I'm not quite sure, but at least I passed. You did a pretty detailed master's thesis then, and I handed my master's thesis in, and I did get a degree, a master's degree.

Swent: What was the topic of your thesis?

Wiegel: Waves--in the laboratory--and using a resistance type wave gage suggested to me by Professor Einstein, who had just come to Berkeley from Caltech--that's our professor Einstein, not his father.

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Wiegel: Einstein worked on sedimentation, and did what is generally considered to be the major theoretical advance in the transport of sediments: that's silt, sand, cobbles by rivers. This is used worldwide today. Then, when he came here, he started to apply some of his ideas to the transport of sand along beaches and things of that sort.

Swent: Were you observing that as well as waves?

Wiegel: Yes. Professor Johnson did most of the work on sedimentation. Einstein worked in it also. Then another person came--he was an older person--Professor Trask, who was a geologist. He started what is known as geological engineering at the University of California at Berkeley. He was brought here by O'Brien especially to do that. He had done much work on sedimentation. His undergraduate degree was in mathematics, and he was, I think, either the first, or one of the first to apply statistical theory to the study of sediments. Again, that was an advantage. At a later date, I shared an office with him for a couple of years, which was great because I was able to learn from him how a geologist thought about these things. But that's jumping ahead.

Project Engineer for Wave Research with the Marine Corps

Swent: So you went to Camp Pendleton?

Wiegel: Went to Camp Pendleton, and my wife--Anne and I got married in early December--then moved immediately to Camp Pendleton. As I said, I did somehow wrap up whatever I was doing and got my Master's degree, but that was the end of my graduate work. I had no intention of teaching; it never even entered my mind. I was going to continue working for a while on amphibious oceanography because it was interesting, and then go out in industry someplace.

Swent: What opportunities would there have been in industry?

Wiegel: Well, I was interested in the oil companies, and I would have applied to Shell Oil or Chevron then called Standard Oil of California.

Swent: Were they doing drilling platforms and that sort of thing?

Wiegel: Not then, that was shortly afterwards. A little later, I would have been able to work for any of the oil companies.

Swent: At that time, I was wondering what might have been your options?

Wiegel: Remember I was a mechanical engineer, so I was thinking in terms of refineries or things of that sort. But what I was doing was so interesting that I just forgot all about it, to be frank. I don't think I even thought about it until you asked the question, in all those years. [laughter]

Swent: This would not have related directly to any oil occupation, would it?

Wiegel: No. So I worked with the Marine Corps in landing craft. Captain Bill Lund was the Marine Corps captain we worked most closely with. But we had two navy officers: a Lieutenant (j.g.) D.A. Patrick of the Civil Engineer Corps was assigned to it, and then a Lieutenant Harold Kimberley, who was what the navy called an aerologist--today we'd call him a meteorologist. Again, we were doing surveying and wave forecasting. We had wave recorders, we were installing them and measuring with them. We were measuring landing craft performance by putting up two photographic survey towers, with cameras that were really photographic transits so we could get the information in three dimensions (by photogrammetry) on the performance of landing craft under different conditions.

We started at Camp Pendleton, but then we moved up to Monterey Bay in a LST [landing ship tank] the navy assigned to the project, and all of our equipment, the Marine Corps, and the amphibious vehicles were on it, and we were in Monterey Bay. We also had a Marine Corps aeroplane for use in taking aerial photos, which was flown up. In those days, you just did something, so we just landed and beached the LST at Monterey Bay right near where now is the Naval Post-graduate School.

Swent: It wasn't there then?

Wiegel: It wasn't there then. It was still Del Monte, but the military had it because they used it during the war--the hotel. But we were on the beach, not the hotel. We were there near Fort Ord, and operated in Fort Ord, which is a very, very difficult beach and surf conditions to operate at. From there, we moved up to Oregon, at Clatsop Spit. That's immediately south of the mouth of the Columbia River. That was, you might say, the absolute opposite condition of Fort Ord. Fort Ord is a very steep beach and Clatsop Spit is a very, very flat beach. To give you an idea; the slope of the beach in Fort Ord may be one foot vertical and ten or fifteen horizontal; at Clatsop Spit it's something like one in 150. So there is a tremendous difference in the type of surf.

But this is what we were studying: how would vehicles perform, what was the surf like under these different conditions. Again, it was this opportunity to study things in the field as they actually are. I've been very fortunate to have been able to do that. Then we came back to Berkeley--just the two of us--Lieutenant Patrick of the Civil Engineer Corps was put on detached service or whatever they call it, and he and I then did the next stage of trying to wrap up the reports on what we had done for the Marine Corps..

Operation MIKI/Manual of Amphibious Oceanography

Wiegel: In October 1949, we participated as observers of a practice amphibious landing [Operation MIKI] on three beaches of Oahu [Territory of Hawaii then], taking many photos to document the beaches, surf, and landing craft performance. It was a full operation with an army division (2nd Division, I believe). Rear Admiral Doyle was Commander of Amphibious Forces, Pacific at that time. Johnson, Bascom, Patrick, and I went on this study, and we prepared several detailed reports on it for the navy and marine corps.

The Marine Corps then wanted us to prepare what they called a manual of amphibious oceanography, and I was asked to be the project engineer and editor of it. This turned into a 1750-page manual which was printed by the Pentagon printing office. I think it was in 1952, and it was classified at that time, but then later they declassified it.

Swent: That was a long three years or so.

Wiegel: Something like that. Yes, there was a lot of work that went into it.

Pioneering Research on Seakeeping Capability of Ships

Wiegel: The navy, at the same time--I guess it was the Bureau of Ships--were funding a hydraulic model study at Berkeley of a landing craft in the surf--that is, waves breaking on a beach in a hydraulic model. Professor Harold Iversen was in charge of that project. So we then started, by design, not by accident--to compare what we had observed in the ocean with measurements and what he and a couple of graduate students had observed in the laboratory. We really made some pretty good advances and when Commodore Schade came to start the department of naval architecture--he came at the invitation of Dean O'Brien--he said this is the first time that he had ever seen details of the naval architectural seakeeping capability of a ship. It was new; nobody had ever done it before. So those of us in mechanical [engineering] who shortly after became civil [engineers], were working very closely with this new group in naval architecture.

Curtis Crooke, Later of Global Marine

Wiegel: One of the graduate students that was doing this was a young person by the name of Curtis (R.C.) Crooke, and I mention his name in particular because after he had finished, he went to work for a small company that had formed in southern California to do off-shore oil drilling. The name at that time was Continental, Union, Shell, and Superior: CUSS. But then they changed and became known as Global Marine, and Bob Bauer was chairman of the board of Global Marine. They developed into one of the two major off-shore drilling companies worldwide. I mention this in particular because at a later date--by then, Curtis Crooke was senior vice president for development. He was still pretty young. Everybody was young in this field; there wasn't anybody old. He was the one that was in charge of the very, very secret program that developed the Glomar Explorer, which was the--I have here a newspaper article--I pulled it out--"ships that came in from the cold"--you know, from the spy episode. That was the ship that either did or did not lift a Russian nuclear submarine off the bottom someplace near the Hawaiian Islands, and that was Curtis Crooke.

He also, on a non-classified project, was largely responsible for the Glomar Challenger, which was the drill ship developed to take the deep-sea cores that have led to remarkable advances in plate tectonics and all of this stuff--by taking these cores all over the world. It was a very interesting group of people that were working in this area.

Development of The Morison Equation

Swent: Yes. The results of all of that have been tremendous.

Wiegel: At that time, there were other ones here: I mentioned Frank Snodgrass and Bob Putz--but there were also other graduate students, Bob Fuchs and Richard MacCamy in mathematics, did their Ph.D.s with Hans Levy in the math department. Then Thorndike Saville, Jr., who did his master's degree, and then Jack Morison--one r, one s--and Jack was the one that did his work under O'Brien on the wave-induced forces on piles. I mention this because that equation is known as the Morison equation. It should be known as the O'Brien, because O'Brien wrote it down and said, "Here, test this." It should be known as the O'Brien-Morison-Johnson-Schaaf equation. We all agree that it should have those four people's names on it.

But it's known as the Morison equation; it's still used worldwide. As far as we know, it has been used in the design of every single pile-supported--or what we call space frame [fabricated with steel tubes] off-shore oil structure in the world. I don't care whether it be Japanese, Korean, U.S., French, Norwegian, British--but every single one uses what is known as the Morison equation.

Swent: What were the four names? O'Brien, Morison, Johnson--

Wiegel: O'Brien, Morison, Johnson, and Schaaf. Schaaf was a theoretician and did a lot of work in rarefied gas dynamics. That's what he became famous for at Berkeley. Schaaf became chairman of the department of mechanical engineering. So that was, again, an interesting group.

Rapid Deployment Concepts for the Navy

Wiegel: We also were doing a lot of work for the navy in concepts of breakwaters for rapid deployment. In other words, not these big, massive rubble-mound breakwaters that take two years to build, but something you could take to a site, and it's either floating or it's something you could drill or drive some piles in--pneumatic breakwaters, compressed air--all sorts of ideas. It's the things that the navy could use to deploy rapidly. You see, getting across and getting your landing craft in, and securing the beachhead is just the first part; then you have got to bring supplies in. For that, you have to have some of sort of harbor.

About this time in the early 1950s, there were other studies going on with the use of helicopters for landing, because it became clear that the best way for the Marines to do their landings, would be to come in and fly and land over the shore by helicopters and secure it. But you still have to have supplies. Now, I would like to just mention something else: this is a <u>Science</u> magazine--

Swent: We could just include that if you want.

Wiegel: --3 January, 1997. A little note "Navigating the shrinking financial seas for Marine geo-science." It says the agency, ONR [Office of Naval Research], previously focused, "'on open ocean and anti-submarine warfare because of the Soviet threat,' says Thomas Kinder, a program manager in coastal dynamics at ONR. 'But now, because of things like Somalia and Desert Storm, the emphasis is on littoral--for emphasis, coastal warfare--and getting landing craft safely onto hostile shores.'"

Swent: Isn't that interesting? Fifty years later.

Wiegel: Fifty years later, and I know they had a meeting with lots of people at the navy postgraduate school.

Last year, there was a meeting at the Naval Post-graduate School in Monterey--unclassified meeting--of people of several of the services and so forth on what research should be done on coastal works, of coastal oceanography to work on this problem. I didn't attend, but I saw the note from the meeting, and there was no one at the meeting who seemed to have had any experience whatsoever in actual operations. They were all scientists, each one saying the work they were doing should be continued--was important.

So I think that the navy would be well advised to go back to some early reports and see what was known and what is known. This is fifty years later.

Swent: Just give me a chance to ask a question that I had. This mentions the Office of Naval Research, and you were working at this time with the Marine Corps--

Wiegel: It was an Office of Naval research contract, but the Marine Corps was paying for it.

Swent: ONR did exist at that time?

Wiegel: It had just come into existence.

Swent: Were there these other organizations like NOAA and--

Wiegel: No, NOAA didn't exist then. The other organization was the Beach Erosion Board of the Army Corps of Engineers. At that time--that is in the early 1950s--they started to fund some research. They did research themselves, but they started to fund some research. They did fund some work here at Berkeley, at Scripps Institution of Oceanography, and at New York University--perhaps other places, but I happen to be familiar with work that was done at each of these three places.

Swent: But you were not working with them yet?

Wiegel: I was starting to work on some of these things. You didn't just work on one thing; very often you might be working on several things.

Swent: I was just wondering when that connection came.

Wiegel: In the early 1950s. I think, for the history--and of course you'll have it in more detail in O'Brien's oral history--but he was doing work with the Beach Erosion Board in the 1930s. In fact, he shortly became a civilian member of the Beach Erosion Board, and because of the work he was doing--and it was largely field measurements in New Jersey, Long Island, California--it was largely because of that--I'd better back up--also there were a few Army Corps of Engineer Officers in the 1930s--and later on also--that were sent to Berkeley to do work in civil engineering. They worked with O'Brien and did bachelor theses, several of them using hydraulic laboratory models of waves and beaches. So when World War II came, this was known, and this is why O'Brien then worked closely with these others to do all this military work. But that's all detailed in O'Brien's history.

So now we have the contact after the war, and the Beach Erosion Board, with the civil work--because during the war, the Beach Erosion Board did all kinds of military stuff for the landings, especially in Europe and North Africa and so forth. So they wanted to get on with research because this was the time it was also recognized that you had to combine science and engineering. Engineering had to be thoroughly science-based; get rid of as much art--you're still going to have art, but substitute as much science as you can for art, and then use art to help out.

So the Beach Erosion Board had let some contracts here at Berkeley. So there was the Office of Naval Research then, and the Beach Erosion Board. The National Science Foundation and NOAA were not in existence at that time; later these became major sources of research funding. Plus, on wave-induced forces on structures, oil companies put money in to do research.

Swent: Was UCLA in the picture yet?

Wiegel: UCLA was in the picture during World War II because they had a professor of meteorology. That is where the navy sent their young, very bright officers to learn meteorology. A small group in the Air Force did also, with Professor Bjerknes. He was a Norwegian who was the founder of scientific meteorology, and he was at UCLA. Professor H.U. Sverdrup, who was an oceanographer--he was a Norwegian also, and director of Scripps Institution of Oceanography--was doing the development of the wave forecasting techniques for the military with Walter Munk. He taught some of the young people in this group at UCLA--so UCLA really had a major impact because of this one professor of meteorology. But there was no engineering at UCLA at that time. You went the first two years at UCLA, but then had to come to Berkeley for your junior and senior years.

Calculating Behavior of Moored Ships: Six Degrees of Freedom

Wiegel: At this stage, then, we were doing research on beaches and so forth for the army, but now we were working on the motion of ships for the navy, both freely floating, but also moored ships. I did a lot of work on the behavior of ships that were moored. There are many problems with moored ships--they move around, this way and that. We also did some of it for an oil company--what is now known as Chevron--for drilling through a moored ship, drilling an oil well. Or first drilling cores, and then oil wells from a moored ship. It's a very, very non-linear--what a mathematician would call a strongly non-linear problem--the mind doesn't foresee these things; that's all there is to it. It's just too complicated. We had all kinds of troubles.

Now, because we noticed that when you set up an experiment so a moored ship should just have certain degrees of motion, that after a while it would be moving in all different degrees. For a ship that isn't moored--its natural degrees of freedom are pitch, heave, and roll. But a moored ship has three more because of the moorings: which is sway, yaw, and surge. So there are what we call six degrees of freedom--and energy was transferring amongst them. Now they have a new term for it; they call it chaos. This is one of the chaotic motions. There are different definitions, but this is one of the real ones. No wonder we had trouble. This, again, is engineering.

We used the envelope of these motions to design for. In other words, we couldn't predict reliably. But we'd get enough information that we could develop envelopes, and then you could design by using those.

Swent: Were you doing this out in the ocean?

Wiegel: No, we did the work in the hydraulic model.

Swent: Here on campus?

Wiegel: No. Now we were at Richmond Field Station. So this must have been in the mid-1950s or something like that. Most of this was for the navy, and we had ship models which they built, and we developed the techniques of modeling this. Here again, we were lucky that one of the young professors was Ray (R.W.) Clough that worked with us for one summer. He developed the manner to be able to model this nonlinear system. Two of the graduate students working on it were R.A. Dilley and J.E. Williams. I can't get into it; it's very complicated. We were asked to give a paper over in the Netherlands, and we flipped a coin and I won, so I got sent to the Netherlands to give the paper. Van Lammeren, who was the head of the Dutch naval architecture group, said that our paper was the most original given. We had really done something new. Of course, we were all young. Ray Clough eventually also received the National Medal of Science, because he developed what is known as the finite element method that is used worldwide in the design of all kinds of structures. Again, this is one of those--

Swent: It must have been exciting.

Wiegel: It was. It was one of those interesting things. So we did a lot of work with the navy on this sort of stuff. But then, shortly after, I no longer did it. By then, we had a real good group in naval architecture here (Randy Paulling and John Wehausen; much later, Bill Webster), and they took over all this sort of thing. But this person I mentioned earlier, Jack Morison of the Morison equation--then we got a contract to measure the forces--

Project Engineer to Study Drag and Inertia in the Ocean for Signal Oil and Gas

Swent: You were just saying that then you began a new project.

Wiegel: Yes, an oil company--an independent oil company called Signal Oil and Gas Company--not Signal Oil, but Signal Oil and Gas Company. Sam Mosher was the founder and chairman of it--he later became a regent [of the university], incidentally--asked us if we would study wave-induced forces in the ocean to see whether there were scale effects. In other words, there were data and studies available that Morison and others did in the laboratory. So we got permission from Davenport Cement Company, which is about ten miles north of Santa Cruz, to make use of their pier that's extended into the open ocean. They were no longer using it for a ship to berth along and to ship out cement; they now were shipping it all by rail or by truck. So they allowed us to use their pier which had deteriorated badly, and we had to do some maintenance on the walkways to get to its end.

This was a big advance, because now we were trying to install things in the ocean, and the forces were big, and you didn't blame a tank, you blamed the ocean. You had to work with it as it was. I was asked to be the project engineer, and others were--Dick Barry, a grad student; Ken Beebe, a graduate student; Jim Moon, who was a private consultant in the oil business; and Frank Snodgrass again, in instruments; and another grad student, Mike Hall. We all worked on it. Incidentally, Beebe eventually became president of a company, PMB, which was bought out later by Bechtel; it's now a wholly owned subsidiary of Bechtel Corporation. That took about a year's work down there, and another year doing the write-ups and so forth.

Swent: How is this different from what you had done before?

Wiegel: This was installing a vertical circular cylinder--one of them six inches in diameter, another a fabricated cylinder two feet in diameter--into the ocean and all instrumented with strain gauges to measure the strains and then find out the stresses and calculate the forces. We had wave recorders there measuring the waves, so we were checking out the Morison equation, and calculating what's called coefficient of drag and coefficient of mass. There are two components of the forces: one is the standard drag force, which is what's used in aerodynamics and things of that sort; but because of waves not being steady, we have accelerations, so we have inertial forces. So it's both drag and inertial. They work both at the same time, but they are in quadrature. Again, it's very difficult to sort out.

Swent: They work at the same time, but they're--

Wiegel: You always have these two force components, but trying to sort them out is complicated. Again, you have to use statistical techniques to do it well.

Swent: There was one word in there that I missed; did you use the word quadra...?

Wiegel: Quadrature. One component is proportional to the sine, and one is proportional to the cosine square. I no longer can remember what the word quadrature formally means, but that's what it means from a practical standpoint. Even though we were doing the work for an oil company and they paid for it all, they said, "Go ahead and publish it." At that time, things had really not been worked out as to what you could publish and what you couldn't. Remember, so much of what we had done for the military was classified; we were just used to writing reports for the navy on a classified project and that was fine. Much of it, the navy said, "Go ahead and publish it after the war." But it wasn't worked out yet with a private company; they had paid for it. But they said "Go ahead" so there was no problem.

The Value of Publication in Journals to Disseminate Knowledge

Swent: Where were you publishing?

Wiegel: In journals. The results of that was in an American Society of Civil Engineers publication. They have several divisions. One of them is the waterway and port division; it's now called waterway, port, coastal and ocean division. It's a formally reviewed journal--technical journal--and we published the results in there. These were the only full-scale measurements of these two coefficients, so they were used by many companies worldwide in their designs. That was a big engineering step forward. Again, a lot of scatter, a lot of uncertainties, but you could deal with the envelopes of the curves. Similar work was being done at Texas A&M for another oil company.

Swent: Was there, at that time, pressure or reward for publication from the university?

Wiegel: For those of us in the research staff, we never thought about anything like that. We published because we wanted people to know about it. We'd attend meetings; people would say, "Will you publish it?" We did. We were encouraged to publish by O'Brien and Professor Johnson. They encouraged us to publish, but I wouldn't say it was pressure. You just wrote it up and published it because you wanted people to know about it. You've done a lot of work, and you thought it was pretty good. During this whole time, there was a lot of work being done by--as I mentioned before--Professors Joe Johnson, Jack Putnam, and then later, Parker Trask, and also Hans Albert Einstein on sediment, beach movements, sand transport.

This was almost all under contract with the Beach Erosion Board of the Army Corps of Engineers. About sometime in the late 1950s or something, NSF [National Science Foundation] came in existence and they started to fund some of it. But I didn't work on those, so I can't remember exactly who funded--. I can't remember when they came into existence.

Swent: Were you writing grant proposals and things like that at this time?

Wiegel: They were pretty simple. Remember we were doing things they wanted.

Swent: You weren't out searching for them; they were coming to you.

Wiegel: They were coming to us. Not one of these did we go out and search for. At this time, as I mentioned before, I was again the project engineer. There was always a professor who was a principle investigator.

We never published anything on the Polaris project; it was secret. That was really classified. We couldn't even say that we were working on it. It was interesting because we could see there was a scale effect-- I mentioned scale effects. A big contract was then let by the navy with Lockheed to develop a towing tank which was in a building that could be pressurized or evacuated by air--change the air pressure--because in turn we could see that you had to deal with ratios to absolute pressure, not atmospheric. Lockheed hired three of our people away like that! They all left here and went to Lockheed. You had to have a secure building--just all kinds of things. I thought I should mention it because the fact that we worked on it is no longer classified at all. It was kind of interesting to have done that.

Two terms have come into existence since World War II: one is called coastal engineering, and that's much of what I have done; and the other is what is called ocean engineering, and sometimes off-shore engineering. This would be your off-shore oil platforms, submersibles, underwater sensing devices--all kinds of stuff for submarine and anti-submarine warfare would be ocean engineering. For the oil companies, it would be more likely called off-shore engineering. The reason is, in the Gulf of Mexico, they went from Louisiana a little off-shore and built a platform, so it was called off-shore. Then they kept going into deeper and deeper--Lake Maracaibo in Venezuela was first, and then the Gulf Of Mexico, and then the North Sea, and now it's all over the place. We had done much of the work for years in both of these areas: both the coastal stuff directly for the Army, but then for oil companies and so forth.

Research on Portable Breakwaters Leads to Studies of Mixing Processes

Wiegel: Well, there was another thing, and I think this is important: one of the studies we were making for the navy for transportable breakwaters, was the use of a ship that could be moored, which had a series of pumps installed in it with holes out its side, with nozzles used to generate water jets. The jets would then cause a surface current, and the current would oppose the waves and cause the wave to break, and you could use it as a breakwater. It works, but whether or not it's practical for a particular set-up, depends on the wave lengths.

Swent: So it's not a physical breakwater at all?

Wiegel: It's not a physical breakwater, no. It was really a derivative of the studies we were doing for the navy or army--I don't remember--it might have been navy--on the use of a pneumatic breakwater--people kept talking about it, but it never really was any good. Finally, G.I. Taylor of Cambridge University showed it really was the horizontal currents at the surface generated by the rising air bubbles which caused circulation--that caused the effect. That's why we decided to jump over and generate the surface current directly. This got us into the mixing of jets.

For quite a few years, we then studied mixing processes, right up until the time I retired. The mixing of a river flowing into the ocean or a bay (an estuary): it's fresh water into salt water; how does it mix? The cooling water of power plants discharged into the ocean: take ocean water in and cool the power plant steam, then discharge the warm water into the ocean; how does this buoyant flow mix? So all these, what we might call water quality, are important. So we really got into the problem of water quality in the coastal regime as an outgrowth of working on a hydraulic jet breakwater for the navy. This again is how one thing leads to another. Of course, now everybody is very interested in water quality; many students have done their theses on water quality. Some of it was supported by the California State/University of California Water Resources Center, some by the National Science Foundation, some by the Pacific Gas and Electric Company; they were interested in cooling water. So this has been a major sub-field in coastal engineering, which is still very important.

Observations on Peak Load Anticipate Science by Thirty-Five Years

Wiegel: I'd like to back up to the problem of wave forces on structures. If you have a pier through the surf--a fishing pier like the one at Santa Monica or Santa Cruz--and you occasionally have big waves that break, the surf forms right there along the pier; you have a breaker hitting a pile. When we were doing these field studies, we noticed a peak load--very short duration--a hundredth of a second or so--but a very high load compared with the normal wave load. We observed it in some laboratory studies we were doing as well, and we filed it away--we wrote a paper and presented the data, or a couple of papers, but did not understand it.

It was many years later that finally, looking at some work that a naval architect was doing on the landing of a sea-plane in the water surface--gave us the theory, or the lead to the development of the theory for wave loads on a horizontal member of an offshore gas structure in the southern North Sea--when I say "we," I mean generally, not me--to be able to calculate that peak load. So thirty-five years after the original observations, finally, I saw how to apply the concept to the force of a breaker on a vertical pile. I had to modify the concept, and include information on breaker kinematics, and published the work. Science was catching up with the engineering in that case.

Analyzing Turbulent Motion; "We Don't Understand the Simplest Things"

Wiegel: Another thing is about the way waves behave--not when they're breaking even--the simplest thing--even in the laboratory--a nice, simple wave that's nice and smooth--the flows around a structural member are rather complicated. It's not just velocities; there are accelerations and decelerations, and there are reversals of flows, so in aerodynamics--and my original training in mechanical engineering was fluid mechanics, including aerodynamics and heat transfer--so I had the background there--in aerodynamics, you study flows in wind tunnels, but the flow is all going in one direction, not reversing.

So what they call the wake in the flow past an airfoil or some other shape--in our case, the wake becomes the leading flow as the flow reverses. So we have this problem of very complicated turbulent motion. Now turbulent motion is still one of the things we know very little about. There's been thousands and thousands of studies, and of course, at Berkeley, A.J. Chorin in the department of mathematics--is the leader, or certainly one of the leaders worldwide in the mathematical theory of turbulence. But even using what they call massively parallel computers, we can--I say "we," not "me;" I can't solve them at all--they can solve some of these flows when it's turbulent.

There was an article in the January 1997 issue of Scientific American discussing this. With all these advances and all these techniques and everything, we can still explain quantitatively only the simplest cases. So again, we're forced to use empirical measurements and envelopes and things of this sort. I remember attending a meeting in Bristol in 1978 on hydrodynamic problems, et cetera, in regard to offshore oil platforms in the North Sea. A well known hydrodynamics person said we don't know enough about the theory to be able to design reliably. I said that I expect all of the oil in the North Sea would be depleted before the theorists would be satisfied. Twenty years later, it looks as if I was right.

Swent: There are still things to learn?

Wiegel: Still things to learn. But one of the things I remember from our Professor Einstein--he was a wonderful person--he said, "Knowledge is the inside of an expanding sphere. What you know is on the inside of the sphere; what you don't know is everything outside of the sphere; but what you know you don't know is on the surface of the expanding sphere. So as your knowledge increases, the sphere gets bigger, the surface gets bigger, so the more you know, the more you know you don't know."

Swent: That's very true, isn't it?

Wiegel: I remember something else Professor Einstein said. Incidentally, we called him Hans when he first came here, but Albert later. His family called him "Young Albert" and called his father "Old Albert."

I asked him, "Did you ever talk with your father about your work on sediment transport and the turbulent processes involved?" He said, "Yes."

Then I asked, "But, what did he say?"

Our Professor Einstein said, "He said it is too complicated." Note the "complicated" not "difficult"; this is important. I used to relate this to my students.

But again, a recent article in Science was on, "Why is ice slippery?" Of course I thought, "Everybody knows that you can ice skate because of the pressure of the ice blades; it liquifies the ice in contact." The two professors--one of them is here at Berkeley at Lawrence Berkeley Lab in chemistry--said essentially, Oh no, that's what we all thought, but that isn't it. He stated: "...very good circumstantial evidence that the surface is molten....the surface of ice is extremely dynamic." He was referring to the top layer, only a few molecules thick. In the article--it's the 20 December 1996 issue--they mention another statement--and this was Professor Steve George at Colorado University--who said, "This illustrates how we don't understand the simplest things we know about."

I love that last quote, because this is how I feel about some of our things. We see them all the time, we see waves coming in, breaking on the beach. We see beaches come and go, and we know about it, but they're so complicated that we still do not thoroughly understand the physics. We understand portions--glimmers of this and that. So here's where we are, and again this is engineering--we have to deal with these uncertainties.

I think I mentioned that we did a lot of work on wave generation, and one of the people who was here-- who after he finished his Master's degree, went on and did a Ph.D. at Texas A&M, made a lot of headway in wave forecasting techniques--by the name of Charles Bretschneider. O'Brien had asked him to study this and gave him directions on the way to go, to modify the forecasting curves used during World War II and afterward by the navy and everyone else, called the Sverdrup-Munk curves--they were both at Scripps Institute of Oceanography. They were developing a physical theory for wave generation, and O'Brien looked at all their data and said, "Well, the way to handle it is so and so" and he told them to use dimensional analysis, which they did--using the similarity principle.

So the real concept on how it was done was really O'Brien's, not Sverdrup or Munk. They had made major headway--I don't want to underestimate it. Anyhow, he had Bretschneider continue it, and got a lot more data and made it a lot more formal. Then two people--again Frank Snodgrass, whom I mentioned earlier with his instruments--and Bob Putz, who was a student of Jerzy Neyman--really went into the statistical and probabilistic models of these things, which are used pretty generally today. They then became known as the Sverdrup-Munk-Bretschneider wave forecasting curves.

Another example of where sometimes your observations get ahead of you: John Isaacs had mentioned to me he had observed, in some conditions in the ocean near shore, waves which look like they have some of the characteristics of a Mach reflection--that is a sonic boom and how it behaves moving in contact with land.

So I started to look into it with graduate students--I was a professor by then--and we developed knowledge of the subject with three or four grad students doing laboratory studies, preparing papers on the finding, and presenting them at meetings--and this has become very well-known. Professor Ed Laitone at Cal was using the method of characteristics to study shock waves theoretically and also once towed an airfoil at the free surface (air/water); he told me that the ratio of C_v and C_p were such it might be valid to study shallow water waves by this means--which is what we did. But the theory was not developed. Professor Lighthill in Britain had done a theoretical analysis of the sonic boom/land surface problem. At the end of the paper he indicated he would continue, for water waves, but I have never seen this--I don't think it was done. This is again a very difficult non-linear phenomenon, and it's only within the last ten years that theoretical advances have been made by Professors John Miles, W.K. Melville, C.C. Mei, Phil Liu, and their graduate students. So sometimes you're way ahead in observations--and developing methods for handling problems. We learned how this would affect a breakwater, for example. So again, this is one of the places where engineering is different from the science, because in science, you'd want to be able to formulate the thing mathematically. Well, nobody was able to do that until much later, but still we could deal with it. I think that's kind of important.

Swent: Did the scientists then draw on your observations?

Wiegel: Oh sure, we (this is the general "we") all worked together. Once they make some advances, then we know how better how to look at things and what to measure. It is an iterative way you advance.

I think I've already covered the rapid deployment for the military and so forth.

Swent: Were there ever any--I don't want to use the word conflict--but differences between projects that were funded by different groups, or requested by different agencies. Were there cases where you were doing research for one--

Wiegel: Never any conflicts, no.

Swent: --person or entity and other research for another agency?

Wiegel: One study always helped out another study.

The Caldwell Equation

Swent: That was my next question: were you able to channel a lot of things from one research project into another?

Wiegel: All the time. There was one other thing: I mentioned that the studies of sediment transport--that is, sand motion on beaches and things of this sort--that were being done here was by Johnson and Einstein and Trask, and I did a little bit, but not much. I've always been interested. They did quite a bit. There was an equation that appeared in a study in Orange County at Anaheim Bay where they had beach erosion problems. It was for the Los Angeles district and a person by the name of Joe Caldwell came across a formula that somebody at Scripps had said, "This should be used." He called it the "littoral drift factor." We don't know who the person was; unfortunately, it's been lost. But it's the right concept that sand transport by wave action appears to be proportional to wave work (time rate of work, "wave power") rather than wave height. My guess was it was probably Walter Munk or Bob Arthur but I don't know, and we can't find out from the written record, and they don't remember.

Anyhow, it was written down and it appeared in this Corps publication and credit given to the input from Scripps. But it requires, again, field measurements, because there's a coefficient in the equation that masks so much that you don't know. Caldwell used the Corps' survey--the measurements made by surveying beaches to the southeast of Anaheim Bay--and came up with an estimate of this coefficient. It's called the Caldwell equation, or later the "SPM formula" (as it appeared in the Corps' Shore Protection Manual), but as he said he didn't do it, but he was the one that got a coefficient for real beaches. It's still useful today. I'd like to jump ahead because it illustrates something: last year I was asked to review a whole series of reports by the Los Angeles District of the Army Corps of Engineers and by a number of consultants to them on the Orange County coast--waves, and currents and tides, sand transport and beach changes and coastal subsidence--all kinds of things.

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Wiegel: --and the result of the study was for use by Orange County personnel and the county supervisors to make long-term plans for the whole coast: "what are we going to do; what should be our policies?" This is this general coastal zone management thing that we like to do--or would like to do--and finally can do when there is funding. The more you know about a subject, the more likely you are to make good decisions. It doesn't mean you will make a good decision, but at least you're more likely to if you obtain good scientific understanding and information.

I looked into--as a part of this--a study made by some South African engineers published in an international journal just two or three years ago. They looked at all of the formulas for alongshore transport of sand used worldwide, and evaluated them to decide which one or ones might be best, using all the good data they could get. The Caldwell equation was the second best of these. I think--that is, of course, the people that developed it had the right idea--there are more advanced theories than the Caldwell equation, but when you get into the more detailed ones, they are so complicated you don't know how to handle them, and interrelationships cannot be entwined. So this I found very interesting.

The measurements used by Caldwell were made at the place for which I was making this review for the Army Corps of Engineers in Orange County. I found that kind of interesting--because you see, 1956 to 1996--

Swent: --forty years.

Wiegel: --forty years. So again, I think you hear a lot about "publish or perish" and all this and that, and "we" didn't do it to publish or perish; "we" did it to make the information available. But this shows you how important it is to publish these things, because it was available to others. Joe Caldwell died years ago and nobody remembered anything about it, so if it hadn't been for the fact that he wrote this report and so forth, it would have been lost. I think that's an important thing: that "publish or perish" is good for several reasons, that being one: to document these things.

Swent: Also I think it's interesting that it had spread as far as South Africa.

Wiegel: It was spread every place: Japan; Australia; all through the European continent; Taiwan uses it; it's used in India.

Swent: These things are valid all over.

Wiegel: Yes. If you do have the right physical concept, then it's valid. Again there is scatter, and this is still a real problem. That, I think, is kind of a broad picture of the research. It should be stated that all of this was done by graduate students--with a few exceptions. I started as a grad student, and then I was full-time research for a while; but I think there were only two or three of us like that; everyone else was graduate students then and all the way through.

I should mention that at about 1957--this will get us into what will be coming up next time--some time--and I can't remember just when--I got into teaching by complete accident. A professor at UC Davis left one week before, or two weeks before school--the fall semester was to start--and they needed somebody to teach Irrigation 117, which is hydraulics. So the dean up there phoned the dean at Berkeley--that must have been O'Brien still--and asked, "Can anybody teach this course?"

I was asked would I mind teaching it. I said, "Sure, I'll do it." So I would take the train up. They arranged for me to teach twice a week, and I'd take the train and I'd do all the preparation on the side. I was still working full-time on my research because I couldn't drop it, but I taught that course. So I got into teaching by accident.

Swent: We'll go into the teaching next time. We might just here mention --you were living in North Berkeley?

Wiegel: That's where I live now.

Swent: Were you living there then?

Wiegel: Yes.

Swent: When you were down at Camp Pendleton, for instance, you mentioned --I think your wife went down with you.

Wiegel: At Camp Pendleton we lived in a Quonset hut on the Marine Corps base, and then we lived in Monterey or Carmel for a few months and then we lived up in Seaside, Oregon, for a few months.

Swent: You just rented places, or did the project provide something for you?

Wiegel: No, we rented places. Then we got an apartment in Berkeley on Ashby. I forget when we built our first house. I designed it, and my uncle, who was a contractor, built it in North Berkeley. That's not the one we're in now, because when we had the third child, it became evident that we needed more space, so we bought one of those big old Mediterranean-style homes. I guess we bought that about thirty-four years ago or something like that. We've lived in North Berkeley for many, many years. I'd say that would put it over forty years.

Swent: Did you have children at the time that you were moving around?

Wiegel: No.

Swent: That was before children.

Wiegel: The eldest is forty-one, then thirty-eight, and thirty-five.

Swent: So you didn't have a child in the Quonset?

Wiegel: No.

Swent: What did your wife do while you were measuring waves?

Wiegel: In the Quonset, nothing. It was southern California, there were beaches and so forth, but when we moved to Berkeley, then she got a job in a bank. Actually, it was Bank of America on Piedmont Avenue in Oakland, and after a short time, she got a job at the Bank of Berkeley, which made it a lot easier to get to work. In those days, it wasn't that difficult to get from one place to another because it wasn't as crowded. So she worked for those two banks until she had the first baby, our son.

Swent: How you were traveling around? You mentioned you traveled to Davis by train. You were using a car, I suppose, to go from here to Half Moon Bay, for instance?

Wiegel: Yes, we'd always use cars.

Swent: What kind of cars did you have?

Wiegel: Well, on the field stuff, it would be a university car, I have no idea.

Swent: Was it a four-wheel drive, a van?

Wiegel: They didn't have such things.

Swent: This is what I'm trying to get at. What sort of vehicles were you--

Wiegel: When we were working down at Davenport and so forth--a couple of the people lived, actually, in Santa Cruz and just drove back and forth from there. I would do down, but we'd have a pick-up truck to take our equipment--a university pick-up truck to do that.

Swent: The pick-ups had come in by then, but I think vans were later.

Wiegel: There were no vans; not that I know of. When I had the military project, we had four-wheel drives because we were using military equipment. So there we had three-quarter-ton trucks for equipment and things of this sort. These DUKWs are four-wheel drive too. But those were furnished by the military and operated by military personnel. We didn't drive them. It was a military operation--some of those things--not the one at Davenport, but the amphibious work was all strictly a military operation.

Swent: Is there anything else that we should...

Wiegel: I can't think of it right now. Remember, I've only sketched the research that we've done because there was all kinds of it. I think that the point is: so much of it was done by graduate students--bits and pieces; we all worked together; we did science, we did engineering, we did research, we did design. We didn't separate these things.

Swent: I think you've given a good feeling of the cross fertilization.

Wiegel: I think that's important.

Swent: I think so, too.

Wiegel: We weren't specialists at all.

Swent: You did mention that you shared an office with Trask and learned so much from that; at this time, where physically were you sharing information? Were you eating lunch together, or drinking coffee together? What sort of social--

Wiegel: Before we moved out to Richmond Field Station--that's when I shared an office with Trask--it was on campus. But we had--first when I came here, space in the top part--more likely an attic--of what is called California Hall now. It was just a big open space. A couple of professors had offices, but the rest of us were just in a warehouse-type of thing at the top.

Swent: Were you working at tables?

Wiegel: We used desks that they brought in and things like that, but then we'd go up to the laboratory that was a hydraulic engineering laboratory in the Mechanics Building which is now gone. So we'd do our testing there or up at the College Avenue pool. Then we were moved into a new building, Hesse Hall, which is a part of the mechanical engineering laboratory here on campus, and we had the top floor--no windows at all. Then we had a series of offices, and we had a coffee mess, but a good deal of it was open. So we just moved back and forth--so we discussed things as they came up. This is where I shared an office with Trask.

Then when we moved out to Richmond Field Station, we had a temp, one of those wooden building out there, and again, we would just have coffee, or rather coffee and donuts. It was just running back and forth; if something came to our mind, we'd go talk to the other person or persons, or they'd come see you. You did it all the time. In some ways, it's kind of like what you read about how Andy Grove and David Packard and those groups behaved--the open office. This is what is was. You were all engineers or scientists or mathematicians, and you were just interested in the problems so you'd talk with each other--nothing formal whatsoever about it. If you went to see the dean it got formal.

V TEACHING AT CAL, 1957 TO 1987

[Interview 3: February 12, 1997] ##

Irrigation 117 at the University of California at Davis

Swent: When we stopped last time, you had just been asked--kind of as a last minute thing--to teach a course in irrigation up at Davis. Irrigation 117, I think it was?

Wiegel: Yes, that is correct.

Swent: That was your initial jump into teaching.

Wiegel: Before I get into that, I think I should just mention that the interview last week was on university research, and I think I should mention that I have been fortunate to have often done research with graduate students and we wrote multiple-author papers. I never once published a paper a student worked on that the student's name was not on the paper.

Swent: Incidentally, the list of your publications is enormous.

Wiegel: I did a lot of writing.

Swent: You certainly have published a great deal.

Wiegel: They've been studied by others, and they've been recognized, and they have been partially responsible for my receipt of several major awards. This is really to verify that the work we did was good and useful. One was the ASCE [American Society of Civil Engineers] Research prize in 1962, and membership in the National Academy of Engineering in 1974. Then I was elected a fellow of the AAAS--that's the American Association for the Advancement of Science--"for leadership and many research achievements in coastal engineering." That was in 1979 or 1980. We'll get into teaching now, but we hear the term "publish or perish," and we hear and read the statement that universities don't recognize people for their teaching, but they insist on research. I would like to say that I think that the universities' insistence on research--doing it and publishing it--is excellent--our functions include both the transmittal of knowledge and the development of new knowledge.

This is the thing that keeps your mind active: the development of new knowledge, and also the assessment of your publications of your research in reviewed journals is necessary. This gives an external look at what you are doing. I think this is very important. This now leads me into teaching more generally.

Swent: Yes, and then later we'll talk more about your editorship of some of these journals, too.

Wiegel: Thank you. That's all teaching, too.

The Benefits and Effects of Consulting to Test Professional Engineering Ability

Swent: All of these things inter-relate, don't they?

Wiegel: That's right. Although today's subject is university teaching, I would like to modify it. Engineering is a profession, and so we should include a few comments on the benefit of research, but also the benefits and effects of consulting, because it is a profession. Consulting confronts faculty members with reality, and it tests their professional ability. But also, by talking about the consulting that you do, where it is germane, in a course, exposes students to what they may do in the future, and it also teaches them some of the aspects of the professional portion of engineering. This doesn't come out of textbooks; this comes out of relating how you can see something--how you design something, both functional and structural--how you interact with the clients, because you always have a client.

Somebody wants something; you try to give them what they want, or if you think perhaps they are wrong, lead them into some other way of looking at something. I've been very fortunate throughout my entire career to do consulting in the U.S. and abroad in a number of countries for government agencies, for corporations. I notice that in some place in the notes you asked to talk about any conflicts between the university and corporations, in regard to consulting. I've never found any problem whatsoever. Any company that have I worked for was pleased to have the information presented to the students, and the students have always appreciated learning about what's going on today, because as a student, they think about a future job. They say: "I want a job that's interesting to me now, who might I work for, and will it be something I'll be interested in, in the future?".

So because of consulting, you're teaching the students something else besides the ability to analyze.

Swent: I'd like to have you be more specific about consulting jobs: how you got them, what they were, and how they came about?

Wiegel: You got them by somebody either coming to you or phoning you.

Consulting on a Breakwater Project at Subic Bay in the 1950s

Swent: What was the first consulting job you ever had? Do you remember?

Wiegel: Well, one of the early ones--Professor Johnson and I were asked by the navy to go out to Subic Bay in the Philippines and look into the possibility of a new breakwater to protect the airfield that the Civil Engineer Corps of the navy had just constructed. We did fly out for the navy, made investigations, and made a report to the navy on how the thing might be constructed and what its effects would be.

Swent: When was this?

Wiegel: It was 1955. That was one of the early ones.

Swent: And was it built the way you recommended?

Wiegel: I don't know. I remember somebody once telling me when I said that for many of the companies I'd done consulting for, I was never completely sure what was done, and the person said, "Were you sued?"

I said, "No."

He said, "It must have worked." [laughter]

Swent: It's true.

Wiegel: A bit of humor, but unfortunately an awful lot of truth in that statement. But I worked for many companies.

Swent: So this one came about--you were assisting Professor Johnson, I take it?

Wiegel: No, it was jointly. Professor Johnson and I were truly colleagues. We worked together. My main field work was with waves and wave forces and things of that sort, and Professor Johnson was more with sediments--sand movement. We would both do these things, but each one of us would put in the portion that he was best suited for.

Swent: What form did your report take to the navy?

Wiegel: At this stage, I can remember that it was a three-quarter-inch thick report in a blue folder. But it did lay out a possible breakwater--I believe it included the wave conditions that might exist inside and outside the bay, and the effect of a small natural island (Grande Island) that was near the entrance. This required development of the wave climate near the site in the South China Sea, and wave refraction drawings. I can't remember the details, but it was a real professional consulting report. I do remember that.

Swent: Just a bit about the mechanics of doing the report when you went out there, because technology has changed. You flew out, of course, to the Philippines. Do you remember what kind of plane you flew?

Wiegel: It was either a navy plane, or shortly thereafter, it would have become a part of military air transport. I suspect it might have been what they call military air transport. I do know that we flew from probably Alameda Naval Air Base--I'm not sure of that--to Hawaii, and then we had to get off the plane and stay a day in Hawaii while we got our new priority, and I guess we flew to Johnson Island, and then Johnson Island to Midway, and then Midway to Guam, and then Guam to Subic Bay. It was something like that. It was not easy. You just didn't go down, book a flight, and go.

Swent: That's right.

Wiegel: It took you a lot of time. Those airplanes were all propeller, so they weren't as fast as--by far--the ones we have nowadays. It was a lot of work to go overseas in those days.

Swent: And how did you take your notes? How did you make your notes? Typewrite?

Wiegel: No, everything was handwritten that we did.

Swent: How did you get your report?

Wiegel: Afterwards, we'd pay to have--this was done in the summers or things of that sort--and then we'd pay to have them typed up outside and pay to have drafting done outside and then submit them to the client.

Swent: Did students help with any of that?

Wiegel: No. Consulting very often led to research on problems that were uncovered by the consulting. Sometimes, in the case of the navy, very often--not this particular one--they decided they wanted research. Then students would work on research as a part of the university activities and be research assistants. So from that standpoint, there was a positive derivative.

Advising Bechtel and GE on a Power Plant in India

Swent: I wonder if you could think of one, and just trace it through the steps of how you got it and how it developed?

Wiegel: Let me talk about a later one. I was asked to do some consulting for jointly--this was quite a bit later--Bechtel and General Electric Corporation on a nuclear power plant in Tarapur, India. It became evident that there was much we did not know, and so the companies agreed that the university should do some research on it under what was called "a service-to-industry contract." So a short proposal was written and a contract--

Swent: Who wrote it?

Wiegel: I wrote the proposal.

Swent: How did you learn that it needed to be--

Wiegel: I don't know; I can't remember why people from Bechtel and General Electric came to see me, it must have been a paper of mine they read.

Swent: Had they have given you a request for a proposal?

Wiegel: Yes. Anyhow, this is a formal--

Swent: I hate to be picky, but how are these things--did they telephone you, or come to see you, or write you a letter? How did this come about?

Wiegel: Well, undoubtedly, it was a telephone call first, and then they came to see me. Remember, this is going way back.

Swent: I know. I'm just wondering how these things progressed.

Wiegel: So we discussed it, and talked about what might be done.

Swent: Was this a power plant on the coast?

Wiegel: It was a coastal power plant, and in fact, it has since been built, and it has been in operation for many decades.

Swent: What was the name of it?

Wiegel: Tarapur. That's on the Arabian Sea. In other words, the west coast of India. So we wrote a proposal, just a couple of pages, and of course this meant that somebody in the dean's office had reviewed it, and somebody in the chancellor's office had agreed to it before our proposal could go out. When I say we wrote it, we wrote the technical portion. The proposal was then written using university boilerplate. So they matched all the university requirements. This resulted in the employment of three or four graduate students as research assistants; Ismail Mobarek and Juan Jen worked on it. It was done in the model basin at Richmond Field Station, which was then outdoors with a high fence (wind break) around it.

We worked on it for at least a full year, including the summer. For one of them it led to what eventually became a Ph.D. thesis at some stage. I can't remember exactly. Of course, with other work. I think about that time, the National Science Foundation got interested in what we were doing, and we got funding from the National Science Foundation to do a much broader based study, and then this continued.

Swent: What was the study? The challenge?

Wiegel: That would be the mixing of the cooling water. You take ocean water in from the ocean, which is relatively cold, use it in the condensers of a power plant. Remember when I say nuclear power plant, it generates steam to drive the turbines, so you need condensers. You use these great amounts of ocean water for cooling and then discharge the heated water into the ocean and it mixes. So we were looking into the intakes and the discharge and how it mixed. The quality of water.

Swent: The temperature was different, was the--

Wiegel: About twenty degrees Fahrenheit warmer would be the discharge temperature from the intake temperature.

Swent: But it was the same chemical water?

Wiegel: The same chemical water, yes. All we did was heat it. That's all that occurs in a nuclear power plant, too, believe me. It's just heated.

Swent: So your question was the mixing of the--

Wiegel: --the mixing of it, and would the water temperature be low enough after it mixed in the first few hundred yards to satisfy the ecological concerns about its effect on fishes and other marine organisms. Not so much fish, but the other marine organisms, because they're in much greater abundance. Fish generally like the warm water, and can swim away if they don't. You take cold water, warm it, and discharge it, and the fish thank you for doing so.

Professors Wiegel and Johnson Did Not Neglect Their Classes for Consulting Work

Swent: Did you get to go to India? Did you travel out there to see it?

Wiegel: They wanted me to. I've been to India twice, but the timing was such that I just could not get out there. Remember when you're teaching, you're teaching. So unless the timing is such that it's at Christmas or summer or something like that, you don't do traveling to a distant place like that.

Swent: This is an assumption that you're making, but I've been told that this was one of your distinctions: that you did put your teaching schedule ahead of any other consideration.

Wiegel: I always did, yes.

Swent: You always met your class responsibilities?

Wiegel: Yes. On a few rare occasions--for one at a much later date I was the U.S. representative to a UNESCO meeting in Paris on ocean engineering curricula in universities. They agreed that, seeing that I was asked to be the U.S. representative, this was quite legitimate to be absent from teaching for a week. There were a few other times like that. Again, Professor Johnson and I traded our courses. If for some reason I had to do something like that, or if I had to go to Washington, D.C., for two days for a committee meeting, he would take my class and if he had to go to Washington for a committee meeting or something, I would take his classes. Between the two of us, we always taught every single class; always. I don't think there was ever a time when a class was missed. We both took it very seriously.

Swent: Yes. You are distinguished by this, I think.

Wiegel: Thank you. We worked hard at it, and we believed in it.

Committees for Engineering Education; Saving the Program at Irvine

Swent: I'm sure your students appreciated that attitude as well.

Wiegel: I think they did, but they always liked to learn about what you found out from these trips. They always enjoyed that. Perhaps we can get back to this a little later. I think that I should emphasize that teaching in a professional field is different than teaching in more academic subjects. I noted while reviewing some records in preparation for this interview that I served on the Chancellor's Committee for Professional Education from 1969 to 1973. I had forgotten there was a special committee for it, but at least at that time, the chancellor recognized the function and wanted to have some input to him about this.

Swent: Do you recall anything about that committee? What you did on it?

Wiegel: No, I can't, because I also found that at the end of that time, I served on the University-Wide Committee on Engineering Education, which is the office of the president, and that was the year I was the acting dean of the College of Engineering. I do remember one thing that was very important in that case, and that was a meeting at the new campus of Irvine. They were considering whether or not to abandon engineering at the new campus.

Swent: They had engineering?

Wiegel: They had started engineering at Irvine. Remember it was a brand new campus at that time.

Swent: Let's see, let's get a year for this.

Wiegel: I was acting dean from 1972 to 1973, so Irvine was formed before that. But it was the meeting of this engineering advisory council to the president of the university. That meeting consisted mostly about whether or not to recommend to the president to continue engineering or to drop it at Irvine.

Swent: What were the reasons for this?

Wiegel: One of the reasons was that a local state college--I think it was Fullerton, but I'm not positive--had a very large number of engineering students and there were not many coming to Irvine. My remembrance is that I argued really strongly to continue it. There were arguments on both sides. These were the deans of engineering at UCLA, Davis, and Berkeley, plus a few other people on the Engineering Advisory Council, I think--to continue. We thought it was very good. Bob Saunders was the person in charge of the program there--he had been chairman of electrical engineering at Berkeley before--and we thought he was doing an excellent job. He was concentrating on engineering systems. So I think that was important.

Swent: And what was the result of your--

Wiegel: They continued it. They developed a fine school of engineering. Remember, it was a little, tiny thing at that time, but they have a fine school at Irvine.

Swent: Which they might not have had.

Wiegel: They might not have had. It was very, very close. I think you'll read, or remember in John Whinnery's history--he talked about Santa Cruz. They wanted to get a program going at Santa Cruz in engineering, and Whinnery and Howard Eberhart did a lot of work towards that, but that never came into existence. It was decided to drop it because of what was called the Terman Report. Terman was dean of engineering at Stanford and then provost and so forth. But all those details are in Whinnery's oral history. But it came very close to losing the engineering at Irvine. It was very fortunate for the state that it didn't come about. As we know, we need these schools. So that was important.

I think I mentioned at the end of the previous session--that was last week--that I got into teaching by chance. I was a full-time research engineer at Cal starting about the middle of June, 1946, but owing to an emergency at UC Davis--by the way, the emergency was the professor who was going to teach a course in irrigation left. It was that simple. The dean at Davis phoned the dean at Berkeley and said, Help. This was just about two weeks before the start of the fall semester. So Dean O'Brien recommended that the dean at Davis contact me--which he did. This was in the mid-1950s.

In fact, not the dean at Davis, but the chairman of the Department of Irrigation, Bob Hagan, contacted me, and I agreed to teach it. I had two weeks notice. I had never taught before. I commuted two days a week by the Southern Pacific Railroad from Berkeley to Davis, and I spent the entire time on the train and when I was up there--except for teaching--preparing my lectures. I was so fortunate in that Professors Einstein and Johnson here, and also Harold Iversen, who was in mechanical engineering--really helped me. Boy, I had question after question which they helped me with. So I was able to go ahead and teach the course.

Swent: Who were the students?

Wiegel: It's interesting, because at Davis a good number of them were graduate students, even though it was an upper division undergraduate course; because it was irrigation they were from many countries--a number from Central and South America because of irrigation, an extremely important subject then and now and will be as long as we like to eat food. So that was interesting.

The Importance of Faculty Socializing

Swent: What were you learning at the same time?

Wiegel: Like somebody said, "The way you really learn a subject is to teach it the first time." You puzzle over every single thing, but as I said, I was so fortunate in having these three faculty people to help me. They were simply great. I would like to say here that I've been extremely fortunate throughout my whole career with the people here at the university. The other faculty--we've always worked together. The staff--you don't do these things by yourself. It's very fortunate. One of the things: please never underestimate the value of having coffee together, which we did later when I became a full, regular faculty member. After lectures, we'd go have coffee, and we'd discuss teaching and course content.

Swent: Where did you go?

Wiegel: The place we first went to at Berkeley was a drugstore at the corner of Hearst and Euclid, and it was Reed's--Herman Reed. We almost made him an honorary member of the College of Engineering at one time, because it was the poor folks' faculty club for coffee, and we'd always go over there after our morning lectures. Then many of us ate regularly--we ate lunch at the Faculty Club. Mostly the discussion was about teaching and research in all the different fields: mechanical engineering, civil engineering, naval architecture, mining engineering. We all talked to each other.

This has almost disappeared at the Faculty Club. I still go there for lunch almost everyday and you do not see that anymore. I don't know what has happened; I think it's unfortunate. It doesn't happen.

Swent: They're still eating lunch there, aren't they?

Wiegel: Very few engineering faculty eat lunch at the Faculty Club. Almost none. I don't know where they go. It's very surprising to me, but it's been a big change. There used to be two tables--large tables filled with professors of engineering that ate there every day. Now you see one or two professors of engineering and that's all.

Swent: That is a change.

Wiegel: It's very surprising. I think I mentioned I took the train up to Davis and back, but then, because of university red tape, I think I taught half the course before I finally got my appointment. So I was, at the same time, working every night and weekend, plus the other days a week on my research contract. I then continued throughout the semester really doing the teaching essentially on my own time. I was putting in more than forty hours a week in research and probably another twenty hours or twenty-five hours a week on the teaching. The first time you teach something--it sounds like a lot--twenty-five hours a week for two one-and-a-half-hour lectures. That's about what it was. Of course up there you graded all your own problem sets as well as tests.

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Wiegel: The course I taught was very much like the first upper division course in fluid mechanics at Berkeley--we used the same textbook--only our examples there would be more of irrigation components and systems: bringing water down to the farm not so much in pipes, but in open channels and things of this sort.

Swent: Was there field work?

Wiegel: No, no field work. That was strictly a lecture course. Later, of course, the students in irrigation would have a number of follow-up courses, and some of them would include field work, but I didn't teach those. There were other people on the full-time faculty at Davis that did that. But one nice thing: there was a tiny, one room, interim faculty club at Davis. It was just set up really for lunch, and I would generally have lunch with an assistant professor of English and another one in political science. So our luncheon discussions were usually very broad-based: English literature, political science, and irrigation. But it was interesting. I enjoyed that.

Swent: Had the previous teacher of this course left any kind of syllabus for you?

Wiegel: No, none whatsoever. This is where Professors Einstein, Johnson, and Iversen were of such great help to me. Otherwise I could have done it, but it would have been a poor job.

Swent: Just to know what you were expected to cover.

Wiegel: The department office had that information. I didn't know any of the techniques--what to look for or anything like that. When I was an undergraduate, I was a reader in a similar course when I was a junior or senior or something like that, so at least I'd had a little bit more. I think what they say is this: "If you want to learn a subject, take a course, then be a reader in a course, and then teach a course; then you know something about the course."

Changing Academic Schedules from Semester to Quarter System

Swent: Then you really do. Were Davis and Berkeley both on the same academic schedule?

Wiegel: Yes. In fact, all of the university was on a semester schedule. Not the same semester system that we have now.

Swent: What was it?

Wiegel: The fall semester continued after Christmas break. Incidentally, we went from the semester, which was that one, to the quarter system at Berkeley, and then back to the semester system. In planning the new semester system, the present system--they recognized the problem of having the Christmas break and coming back and doing another week of lectures, and having your examinations in January. So they modified the semester to finish before the Christmas break (now called the winter break). Now, they're discussing on this campus--I just learned within the past week--they're discussing trying to have a system where you go back, start school later, continue after the Christmas. All I can say is, "Don't they ever learn?" That was no good at all and I hope that they don't fall into the trap of doing it again.

Swent: Isn't that interesting? To go back to the same--

Wiegel: They are discussing it. Hopefully, they will not do it.

Teaching in the Mechanical Engineering Laboratory at Berkeley

Wiegel: Shortly after that, in the late 1950s, I was appointed, on a part-time basis, to be a lecturer in mechanical engineering here at Berkeley. I taught a section of the required senior course in the mechanical engineering laboratory. The portion I taught was more on the pumps, compressors, wind tunnels--that aspect of mechanical engineering.

Swent: Where were your classes held?

Wiegel: They were held in one of the buildings immediately adjacent to this one. This one wasn't built then. This one is O'Brien Hall. But the one I'm talking about is Hesse Hall, (also the Mechanics Building, for pumps) and that was a mechanical engineering laboratory, and it still is part of the mechanical engineering laboratory, but I don't believe that any single item of equipment that we used then is in that laboratory any more. It's all different, which it should be.

Swent: What sorts of things were you teaching then? What were you using?

Wiegel: It was just a laboratory. The students did the laboratory experiments and every single senior student in mechanical engineering had to take this extensive laboratory. I taught one of the sections.

Swent: What sort of equipment were you using?

Wiegel: Pumps.

Swent: What kind of pumps?

Wiegel: All kinds: centrifugal pumps, water hammer pumps, displacement pumps--also we taught about air compressors, and then tested airfoil sections in a wind tunnel. Remember, my original undergraduate work in mechanical engineering was heat transfer and fluid mechanics, so I'd had a reasonable background in aerodynamics. I could handle those.

Swent: You mentioned that the equipment has all changed since, so how has it changed?

Wiegel: It's gone, and new things--computer-controlled this, computer-controlled that, etc. I don't know.

Swent: So the things that you were teaching--how were they controlled?

Wiegel: By hand--mechanical.

Swent: Not even electrical?

Wiegel: A few electrical things, but a good deal mechanical.

The Pelton Wheel: Materials Change, but Principles Do Not

Swent: The materials had changed as well, I presume. Were there any ceramics used or plastics?

Wiegel: This is interesting--you mention materials, and this is one of the major advances that have been made and are still being made--is in the understanding and the development of materials.

But first: more than a hundred years ago, there was a tiny hydraulics laboratory on campus, and a person came down from the gold country and his name was Pelton. He had a model (or one was made here--I don't know which) of a device that he was going to submit--he hoped to win an award on a method of changing from water power to electric power. It became known as the Pelton wheel. It was tested on this campus--probably by Professor Hesse --either report number one, or report number two of the--I guess it was called College of Mechanic Arts or something--back about in the 1880s--was on the Pelton wheel. Incidentally, the little model that was used is on loan by the university to the Oakland, California, Museum and they have it displayed very nicely in the section on the work done up in the gold country.

I was told, many years ago, that after the work that was developed by Mr. Pelton and then I believe it was Professor Hesse in the laboratory here at Berkeley--the main thing was, you had buckets of the double-lobe type, mounted on the rim of a disk runner, and the water jet would impinge on the bucket. The divider split the jet in two sections to balance the lateral forces, which permitted a much lower load on the bearings.

There were some modifications in the shape and spacing of the buckets and so forth during these tests, but I have been told that since that time, there have been almost no changes in the configuration--of course, they got bigger and bigger--it has been the materials improvement that have been the main thing that has changed over the years. Now, Pelton wheels are still used worldwide on practically every single, what they call high-head hydroelectric plant. By high-head, they mean a big elevation difference between the surface of the reservoir and the location of the turbine. I digress, but I think that's interesting.

Swent: No, it's not a digression at all.

Wiegel: I think it's very interesting because these are teaching laboratories. It's how you teach students: they get their hands on it. When Hunter Rouse, who had written the history of hydraulics in the world, then expanded in a separate book the history of hydraulics in the United States, he stated that the first teaching hydraulic laboratory in the United States was at Berkeley. So the hydraulics laboratory you see has been in existence as a teaching laboratory and used for research and development for more than a hundred years. We have new equipment, of course, and we did give--or rather loan--the original Pelton wheel to the Oakland museum; I checked it out last year, and it is well displayed.

I think you'll notice our logo at the north end of the hydraulics laboratory, which is in this building--O'Brien Hall, and the logo is an old--I believe hurdy-gurdy wheel that Professor Johnson found up in the gold country. It was all rusted--it has been cleaned up--but it is an original from some place, so you may want to look at it. But it's the logo.

Swent: So the equipment that you were working with was made of metal in those days entirely?

Wiegel: Yes. Incidentally, there's a very interesting article in the January 1997 issue, or maybe the December 1996 issue of Scientific American on some of the inventions of Hans Albert Einstein. Not our Albert, but his father, of the famous $E=mc^2$. When he was young, "Old Albert", as his family called him, worked with another physicist who later also became a Nobel prize winner, and one of the things they worked on was the development of a non-moving-part pump.

They developed it for use in the refrigeration industry, but because of cost and so forth, it, or modifications of it, was only used by the Swedish firm Electrolux. But when they started--after the end of World War II--to pump radioactive molten materials in some of the devices, use was made of that concept. The electromagnetic way of pumping--I should mention that. We didn't have anything to do with that, but pumps have changed too is what I want to say.

Full Professor of Civil Engineering, 1963

Swent: Yes, they've changed drastically.

Wiegel: Now, after that, in 1960, I was appointed by President Clark Kerr to be an associate professor of civil engineering full-time with tenure. As I said, I never had any intention to go into teaching, I just, step-by-step, became one. I've enjoyed it. I'm certainly pleased that it occurred, but I didn't plan on doing it. In 1963, I was promoted to full professor of civil engineering. Incidentally, John Whinnery was dean when I was appointed associate professor, and when I was appointed full professor, John Whinnery asked me to serve as an assistant dean in the College of Engineering, which I did, and I served in that capacity for eleven years, for John and his successor George Maslach.

I also would like to mention that the department of civil engineering was renamed last year the department of civil and environmental engineering, which I think is a good move considering the way the two words are currently used. It's more descriptive of what is done in this department.

Environmental Engineering: Quantitative Treatment, Costs, and Tradeoffs

Swent: I was wondering whether you might want to talk about how environmental consciousness changed your teaching or your students?

Wiegel: Certainly, the students today--at least the ones that apply to Berkeley--the graduate students--are very interested in the environmental conditions and how you can handle them quantitatively--not just qualitatively, but what can you really do about things quantitatively. We've always been interested in the environment. It started out because the environment would keep belting us--droughts, floods, hurricanes, earthquakes. In my area, coastal engineering, we deal with harbors and ports and breakwaters--the environmental effects are very important; I think that people should read books like Two Years Before the Mast [Charles Henry Dana] and see what life used to be like--how difficult it was.

Swent: I recently read that book, just a couple of years ago.

Wiegel: I used to recommend to my graduate students to read it to get an idea of what life was like back in the early 1800s. We weren't doing so much to the environment, but the environment was doing a lot of things to us: storms, waves. Life was terrible. Floods. It was terrible. Very, very difficult, and many people were killed. Your life expectancy was not very long then. We had to learn how to modify the environment, to decrease the hazards. Then, of course, we got to the place where, because of population pressure, there are just so many people that it became recognized that we had to mitigate many of the things that we had done in the past, and we have to modify what we do in the future.

So you want to examine whatever plans are being discussed; what their effect will be on the environment. But it should be quantitative, because as you know, you have to balance off costs. I remember years ago when I was developing a new course called--it was CE 207--it was on mixing processes. This was a big portion of what you needed to know when you dealt with water quality: mixing one fluid with another, or things in fluids.

I was impressed by a paper I read many years ago on waste disposal in the ocean. Many towns and cities would simply have a pipe or an open channel built and municipal waste and industrial waste--the municipal waste was not that bad, but the industrial waste was, would then go into the ocean and mix with it. Then, people talked about the public health problem. It turned out that it is a public health problem if you're discharging waste into fresh water--a river, a lake--because kids, or adults, when they're swimming, might ingest the water with the bacteria and whatever else was in it. In the ocean, you usually take only one swallow of salt water and learn a lesson.

So basically, you do not ingest salt water when you're swimming, so it's an entirely different problem, but the standards were set, based really on fresh water. There was a professor of sanitary engineering--not here, I don't know where--who wrote an article and he really looked into the public health problems, and then came up with a conclusion which I think is very valid in many aspects. That is: you only have so much money that can be spent. From a public health standpoint, for this amount of money, in his opinion, you would get more for it when you're dealing with discharge into the ocean, salt water, if you spent it for more lifeguards.

He said, "Think about it: if you did ingest this water and you did get sick"--incidentally, there are very substantial and detailed studies by epidemiologists on this subject of the relationship between waste in the ocean and people getting diseases. I'm talking about real professionals that know what they're doing. He said, "Then you get sick, but if you drown, you're dead. What is the better way of expending public funds?" I've always thought about that, because we still have that with us. There's only so much money; what might be the best way of, let us say, having a better quality of life? I don't know. I still read articles on it. Would I be so smart, I would know the answers to these things.

The Junior Course in Fluid Mechanics: Small Lecture Sections

Swent: I'd like to have you be a little more specific about the courses that you were teaching.

Wiegel: Well, there were two graduate courses in coastal engineering.

Swent: You're distinguishing between graduate and undergraduate students.

Wiegel: Yes, and I want to mention it. I almost always taught one or two undergraduate courses. One was the junior course in fluid mechanics, and the other was a hydraulic laboratory section--we had several sections. We made a conscientious decision not to have a big lecture or laboratory section; we had smaller sections, and then when we went on the quarter system, we would give the lecture course once each quarter, and I would teach one of them, Professor Einstein would teach one of them, and Professor Johnson would teach one of them.

Swent: What course was this?

Wiegel: The first course in fluid mechanics, which was a junior course required of all civil engineering students and all mechanical engineering students. At the start, we would jointly work with the mechanical engineering department and we all taught together, and Professor Iversen also taught one, and I think Professor Laird in mechanical, and later, Professor Talbot. So there were quite a few sections because we wanted the classes to be small.

Swent: How small?

Wiegel: No more than thirty students in the lecture course. We found that if you get many more than thirty, you no longer got the students asking questions. That was the reason: you wanted them to ask questions. This is one of things you try to encourage. Our ideas about things change; many of them are right, some of them are wrong; never hesitate to question anything. That's what you want to do as an engineer: question, always question. We did.

In engineering--not just our group--but in engineering, we took teaching undergraduate courses very seriously. We considered the students to be professionals in training. I think that's the difference: they weren't just students, they were being developed into engineers, and we were engineers.

Swent: So you were teaching these lecture sections; how often would they meet?

Wiegel: Three times a week.

Swent: One hour?

Wiegel: Fifty minutes. But we also, because of the large number of students, there were a couple of times we would have two sections in a semester. I guess it was when we went to the quarter system. We had one section that would be of three fifty minute lectures, and then another section--I think this was the fall quarter, when there would be many students taking it--given on Tuesday and Thursday, they would be one hour and twenty minute lectures. This was to give the students flexibility so they could fit it into their schedules. We were serious about it; as far as I know, they still are serious. Of course, having been retired since 1987, I'm not completely sure of what they do today, but we worked hard at it.

Laboratory Sessions Were Also Essential

Swent: Were the laboratory sessions in addition?

Wiegel: The laboratory sessions--we also had teaching assistants, because you had laboratory parties of three or four students and you had to keep your eye on them--remember these are pumps and turbines and things of this sort, and you have to be there, you can't just ask a student to go in and do it. So either a teaching assistant or the professor was there in the laboratory the entire time that the students were there.

Swent: How long was the laboratory session?

Wiegel: I think they were about three hours; that's the best of my remembrance. If they finished it earlier, they finished it earlier. They had assigned--I think--it was three hours. Of course, they had to write up their work. This is another aspect of teaching: writing up. You had the students write about what they did, what equipment they used, present their findings--discuss--and then draw conclusions based upon what they found out, and make recommendations. This drawing conclusions and making recommendations is a vital part of any profession. You have got to do this all the time. You can't just discuss things, you can't just have a debate--you have to draw conclusions and then make recommendations either to your professor--but this is just to train you, so you'll do it for the client.

The only way you teach it is by doing it. It's got to be hands-on. It doesn't do any good just to lecture about it, they have to do it. That requires a lot of work by the professors and the teaching assistants. The teaching--because of many things--cost and other things, and the pressure of adding other things to the curricula--the use of laboratories has gone down and down. Not just here. Last year, at a meeting I was talking to a professor at Caltech [California Institute of Technology] and another one at M.I.T. and both of them said their laboratories have suffered badly. There's not the support for laboratories, and the courses aren't given any more, or in such a reduced form that the students don't get much opportunity.

You get much more in computer analysis. You can't just keep adding things on; things have to drop; the student has got to sleep sometime. That's basically it. Plus the fact that the use of computers is very inexpensive; the use of laboratories is very expensive. So the budget has driven more of it, I think, than anything else. But it's too bad. At least the students who work in research projects have an opportunity to get this hands-on experience. Engineering is still a lot of working with things, or you're designing something. You're designing hardware, or you're arranging hardware into a system. Look at the Boeing 737 and the problem of stability and control. That's a real hands-on problem --just as an example, because you probably have read about it in the newspaper during the last two months.

There are many other examples, but not quite brought to our attention like when an airplane goes down because of a control problem. We have just all kinds of excellent students, and many of them worked in research projects and this gave them an opportunity of doing things themselves. There's nothing like doing something yourself to build up your confidence that you can do something. This is very important in the education of professionals; not only the information, but the self-confidence that I "can do something myself." We've had very, very fine students over the years.

Outstanding Students Condit, Beebe, Prins, et al.

Swent: Would you like to mention any in particular?

Wiegel: Some of them have been particularly successful. The current president and chief executive officer of Boeing aircraft--

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Swent: I would like to have you mention some of the students that you particularly remember.

Wiegel: I mentioned I had a student who was in mechanical engineering as a research assistant when he was a senior, during the summer, and his name was Phil Condit. Phil is the current president and chief executive officer of Boeing.

Swent: He did very well.

Wiegel: Another one I can remember that went way back is Ken Beebe. He went to work for a small structural engineering firm in San Francisco and eventually bought the principals out when they retired, and the name was changed, and his was added to it. It became known as PMB. I forget what the P and the M stand for, but the B is Beebe. It was bought out, and is now a wholly-owned subsidiary of Bechtel Corporation. Those are just two.

We also had visiting scholars from foreign countries, some of whom were Fulbright scholars. In fact, I believe that the first Fulbright scholar in civil engineering was Egbert Prins from The Netherlands. He spent a year with us here, and then eventually--he just retired recently--he became the director of the Delft Hydraulics Laboratory in the Netherlands, which is the biggest such laboratory in the world. Another young visiting scholar was Kyoshi Horikawa from Japan, and eventually he became dean of engineering at the University of Tokyo and is currently the president of a private university, Saitama University, in Japan. When they were here, they were young, so they were the same age as our graduate students, and they all worked together and they learned from each other. Incidentally, many of our graduate students were from foreign countries; many foreign countries.

The Master of Engineering Degree with a Non-Scientific Minor

Swent: Was this something new after the war?

Wiegel: That was after the war. Before the war, there were almost no graduate students in engineering. They were almost non-existent. Of course, remember, many of the graduate students had been our own undergraduate students here at Berkeley, and we, in engineering, really--and that's throughout the whole country--feel that engineering students should be encouraged to get at least a master's degree in order to get a good enough background. This isn't, and can't always be the case, but many of them do go on to graduate work; many of them for a master's degree.

We have two types of master's degree: the master of science, which is a one-year program; and then we developed the master of engineering, which is a two-year program. That is really, in my opinion, the best professional degree--the two-year master of engineering.

Swent: How do they differ?

Wiegel: In one way, of course, it's twice as much work; one was two years, one was one year. But in the master of engineering, you had the same amount of required course work as you would for the Ph.D.--you just didn't spend that extra one, two, or three years of doing a thesis. But there was a big difference. The Ph.D. is oriented toward research and teaching and so forth, but always found to be very useful by companies, and companies hire our Ph.D.'s routinely, believe me.

But the two-year master of engineering had two minors in it as well as the major. One of the minors had to be out of the sciences. It couldn't be in mathematics, it couldn't be in physics, it couldn't be in another branch of engineering--it had to be in, let's say, business administration, and then more in the "person" aspects of business administration. The purpose was to get a different viewpoint; a non-science viewpoint. This has been very successful--for engineers to have that background.

Now, the students were from California, they were from other states, or from other countries, and we always have what we called the bullpen; this is the place where you have a lot of desks and chairs, and the grad students each have a desk and a chair, and they have a coffee mess. They get to know each other, they talk to each other, and they probably learn as much from each other as they do from their professors. But also, because of this worldwide mix--and many of them remain lifelong friends--I think this has done more good than the U.S. State Department, from the standpoint of international affairs. I'm very serious; because I see this time after time after time. Somebody will come, say, from Kuwait, and say, "Oh, so and so from Denmark came to see me. We were both graduate students at Berkeley." It's almost like the English system of the old school tie: they have this tie with that golden bear on it. It's a little bit of humor, but it's very serious. It's very important for civilization.

The Alumni Network of Worldwide Importance for International Understanding

Swent: Many of them have returned to their own countries?

Wiegel: Many of them returned, many of them have become professors, or they became--over the years, director of institutes, heads of companies, and so forth. One of them went back and eventually become professor at the University of Cairo named Mobarek, and then went to work for the World Bank in Washington, D.C., where he still is, and he heads up the desk for port development in all of Asia. They all talk to each other; they still do.

Swent: It's a wonderful network.

Wiegel: It's a wonderful network, and they're friends. You don't fight with a friend.

Swent: And also well-trained engineers.

Wiegel: You get along. They're well trained, they're well educated. There's much more to it than just the technical education that occurs; it's a social and professional education. The university of course has a study abroad program, where you go to Verona or Cambridge, and there's one at Cairo University--many places--and it's still in existence. But also, there have been special programs where we try to help a university get established. Going back twenty, thirty years ago--there was one at Santiago, Chile at Catolica University, and there was one at Kampur in India, and it was one of the Indian institutes of technology (IIT). India wanted to develop institutes in which they could train scientists and engineers who would then graduate and help develop their country.

I think there were five or six of these. One of them was the one at Kampur, and the University of California was one member a consortium of universities to work with IIT Kampur. I remember

M.I.T. and Caltech both being in the same consortium. Between 1969 and 1972, I was the University of California's representative. This was out of the office of the president of the university, then Clark Kerr. The money came from U.S. Congress via the state department or something, to purchase equipment, and to pay for personnel exchanges. We would send faculty over there from these different institutions; they would send faculty here for a year or so--again to get these interchanges of ideas.

Teaching Coastal Engineering at the National University of Mexico

Wiegel: Teaching at a major university is very broad. It's not just teaching undergraduates or graduates, but it's doing these other things: teaching and working with people in other countries and other institutions. I remember once, Professor Johnson and I were asked to go to Mexico and teach--there was a graduate course we gave in coastal engineering, and the Mexicans wanted to develop their coasts and their ports--and they had developed some in Acapulco, for example--coastal resorts. They wanted to learn about all these new techniques that had been developed.

So Professor Johnson and I were asked by Professor Emilio Rosenblath to go, and we did go to the National University in Mexico City[1]--which was a big university--to teach the graduate course--one of the graduate courses we taught here. No modification; they wanted it exactly the same, and taught in English. The students--I think there were seven or eight--and they all were fluent in English, and they all had undergraduate degrees in civil engineering, and we taught the full course there during the summer. This is just an example of the breadth of teaching that goes on. Again, teaching of engineering improved. I think I mentioned already that the big advance made in engineering--at least in civil, and I think in almost all branches, perhaps except for electrical, which had already done it--was during World War II, where it became much more scientific. The capabilities of doing analysis were improved tremendously. Also getting field studies and things of this sort.

Swent: How did the capability of analysis improve?

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[1] Universidad Nacional Autónoma de México. It was done at the request of, and by arrangements by, Professor Emelio Rosenbleuth who was dean of their graduate school. --R.W.

Wiegel: That requires the mathematical formulation of physical problems, and then, of course, later, with the invention of the computer, we could really do some of these things. Before, engineering was really an art, not so much science, but an art. I'd like--I think I mentioned that I had been asked to write a history with Thorndike Saville, Jr.--who was also a graduate student here at the time I was taking graduate courses at Berkeley--so we became lifelong friends, and we're still close friends--he and his wife, me and my wife--

His father was at that time the Dean of Engineering at New York University. Two years ago, we were asked to write the history of coastal engineering in the U.S.A., and this was published last year by the American Society of Civil Engineers in the book that was called, History and Heritage of Coastal Engineering, edited by Nicholas Kraus, 1996.

Digging into the history, I came across a short note by an engineer in New York--his name was Henry Sharp. I don't know who he was or anything, but I'd like to quote this.

> Conditions vary so widely from place to place, that rule of thumb methods are sure to give a large percentage of failures, and a structure successful at one place may be a dismal failure at another. On the other hand, the engineer who wishes to attack this problem scientifically finds that science has done very little to help him. He is almost entirely without trustworthy facts and must work up his data from hasty studies of his own.

Please insert "his/her" for each "his," because fortunately today we have many young women in engineering. (To me they all seem young nowadays.) [laughter]

Swent: Well, they are young.

Wiegel: And especially in the portion of civil engineering that's the environmental aspect. They're very interested in it. It was an art at that time, and it wasn't very good art.

Swent: When was this written?

Wiegel: This was in the late 1920s. It started to change a little bit in the 1930s. Dean O'Brien--that's in my field, coastal and off-shore--was one of the first to really start to change it. But it was World War II, and we have already gone into that in the section on research and so forth. This affected the teaching dramatically, because now we could teach analysis.

Swent: You mean the computer?

Wiegel: Well, no, this is the physics of what was going on, and then the mathematical formulation of these things. That's analysis. Then, when the computers came along, we could do a lot more numerical analysis.

Swent: I just want to be clear. You said that what the relationship is between the war and this change in--

Wiegel: Because, I mentioned this earlier in the interview about the amphibious operations: it was necessary to make reliable estimates of what wave conditions would be at a landing site sometime in advance, and then how these conditions might affect the operation of landing craft and then after the landings, at a later date, the trans-shore shipment of supplies and so forth. This required quantitative information and these techniques were not available, so the military contracted with universities such as M.I.T. and Berkeley to advance these things.

Swent: So it was the urgency of the military?

Wiegel: It was the need and the urgency. It really was urgent, of course. Once in a while you hear in a meeting: "Why did so-and-so write and make this statement when a little more checking might have made it better?" And I'll tell them, "I don't think they had another year at their disposal. 'Let's hold the war off for a year while we check this.'" They don't understand the urgency at that time. I don't think anyone understands urgency unless you were absolutely face to face with having to make decisions.

You see, teaching is, as I said--now this gets me into field work because it's not just analysis, but we didn't have the data, we didn't have the measurements. We had to develop equipment to measure things, and incidentally, I was a mechanical engineer, and some of the first things I did after I was here at Berkeley after the war was work on the development of instruments to measure things in the ocean, and then on how you analyze your measurements and compared them with theories and things of this sort. But I went into that, in more detail.

But the other thing is that we worked in the field, we worked in the laboratory, we worked on theory, we taught, we did research, we did consulting, we did design, we did all of these things. This was a marvelous opportunity develop yourself as a combined professional and teacher; to do all of the things. Of course, this is what makes life interesting too: is that you have this great variety of things that you do.

Swent: I wanted to have you talk also about your development of these instruments for measuring.

Wiegel: The navy supported that, because they knew of their need. There were a couple of people: one of them who was Roger Revelle, who was a navy commander then, but he had a Ph.D. in geology from Berkeley, and he was a geology oceanographer at Scripps, and later became director of Scripps Institution of Oceanography--and he was the one that did come up with the concept which--and the push--to develop what is now known as the University of California at San Diego, and Revelle College is named after him. Because of him, they had let these contracts; Berkeley had one, and M.I.T. had one--Woods Hole--to develop instruments to measure ocean phenomena. We had some very good people. One of them was Frank Snodgrass, who I have mentioned several times.

Swent: Did you work on those as well?

Wiegel: Yes, I worked on one; the thermopile wave meter was the one that I worked on. But there was a non-linear transfer function in it, and I didn't know much about, or anything, about that at that time. Later on, I learned a lot about the fact that you needed to have linear transfer function if we're doing to do spectral analysis of things. That could get into a whole other two hour interview. Needless to say, I learned a lot. Frank Snodgrass knew this; I learned it from him. But he then left and went down to Scripps Institution of Oceanography and developed a number of very fine instruments and installed them, and they did some of the great worldwide experiments because of the development of the equipment. In other words, we did everything; all aspects.

<u>The Successful Work-Study Program</u>

Wiegel: I would like to jump off here, and it's a slightly different thing, because in engineering--at some universities there is something known as the work-study program. It's where after the first two years, a student will get a job with industry or government agency, and work six months, and then come back to school six months, work six months, come back to school six months. So it would take five years to get a four-year degree, including summers. It wouldn't work out, except you had to remember the summer.

At one university, the University of Cincinnati--at least years ago--all students were on that program, but certainly in mechanical engineering. But in Cincinnati, there were all kinds of big industrial firms. O'Brien started it here, and it's still in existence, and it's a good program, I think. It's small. When I was assistant dean, I was in charge of the program; this is one of the main things I did. I was in charge of it for many years. We would always have these students coming back from work. We'd have a little get together in the evening down at the Alumni House--coffee, donuts, cookies--students are always hungry. They would talk about the work experience.

One of the things that would come through is, time after time after time, they seemed to be surprised that, "Gee, the people I'm working for are awfully nice to me." You wonder, did they expect to be bitten or something? It's true; it was something that always seemed to surprise them. One of the other things we found was that the students in the work-study program generally didn't take much longer to get the degree than the students who weren't on the work-study program. One of the reasons is, of course, a lot of students--they were all undergraduate, not graduates, and most students, at least the ones that came to Berkeley, had to work to help support themselves. Not all, but many, many did.

So many weren't taking a full-time load of courses; they would have a part-time job--in the summer they had to get a job and so forth. So it worked very well. Also we found out that most of the students in this work-study program, when they graduated went to work for one of the companies they had worked for in the program. There were two reasons for this: the company had been able to look at them, and they had been able to look at the company. The other thing that really impressed us, was their response when we asked what they did in detail. This was for all the students at the meeting; they all heard each other talk about what they did after they came back. The ones that had finished their junior year--almost all of them had been assigned real engineering work.

Somehow, in the junior courses--they learned enough that they could do a lot of engineering. This made them feel very good. It also made the other students feel good too when they learned, "Gosh, these courses I take are useful." That's a good program, and that's a very important part of teaching if you can get the jobs, and you can have the students take them.

Grade Inflation Did Not Mean Lower Quality Students

Wiegel: We've been asked quite a bit about--after the grade inflation that occurred in the 1970s and 1980s--whether or not this led to a lower quality of the students at Cal.

Swent: What do you mean by grade inflation?

Wiegel: A student thirty years ago, for a certain amount of work, would get a C, and then after a while, the class average became a B, not a C. This has occurred in almost all universities, except perhaps at West Point and the Naval Academy.

Swent: You mean they give more B's and A's now than they used to?

Wiegel: Yes, than they did. This is called grade inflation. It's not so much in engineering as in others, but it still occurred--it is occurring. Well, certainly this is my opinion from my observations--I checked this out with others too--we all agreed that it did not lead to a decrease in the quality of the students. The grades might have been inflated, but the students coming in were still good; what they were learning was very good; the engineers going out were very good. So although there was inflation, it didn't affect the overall capabilities. I think this is important: the quality has remained high. That's very important.

Swent: When did they change from A's and B's to numbers, and what effect did that have?

Wiegel: We still give A's and B's, only each one had a number, so you add up the numbers. An A is 4.0, an A- is 3.75, a C+ is--I forget. But we gave students the grades, A, B, C, and then we'd give these numbers and we would add them up and all that kind of stuff. I heard one person suggest that what you really should do--and I think one university did it that way so you could judge--on the student's grade record would be, say, A, or A-, or B, and then slash, and you'd give the average grade given in that class. In other words, if a student was A-/B+, it wasn't quite as good as A-/C+. But we didn't do that.

Berkeley's Successful Extension Program for Continuing Education

Wiegel: Another aspect of education is what is sometimes referred to as continuing education--university extension. The Berkeley campus has done this very successfully for many, many decades.

Swent: You were a pioneer.

Wiegel: No. In one aspect, yes, I'll mention that later, but not in extension in general, just one aspect of it. The work may be done with full courses, and sometimes these are the equivalent of a course given on campus--the same one. This was for people who were working full-time, so they would take a course at night or weekends. It may be in a subject that was not available when they were a student. The computer field, for example: the numbers of courses given in computer science--hardware, software--by Berkeley is great.

These may be given on campus, they may be given in San Francisco, or down on the Peninsula. They may be given by those people down on the Peninsula in Silicon Valley who are working on developing the newest thing in a particular piece of computer hardware or software--they'll teach, they'll work at night. They don't have to. It's an attitude: they're doing it, they're interested, they want other people to know about it. This has been marvelous. They may be courses entirely different. Especially, say, in the arts and the humanities. I don't know too great an amount about the details of those, but again, this is the quality of Cal.

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Wiegel: The College of Engineering made surveys of the graduates of our undergraduate programs--one year after graduation, five years after graduation, and so forth. After the first year out, engineers would say we wished we had more design in our courses; the first job usually being in design. Five years out, we wished we had more on analysis--mathematics and physics--because they're looking in more depth. Ten years out, we wished we had had more management. Fifteen years out, we wished we had had more humanities. Twenty years out, we wished we had had more in the arts. I'm not talking about everyone, but kind of general--this is the way life changes. So extension activities are useful in all of these. Students go out to classes, if they have the energy, at night or weekends; they catch up on these things that they see they were missing, or that they wanted to take.

The First-Ever Short Course in Earthquake Engineering

Wiegel: Other ones are called short courses. These are something you give in maybe one week. An eight-hours-a-day type of thing for a week. Or maybe not quite that, but usually it would be about a week. In 1965, we gave the short course that was named Earthquake Engineering. Several of the engineering and seismology faculty here at Berkeley developed this, triggered by Karl V. Steinbrugge, who was a Cal graduate, who was a civil engineer with the Pacific Fire Rating Bureau.

In addition to the Fire Rating, of course, and go back to the Great San Francisco Earthquake of 1906--fire did more damage than the earthquake directly, so the Pacific Fire Rating Bureau--which was a kind of consortium of insurance companies--what they wanted to do is to make things safer. In other words, the best thing to do in the insurance business is to make things safe so that they don't burn down or they don't shake down. You prevent the damage. Karl was the leader in that group and an expert on earthquakes and the damage that earthquakes do to buildings, and what you could do to minimize the loss. He suggested we give a course on earthquake engineering, and we agreed. We thought this was great.

A small working group was formed of several faculty members and Karl Steinbrugge. I was asked to chair it--I guess this was because I was an assistant dean. I had worked in one aspect of this: tsunamis. These are sometimes called tidal waves, or seismic sea waves; and these are the giant waves that are generated by the tectonic displacement of underwater earthquakes, or a very high-speed underwater landslide triggered by an earthquake. I had been doing a lot of work on this one aspect. This is an important aspect of underwater earthquakes. For example, back in the 1700s, or something like that, when Lisbon had a huge loss of life--

Swent: 1755, I think.

Wiegel: Something like that--of the earthquake. When you looked into it, it wasn't the earthquake, it was the tsunami that was generated that killed almost everybody. Two of the great earthquakes in Japan along the Pacific coast were in 1896 and 1933, and a lot of the loss of life was because of tsunamis. I was doing a lot of work on one aspect. We organized it, and there was a series of lectures on different subjects, starting off with Bruce Bolt, who was the professor of seismology and director of the seismographic station here at Berkeley. Incidentally, he was chairman of the academic senate two years ago, and is currently president of the faculty club--so Bruce has remained very active. At one time, he was chairman of the California Seismic Safety Commission, which reports to the governor--that's Bruce Bolt.

So first a lecture on seismology, then geology, and then the technical aspects of foundation design--how you design buildings for seismic safety. We did it via University Extension. They sent out a brochure, and we were inundated with applicants. There were so many, there was no way we could get them all in. We talked it over--I think there were fifteen of us--by now we'd gotten all the speakers and so forth. They were from here (Professors Harry Seed, Ray Clough, Joe Penzien, Bruce Bolt, and others), from Caltech; one of them, of course, was George Housner, the great person in earthquakes; another was Don Hudson. The person from the University of Illinois, Nathan Newmark, who, in the United States, was the number-one person on impulse loadings of structures--that is, explosions; Karl Steinbrugge; then the presidents of two San Francisco engineering companies, structural engineers that did earthquake designs: Henry Degenkalb and John Blume. There was also Cal's T.Y. Lin; the famous T.Y. Lin of Cal and of private practice. Another, from Chevron corporation, John Rinne, later became president of the ASCE.

So we decided we would have two sections, one starting on Sunday instead of Monday, and we would have Sunday, Monday, Tuesday, Wednesday, Thursday for one section, and then we'd have Monday, Tuesday, Wednesday, Thursday, Friday for the second section. So each of us gave the lecture twice, and that way we could keep the number of students down in each section.

They weren't just engineers from companies that attended; it turned out that professors of many universities, and many government agency engineers wanted to take it. It was new. I'd like to mention that five of the lecturers have since been awarded the National Medal of Science at the White House at different times (George Housner, Nate Newmark, T.Y. Lin, Harry Seed and Ray Clough), and nearly every one of them became a member of the National Academy of Engineering or the National Academy of Science, or both.

The Resulting Textbook: Another Aspect of Teaching

Wiegel: The people that took the course said, "There's no book on this subject. We want a book." So I was asked to try to get all these people to write a chapter on their presentations. Some of these were the top people. How did you get them to write a chapter?

Also, we had told them they didn't have to worry about writing anything; and then come back and ask them to do so--I didn't relish this.

Swent: Even though they had given lectures at this conference?

Wiegel: Yes, they had given lectures, heard the students' comments, and they understood the need. Also, we said, "It would look funny if we don't have a chapter from every one. People might say, 'Gosh, wasn't his good enough?'" No, I didn't say this, but you twist arms a little. However, they were all nice people and thoroughly professional. So I was asked to be the coordinating editor and Prentice Hall published it [Earthquake Engineering, 518 pp.]. By the time we got all the papers written and everything, it was 1970 before it was published. It wasn't easy to get together, but it was the first book on the subject and it was used worldwide for years. Many universities used it as a textbook. So again, that's another aspect of teaching: not only did you teach directly, but you developed a textbook that many other people can use.

Swent: Did you have any student help with the textbook?

Wiegel: No, this was really state-of-the-art. This was the sort of thing that only the leading person--

Swent: I just meant with the mechanics of it.

Wiegel: Not that, no.

Swent: You had secretaries who helped you?

Wiegel: I had no help whatsoever.

Swent: You did your own typing and everything?

Wiegel: No, each author prepared their own. I can't tell you, for example, whether or not George Housner had help at Caltech--that I don't know.

Swent: You did the editing?

Wiegel: I did the coordinating editing. It was just to be sure there weren't gaps. There's a big difference. When I became editor later on of the journal Shore & Beach, that's different. I read, I edited, and exchanged comments with an author. This wasn't that sort. This was just to look for gaps and to try to get consistency; that's all. You didn't have to edit the writing of these people--they were all pros.

Chair of the University-Wide Extension Program

Wiegel: I guess it must have been because of that successful short course, because, just after it was given, I was asked to serve as chair of Berkeley's academic senate's committee on extension. Not just serve on it, but to be chair of it. Then shortly later, I was asked to chair the university-wide committee on extension. That's all extension activities: here, UCLA, UCSF Medical School--all of these things. Continuing education of the bar--all of that came under the university-wide extension. Each campus had a dean of extension, and we were the liaison between the dean and the regular campus faculty, academic affairs, and so forth. So I've always had a real--

Swent: How did you feel about that?

Wiegel: That was a lot of work. But again, I feel very strongly that this is a major activity of a university. You're serving a very broad-based group of people. You're doing a good service in many, many areas. As you can see, I've had a tumor in my inner ear removed surgically, I also had open-heart surgery. So I'm very grateful that the medical profession has a very excellent, extensive extension service teaching doctors new procedures. [laughter]

Swent: So that they're up to date. That's right.

Wiegel: So you've got the professions. But then there's this whole other portion which is the quality of life: the humanities, the arts, which are made available by so many extension courses. They're very popular. I don't know if you've looked into--

Swent: I've taken a number of them.

Wiegel: So you know.

Swent: I'm well aware of them; they're great.

Wiegel: And people do a good job. Once in a while, it's not so good, but mostly, they do a very good job.

Swent: The ones I've attended have been very fine.

Wiegel: I organized other short courses--which had nothing to do with being chairman of the committee--with some other people like Professor Gerwick and others, and Paulling in Naval Architecture, in offshore engineering. Lectures were given by faculty, engineers in industry, and in some government agencies. This was for the oil companies and engineering companies; also professors from other universities attended--the drilling for oil and gas in the Gulf of Mexico and the North Sea. We gave the first ones given any place, and we were doing much of the research at that time, both in civil engineering and naval architecture. People came from all over the world to take those courses. They would come from England, France, Norway, Japan, when we offered a course in offshore engineering.

Swent: Where did you give them?

Wiegel: Here on the Berkeley campus. Sometimes at a hotel at the Berkeley Marina. Those were always short courses. We learned our lesson after the first one. Each person giving a lecture was asked to prepare a written hand-out that the students could have. Do you notice all the big thick blue binders in the bookcase behind me? Each one of those is a short course series of notes on some course that I was associated with. The last one that I had anything to do with was after I retired, and it was just to be a participant. It was a university extension course held on the Clark Kerr Campus--you know that nice campus that used to be the California School for the Blind before they moved? This was on estuaries and coastal wetlands: the mixing processes in estuaries, geology of estuaries, the biology of estuaries, the chemistry of estuaries--all of these things on estuaries. You know, San Francisco Bay is an estuary.

The leader of this, who organized it and so forth, was Peter Goodwin, who had been a graduate student at Berkeley from England. He did his Ph.D. here at Berkeley. His major professor was Hugo Fischer, who was tragically killed in a soaring plane crash. We were able to arrange to have Norman Brooks, a professor at Caltech and Fischer's mentor, step in and help out with the thesis.

Goodwin then went back to England after his Ph.D. and taught there. But then I guess he found he liked the climate in the San Francisco Bay better, so he came back to the Bay Area, and he worked for a firm in San Francisco that is well known in the environmental field--Phillip Williams and Associates. He was the organizer of it. That was only about three or four years ago. That was very successful, too. Incidentally, Peter Goodwin is now a professor at the University of Idaho.

People are so interested in the environment, and this was all quantitative--all of these different aspects. Again, you see, that's teaching. But the people were not only from government and companies, but from other universities and other countries.

I somehow skipped--and I remember--and it must have been when I was first appointed associate professor and Glenn Seaborg was chancellor at that time, and he had a reception for new faculty in the chancellor's home on campus. We were talking about how long it took to prepare a lecture, and how long in advance it took. I said, "Well, I have to prepare mine a few days before, and then I go over the notes the night before." He understood that, but what he usually did was to prepare three lectures in a row, and go through the notes once for the three lectures in a row. I thought, "There's a difference. What a mind!" I found that kind of interesting.

Member and Chairman of Chancellor's Advisory Committee on Education of Military Officers (ROTC)

Wiegel: There are other aspects of education that I would like to add. One of them is the education of prospective military officers: this is ROTC I was in ROTC when I was an undergraduate, and very appreciative that I had that opportunity. ROTC is very important from a national standpoint. But also, it's about the only place in the university that leadership is taught. How do you lead? As important as how one leads is, how does one make decisions under extreme stress? It may be in war, it may be when you're practicing landing an aircraft on a carrier, it may be when something goes wrong with a power plant, and you have got only a few seconds to make a decision. So the ROTC program serves a use broader then just preparing people to become military officers, most of whom are simply on a reserve status.

I have been on the chancellor's advisory committee on military officers education program since 1978. I'm still on it. I served as chair of it on two separate occasions. Some of the best comments on the value of ROTC programs in universities such as Berkeley are in one of the publications that are the printed version of talks, discussions, question and answers of the Fleet Admiral Nimitz lectures on national security affairs here at Berkeley. It was by John Lehman, who had been, amongst other things, secretary of the navy as well as a navy pilot, and is currently chairman of Lehman Brothers of New York, the investment brokers.

His lecture series were "Naval Power after Cold War Victory." This was the first speaker we had after the collapse of the Russian empire which was called the Soviet Union. A major point he made was that the founders of the United States were worried about military take-overs of government. They had seen the experience in European countries, and they didn't want it to happen. We do have three military academies: West Point, the

Army; the Naval Academy, or Navy and Marine Corps; and the Air Force Academy. But we also have ROTC programs in many universities, and during the troubled times in the sixties and seventies, many universities cancelled them, such as Stanford. The U.C. Regents told the faculty this is a regential matter, not a faculty matter; we will continue ROTC programs!

So Berkeley continued to give ROTC, but there has been a much bigger input from the faculty, and this is via the committee the chancellor established on military education, and it's still in existence. It's still very active. It has good members on it. The current chairman is Tom Barnes, who is jointly a professor of law and a professor of history. Bob Middlekauff is on it--he was provost here and then went down to be head of the Huntington Library, but is back. He's a former Marine, and as they say, "Once a Marine, always a Marine."

Everyone on the committee was a former Marine, Navy, Army, or Air Force. We all had experience. We're running out. This is why, even though I'm retired, I'm still on it. We're running out of faculty people with three, four, etc., years of military experience.

The Importance of Having Well-Trained Civilians in the Military

Swent: You seem to have felt very good about this.

Wiegel: I felt very good about it; still do feel very good about it. Anyhow, the point is that we want to be sure that our military is still civilian and from many levels of our population and from many areas of specialties. We need engineers, we need pilots, we need humanists, we need business administration, we need all kinds of people to get different viewpoints, and to make sure it's civilian, not a military hierarchy. So we need the military to prevent wars or to minimize them or to fight them if need be, but we also want to be sure it's always civilian from throughout the country so there is not a military takeover which has happened in many, many, many, if not all countries. I consider that a very important part of teaching and related activities.

Advising Graduate Students: Matching Talents and Problems

Wiegel: I'd like to mention that I retired from active teaching in June, 1987. I'd been here more than forty years in either research or teaching, and became a professor emeritus. However, I still give several lectures each year in a few courses, and I give a seminar in the interdisciplinary ocean engineering seminar, E 201. Incidentally, I'm scheduled to give one this coming Friday on this subject. I also help, when asked, with students in their individual research. I am in each morning by 8:00 a.m. (0800) with my door open and it stays open the whole time I'm here in case a student wants to drop in.

One of the very nice things about the university is that when you're retired, if you still want to be active with the research and students, you can do so. I have done this. I suddenly realize I never mentioned anything at all about the students and their Ph.D. theses. This is a one-on-one where they work with you and a few of the other professors. I was supervisor of twenty-six Ph.D. theses.

Swent: That's a lot.

Wiegel: That's a lot. And I've been on a number of other Ph.D. thesis committees, and I've supervised a number of mini-theses for the master of engineering program. This is one-on-one working with students, teaching, and so forth. One of the things that you learn when you supervise a student's Ph.D. thesis, you try to match the particular sets of talents of a student with a problem. Every problem is different; every student has a different mix of capabilities. Somehow you have got to match these so the students can go ahead and do a good job.

Sometimes if a student hasn't done a good job, it's because a professor hasn't matched the student with the problem. Then you have got to encourage the student. You talk to them, you keep their spirits up. You give them ideas, directions, who else to talk to, what courses they might have to audit to help out. Then you have to tell the student to stop. You've done enough, write it up, and leave. Get a job. Because if they really get interested, they always want to do one more thing. Sometimes you have to say, "Stop."

Swent: Enough is enough.

Wiegel: So there's four things: match the students, guide them, encourage them, and get them to finish. I'm sure there's many things that I've left out.

Swent: I have time if you do. Do you have time to go a little more?

Wiegel: Yes, if you have some questions.

Swent: I'd like to have you be a little more specific about some of your--

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Swent: Particularly--and it might come into this discussion of Ph.D. theses--how much input or consideration was there for specific problems that industry was facing? Did you have advisors from engineering companies, or would you gear a research--you were doing research for--I'm not phrasing that well.

Wiegel: One main factor of importance in working with companies or government agencies was, say, doing work on projects where you would see that you just don't know the answer to a question. But, from an engineering standpoint, somebody with more experience or sometimes yourself would say, "Here, we can go ahead and do this--using such and such safety factor in there you can proceed." But then you file it away, thinking, "One of these days, somebody should learn more about this."

This is where the matching comes in; trying to match these various assorted problems that you keep on discovering, with somebody's particular set of capabilities. Then, of course, because very few if any of our students come from wealthy families and could support themselves in graduate school--somehow, you have to have a research contract, either from government or industry, or something like that. When I say government--NSF, or ONR, or it might be the State of California. So many of the theses were in research to develop new knowledge in one of these areas in which we didn't know the answer to a problem.

A Practical Problem with Pumps in India Leads to Research Projects

Swent: This is what I wanted to bring out: that there must have been a relationship with--

Wiegel: Earlier, I mentioned the cooling water system for the power plant at Tarapur, India. It is a good example; one of the people who was working as a consultant to the joint Bechtel-General Electric Company, was Dick Eaton. He had retired as technical director of the Army Corps of Engineers Beach Erosion Board. Former to that, he had been an engineer at the Los Angeles District of the Army

Corps of Engineers. Dick was a very good observer and had lots of practical experience. He had come back from India at Tarapur, and we were talking--i.e., Dick, myself, somebody from Bechtel, somebody from GE--about the intake structure.

This was in a rock reef area. There was not sand there; it wasn't a sandy beach, it was rock. They were going to rock dredge an entrance channel and a stilling basin in the rock, and then install a series of big pumps to pump ocean water through the condensers, and out of the condensers through an outlet back into the ocean--maybe a half a mile away or something like that. The question arose: how much wave action can these pumps take, and still behave in an appropriate manner? The companies had asked the pump manufacturers, and the pump manufacturers said, "As little as possible." That's not a very good answer. We never did find out how little "as little as possible" was. This is engineering, as opposed to science.

Dick Eaton had observed wave action at several places where inlets to the harbors had been dredged--that is, dredging a channel deeper than the natural water depth. In some cases, they dredged the channel seaward of the entrance jetties; he observed that the waves coming in from the ocean got smaller for some reason, so that the waves that penetrated into the harbor basin were much smaller than if they didn't dredge that entrance seaward of the jetties. Engineering observation, no theory. But he had observed it. So we at Berkeley made a hydraulic model of this, and dredged it, generated waves mechanically, and sure enough the waves were smaller. Only now, we could measure them, and we could say how much smaller. By lengthening the channel out from the pumps, and increasing the dredged depth, we could get the waves small enough that the pump people said, "Yes, we think that's okay." Why? Now this gets into science, research, Ph.D.s.

A little later, there was a match: there was a student that had the background, had the courses, had enough ability, and he went to work on it. This, of course, is again an example of why you have several faculty members on a Ph.D. thesis committee, because I understood a certain amount about it, but some of things were pretty darned--very subtle, to be frank. Professor John Wehausen, a professor of engineering science, was in the department of naval architecture at Berkeley--he was aware of some theory that would indicate that this should be the case. He couldn't get the exact answers, but he could get regimes.

So we had this student, and he was from Egypt. He had been working for the Suez Canal Authority, and the Egyptian government said he could come, and helped him, and he met all the requirements and he was here. He was a very bright person. He has incidentally become--step-by-step--the director of research, the director of engineering, and he's in now the number-two position in the Suez Canal Authority, I understand. The top technical person.

Swent: Do you recall his name?

Wiegel: Nabil Hilaly. He did his Ph.D. thesis on this subject; both in hydraulic model and theory, and developed, then, the techniques to be able to analyze and say why this would happen. Professor John Wehausen was on the committee, and provided substantial theoretical insight. There's the consulting thing by this other person; the work we did on a contract between the university and the company solved the engineering problem, but did not answer the scientific question. The Ph.D. thesis answered the scientific question. His work was published in a refereed journal.

Swent: Then you have the whole thing.

Wiegel: I think that's a very good example.

Swent: That's an excellent one.

Wiegel: There are several others, but that's the one that I remember most clearly.

Swent: If you could detail a couple of others, it would be very helpful.

Wiegel: Let's see.

Swent: You mentioned Bechtel. Have you done much with Bechtel?

Wiegel: Yes, over the years; one project was concerned with the possibility of a future mudflow causing wave overtopping in the design of the rehabilitation of a dam and reservoir after the Mount St. Helens volcano eruption in 1980.

One of the things that we worked on for a long time was wave-induced forces on off-shore oil structures. I talked about this earlier: we did both laboratory and field work. One of the things we observed when we put an instrumented pile in the surf zone--this was down in Monterey Bay--and measured the waves with a recorder and measured the strains in the pile induced by wave forces on the pile--we saw that when a wave broke against the pile, suddenly you would get a sharp peak in the force--just up and down--not over a tenth of a second in duration.

Swent: I think you have gotten into this.

Wiegel: We did it in the laboratory, and instead of a tenth of second, it was a hundredth of a second. Filed away. No idea as to what in the world was responsible for it. Decades later, Paul Kaplan, a naval architect who had been working on the problem of the landing of sea planes--which incidentally was the outgrowth of what the great von Karman at Caltech had done during the war on the impact load on a sea plane or other airplanes being "ditched" hitting the water surface--. Paul Kaplan was looking at a problem with a British company that had an off-shore platform in the southern North Sea. This was one of the first ones for recovering gas.

One of the cross members failed--what they called fatigue failure--they blame much on fatigue failure. They don't always understand what has happened, I hate to say it. A lot of it is fatigue failure, but lots of times fatigue simply gets blamed because you don't know what it is. It got worn out, so you could say it was fatigue, low-cycle fatigue. But it broke, and another one broke. That person, Paul Kaplan, had a flash of an insight that it was an impulse load--remember engineers use F=MA; force equals mass times acceleration. But really Newton used the summation of forces is equal to the change in momentum, and momentum is the product of mass and velocity.

When it came to water loads--what part of the ocean is exerting the load? Is the small portion of the mass of the ocean imparting the load changing with time? The answer is yes. So he saw it. Then, I took that--this wasn't a Ph.D. thesis, because this was much later--there was nobody to match with the problem. For a vertical pile in the wave breaking zone, the surf zone, using the same concept, I found that we could predict these things--making use of this concept. We had been designing these things by putting a safety factor in. Now finally, after maybe twenty-five or thirty years, we finally understood the physics of it.

You see how much better that is when you teach the students, and you could teach them the physics, and you say, "You see this peak? You see how high it is, and how short the duration is? Now here's the formulation and the mathematics." It's not easy to make the calculations without computers, but now we can compute it. So that's a second example.

Pioneering Research on Mixing of a Surface Buoyant Jet

Wiegel: Another example is concerned with a part of water quality, thermal quality. Much of it was done after our work on the power plant in Tarapur. When the intake water's heated and then discharged, it's not that it's warm, although the temperature is what you're interested in for marine biology studies. But the mixing has nothing to do with the temperature as such, it has to do with the fact that it's less dense after you heat it. It's called a buoyant flow. The discharged water wants to stay at the surface, because it's lighter than the ocean water. It doesn't want to mix with the cooler water below, but it does mix because of turbulence. There was little that anyone had done on the mixing of a surface buoyant jet. Almost nothing.

We, and others at MIT and Caltech, had worked on it; this is why GE and Bechtel asked us to do a hydraulic model study for the Tarapur project. We knew that from a practical engineering standpoint, engineers could go ahead and design a system; there were systems in existence. But there were, over the years, additional studies--and part of this was supported later by Pacific Gas and Electric Company, because of the Diablo Canyon power plant--we did a lot of work for them. Hydraulic models--we worked with them when they were making their full-scale tests--compared model results with full-scale measurements. I think there were either two or three Ph.D. theses that were supported before we really understood the physics and could do the analytical and numerical calculations.

It wasn't until we compared the results of our hydraulic models with the extensive, very expensive full-scale measurements that PG&E made at Diablo Canyon that we knew the problem was solved through the use of an undistorted densimetric Froude number in modeling it. Now perhaps one can make useful estimates using a numerical model--but I don't know for sure as I haven't kept up with this type of work. But still, I believe a large hydraulic model, using undistorted scales, might have to be made because of the complexities. I wish I had the time to go into this again. Some of the support during the early days was by National Science Foundation, some was supported by the State of California water resources group--because it's a water quality problem--and then later a large part by the Pacific Gas and Electric Company.

For decades, there were three groups working on this type of problem: myself and Fischer here at Berkeley, Norm Brooks and his colleagues at Caltech, and Don Harleman and graduate students at M.I.T. Later, many around the world were doing it, but for years there were only these three places where this sort of work was done. That's a major input to the water quality thing: to get quantitative answers so you know what's going to happen. That's needed by marine biologist as an input to his/her work. They want to know what the temperatures are and things of that sort, and we have to be able to tell them what they are.

Swent: You say everybody's doing it now?

Wiegel: A number of universities in the U.S. and France and Japan and England. But the early work was done by these three groups.

Some Special Graduate Students from Egypt

Swent: Just one other thing: you mentioned Egypt a couple of times. Did you have a lot of students from Egypt?

Wiegel: Yes. At one time--and this has got to be thirty years ago or more--the dean of engineering at the University of Cairo--shortly after Egypt took over the Suez Canal--of course, you've read articles, you heard statements at that time of how they couldn't run it, but of course they could; they have run it, and have done an excellent job--he wanted to have some of the brightest students do graduate work in this field of coastal engineering because it would apply to the Suez Canal, the inlets, the canal operation, and so forth. I don't know the connection, but somehow at some place, he had met Professor Johnson. Joe said they would have to meet the university requirements. That's all there is to it.

So the Dean of Engineering at Cairo University chose these--I think it was five--and they were carefully chosen. Believe me, they _were_ and _are_ smart. They were the cream of the cream because they were with the biggest money-maker in Egypt--the Suez Canal. So they sent their applications, and there was no question. The graduate division read them; accept them! [laughter] Not going to get anybody better than this. So we did, and I don't think they all came at one time; I think there were three, and one and one or something like that. I think there were about five. I know one of them is currently professor at the University of Cairo; one is now the number-two person in the Suez Canal Authority; one, Ismail Mobarek, is with the World Bank and prior to that, also a professor at the University of Cairo; one, I think, died of a heart attack.

Swent: Are you still getting students from Egypt?

Wiegel: No. We have got a professor from Egypt, Mustafa Foda. He didn't do his Ph.D. here; he did his Ph.D. at M.I.T., but he's professor here now. He was an undergraduate student at the University of Cairo, and studied there under Professor Mobarek.

Swent: But this was sort of a one-time thing?

Wiegel: That was a one-time thing. We had one or two before; we've had several after. But the ones in my mind are the ones--we had others that come in structural engineering, and sanitary engineering--I have got to be careful. I'm talking only about my personal experience.

Swent: Yes, that's what I'm asking about.

Wiegel: In water resources and so forth, we've always had them from Iraq, Iran, Lebanon, and Israel.

Swent: What other countries in particular?

Wiegel: Canada, Mexico, Venezuela, Australia, the Philippines, Korea, Malaysia, Japan, China, Taiwan, England, France. Many, many--even one from Iceland.

Swent: Everywhere.

Wiegel: As I said, so many of the students became good friends and remain good friends. I think that responded to your question.

Swent: That's fine, yes. Is there anything else you wanted to mention before we wind up here?

Wiegel: I'm sure as soon as you leave, I will. That's the standard operating method.

Swent: Make a note, and we'll catch it up next time.

Wiegel: The first thing I said this morning was: by the way, last time I forgot. Next time, I will start off, "By the way."

Swent: That's fine, that's just fine.

[Interview 4: February 19, 1997] ##

Swent: Before we get started on anything a little more formal, we were just chatting here about how things get forgotten, and that, of course, is what oral history is all about: trying to preserve those things. You were pointing out to me a report of a conference held in 1965. It's a big, inch-and-a-half-thick volume called <u>California and Use of the Ocean,</u> which is the proceedings of a conference?

Wiegel: No, this was a plan.

Swent: A plan that was put out by the University of California Institute of Marine Resources: <u>California and Use of the Ocean</u>. What started this is we were talking about a conference that Orville Magoon is co-organizing or co-chairing which is going to be held March 24-27, 1997, entitled, "California and the World Ocean '97." It's almost the same topic, and going to be held down in San Diego. Bob was saying the importance of updating things and sometimes repeating things just so they don't get lost.

Wiegel: Well, the thing that came to my attention was that yesterday afternoon, we were having a two-hour meeting for--you might call a steering committee, or something like that--for the Water Resources Center Archives. One of the things that was mentioned is how many consulting engineering reports that are made for clients about water resources, and other subjects of course, are forgotten after a few decades--many of which have extremely interesting data and recommendations. It's not that anybody does it on purpose; it's that everybody is busy and they may or may not write a paper on it or an article on it, and after a few decades it becomes lost except if these companies are willing to give copies to something like the Water Resources Center Archives.

This recalled to me that you may even print large numbers of reports and they get lost; people don't know about them. You can talk about bringing things up on the computer, but you have to have something to start with. Unless you put in some of the correct code names, you never will find these things. I don't know whether or not the people that are organizing this new conference are aware of what occurred more than thirty years ago, but I'm going to certainly send a note to Orville Magoon and mention this to him.

He may well be aware of it, but I'm going to mention that we have a copy of the report in the archives here on the Berkeley campus.

Swent: It's very possible that if everyone assumes everyone else knows it--

Wiegel: I know things can get forgotten because--

Swent: They certainly can.

Wiegel: --I have been trying before each of these interviews to go back and refresh my memory about things, and some of the things I brought up I remember fairly well, others I just remember, and then I find things that I had forgotten completely about.

Swent: That's very easy to have happen, isn't it? We do want to talk about the water archives.

Wiegel: Let me pick that up just a little later this morning, because it is a very important part of the university, and it's well thought of; I know that. The people that work in it are highly professional and dedicated and work well with the students as well as people from the outside. It's not just a university activity; it was founded by an act of the state legislature many years ago. O'Brien was one of the people that worked with the state legislature in establishing the Water Resources Center, and then the archives became an important part of that center.

Professor Joe Johnson of hydraulics was the first archivist, and one of the reasons the collection is so good is that O'Brien and Johnson knew many of the early engineers that worked on water supply in the state of California, and because Professor Etcheverry taught here and practiced irrigation engineering in California and elsewhere for decades, and was well known and thought of. That is: irrigation, hydro-electricity, water supply for cities, flood control. They knew these people, and they talked to them, and said, "Why don't you turn over your consulting reports and data to this new archives so it won't get lost." This is what they did, and something they have been doing ever since. It's a very important thing to this state, and it's important to people in many other parts of the country and the world. It's well used. It's now on w.w.w. worldwide web.

Swent: So that means it's really important.

Wiegel: Yes. People become more aware of it.

Swent: Well, I think that everybody has become much more aware of the importance of water within the last few years worldwide.

Wiegel: We had a little joke yesterday. Don't say scientists don't have a sense of humor. We were thinking of California's water, and we were thinking of floods and droughts, and this is the California equivalent of the Chinese yin and yang. But ours is flood and drought. [laughter]

Swent: That's right.

Wiegel: I'll mention that a little later under governance of the university.

Swent: Yes, all right. That's what we were going to talk about today.

The Richmond Field Station

Wiegel: But I'd like to, if you don't mind, pick up a couple of items that I had forgotten about last week when we talked about university and teaching. As part of it, you asked about the Richmond Field Station. I made a few comments at that time, but I would like to add to what I said. Originally, it was named the Engineering Field Station, but the name was changed by Chancellor Kerr when he was chancellor of the Berkeley campus, I believe. The property was purchased by the UC regents shortly after the end of World War II, and many details about this development are given in the oral history by Mike O'Brien.

Most of the large engineering laboratory facilities were built there. It's located in Richmond, at the edge of San Francisco Bay, and it's about a twenty-minute drive from here, which made it rather inconvenient for students and faculty to use, but the alternative of not having these large facilities would have been detrimental, so it was well that this was done. But it was inconvenient, unless you were there full-time. I spent many years there; in fact, until I was full-time on the faculty. Then I maintained an office there for many years thereafter, and my graduate students and I used the facilities right up until I retired in 1987.

Swent: Is it at the water's edge?

Wiegel: It's at the water's edge, and several of the large facilities are the model basin and the naval architecture laboratory--the ship model towing tank. Specifically, what I wanted to mention about that--because it might get lost--the information--the rails on the towing tank, and also on the model basin, are very special rails. They were shipped to the university by the navy's ship model towing tank (located in the vicinity of Washington, D.C.), when they upgraded theirs in the mid-1950s. So we were fortunate to get these. Also, the towing tank is built on deep bay mud, so the whole thing floats like a reinforced concrete ship and the rails can be adjusted both for alignment and for vertical accuracy. This has had to be done several times since the original facility was built, but it permits the facility still to be used.

Swent: You say, "still to be used?"

Wiegel: To use a towing tank, you have to have very accurate alignment of the rails, and it must be level. By being able to adjust this, you can continue to use the facility. If we had not done it, it wouldn't have been operational.

Swent: Because of the underlying--

Wiegel: Because of the gradual differential settling. Because of the fact that it was built on this deep mud.

Swent: I see. Was it affected in the earthquake of '89?

Wiegel: I can't tell you whether or not it did settle--I was retired then --but it could have been easily--it's quite a bit of work to align up and so forth--but they're still using it, I know. Only now, what they're towing instead of a ship is a three-dimensional model of an airplane, and it is towed underwater in order to study the tip vortices if formed. This is being done by the department of mechanical engineering, and I think it should be mentioned that the department of naval architecture and offshore engineering--as it's now called--is being disestablished at the end of this academic year. Some faculty will go into civil engineering, some into mechanical engineering, and the ship model towing tank will become a part of the department of mechanical engineering. The hydraulic engineering model basin and wind wave tank at RFS are in "mothballs"--closed down and locked up. There is no staff. I find it sad.

Some of the details of the facility have been described. I looked up to find out about it. Forty years ago almost, Mac Snyder, Karl Bermel, and myself wrote a paper in which we described all of these facilities, plus the ones on campus, in hydraulics and coastal engineering with drawings on how the things operate and so forth. So that's in the record in what is called a--there's two terms: a refereed journal and an archival journal. In other words, it's in a journal that's kept by many libraries.

Development of Pioneering Wave Generators

Wiegel: As you can see, I'm using words I heard yesterday at the meeting. The most important thing about the facility, I think, is that the first programmable wave generator anywhere in the world, and probably the first wave spectral generator anywhere in the world, was designed and de-bugged for use in that tank between 1959 and 1961. This has been copied by other people, but this was the first one, done here by J.W. Cuthbert, Oswald Sibul, and Bill Webster. Webster is now professor of naval architecture here at Berkeley and currently an associate dean at the college of engineering. The late Oswald Sibul was from Estonia. In fact, he escaped from Estonia when the Russian Empire took over the Baltic countries at the end of World War II, and he spent his professional career here in naval architecture at the university.

Also, the pre-feasibility studies for the Polaris missile were done in this tank. This work was classified and was never written up, and I think it's well to get in the record that that's where it was done.

Pre-Feasibility Studies for the Polaris Missile

Swent: Did you work on that?

Wiegel: Yes. We had several of our graduate students work on it, most of whom were hired away from us by Lockheed Aircraft Corporation that then continued to do the real development work. So we did the first work--obviously it was good because they hired all of our students from us.

Swent: How does it work to have graduate students work in a project like that that's so highly sensitive?

Wiegel: One way you can do classified work is to break it up into bits and pieces. These students had clearance--

Swent: Of course.

Wiegel: --because, remember, some of our students were--I don't think I mentioned, but the Army Corps of Engineers, for years sent, or permitted young officers to apply, because they had to be accepted as graduate students like any other graduate students. There was an unwritten law in the Corps of Engineers that to be promoted to field grade you should earn a Master's degree in engineering. So we always had several students who were in the military, but also, we had others that did have clearance in those days. People now can't conceive of the fact that we did these things, but we did. You could keep the security level low by breaking up the project into little bits and pieces.

Swent: But the person at the top had to be aware of the whole project?

Wiegel: Oh, yes. The person at the top who was undoubtedly back in the Pentagon knew many things. I knew a certain amount because I had high clearance. But the students wouldn't have known anything at all about any of the details. Even though they had low clearance, they just were working on it. They did not analyze the data. To analyze, we used navy photographers to take the data on--you would call them motion pictures, but they were really photographic transits. They had been very carefully calibrated at the navy gun factory, and they used photogrammetry to get motions and things like that. But that was all done on a navy facility, so navy personnel took the photographs and they analyzed them on a navy facility. So that's how you handle things when you're under extreme pressure to get something done rapidly.

Swent: So what were you doing here then?

Wiegel: Setting up the procedures and then thinking about the value of the work. We then realized that the next step had to be done in a facility which would have been quite different from what we had, because you would have to have a towing tank in a building in which you could adjust the atmospheric pressure because you had to--as a part of the modeling--worry about the ratio of absolute pressures, and we were working in atmospheric pressure because the tank was in the atmosphere. So the next test--and this is what Lockheed did--was to build a facility to do this--in Sunnyvale. So I made that recommendation.

Swent: Polaris was launched from underwater?

Wiegel: From a submarine underwater. Correct.

Swent: So it had to cope with water pressure, and atmospheric pressure and--

Wiegel: --and very complicated things at the free surface. We did learn some interesting--

Swent: Complicated things at what?

Wiegel: At what they call the free surface, when the missile came up and was about to go from water to air. Interesting things did occur: it could come right back down and hit the moving submarine if it wasn't designed properly. But that sort of thing was thought about in advance; it wasn't a surprise--that was one of the reasons for the experiments.

Swent: Were there ever any cases where someone was denied clearance?

Wiegel: I'm not aware of a single case, no. Incidentally, you had asked at one time about the loyalty oath and so forth; well, I wasn't on the faculty at that time, I was full-time research staff, and of course, people like myself really didn't quite understand what was bothering them because we were all cleared. We all just automatically had done all of these things. We just didn't understand it.

Graduate Students and Surfing

Wiegel: I think I mentioned that I supervised twenty-six Ph.D. theses, and many master of engineering mini-theses, and much of the work on these theses was done using the facilities at the Richmond Field Station. I think I just mentioned to you that a number of students were young junior officers. If you're a junior officer, you're young, because it's up or out in the military.

Swent: Are there any in particular that you remember?

Wiegel: I remember one of them: Homer Johnston. Homer became a brigadier general--several of them became general officers--but Homer wrote an article--he was from southern California, and he loved to surf, and he wrote a serious article on surfing: what makes good surf sites? It was the first paper that I am aware of, in which anybody has ever tried to figure out why a place was good for surfing; the combination of waves and the bathymetry and so forth.

Swent: What? Bathymetry?

Wiegel: Yes, bathymetry: underwater depths and configurations. What makes a good surfing site? Homer Johnston looked into this, and came up with the first recommendation as to what he thought might be a good combination. A few years later, a person going to the University of Hawaii--Kimo Walker--did a Ph.D. thesis on this. This is very serious because surfing is an important subject--because so many people enjoy it.

Teaching Students to Write and Speak Persuasively

Wiegel: I mentioned the master of engineering degree as a two-year professional degree which I believe to be the most cost-effective degree that we give in engineering from the standpoint of people becoming practicing engineers, rather than research and teaching.

I think that somebody should do a history of that degree: how it came about, why it's important, and how valuable it has been. I think--at sometime--somebody should do that. Not me, but somebody should.

Swent: Had it already been given?

Wiegel: No, it was developed in the 1960s.

Swent: What was the impetus for it?

Wiegel: It was probably a few people like Harmer Davis that recognized--the reason I say Harmer Davis is that Harmer Davis was the director of the Institute of Traffic and Transportation, and he worked closely with people in regional planning, city planning, and things of this sort. You just don't design a highway, you don't just go take soil samples and decide what sort of foundation you need, and how wide your cars are, so how wide your lanes have to be--but you have to work and see how this will fit in with the entire concept of how you move people and things from place to place.

So Harmer saw the need--and others too, but I remember Harmer--of an engineer who would take a minor, similar to a minor for a Ph.D. that they would take in mathematics--take a minor in something that's completely out of engineering to get a viewpoint of some other discipline. In regional planning, in history, in public policy, in portions of business administration--those portions that deal with people. At least in civil engineering, this has been very successful. I think that somebody should document that at some time.

Swent: Has it been a popular degree?

Wiegel: It's been a very popular degree.

Swent: It really met a need, then.

Wiegel: It's two years, and the number of units of courses that have to be taken are about the same--certainly no less--than for a Doctor's degree. There are two minors, but one has to be out of engineering or physics or mathematics; it can't be in any of those, it has to be for engineers to learn something about the way other people think about problems. This is valuable. Practicing engineers, and of course all others--this is connected with this--have to sell their ideas. It's no good just having concepts, design procedures--you have got to sell them.

Look what's going on right now with the proposal to replace the east side of the Oakland-San Francisco Bay Bridge. You have to come up with designs or concepts--but you have got to sell these to somebody. First your professor, or your boss, or the public. You always have to sell these things. This is done in writing, or more and more with visual aids and things like that--orally. Time after time, you would hear senior engineers tell the students when they came and met with them in student chapters of the societies--one of the big problems with engineers is they can't express themselves well--or at least many cannot express themselves well either in writing or orally.

I took this very seriously, and in one of my graduate classes, I had every student prepare a term report in lieu of a final examination. The final report, on a subject directly related to the course, had to be written up--it may include a design--but this was a written package of the type you would prepare for your boss or somebody. Then, in another graduate course, I had each student give a twenty-five minute oral presentation. You see, this is what you call "learn by doing." In addition to learning by doing--that is, you learn how to make a presentation--but there's also no better way of learning a subject than preparing and giving a lecture on a subject. So this was always useful.

In the written paper and the oral presentation, I made them use the library, because I wanted them to get used to digging things out for themselves, and learning how to use the library to find material. I think that this has worked very well, because I still see former students coming back and using our archives to get material from it.

Teaching the Use of a Specialized Library

Swent: These were graduate students who still needed to learn to use the library?

Wiegel: These were graduate students. They still needed to learn how to use the library as a real resource. Remember this is not a standard library. This is finding not just published papers and things like that, but old consulting reports and data reports and that sort Of information. That's all part of teaching; that's all part of learning these things. What's new about the present so-called "information age"? Probably more rapid access and better ways to manipulate data.

Inventing the Term Oceanographical Engineering and Authoring the Book

Wiegel: As a part of teaching the graduate courses in the first few years, I did revise and add--

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Wiegel: --to my class notes--with them gradually approaching the format of a textbook.

Swent: You developed this out of your courses?

Wiegel: Out of my lecture notes that I had been giving. I would say about three years of lecture notes. I was asked if I would be willing to do it. I didn't realize how much work was involved.

Swent: Who asked you?

Wiegel: Prentice Hall, the publishers.

Swent: They came to you?

Wiegel: Yes. They did a very nice job, because I at least had enough sense to say I wanted it in a large size format with many, many figures and photographs, and they agreed--but that's an expensive thing to do.

Swent: How had they heard about you?

Wiegel: I don't know. After I did that--after it was published, it was used worldwide. One of the reasons for this I think is: we had a number of young visiting scholars in the 1950s from many countries, and because of that, they were familiar with my lectures and class notes, and later, with the book. The book was used worldwide as a textbook.

Swent: They took it back home with them?

Wiegel: Yes, and it was used by those who came later for several decades before it became out of print. It's still in use by consultants as a reference book. Engineers still tell me how useful it is to them. Let me back up now. I gradually changed a course that I inherited when I first started to teach, on the design of hydraulic structures such as spillways, irrigation, canals and so forth, into more and more of the water quality and mixing. I developed really a completely new graduate course in that field, and for a decade, that was taught only in three universities: here, at Caltech--by Norm Brooks; and at MIT by Don Harleman. Now, of course, it's taught at many places.

Swent: This term: oceanographical engineering. Was that one that you invented, or had it been used before?

Wiegel: I invented the term--

Swent: That's what I thought.

Wiegel: --and people ask my why. I said, "I was thinking of mechanical and electrical, so there's oceanography, and so I coined the term 'oceanographical' to be the equivalent of mechanical and electrical." I would have called it coastal and offshore engineering if I had to do it over.

Swent: Coastal seems to be the term that has--

Wiegel: Yes, but it's broader than coastal. I did much in what is called offshore engineering. That is some of the things needed for the design of offshore oil platforms. So it was broader, but I coined it for that reason. I think I did the right thing, but I would have done it differently.

Swent: This book is referred to as the book that really established this branch of study.

Wiegel: Yes, that's right. That was worth it. I'm very pleased that it was done, and of course I would modify many things in it now, but as far as I know, there's nothing in it that is wrong. I don't mean there are not mistakes; I corrected a number of mistakes. When you have a book with hundreds of equations, you're going to have "y's" without the square in them or something. Those are just plain mistakes--typos and stuff. But as far as the fundamentals are concerned, it's still correct, I think. Although I would update it in many ways, life became so busy--I never got a chance to try to put a later edition, although Prentice-Hall kept asking me to do so. It was reprinted a number of times with corrections of the mathematics and so forth. Now I finally got to today's subject. [laughter]

Swent: All right.

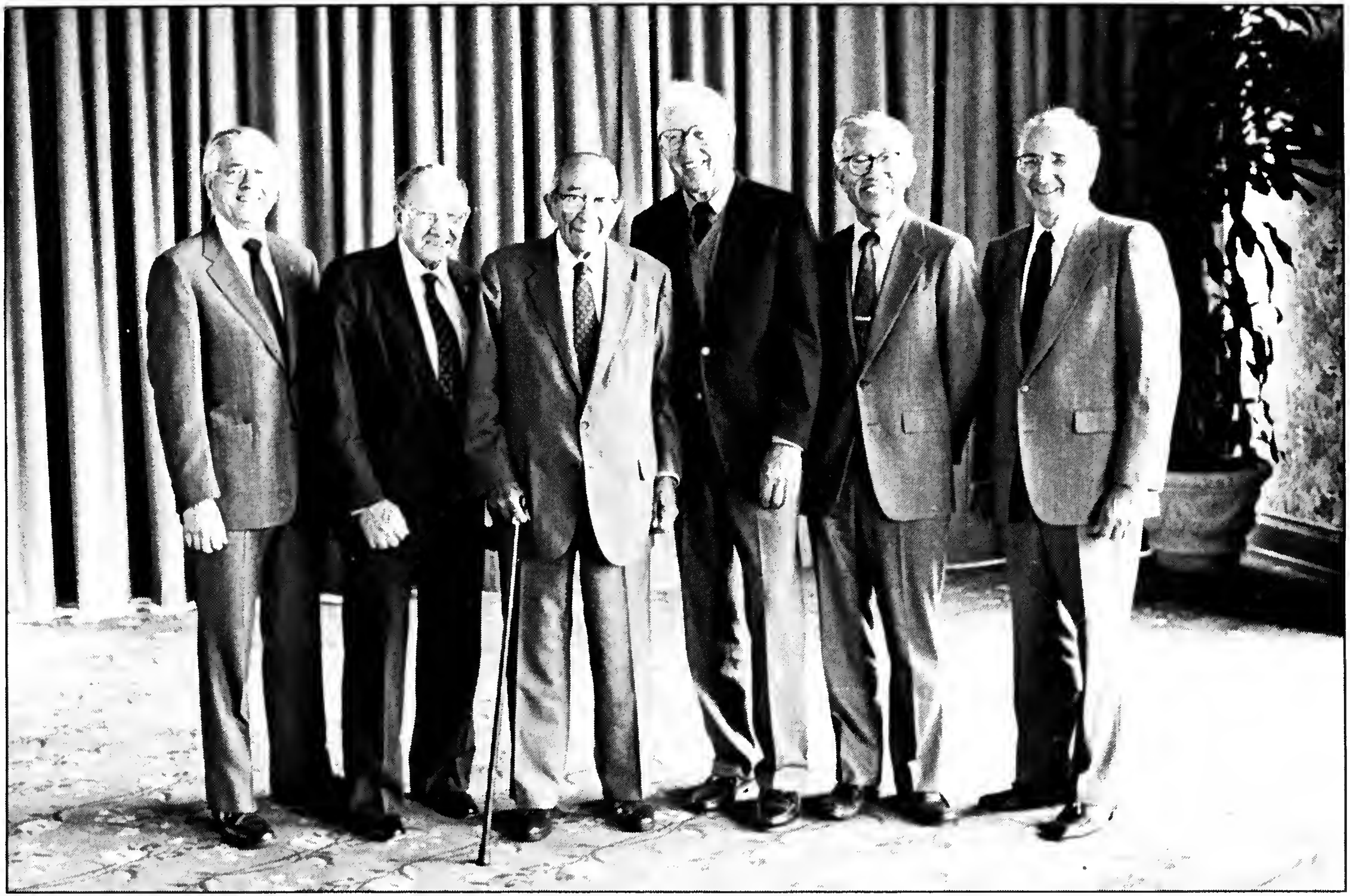
Wiegel: When I got thinking about some of these things--it was a week ago we had the interview, and my mind starts turning over--

Robert L. Wiegel with Mark V Wave Gage, 1949.

Tests of amphibious vehicles, Fort Ord, California, February 1950. Left to right: Robert L. Wiegel (kneeling), unidentified, Captain Lund, Rear Admiral Doyle, Morrough P. O'Brien.

Lunch at Spenger's Fish Grotto, Berkeley, California, October 1957. Left to right: Robert L. Wiegel, Hans Albert Einstein, Joe W. Johnson.

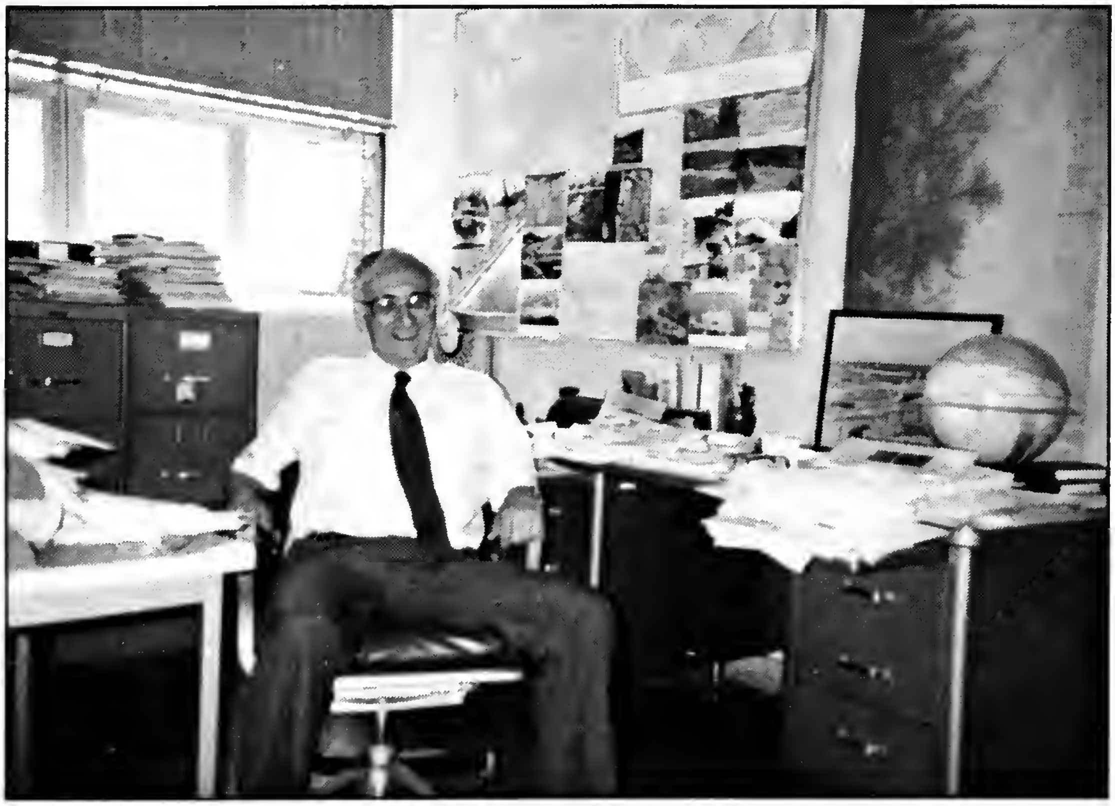
May 3, 1984. Deans and associate deans, College of Engineering. Left to right: Robert L. Wiegel, Karl S. Pister, John R. Whinnery, Everett D. Howe, Clyne F. Garland, Donald McLaughlin, Arthur M. Hopkin, George Maslach, Ernest S. Kuh; and budget officer Frances Woertendyke Eberhart.



Left to right: Karl S. Pister, Everett D. Howe, Morrough P. O'Brien, George Maslach, Ernest S. Kuh, Robert L. Wiegel. 1987.

Dean Emeritus Morrough O'Brien and President Emeritus Clark Kerr at
1987 Symposium in O'Brien's honor at the University of California.
Mrs. Mary O'Brien and Professor Robert L. Wiegel look on.

Photo courtesy College of Engineering Public Affairs Office



Top: ASCE Coastal Engineering Research Council meeting, New York, 1989. Left to right: Eco Bijker, Ron Noble, Bob Dean, Orville Magoon, Thorn Saulle, J. A. Battjes, Robert L. Wiegel.
Bottom: Robert L. Wiegel in his UC office, May 1987.

Opening Ceremony, 22nd International Conference on Coastal Engineering, Delft, The Netherlands, July 2, 1990. Left to right: Egbert Prins, unidentified, Prince Klaus, Robert L. Wiegel.

VI UNIVERSITY ACTIVITIES IN ADDITION TO TEACHING

Director of the State Technical Services Program, 1965-1968

Swent: That's very good, I'm glad you did that. We had planned to talk today about your activities in university governance.

Wiegel: I'd like to modify the governance a little bit, and expand, just in terms, some of other activities that the university does for the benefit of the state and various segments of government and the general public. The university does many things that are useful.

One current example is technology transfer. There are articles, almost on a weekly basis in the popular press--a number of articles in the technical press--on this subject. Technology transfer is to take new ideas coming out of research and turn them into products or systems to make life presumably better, or to go back and rectify mistakes that we made in the past. This is done on an accelerating basis. We're running now, when we used to walk on these things.

It's been done ever since research has been done at universities and has been used under different names. The reason I chose this, is that more than thirty years ago, the Congress passed a law on what they call technical services. They call it the State Technical Services Act. This was technology transfer by a different word.

Swent: And this is the national Congress.

Wiegel: That's the thing that was passed by Congress. Lyndon Johnson was president, and when he signed the law, he used something like the expression maybe this is the "sleeper" of the XXX Congress--I forget which Congress it was. They all have a consecutive number. I did the legwork between the university and the governor's office in Sacramento. Governor Pat Brown designated the University of California as the agency to run it for the state of California. Clark Kerr was president, and Harry Wellman was the university vice president. In other words, when Clark Kerr was on a trip, Harry would be the acting president and so forth. I was asked to be the director, after being recommended by the dean of Engineering, George Maslach. I was in their office complex and I had an office down the hall from them in University Hall. For three years--this was 1965 to 1968--I was the director.

Swent: What was the exact title?

Wiegel: It was called the State Technical Services Program, and the idea was to develop better means than those that were in existence to transfer all of this research that was being done at universities --remember, at the University of California, this also included the Lawrence Livermore Lab, and many things that were developed for security projects, but what you would call spin-off to private uses or public uses. So there were tremendous amounts of this information; but how to get people to make use of it in better products or better systems?

I had an advisory council that was set up by President Kerr. These were very distinguished engineers and scientists mostly, although there were a couple of other types to add breadth in public policy and things of this sort. One of the members was David Packard, the co-founder of Hewlett-Packard. Another was Bill Gould, who at that time was vice president of Southern California Edison, and later became the chairman of the board of Southern California Edison. Another person was Abe Zarem. I remember him, because he was the founder and inventor of electro-optical systems. You have heard of Xerox?

Swent: Yes.

Wiegel: There are two parts of Xerox: there's the actual way that the image is made, but this can't be done without the electro-optical system to sense everything and tell it what to do. Abe Zarem was the one who had invented that, and his company was merged with Xerox, and he ended up owning huge numbers of shares in Xerox. I think at the time of this, he might have been worth--they used to laugh, "How many millions did you make or lose today while you were attending this meeting, Abe?" But he wasn't interested in money except for its use in improving his research facility that he used to develop new concepts. That's what he was interested in.

Bob Johnson was another member. At that time, Bob headed up the aerospace portion of Douglas. Later, Douglas aircraft was bought out by McDonnell, to become McDonnell Douglas Corporation--and Bob Johnson ended up corporate vice president for engineering, and then eventually, the head of all McDonnell-Douglas West Coast operations, which is their space program, Douglas Aircraft Corporation. So these were people that had done this sort of stuff.

We were interested in inventors. What was it that made an inventor? There aren't many people who are real inventors. Occasionally you'll read that it's just by chance that people invent this or that, but it's not that simple. People who are inventors--there's something different in the way their mind works. We looked into that.

Swent: What exactly were you doing?

Wiegel: I was the director. One thing I did was to serve as the liaison between the university and the governor's office, via Hal Walt who was Deputy Secretary of Finance. Mostly, I would go out; I would meet people; I would give talks. I should say that I think one of the reasons why I had been appointed to this was: there was a San Francisco Bay Area relation with industry committee, and I was the university representative on it. So I knew many of the people in the Bay Area of this sort. I think that was one of reasons: I knew a lot of people. I had a lot of energy, and I've always been a great reader. I would just read books, journals, magazines, newspapers, on all kinds of subjects. I got very interested in entrepreneurmanship and management and financing of businesses and things of that sort.

At the faculty club at lunch, I would always be talking about these kind of things, so I think--you never know for sure why anything really happens, but I think those had something to do with it.

Swent: During those years were you continuing to teach as well?

Wiegel: Oh, yes.

Swent: But you had an office?

Wiegel: I had an office on the seventh floor of University Hall. That's the building down just to the west of campus at the corner of University Avenue and Oxford Street.

Swent: Precisely what were you trying to set up? Meetings or courses or fund-raising?

Wiegel: Not fund-raising, no. It was funded, but matching funds were required by any group who received a contract.

Swent: What were you trying to do?

Wiegel: One of the people was with the Lawrence Livermore Lab. He was a physicist or electrical engineer, Hy Olken, but he got very interested in how to take new ideas and turn them into products, and had been doing a lot of thinking about the subject. We funded him with a university contract to write a book on this subject: how he found you could do this--making use of ideas coming out of the Lawrence Livermore Laboratory. We contacted--I think it was a contact with Bill Gould, or Bob Johnson in southern California--a person down there that made a series of television programs for public television on people who had done this, to give examples, to try to encourage other people to do this sort of thing.

But there was just too much direction from Washington. I think in engineering, it's something like where the client over-specifies a project, and because of over-specification, it really denies the engineer designing something to use much judgment about what to do. This was what they did, in my opinion--after three years, I recommended that the university no longer continue to run this in the state, and they accepted my recommendation and dropped out. A year or two later, the whole thing fell. But a good deal of it was because of too much interference from Washington, over specification.

I would like to say I've been on other things since then in an advisory capacity to committees in Washington. I find that this is a tendency of people in Washington: that they know what's best for you, and they want to tell you what to do. This is especially true in standards and things like that which I've had some experience in.

Swent: I just can't figure out exactly what it was all about. It sounds terribly vague to me.

Wiegel: We knew what we wanted to do.

Swent: Which was?

Wiegel: When I say "we," that's editorial "we." We didn't know how to go about it. We knew bits and pieces.

Swent: You were trying to speed up this transfer?

Wiegel: Trying to speed it up. Of course, it probably had nothing to do with what we did, but in the Silicon Valley today, this sort of stuff is done routinely with a speed that's almost mind-boggling, as I think you know. So there are people that know how to do it. But there are the inventors, and the entrepreneurs. The entrepreneur--and you will be talking to an entrepreneur in a few weeks--that's Orville Magoon--they don't give up; they try this, they try that. If something fails--or it may not fail; it just isn't successful--they don't quit. They go on. They work on that again, they may work on a modification. So you have got the concept, the idea, but you have to match with it these other people that can and do push.

Then the hardest thing is trying to judge what people might want, or they may not even know they want it. Now, you market it, and get them to see why they might like to have it. So what we wanted to do was fairly clear-cut; how to do it was what we were all struggling with. We made some headway, but I think you'll still find out today that people still don't know too much about why this one person seems to be an inventor, and the next thirty people are not inventors.

Swent: I'm still unclear. This commission--was it a commission or council that were you chairman of?

Wiegel: I was the director for the state of California, and I was to go out and search for ideas, for people, not to transfer the product, but <u>how</u> this is done; how it can be done, not to do it. The wish for more/better technology transfer keeps coming back in. I forget what they call it in Washington, D.C., now. President Clinton and Vice President Gore were pushing it--to put some money into developing things that might turn into new products and this and that. This is the top-down thing, and it doesn't seem to work very well. You need this one person here: somebody that gets an idea, and then somebody that will work with them and push, push, push.

So the concept--the idea--what we were trying to do was a good one, but it went up and then came down. Now it's back in fashion again. As I said, the state's got an advisory council in technology transfer. Good luck. [laughter]

Swent: So that was something you did for three years?

Wiegel: Yes, on a part-time basis.

Attending Meetings of the California Coordinating Council for Higher Education

Swent: That's quite a while.

Wiegel: But I also taught my courses at the same time; I had my graduate students. I had my research contracts. I've always worked long hours. As part of that, I had to attend a few meetings of the California Coordinating Council for Higher Education, and on a couple of occasions, make reports to them. Remember this was a program for the state of California, and the Coordinating Council consisted of the president of the University of California, the president of Stanford, the president of USC, the chancellor of the California state college systems, and the head of the community college systems. I forget the others, but it was the overall body that coordinated higher education in the state of California.

Swent: Very important.

Wiegel: I found, being able to attend several of those sessions and to report to them a couple of times, that this was very interesting. I learned a lot about how the state's higher education worked. In governance, a lot of it is because of people talking to each other. In fact, I've given quite a bit of thought to how universities govern. It's very different than the military, which I know quite well. It's very different from most corporations, and so many old, large corporations were set up under the model of the military, as you well know. Very different from government agencies, where you have got the chief executive, you have got the commander, you have got the director, and they tell people what to do. At universities, you don't. You may try, but it doesn't get you anywhere.

Swent: You can try.

Wiegel: So governance is really through committees, and meetings, and selling. Ideas are going up, ideas are coming down, ideas are going sideways. In the higher education system in California, the fact that you had the people like the president of the university of California, the president of Stanford, the chancellor of the state colleges--they all talked and worked with each other. You try to get rid of friction. This state, I think, is wonderful. The idea of community colleges where people can live at home and go to school at the advanced level for the first two years--because as you know, when you go to college, if you have to live away from home, this costs a lot of money, and if you can have in most communities a school where you can take your first two years and not have this expense of boarding out--this worked. I assume it still is a very good community college system.

Then you had your state colleges, which were four years; the derivative of the old state normal school that were at the turn of the century for training teachers, but then became much more like universities in that you take many different subjects, so you can go to a state college. Practically every big city had a state college. San Francisco State, Fresno, Los Angeles, Long Beach, San Diego. We have many; and these are fine institutions. Then you had the University of California which had the undergraduate programs, but also had the graduate programs and the medical schools and the law schools and things of this sort.

Swent: When were you on this council?

Wiegel: No. I attended some--

Swent: You attended--

Wiegel: Yes, President Kerr was on the council.

Swent: When were you attending, working with them--

Wiegel: That was in 1965 to 1968, when I was the director of the state technical services.

Swent: And the state master plan for education was--

Wiegel: It was in existence.

Swent: But it wasn't very old. It had been--

Wiegel: No, they had just started it.

Swent: That was--I have the date here somewhere.

Wiegel: It was just before that, because Clark Kerr was one of the architects for it. I forget--was Wallace Sterling the president of Stanford?

Swent: I think so.

Wiegel: I think it was Sterling.

Swent: So they were still kind of working through that?

Wiegel: That's right.

Swent: An interesting time to be there.

Wiegel: For me it was very interesting. As I said, I became absolutely sold on the way the state of California was doing this.

Swent: You were taking reports from your task force?

Wiegel: I had to report to the council on a couple of occasions because it was a project for the state of California. The University of California had been delegated the responsibility.

I've always been interested in what makes entrepreneurs and things of this sort, and how you get new products. People say, "Gosh, all these committee meetings and all of these things are a terrible waste of time and a terrible waste of money." Well, it certainly takes time, and it certainly takes money, and there's undoubtedly time and money wasted, but I don't think there's an alternative to it. I don't see how you can have a university operate without this.

University Organization More Like a Symphony Than a Business

Wiegel: In fact, I used to semi-joke that when chancellors are looking for faculty, they always want somebody of independent thoughts, and then they go through this big search program, and they choose a faculty member and then another, and these have been chosen for their independence. Then the chancellors want to get something done, and they find out they can't because these people are too darned independent. In most companies, you hire people to work as teams. In a university, you don't. You don't hire a team of professors. You hire this one, this one, and this one.

Somebody once said that the concept in industry and the military where there's a commander, a chief executive, and five people report to this person, and each of these have three or four report to them and so forth--he said, "That may be all right, but then there's other kinds of organizations like a symphony. Can you imagine the conductor conducting, and it's the only the lead violinist, the lead horn player, the lead tymbalist watching him, and then three or four other people watch each of these? You wouldn't have much of a symphony."

Swent: That's true.

Wiegel: Only whoever it was expressed it much better. But in some ways the university is like that. In some ways, the head of the university must be simply like a conductor of a symphony. He's got to see, hear these nuances and modifications and reflections off the walls every single place you give a symphony. In every single building the acoustics are different. It's different with people in it; it's different in the summer than in the winter, because in the winter people are dressed heavier. I'm not joking; this affects the acoustics. Somehow, a university is kind of like that, and it's not easy to get it to run.

Swent: No, it isn't.

Wiegel: But that's part of governance; that's the way it's done.

Swent: It's critical for it, isn't it?

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Wiegel: One example of a mechanism to help the university--there's a university-wide governance, and remember there are the several campuses, which I think of as independent dukedoms. You call the duke a chancellor, but in some ways it's similar to the days of old. But there's university-wide governance, and this is more difficult.

The Engineering Advisory Council Develops a Master Plan, 1965

Swent: This is sort of like your violin sections.

Wiegel: Yes, that's right. One of the ways they do it, is the president of the university has to get inputs and advice. They get it through the university academic senate statewide--I think it's called the Academic Council or something. But there also was the Engineering Advisory Council set up by Robert Gordon Sproul, and continued by Clark Kerr when he became president, and this consisted of a number of distinguished engineers throughout the state from industry, universities, state colleges, representatives of community college.

Perry Yates--and those are the early days--he was the senior vice president of Bechtel Corporation, and he was the number-two person to Mr. Bechtel at that time. Not young Steve, who's now retired also, but old Mr. Bechtel. Perry Yates was really the number-two person and he chaired the Engineering Advisory Council when Kerr was president of the university. Much earlier, George C. Tenney was chairman (1949-); he became president of the book publisher McGraw-Hill. They were good friends of Mike

O'Brien. One of the things that President Kerr asked the Advisory Council to do was to consider what should be done in the university system for engineering, because remember, these new campuses were coming. Until after World War II, for example, to get your engineering degree, you had to come to Berkeley the last two years. You could do your first two years to UCLA, and I believe your first two years for a while at UC Davis after it became broader than an agricultural experiment station. At the end of World War II, of course, UCLA developed into a four-year college and not too many years later, Davis did too. Then other campuses were coming on--UC Irvine, San Diego, Santa Cruz, Riverside.

So they did a lot of thinking, and they came up with a report which was called An Engineering Master Plan Study for the University of California. This was September of 1965. I have a copy here.

Swent: This is a beautiful gold-bound book.

Wiegel: It's a beautiful gold-bound book. It's 177 pages.

Swent: A big report.

Wiegel: They had a lot of input from faculty on the different campuses, and also what became known as the Lawrence National Laboratories. A lot of these recommendations were implemented.

Swent: Were they?

Wiegel: Yes. I went back to look at one chapter last night. The one on continuing education, which is engineering extension. Much of what they recommended has been implemented. Some of the things were not, and reading them, I think should be. I think they gave very good advice. But that's part of governance in this symphonic way of looking at things. Ideas flowing in all directions.

Swent: One of the questions that I asked someone recently was whether this advisory council was still used, and evidently, it isn't used at all now.

Wiegel: It could be.

Swent: It doesn't even meet once a year. I'm not sure if it even still exists. Those are some stellar names on that list.

Wiegel: Well, I think you have a lot of background in mining. One of the members was Phil Bradley, who was "Mr. Mines" in the state of

California for decades. He was a real hard-bitten outdoors mining type. I remember him very well.

Swent: A great lover of Cal.

Wiegel: They all were.

Swent: I think this advisory council doesn't really--

Wiegel: Doesn't each campus have one of their own now?

Swent: I'm not sure about that--

Wiegel: I'm not sure, either.

Swent: --but I did ask recently and was told that they no longer meet.

Wiegel: This study is a good study.

Swent: I think that council also served as liaison between the university and industry.

Wiegel: Oh, yes. Ideas were flowing in all directions: industry and government agencies and other educational institutions too. Another way that it is handled is through internal institutes and things of this sort. I give one example because I mentioned it before just briefly, "California and the Use of the Ocean," and it is an IMR report [The Institute of Marine Resources].

Member of The Institute of Marine Resources Executive Committee, 1966-1972

Swent: What was your role in all of this?

Wiegel: I will mention it just shortly. It was established in the university. It's kind of like the Water Resources Center, established really because the state legislature had become convinced that the state needed a Marine Resources Institute. This is--fisheries, for example; fisheries are extremely important. Not only commercial fisheries, but sport fishing and things of this sort. Then the fact that cities along the oceans would discharge their wastes, agriculture waste goes into rivers, rivers into estuaries. All of these things. So Roger Revelle--people had listened to him and heard him, and people in the state legislature decided that the state of California needed an institute of this sort that was broad-based, and went ahead and established it with a certain amount of funding.

The idea is: you then get research contracts and things like that to do the real in-depth work. There would be enough to fund a director and so forth. President Kerr's office established an executive committee for it.

Swent: Were you involved with this?

Wiegel: Yes. Bill Nierenberg, who was director of Scripps, was a member. I was a member representing Berkeley. The dean of engineering at UCLA was on it. The chair of it was retired navy Rear Admiral Charles Wheelock. Wheelock had been--just before he retired--inspector general for the Bureau of Ships of the navy. A very fine person. Roger Revelle talked him into coming to Scripps after he retired as a rear admiral. Officially, he was acting director of this new institute from when it was founded--I think 1954 to '57--and then director for another five or six years. But unofficially, he was filled in as Roger Revelle's person that really ran Scripps administratively.

Roger was the idea person scientifically, but as Roger said, he was not a good administrator. Admiral Wheelock was a superb administrator. When I was on the executive committee for the institute, Admiral Wheelock was chairman of the executive committee for the institute. I got to know him very well. He worked on other things in addition to this--he worked a lot with Clark Kerr and Harry Wellman in the physical planning for the new San Diego campus. How many acres did you need? Where might be the best place? What portion of it should be dedicated to teaching facilities? What kind of housing for students? These things. Then Charles retired from that area, and moved up to Carmel, and later on to Aptos. He also worked with Kerr and Wellman on the physical planning for the Santa Cruz campus. So he added a lot to it, and he was a very, very fine administrator. I remember one thing about him: he would ask me, "What time is it, Bob?"

I would look and tell him, and finally I said, "Admiral Wheelock, why don't you ever wear a wristwatch?"

He said, "Bob, my whole career in the navy was controlled to the second. When I retired, I took my watch off, and have not worn one since." [laughter]

I don't remember how long I was on that, but I remember the director of the institute at that time was Benny Schaefer, a professor of fisheries. He was one of these very broad-based people too. He knew a lot about biology, but he knew a lot about economics, he knew a lot about commercial fishermen, who were about as hard-hitting and independent group of people as exist in this world. He served for two years as science advisor to Morris Udall when Udall was Secretary of the Interior, so Benny knew his way around.

He served with Udall for two years just prior to becoming director of the Institute of Marine Resources. What we did in the executive committee--we tried to work out what type of work should be done on which campus. To match the capabilities of people on different campuses, and then how to distribute the few funds that were available through the state, but then also to encourage these people to get research contracts. So we did a lot of that. Benny died and we needed a new director, and maybe because I was the person furthest away from Scripps, Vice President Wellman asked me to be chair of the search committee for that.

We made a complete--not just national, but international search for a director. We ended up with a person who was down there: John Isaacs. Isaacs is the only person I have known well that I really consider to be a genius. I know an awful lot of very smart people, but John Isaacs--he died a few years ago--he would just see how things worked and why. Then he'd look at you and wonder why you couldn't see it also. He would see these things, and sometimes they were wrong, but very often they were right and they were new. Then professors or graduate students would work on these things for one, two, three years, and finally be able to find, formally, why what he saw was a truism--why it was. He was really quite a remarkable person. He was always very interesting.

Swent: I see that was in 1970 and '71 that you were doing that.

Wiegel: Schaefer died in July, 1970. That's right. We spent probably six, seven months searching for a director. John Isaacs would have come aboard probably in 1971 or earlier. The institute is still in existence, and still university-wide, but it's governed by this way of talking to each other regularly. I was going back through some of my records, and it's kind of interesting, but the last job that I did outside of my teaching and so forth--just before I retired--I was asked to be the chair of the five-year review of the Institute of Marine Resources. It was kind of interesting. Years after I had served on the executive committee to go down and have to interview everybody.

By then there was an assistant director at Davis, one here at Berkeley, one at Santa Cruz, one at Riverside, one at UCLA, one at Santa Barbara. I found that really interesting. I jumped back in, and learned all over again what they were doing. They were doing lots of good work. It's a mechanism that had worked.

Swent: Just to get the dates right: you were on that executive committee from 1966 to '72. That was six years.

Wiegel: That's probably about right.

Swent: And the search committee was 1970 to '72, and then the review committee was in 1987. Almost twenty years later.

Wiegel: Almost twenty years later.

Swent: From 1966 to 1987. In that time, it had grown from one location to many.

Wiegel: It was every place.

Swent: Every place.

Wiegel: I said, Riverside. Why Riverside? They have a lot of pretty good agricultural people and things like that out there, so my guess is that may have been the reason. Davis was straightforward because that was food--fish--how you process food. You notice how fast the oils in fish change to where you don't--I remember asking a famous fisheries person at Scripps--because I did a lot of overseas traveling, and I would be in the Philippines, or New Guinea, or in the airport--about eating things. He said, "Not shellfish, but you can eat fish, because if fish has gone bad, you can smell it. You won't want to eat it." You don't eat the guts; I always remember that. So when I go to these type of places, it would be fish and yogurt--those are the two things--

Swent: Always safe.

Wiegel: --that seem to be safe.

Swent: That must have been very gratifying.

Wiegel: It was. Remember, I've always had a lot to do with the oceans--we'll find out more about this a little later when we go into professional activities and so forth--but this is why I was asked to serve on it. Not just coastal, but I was very interested in shipping and such off-shore things as waste disposal, water quality. All these things tied in, you see, with fisheries.

Service on Coordinating Boards and Academic Senate Committees

Wiegel: Another one I was on was the coordinating board for the Water Resources Center. That was from 1982 to 1987, when I retired. In my last year I served as chair of the search committee for the new chair of the Water Resources Center, who reported to the university vice president for agriculture. I was also on the Sea Grant Coordinating Council. Sea Grant was established by act of Congress. They got a lot of research money, but it's not only research; they want people--in a manner somewhat similar to the agricultural extension service--there was a fisheries extension service to get out and work with commercial fisherman and people like that. This is state by state. Texas has a big program going on; Oregon has a fairly good-sized program; Washington on the fisheries; California on the fisheries. Fisheries is important.

Swent: What was your role in that?

Wiegel: I was on the university's Coordinating Council from 1970-1974; this was appointed by Vice President Harry Wellman, and reported to him. It was to set up how the university would handle the various proposals that were made for funding from Washington under the Sea Grant Act, because that was funded by Congress, and then it was handled under NOAA--National Oceanic and Atmospheric Administration. Bob White was the administrator of NOAA. That's within the Department of Commerce. Part of it was because Bob White had been the head of the U.S. Weather Bureau, and the weather came under NOAA. The United States Coast and Geodetic Survey became a part of it. That's now called--I don't know what name they have it now--they did all the hydrographic surveying. Bathymetric surveys for navigation and things of that sort.

There was a rear admiral who was director of the survey--Admiral Nygren. Much of the fisheries were--and incidentally, at one stage, Bob White did ask me a couple of time to see if I would become the assistant director of NOAA for the marine laboratories, but I talked it over with the family, and we didn't want to move to Washington, D.C. It would have been a very interesting job, but I didn't really want to move to Washington, D.C., either. It's hard to move from Berkeley. I like to go places. I go all the time, but not to move. So there were all these things, and they all kind of inter-tied together.

There was the way the research would be done, where teaching would be done, and I was kind of a liaison amongst these things because for one reason or another, I happened to have been appointed to a number of them at that time, which tied into other committees I was on in Washington. I think that was part of the reason.

I think I mentioned another governance activity--during the late 1960s and early 1970s--when all the unrest that we had--both student and a number of faculty--and some of the faculty were trying to get the R.O.T.C. off campus. Incidentally, it is off campus at Stanford and Princeton and other places. But the regents said, "This is a regential matter, not a faculty matter. You will have it." So we have it to this day. It was when Al Bowker became chancellor--he established an advisory committee on military education--I think I've mentioned this before.

Swent: Just briefly, but we didn't go into detail.

Wiegel: So a good deal of it is done through faculty, because all members of that advisory are members of the faculty. Originally, we reported directly to the chancellor, but because the chancellor can really have only so many things reporting directly to the chancellor, currently, we report to Carol Christ, who's the vice chancellor, but also provost of Letters and Science. I think we report to her, not just because she's the vice chancellor, but because she's the provost, and then at the same time, report to her as the vice chancellor. But I did go into ROTC before, and why I think it's important and so forth.

I didn't mention that as a part of this, a couple of us are also members of this advisory group on the Fleet Admiral Nimitz Memorial Lectures in National Security Affairs, and I'm still active on that. That was funded by donations. The group raised almost a half a million dollars, I think, in a few months. The money is in escrow, or whatever the university agency is that manages accounts. It keeps building up and up. The first lecture was given by Samuel Huntington of Harvard University. I mention that because Huntington just had a new book published recently that I read. I bought it and read it when I was out it Hawaii. It's called The Clash of Civilization and the Remaking of World Order. This has been highly recommended by the New York Times, The Wall Street Journal, Forbes magazine. A variety of people have said it's good, and it is. It's very good. He's certainly pessimistic, though--more so than me.

Swent: He was the first?

Wiegel: Yes. After that, we had a former secretary of the navy, John Lehman.

Swent: When was the first lecture? Do you know?

Wiegel: 1986. It's been a decade. Incidentally, the one coming in April is four-star Admiral "Snuffy" Smith, who has just retired as the head of naval operations in the Mediterranean; which includes the NATO operations in Yugoslavia. We're looking forward to his lectures in April. He said he will address the Yugoslavian situation.

Swent: So are these speakers always--

Wiegel: And another was secretary of the air force, Hans Mark--he had been professor of nuclear engineering at Berkeley back in the 1950s. Then Admiral Bobby Inman, the famous Inman of the national security affairs. Another was General John Galvin, who had just retied as Supreme Allied Commander, Europe (NATO).

Swent: So these are always military people?

Wiegel: No. We had a U.S. congressperson. Huntington is a professor. We've had two professors of history, Huntington being one. So it's a mix. These are public lectures. Open--everybody invited. But in addition, they always give lectures to the ROTC students--the navy, the air force, and the army--to try to get across to them ideas and give these people the opportunity in asking questions of ones who have really done it. A four-star, or a secretary of one of the services, or a congressperson who had been the head of a related committee. It's a very good opportunity, and of course it's good for the community because people from all over can come and listen to these lectures.

Swent: This is every year?

Wiegel: Every year. So you see, this is another way of governance. It's, again, ideas up and down and back and forth all the time. Deans O'Brien and Whinnery and Maslach, all encouraged faculty to participate in the academic senate; especially on the committee work. I did this throughout my career.

Swent: You did a lot of it.

Wiegel: I did a lot of it. As you know, a year after I retired, I was asked to be secretary of the Academic Senate, which I accepted. But I resigned the position--

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Wiegel: I had a major operation; I didn't get my energy back, and I became completely deaf in one ear, and lost the balance control from one inner ear, and also I found the job wasn't all that interesting.

I had got much more interested in the professional fields again: engineering.

Swent: I was going to ask: is this unusual for an officer of the senate to be a retired professor?

Wiegel: I was chairman of the committee of rules and jurisdictions at one time, and thirty years ago I could have answered you about that. I knew the by-laws of the regents, I knew the directions that the regents had given, I knew the by-laws of the Academic Senate, and the answer is: it's someplace in there, yes, you can be. But I forget exactly why.

Swent: I would guess this might be quite an honor to be asked?

Wiegel: Oh, yes, it was. I was pleased to do it, but as I said, I unfortunately had this serious medical problem. It took quite a while to recuperate. I think I mentioned earlier two of the committees on continuing education--the one on extension of the Berkeley senate, and another one for the university-wide extension, which was--I chaired the Berkeley campus committee, and then for a few years I chaired the university-wide committee. This was the working relationship between the full-time academic people on campuses--the Berkeley campus committee was just Berkeley, but the university-wide one was for all campuses including the medical schools and so forth--and how you got information flowing and back and forth between people who are regular faculty, but were willing and able to give these courses in extension, as well as others.

Sometimes extension courses are given by regular faculty members; other times they're not. For example, the ones so successful in San Francisco and the Peninsula in computer science--very often these are given by the people down in the Silicon Valley that are doing all these things right now. Yet, they're willing to take the time to give these courses at night and on weekends for other people, to upgrade or update them. It's a wonderful activity. That's part of the governance: through these academic senate committees.

Swent: That would fit in with your technology transfer.

Wiegel: That would fit in with technology transfer, because it is idea transfer. Isn't that what education is about? Ideas; not only transfer, but questioning ideas.

Swent: That's right.

Wiegel: I remember seeing a bumper sticker: "Who are you to tell me to question authority?" [laughter]

Swent: That's a good one.

Wiegel: That's a Berkeley-type of sticker.

Swent: You were chairman of the university extension...

Wiegel: It's the Academic Senate Committee.

Swent: Starting in 1965 and then from 1966 to '72, you were chairman of the university-wide committee.

Wiegel: Yes. That's the one that was very broad, because that's where we also worked with the people at UC San Francisco, UCLA, and so forth. The medical schools have done a great job, as you know. Thank goodness doctors keep up to date. Most people seem to realize that this is necessary for doctors, but of course it's just as necessary for engineers, and for people in any other activity--business--any other professions.

Swent: Teachers certainly.

Wiegel: You have got to keep up to date with all this new information. This is what is done through these extension activities. The university has done a very good job. But in addition, they have these wonderful broadening things in the humanities, and the arts, and things of this sort. It's all self-supporting. I don't know about the medical school; I have to be careful, because I don't know. But all the other programs have to be self-supporting. Not each individual one, but in the aggregate. You may make a profit in this one, and you may lose in this one, but in the aggregate they have to be self-supporting. I'd just like to thank all these people who are willing to teach these courses because they don't make much money. It's almost a public service on the part of the people who give them.

Swent: What a wonderful one.

Wiegel: A wonderful one.

Swent: So you clearly enjoyed that very much.

Wiegel: I thoroughly enjoyed that.

Swent: You were also on the committee for rules and jurisdiction, and that, again, went to the university-wide one.

Wiegel: Yes. That was a surprise. I was asked to chair the Berkeley committee, and I wondered, "An engineer?" I thought it would be a professor of law or something. Incidentally, the dean of the law school was on the committee. Anyhow, I was asked to do it, and I also was parliamentarian of the senate for a while.

Swent: Yes.

Wiegel: It wasn't until many years later that I found out the famous book used by so many parliamentarians, Robert's Rules of Order, was written by an Army Corps of Engineers officer 130 or 140 years ago, by the name of Henry Robert. He eventually became chief of engineers, retired about 1900, and shortly after he retired, he chaired the consulting board that came up with the plans to rehabilitate Galveston, Texas. In September of 1900, there had been a terrible hurricane that killed about six thousand people and destroyed somewhere around thirty-six hundred buildings. They always talk about the Galveston seawall; well, it was more than that--it was the Galveston grade-raising, seawall, and revetment. They recommended that part of Galveston Island, where the buildings were, be raised by ten feet or something like that, using sand dredged from Galveston Bay. People always talk about the seawall; they never seem to know that the seawall was largely to protect this raised material that had been placed. He was quite perceptive. I'm sorry that General Robert was not asked to make plans at a number of other places at the same time, and use the same concept.

So I guess the engineers can work well in rules, jurisdiction and parliamentary procedures. One thing is: the committee on rules and jurisdiction did not have--that is, the Berkeley campus, did not have much power. The statewide one did.

Swent: You went on to be on that for a long time.

Wiegel: Yes. But the Berkeley campus committee, by design, did not. This was not oversight, but design. But we were asked questions by many people. We did our homework well; we did a lot of work.

Swent: You were on that statewide committee for fifteen years.

Wiegel: Was it that long?

Swent: From 1976 to '77, and '78 to '81.

Wiegel: That's five or six years. I went on sabbatical, I think or something, during fall '77-'78. I was chair of the Berkeley Committee from 1968-1970, and then 1975 to 1981, except fall of

1977, and on the university-wide committee during the 1976 interval.

Swent: I'm sorry; five years.

Wiegel: But because we did our homework--

Swent: But before that, you were on the--

Wiegel: Yes. --almost all of our recommendations of the Berkeley committee were accepted by the people involved.

Swent: What were some of them?

Wiegel: I can't remember. Lots of them were pretty sensitive anyhow, but I can't remember the details.

Swent: I wish you could.

Wiegel: It would be like: who would have voting rights in departments; would an adjunct professor be able to vote on this or vote on that; what rights do assistant professors have, or do they have any when a promotion of someone to full professor is being judged? These kind of things. Lots of things like that. Many of them would be individual things, but as I said, we did our homework, and almost always our advice was accepted.

Swent: You reported to the senate?

Wiegel: We reported to the senate, yes. Certain things, if they required action by the senate--maybe somebody said, "We don't think this senate rule is right"--then we would write a motion to bring before the senate, and then the senate would make a decision if they were going to change something. Those are all buried in academic senate minutes one place or another. I did learn something there very well: you need a good minutes of senate meetings because very often the question would be, "What was the intent of the senate when they approved this?"

Here you have a bare-bone one-sentence motion that was passed, debated, and all this and that; but then somebody said, "What was the intent, because I don't think it applies to this, and I don't think it applies that." So we would dig into the minutes, and we always wished they had been more thorough. They were pretty good, but there often seemed to be something missing about the intent. It was what one professor might have said, and that kind of swung the vote.

Swent: Then the next important one was the committee on committees.

Wiegel: That is the only elected committee of the academic senate, and is for two years. I think of Herb Caen, because he died just recently, but some years ago he had what he thought was a very humorous little tidbit about the fact that the Berkeley senate had a committee on committees. He liked that. He said, "Only at Cal would they have a committee on committees." Of course, it was the committee the entire senate membership elected by secret ballot, and the committee on committees is the one that then appointed all other committees of the academic senate. This includes the budget committee, the graduate council which had graduate affairs, the committee on courses of instruction which reviews all courses. So courses, faculty, committee on graduation standards. All of these things.

That's the gut of governance of the academic part of the campus. All of these are appointed by the committee on committees, and the chancellor would always ask the committee for names of faculty to serve on chancellor's committees, or the five-year review committee of departments, or organized research units and so forth. I remember most everybody that was elected was broad based and well known, because otherwise, they wouldn't have got elected. They were for two-year terms. Marion Diamond was a member and John Whinnery was a member when I was on it, 1982-1984.

Swent: That's the key committee.

Wiegel: We were really conscientious on this, but we also had a lot of input from the secretary of the academic senate, who at that time was Milton Chernin. He was a retired dean of the School of Social Welfare, and he was an extremely knowledgeable person. We had to find out not only what people could do, and most of us would know this, but they would know the ones that seemed to be willing to serve in committees. But sometimes you had to twist arms. We all twisted arms of individuals to get them to serve. It's unfortunate, but not a great number of people are willing to serve on these things, but they're sure willing to complain when something goes against what they would like to see done.

I used to be very blunt: "Put up, or shut up. If you're not willing to serve, then don't come complaining if some committee makes decisions that you don't agree with." This is how we govern ourselves.

The Most Difficult Year: Acting Dean, 1972-1973

Wiegel: Back then, the new chancellor was Albert Bowker and the dean of engineering was George Maslach at that time, and Bowker asked him to become one of the two provosts; one was for the College of Letters and Sciences, the other was for professional schools. I was asked to serve as acting dean, and I did this in 1972 and 1973, but I still taught my graduate courses. I could drop the undergraduate course, but I taught the graduate courses.

I was supervising five Ph.D. theses. I was principle investigator on a couple of research contracts. I was on the University Sea Grant Coordinating Council, which I mentioned earlier, the Chancellor's Executive Committee on the Earthquake Research Center, the Board of Directors of the Faculty Club--in fact I was president for one of the years--and I had just become the founding president of the International Engineering Committee on Oceanic Resources, which I had been working to get going for three years, and which was and is advisory to UNESCO. I was on the Governor's California Advisory Commission on Marine and Coastal Resources, which is the predecessor to the Coastal Commission. I was an ex officio member of the National Research Council's Marine Board. I was vice chair of two ASCE units: the executive committee of the waterways and harbors division, and the coastal engineering research council. Both of which I became chair of a little later.

Swent: Those were terribly busy times.

Wiegel: I inherited several personnel problems that had to be resolved which I did.

Swent: In the dean's office?

Wiegel: In the dean's office. These were not told to me in advance. The budget cuts had started, so suddenly there was less money also. That had nothing to do with the personnel matters; this was separate. Fortunately, the college had good staff--and still does--including Dave Brown, who was the budget and facilities planning officer in the office of the dean. He retired only last year. He was very good, and all of the rest of them were.

Also, the great help of our secretary (later an administrative assistant) in the hydraulics groups, Mabel Iwamoto, should be recognized. My handouts and other classwork was always taken care of to help me in giving my courses. I think I have already mentioned someplace the accolades President Kerr has given to some university administrative assistants. Dave Brown and Mabel Iwamoto are of that caliber.

Swent: Where was the dean's Office at that time?

Wiegel: On the third floor of O'Brien Hall. It wasn't set up like it is now; a little different. The staff is now bigger, and things of that sort. But the department chairs were all very good. One of them, incidentally--the chair of electrical engineering, Tom Everhart--is currently president of Caltech. That gives you an idea of--when I said the chairs were pretty good, they were very good.

It was a very hard year. In hindsight, I should not have taken it. It was a terrible overload. I was always a team player, but, boy!

I read John Whinnery's oral history again, and John said that when he became dean, Fred Terman--who had been dean at Stanford, and then provost at Stanford--told him never to take a problem to bed with him, but John said that Fred never told him how he could do this. I never had that problem of taking problems to bed, but on the other hand, I don't think I got much sleep that year, so I don't think there was anything I could do about it.

Swent: That was 1971, you said?

Wiegel: It was 1972 to '73, I think. It was in the early 1970s I know. I also had good advice from several senior faculty, particularly Earl Parker and Harmer Davis; both of whom John Whinnery mentioned. But also John Whinnery. So when I had some tricky things I would go talk to these three--I knew them all well.

Swent: You had a young family at that time too?

Wiegel: Yes. We always liked to ski. I don't think we got to ski much that year. I suspect we didn't get up to ski. We like to ski, and in the summer we used to go up to Tuolomne Meadows in Yosemite and camp. I'm sure that we went camping. We've always liked the outdoors.

I remember one of the things Harmer Davis told me: "Bob, there are some problems that you can solve, and there are other problems that you can only cope with." Just last month I read the book by Henry Kissinger called Diplomacy, and of course on a very much higher level, Dr. Kissinger said he learned that there were some problems you could solve, but many you could just cope with. What is the term the British use? "Muddling through." You couldn't solve them! That's all there is to it. So what do you do when you can't solve a problem? It won't go away; you just kind of hope it might. But there are things you can't solve.

Swent: Americans love to try to solve everything.

Wiegel: I remember reading, years ago, about--it was a joint venture, but I think it was largely British--it was in the end of the last century. A cable across the Atlantic from some place in the British Isles to Canada and then down to the U.S. had been installed. Some of the great physicists in the U.K. were working on the transmission problem, and of course, you had in the U.S., people that were also on it: Thomas Edison on transmission; the telephone transmission by Bell. There was a proposal in Britain to put a cable from London across the Continent, across the Mediterranean, Arabia, then all the way to India. So the foreign office in London could be in almost instantaneous contact with the representative of the queen in India. Only it wasn't proposed by the foreign office; it was proposed by some entrepreneurs who thought it was a good thing.

The story that I read some place was the foreign office turned it down. They said that one of the great advantages of the present system was: a letter comes from the viceroy, or whatever they call the person, in India--comes by packet ship to London; we read it, discuss it, and send an answer back by packet ship to India, and by then the problem's resolved and our bad advice will not have to be taken.

You asked a little bit about departments. When O'Brien was dean, there was only one department in engineering; it was the department of engineering. O'Brien was dean of the college of engineering and chairman of the department of engineering. Then there were a number of divisions: mechanical engineering, electrical, civil. But in the transition from O'Brien to Whinnery, the chancellor established departments.

This is very important in governance because in the by-laws of the regents, the department faculty are the ones that have certain responsibilities and certain authority. Kerr wanted to have more than one engineering department, a number of them. This has been well covered in John Whinnery's, so I won't go into that.

Swent: I was wondering how it affected you, though?

Wiegel: Well, you mean as a faculty member?

Swent: Yes.

Wiegel: I don't know, except that one time, O'Brien said--right at the end--I had been in mechanical engineering; it wasn't civil engineering; I was, and still am, a licensed mechanical engineer--he decided that if you dealt with water as water, you're going to be in civil engineering, if you were in steam--if you boiled it or froze it, you stayed in mechanical--that was air conditioning, refrigeration, and steam. That's my only personal experience. But the most important experience I had with respect to this was when I was acting dean. A report had been made by a committee--Don Pederson was chair of the committee--because there was a department of computer science in the College of Letters and Science, but in the department of electrical--I guess at that time, it was known as electrical and electronics engineering, but that was where hardware was being developed for computers.

A lot of software concepts were developed in the Letters and Science department; software was also developed by people in different engineering, physics, and chemistry groups--developing their own programs. All kinds of people were working on it. Anyhow, this committee report by Pederson recommended that there should not be two places that this is really taught; that the combination of hardware and software affects the architecture of the hardware. It would be better if the diverse group of faculty talked to each other and worked together. I read the report; it made a lot of sense. I discussed it with the department chairman, Tim Everhart, and then recommended to the chancellor that this be done. Bowker acted on the report and recommendations. The department in Letters and Science was disestablished and the department in electrical engineering expanded--it's called electrical engineering and computer science: EECS now. There's a big building for electrical engineering, Cory Hall, but the new one, Soda Hall, is almost all computer stuff. The one across Hearst Avenue. Of course, this was the proper thing to do.

The report was right, the actions taken were right. It has been very important. Since then, I believe, MIT, Stanford, and others have combined their departments too.

Swent: Was this a difficult decision to make?

Wiegel: No, not to make; it might have been to implement. Bowker is a very strong person. I didn't know him very well then. Since he retired, he went back East, he was head of some big programs. But he's now retired. He lives out here in Berkeley, and comes to the Faculty Club. There is a group of us that have lunch at least once or twice a week together. He's a very strong person. Of course, he was professor of statistics. He had a good background for understanding these things. His father was with the National Bureau of Standards, so Bowker grew up hearing about all these kinds of things. I'm sure he had no trouble whatsoever in making the decision, and he's a very strong person, so if he thought something should be implemented, it was.

Swent: I'm just thinking, as acting dean, it seemed clear to you, but what would have been an alternative?

Wiegel: Just to have the two separate departments, and in hindsight--

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Swent: Was there any resistance from the Letters and Sciences to giving up?

Wiegel: Yes. I don't know any of the details; I know there was. As I said, Bowker was a very strong chancellor. We were lucky; not just from that standpoint, but in many ways, we were fortunate that we had Bowker as chancellor at that time. I'm sure he just said, "This is it." Pederson and his committee, had done a really good job; they had done their homework well. They analyzed the problem well, and they had come to logical conclusions and recommendations.

I served on many college and department committees also. This is all apart of the governance. One of them was the interdisciplinary committee on engineering in the ocean environment. That was from 1964 until 1996; long after I retired. I've always been interested in this subject. This is a part of the governance: how do you handle an interdisciplinary program? It's very difficult; it's hard to get them to work well. It's hard to get them to work together at all. It's a continual effort; it still is.

Swent: How large a committee was it?

Wiegel: Six to eight people usually; something like that. It being civil, naval architecture, mechanical engineering--I still call it mining--mineral technology, because of the petroleum, the offshore--oil and that sort of stuff.

Swent: All senior faculty?

Wiegel: No; across the board. Assistant professors and full professors. It was done by design that way. I just use it as an example because it's always been difficult. Now, with the disestablishment of the department of naval architecture and offshore engineering, and people going into two different departments, I'm not really sure as to what will be the long-term survival of it. It sounds like more interdisciplinary effort. I am worried about it.

Swent: Which of these things was the most gratifying to you?

Wiegel: Except for the year of acting dean when I was not getting much sleep, I enjoyed all of it, I think. I think maybe the Institute of Marine Resources, because I would go down to Scripps Institute of Oceanography at La Jolla quite often. I've always gone to see the people at La Jolla, because I've always done coastal oceanography, and I knew many of the people there. The people I worked with that were in physical oceanography and in fisheries and things like that--I liked to hear what they were doing. Not that I fish; I don't mean that, but trying to have what they call sustainable fisheries. You still see that.

I read an article just over the weekend. It was probably in the New York Times, or something like that, on sustainable fisheries. It's a terribly serious problem worldwide. I was meeting and learning what these people were doing about it; these top scientists. But so much of it is economics. The Water Resources Center--I may have mentioned it--I was on the Water Resources Coordinating Board for years, too, right up until I retired. In fact, I did two things my last year: one was chair of the five-year review of the Institute of Marine Resources; the other was I chaired the search committee for the director of the Water Resources Center, which is university-wide. I've always liked these things that cut across different disciplines.

So, I would say the Institute of Marine Resources is a type of governance. I would have liked that technology transfer, if we had not had so much over-direction from Washington. Not when you were working with an advisory board with some of the finest people who had ever done that sort of stuff. I think I answered your question.

Swent: Is there anything else that we should talk about today?

Wiegel: I can't think of anything right now.

More About the State Technical Service Program

[Interview 5: March 10, 1997]##

Swent: We're beginning our fifth interview here, and at the end of our last session, we were talking about the State Technical Service

Program, and you had a little bit more you wanted to say about that.

Wiegel: Yes. I think as I mentioned, my entire set of files, which would be probably a full file cabinet--certainly several drawers--is not here, and not up in our dead storage either, which is up in the attic here. I now faintly remember, many years ago, somebody from one of the university institutes--probably governmental or something like that--wanted to make use of them for a study they were doing for the state. The only thing I can think of--because I'm always pleased to help--is that I said, "Go right ahead," and I've lost track. But since I talked to you last time, which was several weeks ago, I believe, I have been able to find a few documents--maybe four or five--that give some of the background. I was trying to reach for names, and certain specific examples of some of the programs, and I've done that. I think it is important because they're talking about this again at the federal level, and have been for the last four or five years; the state of California has a group going and so forth. So much of this has been looked at before. One of the people that was very useful was Hyman Olken.

He was with the Lawrence Livermore National Laboratory. He's an electrical engineer; but also a writer. A very good technical writer. He loved it. He was a very good thinker. He did quite a bit of the work for us. One of the things he did do, and this was under contract--this is one of the items I did find, and I'm going to turn it over to the Water Resources Center Archives after today's interview, so we'll read this. It's called, "Information Resources for Promoting Advance Technology Development in California." This was back in 1966--thirty years ago. This lists all the various technical libraries, whether they are with the government, or with private corporations, not only in California, but throughout the whole United States. The National Bureau of Standards special retrieval systems; the way you retrieve information from the patents office; how you get information from the small business administration. Then Lockheed Aircraft's in-house system; Dow Chemical's in-house system.

He collected all of this information and made this available. I think we could refer to what President Johnson said when he signed this bill, because when the bill was signed he was a very--

Swent: Which bill was that?

Wiegel: The State Technical Services Bill. This was in 1965. I think they called it the State Technical Services Act of 1965. You see every once in a while the same statement: "This may do what for

American businessmen what the great Agricultural Extension Service did for agriculture." The things were so entirely different, though. One, you did the research, you did the teaching, you brought it to the individual farmers. Here you're trying to do something with massive research activities of the Atomic Energy Commission and Institutes of Health, and make them available to everybody including very large corporations. So there's really no comparison; it's rhetoric.

Swent: I wonder if the Internet will make all of this easier.

Wiegel: An internet (lower case) was a very critical part of what was done then--you can do it now much faster with the (upper-case) Internet.

Swent: I was thinking that that will, in a sense, supplant this, won't it?

Wiegel: The two biggest contracts that we let in the state of California: one was jointly with the state of California Library--but they expanded it in-house to include the public libraries in San Francisco, Los Angeles, San Diego, and Fresno--to spread throughout the state to make all sorts of the technological information available to people. There was a joint contract with the Institute of Library Sciences, which was a university-wide institute of the University of California, and they worked closely with the California library system. Remember this was thirty years ago. They were developing how you use computers to access technology information, and much of this information was being put on tape, at that time, by some of the government agencies in some of the private companies.

The university library research group was working on how you could make this available; how can you access this via computer. Now, it's almost impossible to access it without the use of the computer. So we funded some of the first studies of this.

Swent: We?

Wiegel: The State Technical Services within California. I mentioned, I think, several of the people that were on the advisory council. Bill Gould chaired it, and he was chief engineer for Southern California Edison, and later became the chief executive officer of Southern California Edison--or CSE, Inc, as it's now known. Dave Packard was on it. Terman, of Stanford, who was the mentor of people like Hewlett and Packard; I've heard that he also was the one who thought of the Stanford Industrial Park, but I'm not sure. Because when Stanford left the farm and all of the money--and his wife who did so much of it--to Stanford, they entailed the property so it couldn't be sold. They were very perceptive.

So Terman thought about leasing it, and leasing it to these companies that became high-tech; which was the start of Silicon Valley. So Terman was a member. General Doolittle was a member. General Doolittle is known by most people because of leading the flying raid on Tokyo. But prior to World War II, he was an outstanding engineer, and he worked with Shell Oil Company. I remember O'Brien either telling me at some stage, or it might be in his oral history--I'm not sure--that General Doolittle was the one who conceived of the system for high-performance aircraft. He was a well-known pilot; he broke all kinds of speed records and things like that back in the 1930s. He knew that you had to have high-performance aircraft, but this required high-performance fuel that didn't exist, and no company was going to spend the huge sums of money to develop high-performance fuel unless there was a demand for it.

The military couldn't develop high-performance aircraft unless there was high-performance fuel available. Doolittle was the one that thought of this and pushed it through. He was what is now known as a systems engineer, and he was on the committee. Blackwelder was on it. Blackwelder was the one who developed the automatic tomato picker. You don't just pick tomatoes; the type of tomatoes that were growing would bruise. So UC Davis people jointly, at the same time, developed tomatoes that could be picked mechanically. There's another example.

Swent: Those nice tough-skinned tomatoes that we have today.

Wiegel: Those are great for making ketchup and making soup. They are no good to eat as a tomato.

Swent: But they don't bruise.

Wiegel: They don't bruise, and they serve the purpose for soup and ketchup, and you get away from having to have stoop labor. This is very important; you want to improve the quality of life for farm labor. That's extremely important. This is a very important function of engineering. It's always to make life easier for people doing things. Substitute energy for brute labor; substitute better ways of doing things and safer ways of doing things. So many of these things are really to improve both the standard of life and the quality of life. So they were very good people on the council.

So this library and this access and the use of computers were some of the first things we did, because these people were doing that. Another thing is to get it out; get the information out. It was recognized that television was the way things were going. Remember this was thirty years ago. There were lots of popular programs; public television had started. The public television station in Los Angeles, KCET--Lee Dubridge was chairman of the board of it.

Swent: He was at Caltech?

Wiegel: Dubridge was also president of Caltech at the time. They had this program called "R & D Reviews," "Research and Development Reviews." Los Angeles was the center of R & D in those days; no question about it. There's an attitude in Los Angeles. It's in the Silicon Valley now, but you go back thirty or forty years ago after the war because of the aerospace industry, in this country, the center was the Los Angeles area. All kinds of people down there had new ideas and ways of doing things.

They developed a series of these programs and we helped to fund it substantially. They produced thirty-nine separate programs. These were interviews with people that did research and development, but you could have good video shots if it was in aerospace and things like that because you didn't have to pay for how much it would cost to take a tape of a launching or something like that. Nineteen other public television stations throughout the U.S. took it up, and broadcast them.

Then they were going to have another program called, "Innovations," where they would interview people who innovated. That was about the end of it. The State Technical Services was a five-year thing and it was not continued by Congress. One of the reasons was that it required matching dollars: half by the federal government, half by whoever was doing it. Then they said it had to be non-profit institutes like universities, and things of that sort. This hamstrung it, because that meant that anyone who put a proposal in would somehow have to get the other funding from someplace else and that's not easy.

Swent: No.

Wiegel: The program died at the end of five years. At the end of two years, I recommended that the university move out of it, and the State of California take it over through some office in Sacramento; which they did--I think the Finance Department.

Swent: It's interesting; you seldom hear of anything dying.

Wiegel: Many things die.

Swent: These things get a life of their own so often.

Wiegel: Yes.

The Challenge of the Information Age and Data Management

Swent: I have some questions that might fit in with this just as I've been reading about some of these things. I'm referring to your History and Heritage of Coastal Engineering, which was published by the American Society of Civil Engineers for your conference on coastal engineering in 1996. You did a history in the USA which is very good, and you mentioned at one time that the Beach Erosion Board did a wave measurement program at Huntington Beach, California. You just did a little parenthetical note that the strip charts and digitized data are no longer available. Then you said the Los Angeles District of the Army Corps of Engineers--they didn't keep those kinds of data after they collected them?

Wiegel: This is a very serious problem. We talk about the information age; there is so much information generated that nobody knows what to do with most of it. Think of the amount of information coming back from the space probes: billions and billions and billions of bits. It's the storage, and certainly nowadays, we have much better storage techniques: compact tapes and discs and things of this sort. In those days, that information was on, probably, paper tapes--punched holes or something. I can't remember.

Swent: Strip charts.

Wiegel: You're right, it was strip charts, which meant that somebody had to look at it and read data from it, and then put it either on another piece of paper, or punch it in. Remember we had punch cards?

Swent: Yes.

Wiegel: So there's much data lost, and this whole idea of data management is one of the main things that the engineering profession, the science profession, economists, people in business management, medical management must cope with--how to handle all these great amounts of data. I remember reading an interview with a top combat pilot. I forget which battle he was talking about--this goes way back, but after World War II, it goes back to when so much information was coming in, so it may have been Korea or Vietnam.

He said that the problem was that so much information came to him while going into a combat mode--he would just disconnect the stuff because he had to have his mind free to concentrate on what was happening within the next ten or fifteen seconds. Now you get into another type of information and management--I've always been interested in management--as to how does a person make decisions under stress? This stress can be combat, which is pretty high-stress; you're either right or you're dead. But it's also dams and floods.

You have got a few hours to make a decision as to whether to start releasing water from a dam because of possible flooding, or if you release it too much, maybe it's going to stop raining and you're going to not have enough water next summer for your agriculture and your home use. Or it can be a nuclear power plant or something. There's all kinds of these things that you have to make decisions. There's so much data.

We talk about the information age: it's not the amount of information; it's the how can we manage information? This is what we're really struggling with today: how to handle this great influx of things. That's a real thing.

You mentioned wave recorders. I was trying to think of an example of this technology transfer, and you triggered it. Here's a camera, and what do you do now with the new cameras? You point them, you look, and you push a button because an acoustic signal has gone out, and reflected off whatever you were pointing that portion of the lens system at, and it comes back, makes a calculation with a chip in the camera that said this is the distance, it then gave this information to a little system running off batteries, that adjusted the lens to focus it. That's technology transfer.

People were studying--during World War II--bats and how bats, when their signals went out, told them where things were. This concept was used underwater; underwater acoustics by the navy. You can't see underwater very much, but the acoustics are marvelous. Acoustic waves will go through the whole ocean. Walter Munk's experiments at Scripps now where they're setting off noise in the Antarctic and they pick it up off of Baja California and off of Japan and so forth. They're looking at temperatures; temperature variations. But underwater acoustics is what you use. So the navy did a lot of development.

The Honeywell Corporation--Minneapolis Honeywell it used to be known as--they did a lot of work on acoustic ranging. But it's not just to send the signal out, it's massaging it. You have got to look at the time it took to go out, be reflected, and travel back, which is very small. Have information stored, what is the acoustic velocity in that fluid, at that temperature. So it's not easy. You have to have all sorts of information in there--[interrupted by telephone call].

Swent: We just had a brief intermission. An interruption while Mrs. Wiegel called to say they had reservations at a hotel in Paris in May.

Wiegel: In May, yes.

Swent: Going over to Paris from London for lunch. That sounds great.

Wiegel: It ought to be a good lunch. We are going over from London to ride on the "tunnel-train." We will stay for a couple of days, and then back to the U.K.

Swent: It should be. That's wonderful.

Wiegel: Anyhow, Honeywell didn't develop it for cameras; it was a Japanese group, and they developed them for use in cameras and we all use it now. Honeywell had a big lawsuit against several of the Japanese firms, and they got a multi-million dollar judgment; I think it was two years ago. But still, it was technology transfer. The information is there; it's how do you modify it that is useful. This is what Hy Olken had discussed thirty years ago; this procedure. Because you need, not only the information, but you have to have entrepreneurs that think about that information in a different context.

Wave Sensor; Example of Technology Transfer

Wiegel: The next step--and this is the wave recorder. One of the university staff, Lyn Magel, working with the ship model towing tank at Richmond Field Station thought, "Why don't I just take this camera and take that piece out of it, and mount it on the towing carriage when we're moving over water waves, and measure the distance down to the surface and then record it."

As the model ship is being towed down the tank in waves, you measure the waves. He thought, "I don't care about the camera, but that's an awful nice way of trying to measure waves." You don't just do it, because now you have to get it into--not a strip recorder, but a digitizer, and then put it on a magnetic disc and develop a program to analyze the input--so you see how these things go. Technology transfer, information retrieval--it's just part of it. It's how do you put these together and come up with new concepts and new ways to use things. This is what requires that Silicon Valley type of concept.

Swent: Mentality, yes.

Wiegel: The mentality is: what can I do that's different, and better?

Swent: I have some notes here that I'll just toss out. In 1960, you developed a practical means for calculating the shape and kinematics of breakers. Was that a technology transfer kind of thing?

Wiegel: When was that?

Swent: In 1960.

Wiegel: Gee, I don't even remember.

Swent: You don't remember, okay. Perhaps we'll fill in that later. You also got--this was before then even--you developed the first quantitative data on the relation of waves. This is a separate thing.

Wiegel: That was again one of those things where--science--you don't always do these by plan. This must have been back in the late 1940s.

Swent: The dates that I have are 1949 to 1957.

Wiegel: That may have been when it was published and so forth. We were measuring waves up off of Oregon and also waves in northern California and central California. I remember talking with John Isaacs--who was still here; he hadn't gone to Scripps Institution of Oceanography yet--and he said, "We ought to see if we can't see these coming to Oregon first, waves coming from the Gulf of Alaska. Then let commercial fishermen and others know that they're going to be here in California in a day or two."

So I was looking for correlations between locations. I found something else that I wasn't looking for at the start--very good correlations from the statistics of a given record between the mean of the wave height and the maximum wave height and things like that.

A British mathematician/scientist by the name of Michael Longuet-Higgins was working on it also, and there was a fellow at Wood's Hole, Dr. Seiwell. This was all independent; three people doing this work independently. We all came up with about the same sort of thing, only Longuet-Higgins came up with the theoretical aspect and I just had the data. So that was almost luck. Bob Putz here at Berkeley came up with about the same theory at about the same time, but then shortly after he and Frank Snodgrass advanced the theory and practice substantially.

Swent: How were you aware of what he was doing?

Wiegel: Well, the British and the U.S. worked close together on waves. This was still an outgrowth of the amphibious type of work and the cross-channel landings.

Swent: How did you communicate with each other?

Wiegel: By letter originally, but then Michael visited here, and then a couple of years later, I was asked by the navy to visit a number of groups in the U.K., and also in France and the Netherlands.

Swent: This was part of that expansion to an international awareness.

Wiegel: I've always been very internationally oriented, because these things are all international. A person here, another person someplace else. You become a little community.

Swent: Evidently, the first foreign people that came here were from the Netherlands?

Wiegel: The first Fulbright scholar in engineering to come to the U.S.--some Fulbright scholars go from the U.S. to other countries--was from the Netherlands, Egbert Prins, and he came to Berkeley. Egbert eventually became the director of the Delft Hydraulic Laboratory, which is the biggest hydraulics laboratory in the world. The Dutch have always had good engineers.

Swent: That leads me to another thing that I thought we might at some point discuss: in the Netherlands of course it's perfectly clear that the control of water and the coastal zone is of national significance.

Wiegel: We always talk about the Dutch boy with his thumb in the dike.

Swent: That's right; it keeps the nation alive. In this country, that's not so clear. Now, we have most of our population living on coasts, but there's still a large portion of the taxpaying public that doesn't care whether there's a beach or not.

Wiegel: I don't think they even think about it. I think they just think that beaches are there and they should be there.

Swent: These billions of dollars that they're spending to nourish beaches for private use. Has there been any resentment of this?

Wiegel: Not billions--hundreds of millions. From 1950-1993, the U.S., via the Corps, spent $403.2 million, an average of $9.3 million per year. A lot of this goes back because of one person; a campaign by one person primarily, Orrin Pilkey, a professor of geology at Duke University.

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Wiegel: In my opinion, his conclusions, which are widely publicized, are based on selected examples that match his opinions, not a representative spectrum of beach behavior--at the best, very poor science; some of it almost non-science.

Swent: He's the one who wrote the book, The Corps and the Shore?

Wiegel: That's the one, but he's written a number of things. I know him well. Some of the things Orrin [Pilkey] says I agree with; other things I simply disagree with. I've told him many times that some of the things he says are just simply incorrect; period. There's no scientific basis whatsoever for some of his comments. When he was young--it's my understanding; I think he told me this--his parents had a home on a beach someplace, and it was destroyed during one of those big storms. Whether it was a northeaster or a hurricane, I can't remember. So his whole way of looking at things has been biased from a very bad personal experience, which is understandable. This is what I understand.

It's fine to have a point of view, but I still think scientists should try to separate out correct science from stuff that doesn't have a scientific basis. I don't like to get into personalities.

Swent: Personalities affect our positions.

Wiegel: I realize that. The Dutch are going through a big change too. This has to do with how people's perceptions change. In the early 1950s, there was a severe storm in the North Sea, and we talk about a hundred-year event, and a two-hundred-year event, and a fifty-year--but most of these aren't based upon any statistics, they're just people talking. But the event that hit the Netherlands and parts of Belgium, southeastern England was--and this is based upon hard statistics--about a one-in-four-hundred-year event.

There were hundreds of people killed; hundreds. Thousands of hectares, or thousands of acres--inundated by salt water.

Towns, villages were destroyed; farm animals killed. The U.S. and NATO--but mostly the U.S.--with their equipment--helicopters, DUKWs--that's the amphibious vehicle--were great in working with the Dutch authorities to rescue people and animals and things like that. They then started what is known as their Delta Plan to protect the Netherlands from such a terrible event in the future. This is what really started their modern coastal engineering, just like our amphibious operations started ours in the United States; it was this event in the Netherlands.

Swent: That was when?

Wiegel: Probably 1953. Professor Thijsse was the professor there and the director of the Delft Hydraulics Laboratory, which was a small laboratory at that time; it developed into a large one. But also, many new young people came out of physics and engineering to work in that field because it was a national disaster, and they put in a tremendous amount of effort, and they developed what was known as the Delta Plan. It had to do with how to protect the lowlands with levees, tidal gates; with the Rhine River and the Schelde River having to flow into the North Sea. I was over there first in 1954, and several times later. More recently, I was there at an international meeting; I was still chairman of the Coastal Engineering Research Council--I was general chairman of the 23rd International Conference in Coastal Engineering in 1990, held in Delft. This was much later.

At that time, 1990, the Dutch gave a series of papers on what they had done on the Delta Plan, and we visited the last of the big structures which was just being completed on the Schelde. By then a whole new generation of people had grown up, asking why are you doing this? Why don't we just let things flow? You see, no knowledge of hundreds of people being drowned--apparently, the average person can't read about this and get any comprehension. You have to see a disaster, to live through it, I think, in order to have it hit you. The young generation--I don't like to say that--this is the so-called "generation theory of history," which sometimes works, and sometimes it doesn't. Certainly the generational theory of history hasn't worked at all in Yugoslavia; they still remember what happened a thousand years ago and are still fighting the same wars they fought a thousand years ago.

So the Dutch people have had a big change. They do all kinds of beach nourishment, incidentally.

Swent: Do they?

Wiegel: On a percentage basis, they do much more than we do. The Spanish do much more than we do; the Portuguese do much more than we do;

the Germans do much more than we do; the Japanese also--percentage-wise. I think it's because of the fact that they don't have as much coast, and a lot of the Dutch like to go to the coast. The main source of income for Spain is the use of the coast for tourists and so forth. I can get into this again a little later, I think. I'd like to just wrap up the State Technical Services to say that the person that was of great help--beside Hy Olken who did a lot of the technical work was Frances Eberhart. She was the executive secretary for the Engineering Advisory Council of the university in the president's office.

Also, Harry Wellman asked her to serve as special assistant to me on the state technical services program--because through her other work for many years with the engineering advisory council, she knew many of California's major engineers personally, and was greatly respected by them. Many of them were either on the advisory council or had met with members of the advisory council and so forth. I just want to be sure that people who did the work then are credited for it--they really did so much. Fran Eberhart certainly did this.

I would like to quote here a statement made by Clark Kerr, President Emeritus of the University of California: "A great secret of the administration of the University of California is that it is not really administered by the dean or the department chairman or the chancellors or the president. It is administered by an extremely good group of administrative assistants who really run the place." He then mentioned Fran Eberhart as a model for all of them.[1]

As I said, most of the information is gone, but I do have some of it which I've now decided--because so much of it is connected with library stuff and retrieval--I will turn it over to the Water Resources Center Archives so that--

Swent: We haven't really mentioned your archives yet.

Wiegel: That I think we'll get to at the end.

Swent: Should we do that later?

Wiegel: --under mentoring.

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[1] This was given at the banquet of The Symposium to Honor Morrough P. O'Brien, 23-24 March 1987. ["Mike O'Brien--A Master Builder of Berkeley," by Clark Kerr, Shore & Beach, vol. 55, nos. 3-4, July-Oct 1987, pp 31-33.] Fran and Howard Eberhart were at the banquet at the Faculty Club.

Swent: Because that's terribly important.

Wiegel: You can see I've always been interested in information retrieval and things of that sort.

Swent: It's the theme that goes all the way through, doesn't it?

Wiegel: The other was the use of television for education, I think.

Introducing Television Instruction through University Extension

Swent: Yes, and SCUBA [Self-Contained Underwater Breathing Apparatus] is another thing I thought we might, at some point, mention. Some of these technological things that helped a lot.

Wiegel: The television--as I mentioned, thirty years ago, people were using public television for education--and at Stanford, just about that same time--Joe Pettit was Dean of Engineering at Stanford then. Joe is a Cal graduate--I think, 1938 or something like that--he was in electronics engineering, and became dean of engineering at Stanford, and then later on, president of Georgia Tech.

In the early 1970s, George Maslach, who was dean here at Berkeley, asked me to look into the use of television for engineering courses. One of the members of the university Engineering Advisory Council--I can't remember the person's name now--was pushing this very much; he knew what Stanford was doing. Stanford was broadcasting these courses throughout the Peninsula. They had an antenna up on Black Mountain, which is to the west of the Stanford campus, and they were doing a very good job. So I looked into it, and Joe Pettit was very helpful to me. I met with him several times about it. Our university had a small television office which was mostly to videotape a few things, and to use close circuit TV for overflow classes--not engineering, however. We had some funds available--I'm not sure where those funds came from--to establish a little studio and transmitter. We thought it might work out.

I did a lot of work on this and got a lot of information. I remember meeting with Bob Connick, who was acting chancellor at the time. I spent an hour or two with him on it at his request, because the problem that bothered me wasn't one of the technical problems; we could do it technically. Jim Cumming did most of the technical development. It wasn't getting it started, because somebody had given money that we could use to build a studio and build a transmitter and so forth. It was how you could fund it operationally. Stanford had no trouble; they could simply charge. Stanford's a private university; people are used to paying. People weren't used to paying the university. But if we did it via extension, then maybe we could charge, because the extension had to be a hundred percent self-supporting.

We were talking about televising regular courses; full for-credit courses. We had to set up a method that people could contact the professor. If you take a course on campus, if you have got a question, you just go ask. There's an interaction, and you don't have this interaction so much via television; although we worked on ways where a person receiving it could push a button, a light would go on, and the professor would know that somebody wanted to ask a question in a remote classroom. A couple of companies were willing to set up remote classrooms to receive it. Some already had such a facility because they were receiving courses from Stanford.

We didn't want to compete with Stanford--this wouldn't be fair--we wanted to offer things that Stanford wasn't able to offer, but still the local industry wanted. I guess that somehow the funding worked its way out, because I did go down there to the studio in McLaughlin Hall just last Friday and there was a list of five graduate courses and one undergraduate course being given this spring; all in electrical engineering and computer science. In fact, almost all were in computer science. It makes use of both live broadcasts, but also video recording. That is: cassettes will be sent over to a place; they don't have to receive it live. I believe it's now done worldwide. I don't know, because I've been out of this for a long time. So that's one thing I--

Swent: You got in and started.

Wiegel: --started. As I say, they must have somehow worked out the problem for funding because it's still in operation.

Swent: That was in the 1960s?

Wiegel: That was the early 1970s; 1972 or 1971 or something like that. It's been in for twenty-five years now.

Swent: At least.

Wiegel: That was just catching up.

Swent: Yes. Some wave motion goes on in the interview.

VII ENGINEERING ACTIVITIES AS PUBLIC SERVICE

Publications Resulting from Conferences and Workshops

Wiegel: What is it we're supposed to be talking about today?

Swent: We wanted to talk about your work in the wider engineering community. It seems to me the publications might come later, because weren't they an outgrowth of your committee work, or would you rather begin with the publications?

Wiegel: I think I've already mentioned a couple of publications like the book I wrote on oceanographical engineering, and the one I edited on earthquake engineering. This ties directly in. I did pull out one of the earlier ones here. It's back in the mid-fifties.

Swent: Let me just read the title; may I?

Wiegel: Yes.

Swent: Coastal Engineering Instruments. Beautiful cover photo of the Bay Bridge, isn't it? "Proceedings of the First Conference on Coastal Engineering Instruments, Berkeley, California, October 31 to November 2, 1955. Edited by R.L. Wiegel, published by the Council on Wave Research, the Engineering Foundation, 1956."

Wiegel: What the Engineering Foundation was: there was something called the Engineers Joint Council. It's still in existence. It's the presidents of the American Society of Civil Engineers, and American Society of Electrical Engineers, and Mining Engineers and others. The big engineering societies. The presidents of these meet in New York--at least they did, once in a while. They had an operational arm which was called the Engineering Foundation. They would fund certain things, and they still do; they still fund certain conferences. About five or six years ago, they funded a conference which I attended, on the possibility of global warming, the possibility of sea level rise, what this might mean to infrastructure and other things along the coast and things of that sort. I had just come back from a trip to France, and the U.K., and the Netherlands for the navy, and we also--at the same time I was in Grenoble at the fifth conference in coastal engineering, which was the first one held outside of the United States. I was a mechanical engineer...

Swent: That was in 1957?

Wiegel: 1954 was when I was in Europe for the first time after the war.

Swent: Right. The fifth conference was in '54 in Grenoble.

Wiegel: That's correct. So I knew many of these people. In fact, I was doing all the detailed work because Professor Johnson was on sabbatical leave.

Swent: They had established this Council on Wave Research in 1951, and you were the executive engineer.

Wiegel: I was the executive engineer. I mentioned to O'Brien and Johnson that we really needed a specialty conference on instruments: how do you measure things, how do you analyze them, how do you store information?

Swent: Was this--

Wiegel: It was accepted.

Swent: Because it was your interest?

Wiegel: Yes, I was interested. I was designing instruments and working with people who designed them and so forth. So it was accepted, and I organized it. It was held at the university, at the International House, I think. I remember Tommy (M.J.) Tucker and also Carruthers from the U.K. came over; the French, the Dutch. There weren't a great number of us in those days, but they included John Isaacs, Bill Bascom, Frank Snodgrass, and Don Pritchard. When I was going through this and looking at some of the authors--there's one of them listed who I would like to mention--Glenn Schurman. He gave a paper on storm wave force meter. The reason I mention Glenn Schurman is--I think he did his Ph.D. at Caltech, but he was in the research group at La Habra, but then he became interested in the operational group; it was called Standard Oil Company of California then. Eventually he became head of all--what became known as Chevron later on--all of Chevron's operations in the North Sea and in the U.K. He became the managing director of Chevron U.K. and then was brought back to be corporate vice president of engineering and then corporate vice president for production worldwide at Chevron, and on the board of directors. Glenn gave a paper at this conference. I had forgotten all about that until I picked this book up.

Anyhow, it's not just a publication; it's thinking of what's needed and then getting a group of people to get together in giving this. We did it, and this publication which the Engineering Foundation underwrote the cost of, was used worldwide because it was the state of the art. One of the reasons I guess that they said to go ahead and do it is that the fifth conference on coastal engineering which was in Grenoble--because Professor Johnson was on sabbatical, I was asked to really take care of it. I did it, and I was over there several days ahead of time because I was in Europe for the navy for other reasons in Britain and France and the Netherlands, and in Rome for the IUGG congress [International Union of Geodesy and Geophysics].

I can still remember the French in Grenoble asking me to please phone the U.S. ambassador to France in Paris and ask him to come down and give the opening of this conference. How old was I then? 1954. So I was thirty-two years old. I wasn't about to make a phone call to the embassy--

Swent: Really?

Wiegel: --in France. So I did contact someone back home in the U.S.; in those days, trying to make a telephone call was almost impossible. It's hard to believe now how difficult it was to make a telephone call trans-Atlantic from a place that was not a capital. Thorndike Saville, Sr.--you notice the History of Coastal Engineering of the U.S. was by Thorndike Saville, Jr., and myself. We were graduate students together, but his father was dean of engineering at NYU, and it happened that at that time, he was president of the Engineers Joint Council.

So I mentioned this to the French and that was great because he was going to be there. So they knew they had, and they introduced him as, the premier engineer in the United States. I can still remember getting off the hook; you can imagine me getting through to the ambassador, but this worked fine. That's the sort of thing, you see, with publications. You have got to do this, and you have got to think of these things. Nowadays they call it networking, don't they? We have always networked; only with letters and telephones and things of that sort. Incidentally, a telephone call to a leader in a particular field is in many ways superior to computer networking--ask this authority what are the best two or three publications on the subject of interest. Another way that we get things published is to have a little specialty conference or workshop. We actually worked in these, with the discussions included in the proceedings.

I was editor of Shore & Beach for a little over eight years, up until last year. When Hurricane Hugo--

Swent: Who publishes Shore & Beach?

Wiegel: Shore & Beach is the journal of the American Shore and Beach Preservation Association. When I became editor, I turned it into a reviewed journal. It's also archival. "Review" is pretty clear-cut what is meant; "archival" is not so clear-cut, but as far as I can see, it means it's available in libraries on a permanent basis. But that's important. To me, archival means it's kind of generally available for people to check out of a library. I don't mean that you can find it in your Kensington Library (a neighborhood public library) or something, but I mean certainly the San Francisco library should have it, or the University of Pennsylvania, or Cambridge University.

So it was archival, but I turned it into a refereed journal. I also thought after Hurricane Hugo hit and did so much damage in South Carolina, that it would be well to have an issue that was focused on that. So the following year, I got some people's interest in this--Bob Dean at the University of Florida, Margaret Davidson who was the--I think she headed up the Office of Sea Grant in South Carolina, and Billy Edge who was a consulting engineer in Charleston at that time--is now professor of coastal engineering at Texas A&M--they organized a workshop in Folly Beach, near Charleston, on Hurricane Hugo and we put out a special issue. It was much larger than a normal issue because the local Sea Grant agreed to underwrite the additional cost.

So you could get a publication out of a workshop where you have something that really should be studied. What happened? What did it do? What were people doing to recover?

Swent: You had the workshop first and the publication followed that?

Wiegel: The publication followed it. Things like that are very successful. I'm not going to talk about regular publications where you just send them to the American Geophysical Union, or American Society of Civil Engineering. But another type of publication that's very important are from the National Research Council.

Swent: What is the National Research Council? Whose baby is that?

Wiegel: It's a baby of Abraham Lincoln, I guess.

Swent: I guess so, isn't it?

Wiegel: The National Academy of Sciences was established under some sort of an act of Congress; I don't remember the background to it. I think it was when Abraham Lincoln was president.

Swent: I think you're right.

Wiegel: I think so.

Swent: I'm just trying to get a little clearer what your relationship with them would be.

Wiegel: At some stage, the National Research Council was established which was really, you might say, the operational arm of the National Academy of Sciences. Then many years later, the something of health--I forget what it's called--but the medical people--

Swent: National Institutes of Health?

Wiegel: I forget what it's called. But then the National Academy of Engineering was born. So there's the National Academy of Science, the National Academy of Engineering, and I think it's probably the Institute of Health. These are honorary things. But the operational--

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Wiegel: I mentioned the Institutes of Health--it wasn't. While you were changing the tape I checked; it's called the Institute of Medicine. Those are the three honorary organizations. But the National Research Council is the operating end; it does research evaluations/state of the art assessments very often. One or more government agencies will ask for a study or Congress will ask for a study, and unfortunately many of these have become very politically sensitive, as you well know.

Swent: Yes.

Wiegel: Especially at the medical end. One of the subjects that was studied of direct importance to the field I've been in--it was done in the late 1980s, or early 1990--and it was called <u>Managing Coastal Erosion</u>. This was done because FEMA--Federal Emergency Management Administration--which is the one that you read about--

Swent: We hear a lot about that.

Wiegel: --concerned with floods and hurricanes and earthquakes. It was established, of course, originally because of how to respond in case the U.S. was hit by nuclear action, and you had to have some way to set up some form of operations outside the military immediately to repair things or get things operational again. But it's much broader than that. Thank goodness, it mostly considers other things.

So Managing Coastal Erosion was one, and then a more recent one was published about a year ago: it's called Beach Nourishment and Protection. These are working groups that are established and funded mostly by government agencies and they try to get them funded by several government agencies. So you build up a balance of stresses there.

Swent: Did this publication come out of a workshop?

Wiegel: It's not a workshop; I'll mention what it is.

Swent: All right. This is a full hardcover book.

Wiegel: Oh, yes. As somebody said, "By National Academy standards, it's a best seller."

Swent: Is it?

Wiegel: Yes. They had to reprint it within six months of it being published; they were sold out because of worldwide demand.

Swent: It was published by the National Academy Press in Washington, D.C. in 1995; sponsored by the Committee on Beach Nourishment and Protection, Marine Board Commission on Engineering, and Technical Systems of the National Research Council in 1995.

Wiegel: It barely made it. It was really December of 1995.

Swent: Really? So this was the outgrowth of the--you tell what it was.

Wiegel: In a way. I've been on a number of these National Research Council committees. When I was younger, I chaired several of them; one of them being on the safety of off-shore oil structures, ("Verification Guidelines for Offshore Structures") for example. That was a politically hot issue.

Swent: I'm sure it was. We'll talk about that in a minute.

Wiegel: The one on beach nourishment was over about two and a half years, and we met--the members--Orrin Pilkey was one of them--Bob Dean. The way the members are chosen, we--when I say "we," that's the editorial "we"--the National Academy realized that you had to have people that were biased because almost everybody that was a technical expert of something is going to be biased some way or another, and you wanted technical experts. So what you wanted to do was: at the start, what are the biases, and then how can we have a balance of biases? That's the point.

Swent: Representation of all the biases.

Wiegel: We represented the biases because otherwise you're not going to get experts. You'll get people that don't know anything about the subject, and that isn't what the academies are about. So we'd have in-depth meetings. We'd first just--this is the way it's operated--you get together and you discuss what you might do--before, this is done within the academy, it worked out things that should be done--you'd done a lot of homework before and this is back and forth and so forth. You got kind of a statement of what your overall mission was. When you first meet, you now say, "What do we need to address this mission? What kind of technical things do we do?"

Different people will be assigned tasks--at the next meeting will you please report on this, and you report on this, and you report on this and you get together again. Then, in this case of beach nourishment, it's quite evident we should go out and see some of things because the scope had been expanded. I should say I take a lot of credit for this because I had written to the chairman of the Marine Board of the National Research Council--which I had served on years ago--stating that I thought they needed a technical assessment of beach nourishment. I had read so many articles in the papers like the New York Times, and I know some of these people: Corey Dean, of the New York Times, who is one of its science editors. I said there's so much misinformation, what's needed is really a technical assessment.

Sponsoring Technical Assessment to Correct Public Misinformation/ Perceptions

Swent: I think you mentioned in this history article the problem with monitoring; that they love to get things going but then they don't pay attention to them after they're started.

Wiegel: Monitoring--

Swent: That's different.

Wiegel: Here I'd like to make a very serious statement, because I've done a lot of thinking about this, and I've had a lot of experience.

This is my opinion: politicians do not want to know about something once they've made a decision, because the findings might well show they made the wrong decision. This an opinion I've formed. Once they've decided we're going to spend this money on this--I don't care what the subject is--I don't think they want to find out if it didn't work. This is one of the reasons why it's difficult to get money to monitor things. We can do the technical monitoring; we can get some money. But it costs a lot to maintain detailed files of this and that. If you're talking about what was the economic impact, what was the social impact--these are the things they don't really want to find out about. That's my personal opinion, and I think I'm right.

About four or five years ago, I was at a meeting at the National Academy of Engineering that was held at the Beckman Center in Irvine. That's not UC Irvine, this is apart--Arnold Beckman of Beckman Instruments gave a lot of money to the National Academies of Science and Engineering because he said, "Everything's in Washington, D.C. You have got to get away from Washington, D.C." So he said, "I'll put two, three, four million --whatever it is--to build a new center for meetings." The Irvine Corporation said, "We'll give you the property free and it will be immediately adjacent to UC Irvine, which seems to be a nice reasonable place to have it; next to a university and of course there's an airport nearby."

There was a meeting there, and it was chaired by the president of the academy of engineers, Bob White, and it was on the environment. Who was the Canadian who was deputy secretary for the U.N. for environment?

Swent: I don't know.

Wiegel: I think his name is Maurice Strong. This was the top person. He was head of the Stockholm meeting and head of the Rio [de Janeiro] meeting, and then the top people from some different companies--Japan was there and so forth. This was an academy event--other people were invited, but largely they were members. The president of Caltech, Tom Everhart, was there; I knew him well and we were sitting together. Anyhow, I got up and I said, "I'd like to ask a question. All these environmental issues--because my experience in my own field is that we never seem to be able to get money and do a good study and monitor and find out what has been the real effect of something. Is this general or is that just specific in my area?"

The response was--and this was a considered response--this is a general problem. They don't get the money to really do adequate monitoring. This is one of the reasons I feel the way I do. So that's the digression. You asked about monitoring.

Swent: You were talking about assessment, and they're certainly closely related.

Wiegel: I had written to the chairman, and he asked me to make a presentation at the next meeting of the Marine Board, which I did. That meeting was in Monterey. It was accepted and modified, but it ended up that it was more than just technical. When I say "technical": engineering and science is what I meant. They wanted legal considerations, they wanted economic considerations, and then political management considerations. So on the committee that was established, there were several people who were very knowledgeable in their own field but didn't know anything at all about the coast; or almost nothing. So we decided we needed several field trips. They were interesting people, and I learned a lot from them--we all did, from each other.

We had to get out; we had to see some of these things. We had to see areas that were degraded; we had to see areas where nourishment had been done; we had to talk to people locally. One site, for example, Ocean Beach, Maryland, which is shown on television every time there's a northeaster there. In Florida and southern California. We had to get additional funding because this all costs money to go out on field trips for large numbers of people. These things are hammered back and forth. Orrin Pilkey was on the committee, and I spent quite a bit of time with Orrin. I said I want to be sure that the worries that you have are all addressed. He said, "Yes," and he came up with a number of things, and this is a unanimous report.

And yet in that book of his that you just mentioned, he says, "Of course, you all know that when you're on the committee you sometimes go along with them and you don't really mean it." This is about the only personality I will get into, but to me it's a strange attitude for a scientist. If there's a scientific thing, you hammer it out, and you don't agree if you don't agree. You say, "I disagree. This is why I disagree." This I think is an example. These are the ways publications get out too, and these are very useful publications. And will be used worldwide; we know that, from the number of sales that have been made all over the world of that publication.

Swent: Was there a publication on Managing Coastal Erosion?

Wiegel: It was another hardcover.

Swent: There we are, yes, okay. You had mentioned that you had that.

Wiegel: That's a--

Swent: That was National Research Council.

Wiegel: Yes. It was a little earlier.

Swent: 1990, Committee on Coastal Erosion Zone Management, Water Science and Technology Board, Marine Board, Commission on Engineering and Technical Systems, and the National Research Council were all the groups that sponsored that. It was put out by the National Academy Press, Washington, D.C., 1990. This, again, was the result of a conference, or series of conferences?

Wiegel: Series.

Swent: I see.

Wiegel: This is kind of sad, because the chairman of this was Bill Wood, a professor at Purdue, and he died of a heart attack relatively young just last month. I received a phone call from friends of mine back there telling me about it. He was an up-and-coming young person. Interesting, too, because on the side at Purdue University--not only a very good scientist, but he started and was coach of the women's water soccer team. He had been a nine-time All-American swimmer at Michigan State University. I found this quite interesting. This was about as broad a stretch from--but he was always a community active type of person.

Swent: Was there a publication as the result of--there's a first international conference on waste disposal in 1959. That was an important one, was it?

Wiegel: That was very important. I gave one of the papers on that. That was--

Swent: "Mixing of Buoyant Flows?"

Wiegel: No; this one was on measuring ocean currents. Erman Pearson was a professor at Berkeley. He was a professor of sanitary engineering here at Cal. He went to the University of Washington as an undergraduate. He was a football player--tackle, I believe, and he was all-conference tackle. He was a big man, I'll tell you. He was very, very bright, and he was one of the real leaders in water quality. Berkeley has been lucky. McGauhey was the one that hired him, and McGauhey was chairman of civil engineering when I came on the faculty here.

McGauhey was chairman of civil at that time. Pearson was doing quite a bit of work on water quality in the ocean;

estuaries. When I say the ocean, not deep sea, but immediately adjacent to the coast. He had all these international contacts too, so that was an international meeting; it was held at Berkeley. For years it was about the only book on the subject.

Swent: What was the book?

Wiegel: It was probably just called the proceedings of the conference. [Waste Disposal in the Marine Environment. Edited by E.A. Pearson, Pergamon Press, 1960, 369pp.]

Swent: The proceedings of the conference. Was this when you first got interested in this mixing?

Wiegel: No, I was invited to give it because Pearson knew I was interested in mixing.

Swent: --because you already were.

Wiegel: It was the other way. That's another digression. It was at that meeting that one of the papers given by a British specialist--I forget and I can't pronounce the word, but it's where you look at how epidemics occur.

Swent: Epidemiology?

Wiegel: Epidemiology, and the study of these things, which is very, very tricky. He was from Britain, the U.K., and they were tracking down--it was something like a fever, and I forget--it wasn't scarlet fever, but it was a very serious one. They all knew it was because it was--say at Brighton; I forget where, but one of the big seaside resorts--and they all knew that it was from the sewage being dumped into the ocean and people swimming in it until they started to really try to track it down. It turned out, no, it wasn't. Typhoid. I remember, Typhoid Mary was what they said. This turned out to be a Typhoid Mary--a person who was selling ice cream cones at the beach.

Swent: Oh, my.

Wiegel: This shows you one of the difficulties that those people have trying to sort things out. Everybody "knew" what it was, only it turned out not to be that. Then they got to finding out--there were other big studies done, one in Rio de Janeiro--because it had a big sewer outfall, big population. I don't want to downgrade the disposal of sewage in the ocean, because there's other types of things--toxics--and they get into fish and so forth; but getting a disease directly by swallowing salt water with bacteria or something in it, is practically zero.

Two reasons: one, the type of enteric diseases, salt water kills them, they don't live very long in salt water, that type of bacteria--which is very fortunate; the second is, people don't swallow salt water very often. A kid swimming in a lake or a river will ingest fresh water. That's a real serious problem. But you only take about one swallow of salt water in your lifetime if you can help it. So it's not that big a problem.

Politically, of course, all politicians like it; that's another thing I learned. I hate to say this, but they don't seem to be interested in what you find as scientists or engineers. All they want to do is--the next election say, "Look, I appropriated X dollars for it to be studied." Maybe it's just me that feels that way, but I have kind of a bad opinion of their real interest. Their real interest is getting re-elected.

Swent: Getting elected, yes.

Wiegel: I think I mentioned to you--once, on a commission I was on--Senator Ralph C. Dills, I believe it was, telling me--because I was worried about getting things fixed up, and he said, "Bob, a person never gets reelected by dedicating a filled pot hole. A new bridge, a new this or a new that, but not a filled pot hole." So this problem of maintenance is always difficult, and engineers are faced with maintenance all the time. Right now the classic one is either to replace or repair the Oakland section of the Bay Bridge. But, this will get a lot of attention.

Swent: They seem to be making the decision on the basis of interviews with the man in the street.

Wiegel: Well, I still have to go back to Sir Winston Churchill: "Democracy is a terrible system, except compared with all others." Interview the man in the street; many of them. That's the important thing--is to get a big sample.

Swent: If you get enough of them, maybe you'll--

Wiegel: It's like our committees on the National Research Council; you want a variety of conflicts. If you get a big enough sample, you're going to get a variety of opinions.

Swent: Then you'll get some good ideas.

Some International Conferences on Coastal Engineering/Councils

Wiegel: I think that's a sample there of--I should mention the names of the people who worked on the fifth conference of coastal engineering because it's nice to see the people that who did the work get credit.

Swent: Oh, yes.

Wiegel: Pierre Danel, who was the French engineer and the head of the research laboratory for the NEYRPIC--N-E-Y-R-P-I-C--which was the biggest turbine manufacturer in Europe. He was a genius; he really was. He made all of the engineers and scientists read, read, read. I forget whether they had to read an article a week and sign that they had read it--but also, in his information retrieval system, he asked each person what other title, or what other subjects should it be catalogued under. They had to write it in, and then he'd have them put a hand-written card in the file system; so the more it was used, the easier it was to retrieve it from different ways.

Some person reading the article might say, "This footnote in here is very important," or, "The fact that they used this type of instrument is very important and one subject should be the name of an instrument that was used." So every time anybody used it, you increased the ability to get back to it from some other way of looking at what the subject was. I don't know anyone else that does that.

Swent: Good idea.

Wiegel: The other was Professor Kravchenko, who was a professor of mathematics at the University of Grenoble. We've had close contacts--well, Pierre Danel died many years ago--but even today, Professor Weyhausen here had very close contacts with Professor Kravchenko, until Kravchenko died recently; that's lasted now since--forty years. So these things are valuable.

I would like to mention one other thing, because I think it's really important: as I said, I've always been interested in international things, and when I was chairman of the Coastal Engineering Research Council, I had a lot to do with having the international conference in coastal engineering set up in Spain, and then a few years later one in Italy, in Venice.

Swent: Let's get the dates on those.

Wiegel: The one in Venice would have been 1992. The one in Spain was probably 1988.

Swent: It was. That was the twenty-first in Malaga, Costa del Sol.

Wiegel: A couple of years later, Doctor Palao, who was the head of the local organizing committee, and was also director general for all ports and coastal development in Spain--he was on a high level. He was an engineer, but a high level official. He said that holding that meeting in Spain was extremely important, because for the first time, the high level government--i.e., his minister of public works--said, "I didn't realize that the Spanish engineers were this good internationally." So he said it was very important.

Something somewhat similar was told me about two years later after the one in Venice, by Professor Ugo Tomasicchio, that this had done a tremendous good for Italy--the Italian engineers and scientists--to have the international conference there. So these things can be very valuable.

Swent: You had them in wonderful places.

Wiegel: Yes.

Swent: Cape Town...

Wiegel: Cape Town.

Swent: Taipei.

Wiegel: The last one was in Orlando, but the one before was in Kobe--

Swent: Kobe.

Wiegel: --and we were there in early November 1994, and in January two months later was when the earthquake hit, and the hotel we were all staying at--and that's interesting coastal engineering--I'd been out in Japan thirty-five years ago as a member of the U.S./Japanese science exchange program. I was out twice, two years in a row. There were only five people from each country: one was on coastal; the next was on tsunamis. For the one on coastal, we looked at all the various ports that were being developed, and one of them was Kobe. They were bringing all this soil and rock down from the mountains there, and filling a part of the bay.

This is a huge bay; it's much bigger than San Francisco Bay. It's not a closed bay; it's more of an open bay. So you have the mountains coming down--and Japan doesn't have much room; they have got all kinds of mountains, but they don't have much usable space. So the whole port area was built on fill, and it was obviously unconsolidated fill. The same thing happened here in the San Francisco area during the Loma Prieta earthquake, only it was much worse in Kobe.

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Wiegel: In 1989--that, because of liquefaction, there was a couple of reasons, one of which was part of the port of Oakland liquefied and settled. That's what caused such great damage to the port of Kobe. The hotel we were at and the convention center was on a man-made island connected by a causeway and bridge, just off-shore. That all settled--I didn't go out after, but friends of mine who went out after said the hotel was there; it was on a whole series of very deep piles, and all the land around it had settled about a meter--i.e., about three feet. So here was the hotel, and you could see the piles underneath, reinforced--concrete piles, of course.

Swent: So the hotel survived all right?

Wiegel: The hotel survived.

Swent: That's a tribute to the engineers, isn't it?

Wiegel: But it also was another example that you have to do a lot to the fill on these things. The amount of engineering has got to be great.

There's another thing that should fit in there, and that's commissions, associations, council, and boards. This is opposed to committees as such. The most long-lasting for me--and this ties in with some of the other questions that you asked earlier, and I'll get to it now--was what was originally called the Council on Wave Research formed about 1950 under the Engineering Foundation.

Swent: Everything came out of that Long Beach conference.

Wiegel: That came out of the Long Beach conference; O'Brien and Johnson thought of that. They organized it, and each paper was by invitation; each was by invitation. After that, one of the people who was a senior engineer in New York and a professor at Columbia University (Boris A. Bakhmeteff) said, "You really need to have a council to study waves and what they do." It was established at that time, and Dean O'Brien was chairman of it, Joe Johnson was secretary, and they asked me to be the executive engineer.

Later on--because, you see, the Engineering Foundation will only have a council for five years or ten years, but then it's either disbanded or will have to find a home someplace else--and the American Society of Civil Engineers said, "We think this has got to stay in existence, but we think it should be broader." It was transformed into the Coastal Engineering Research Council; and at that time, O'Brien was chairman of it, Joe Johnson was secretary, and I was vice chairman. I was active in it, and I think it was 1978; it was in Hamburg, Germany that they asked me to become chairman of it, and I was chairman of it until 1992 when I knew that I didn't have the energy I had before. I didn't know what was wrong with me then, but I knew I didn't have the energy. So I chaired it from 1978 to 1992.

Swent: A long time, fourteen years.

Wiegel: It's quite a long time.

Swent: You retired at the meeting in Venice?

Wiegel: That's when I turned it over--

Swent: That was the last one that you chaired?

Wiegel: Yes, and Bob Dean became the new chair of it.

Swent: The meeting in Hamburg was when you--

Wiegel: That's when I--

Swent: --began.

Wiegel: It's still going. It has grown. You saw--the proceedings of the first conference of coastal engineering would have been the same size as this one on coastal engineering instruments--say three hundred pages or so. The last one which was just held will be four volumes, but the last one published until now is Coastal Engineering, 1994--this is the one that was held in Kobe, Japan. It's in three thick volumes; it's over 3,500 pages of proceedings.

Swent: Oh, my.

Wiegel: This is what has happened: it is expanded, it is worldwide. One of the reasons is that there's an awful lot of money in the world. There's a lot of poor distribution of wealth, but there's a tremendous amount of wealth. There's just all kinds of people that have enough money and enough leisure time that they can go places. Beaches are one of the places they like to go; certainly if they have children they like to go to the beach. Kids love to build sand castles.

I have to tell you a story: I was in Egypt, and I was invited with a couple of other people for the United Nations Development Agency. It's about the Nile Delta and some of the problems they're having there. So we were there meeting, and we met with the governor of Suez, the governor of Alexandria--the four governors along the Mediterranean coast at one time or another. I was in this new area which--just as in Spain, just as in the south coast of Turkey, just as in Miami Beach--to the west of Alexandria--which was all desert and that leads into Libya--condominiums going up; fine beaches.

So we're down looking at them, and I see these three Egyptian kids building a sand stepped pyramid. I took a photograph of them because kids the world over--. You get this nice, wet sand, and you build something out of it; and of course, being Egyptians, I guess, they're building a pyramid, not a sand castle. [laughter] I thought that was wonderful.

Swent: [laughter] It is.

Wiegel: But the thing is that people like to be on the coast. There's some very apparently deep psychological reason for it. People are looking into this. It's a worldwide thing. A few years ago, I was over in Thailand. I flew into Bangkok--I'd been there several times before, but I wanted to go see these new developments on Phuket Island, which is on the southwest peninsula side of Thailand. It's on the Andaman Sea. They have some of these new hotel resorts which are the same type as the luxury ones out on the Hawaiian Islands--ones that cost three, four hundred million dollars. A lot of Italian tourists go there, and big charter flights come in from Hong Kong loaded with tourists; another one from Tokyo loaded with tourists.

People like to go to the coast; they like beaches. From an economic standpoint, this is very important because you can only sell so much manufactured goods. How many pair of shoes can you have? How many jackets can you have? There are some distribution problems; we all know that. But in the developed countries, there's other ways you have got to develop jobs. People have to use what surplus money they have for other things. Tourism is one of the great ones. I think you just went, or are just about to go on a trip some place, I think you said. I know my wife and I are. We just got back from Hawaii, and we'll be going to the UK. People do this. Beaches are one of the great places people like to go.

I did a study of Miami Beach--on the beach nourishment there--and it was published a couple of years ago. One of the bits of information I got was the number of beach uses--i.e., numbers of people that went to the beach for a day in Miami Beach since it had been renourished compared with before; it was substantial. The increase in tourism income--the number of tourists who go to Florida and spend some of their time on the beach--a lot of them go to Disney World--but a lot of them go to Disney World and then to a beach. Or in winter, many, many of them simply go to Florida from Europe, simply for the weather, and stay on the beach.

Europe--first it was Spain, and then it was Italy--and I know much of both of the coasts--and then Turkey--I've been there a couple of times, because the south coast of Turkey is developing. It's not just tourists from Germany, but it's from Europe in general going to the south coast of Turkey. The Arabs--money--uses a lot of this area in Egypt that I have just mentioned to you--west of Alexandria. So all kinds of money from the Saudis and--not Lebanon, because Lebanon doesn't have much surplus, and they have a marvelous climate in Lebanon anyway. So these are developing.

Mombasa: this is down on the east central coast of Africa, in Kenya--big new resort areas on the beaches in Mombasa. Charter flights from Frankfurt; charter flights from London and so forth. So it's a worldwide development. South America of course--Argentina and that area--they make use of their beaches very extensively. Rio de Janeiro--I think everybody's aware of the fact like the Brazilians like their beaches. Australia, Japan, Mexico. Maybe this answers it in part; the other part is sailing, marinas, and surfing. Now, this gets you into conflict.

Sitting on the beach--little kids want to go in the surf or build sand castles, older ones want to surfboard, other people want to fish. You build cities; they have got waste disposal. If people want to drive to the beach, where are they going to park? All of these things build conflicts. So more and more, what we as engineers are working with, are the political people--as to how do you try to resolve these conflicts. We put in our technical portion. That's what we're good at. Resolving the political things--I'm not sure they can always be resolved.

Swent: Maybe not.

Wiegel: Orville T. Magoon--you've met Orville--and Billy Edge have organized a series of conferences that have been very helpful though. Coastal zone management, and the ASCE published the proceedings. Engineers are involved in this worldwide now, and our students find it interesting and challenging.

I remember when I was acting dean, one of the bits of advice I got from Harmer Davis was the two classes of problem--I think I mentioned this: some you could solve, others you simple cope with. I'm afraid some of these are the type you cope with. When you have great numbers of people and they all want to go to about the same place--whether it be Yosemite, or whether it be Santa Monica Bay--you're a democracy, you say "yes," but then you say, "Gosh, you're damaging what you want to go see." This is what we're trying to work out today, isn't it? Lots of people are working on that and it's worldwide; it's a worldwide problem.

I think I know about every mile of the Normandy, Brittany, Atlantic, and Mediterranean coast of France because I've been there a number of times--the French love to sail, and they've got new little towns, little developing places that never were developed before--with marinas. This is what they like to do. This is one way of trying to absorb--one of the problems in France, as you know, is the big unemployment--same in Germany. You have got to develop new types of jobs, and these aren't in manufacturing and so forth; it's in these various service types of things.

So the Coastal Engineer Research Council--we serve a very good purpose. We had no intention of being an international council; and it is a council of the American Society of Civil Engineers. We appointed ASCE members in other countries to the Council--from Japan, The Netherlands, Australia. It's just there was nothing else around, so everybody started to come to our meetings, and it was successful. Well, if something's successful, you continue it. It's still going. As I say, the next one will be in Denmark next year.

Swent: You had one in Denmark long ago.

Wiegel: Yes. We're starting to repeat.

Swent: You're repeating now.

Wiegel: In the year 2000, it will be in Sydney, Australia, which will be a repeat, and in the year 2002, it will be in south Wales. It was kind of interesting, because the representative of the Institution of Civil Engineers (London) said--when he was making a presentation--"You've accepted the next--in the year 2000 in Sydney in New South Wales"--he said, "Let's have the year 2002 in Old South Wales." Of course, that wasn't why it was accepted;

they had made a very good presentation, and the Institution of Civil Engineers is going to host it.

Swent: The conference was in Denmark in 1974.

Wiegel: Yes, and in next year, 1998, it will be in Copenhagen again.

Swent: What about Cape Town? You haven't mentioned Cape Town. You were there in 1982.

Wiegel: I had all kinds of political problems with that one. It was going to be in Venezuela, and I got a phone call that they couldn't get it together because it required some money and this and that, and at the last minute they couldn't get it together. I was on my way to Australia for the conference there. I had to make the announcement that the next one, which had been accepted for Venezuela, couldn't be held. The representative from South Africa said, "We can host it." But then we had problems.

I had to contact state department and all kinds of things. This was all within a couple of days. So the South Africans got an agreement from their government that anyone who is accepted by an external review to give a paper would receive a visa--I mean, unless they were criminals--you know what I mean. That was one of the things we insisted on: anyone who had a paper accepted--and this was by an external review, no South Africans. That was agreed to, and we had it in writing.

That was when there was so much pressure with respect to South Africa. But our opinion was that we were engineers and scientists and we all work together; we work closely with people. It's not the engineers and scientists that cause these problems; these are university people. So we decided to go ahead, and I got in trouble with a number of people. But in hindsight, they said it was the appropriate thing to do.

Swent: Who? What kinds of people?

Wiegel: I don't want to get into individual names.

Swent: But what? Were they from other countries?

Wiegel: Yes. One of them in particular--I said, "Well, can you explain why your country--one of your biggest construction firms is doing a major job in Cape Town right now?" That kind of took care of that. That was money. Two different countries which were very pious, but when you looked into it, found they were working with South Africa because there was money in big construction contracts.

An Innovative Sand Breakwater at a Namibian Diamond Project

Swent: Did you have any trouble getting your American permit to go there?

Wiegel: Nobody had any trouble. It's a beautiful location. I was able, fortunately, to go up to Namibia to the diamond--where they were working along the coast.

Swent: Oh, yes, the west coast.

Wiegel: It's just north of the Orange River in Namibia. This is interesting because the Orange River is the river that feeds through the main diamond source in Africa. Sand comes down the river and other sediments. A very small part of the gravel is diamonds.

Swent: Diamonds.

Wiegel: For thousands of years the river has transported material to the coast, and then what they call littoral drift--that is the current along the coast right on the beach, driven by the waves--this moves sand and gravel--this is the so-called littoral transport of material, and in that sand and gravel are these diamonds. So what they've done: they've bulldozed the sand off the beach, because diamonds are a little more dense, so they're on the bottom. They didn't just start bulldozing sand off the beach; there was first exploration by mining engineers, geologists. They found where the diamonds were; they were in cracks--surface cracks in the bedrock. In other words, you took the sand off and got down to the bedrock, then you got gravel out of the cracks of that bedrock there, and a certain amount of that gravel is diamonds.

So they bulldozed the sand off the beach into the surf zone; about a kilometer long, and then a cofferdam to shore. But in order to maintain it out there--and this is a true invention--what engineer thought to do this, don't ask me--but they decided if they put a lot of horizontal wells--they call them points--from the inside of the beach side of the sand--and pump out the salt water that penetrated from the ocean, that this sand breakwater could be maintained. It worked. Who thought of that; why they tried it--I haven't any idea. It's lost, but a great idea.

The chief civil engineer of the whole diamond works there was--did his master's degree here at Berkeley in civil engineering right after the end of World War II, and that's how I happened to--

Swent: Do you remember his name?

Wiegel: I can't remember his name. But I remember vividly him telling me he had been shot by Germans during World War II and left for dead --but he survived.

Swent: What company was working there?

Wiegel: Anglo American Corporation, which is a wholly-owned subsidiary of Debeers. But Anglo American Corporation is the actual mining company for diamonds.

Swent: There were a lot of other people trying to get in there.

Wiegel: Yes, but this is the big operator. You don't just go in. You're flown in by charter plane, and after you land, an escort is with you the whole time. You just don't wander in this area, believe me. So then they moved all the sand--

Swent: So these were horizontal pipes were used to pump the water out?

Wiegel: Yes. They moved the sand off the beach out into the surf zone. But now: how do you maintain it there with all the wave action? Somebody thought about putting in horizontal wells and pumping the salt water out, and somehow this would make it better, and it does. Other people are now trying this concept out in different parts of the world to try to maintain regular beaches by doing the same thing. Back to Namibia--then they get down to the bedrock, and it's down there with men with whisk brooms. We said, "Why don't you try this or that?"

"We've tried everything anybody has ever thought about. Every kind of mechanical equipment, every kind of separator, every kind of everything. There is nothing like a person with a whisk broom and a pair of eyes to be able to segregate the 'most likely' from the 'obviously not.'"

This is taken in and it's washed, and put into a centrifugal separator, and you can sort it out by densities. Then finally, when you get the choice of the choice, it goes out on a black velvet conveyor belt, and you can see clearly which are the diamonds, and they're then picked out. Then they wash them in kind of a weak acid, because if I looked at it, I would think it's just another pebble. Then they wash them; then you can see the color. But they come in all colors. My favorite were the yellow ones. Of course, they don't say, "Here, have a sample." This is all behind glass with a person right next to you. But it was really interesting, and that was a very good opportunity. We were there to see the beach operation primarily.

I'd been to South Africa before, at other places. They have very good coastal people down there. They have had some work published by the American Shore and Beach Preservation Association, and I was a director and vice president of it for a decade, and I was editor of Shore & Beach. I was asked to be president of it, but that's when I didn't have the energy, and I said no. I would serve as chair of a search committee for a president, but I wouldn't be able to accept it myself. I'm still kind of active in it; I attend their conferences.

Swent: There are two things that I wanted to bring in at some point: one was your work in maintaining the high level of these papers--your insistence on jury reviews for the papers. I'm sure sometimes you were pressured to accept papers that you thought were not up to snuff.

Wiegel: I think what it is: I'm old enough; I've been around enough; they don't want to take a chance with me, so I was never pressured.

Swent: How did you get to that?

Wiegel: Oh, I sent them out for reviews. Every paper was sent to at least three people to review.

Swent: You were given credit for maintaining a very high level of papers in these things.

Wiegel: Maybe it's because I'm a professor, and I'm used to looking at student papers; I don't know. You're judging them all the time, aren't you, and you're saying, "This isn't up to snuff; now you do a better job." I look, and I know the kid had fallen asleep about three in the morning; I knew what happened; I'd done the same thing. I think being a professor is very useful from that standpoint because you're used to judging things. You review a paper for I think three reasons: one is that you are looking for something interesting--remember this journal is for a broad-based group of people, and some of them are scientists or engineers, but many of them are not. So I continued the policy of my predecessor, Joe Johnson: that was, I didn't want any equations in this journal. I wanted it stated in words or sketches, photographs--we used a lot of photographs, and we used a lot of sketches. I wouldn't rule it out completely, but basically, no equations. So the first thing in the choice of papers is, do you think this is of interest to a large number of people? That's the first thing.

The second is that: are there factual problems? Very often there are. There are things that are wrong, and with these expert reviewers you can catch those kind of things. The third is: have they just overlooked something? Because of course it may be obvious to the person or persons who wrote it, but it isn't clear to somebody else because they have another set of information in their mind compared with the people who are reading it. If you have three reviewers, you can pick up quite a bit of this. These are three good reasons to review, and it makes for a better journal. It's hard work, but I think very well worth while.

I came back from a trip--I had been up to Alaska in the north slope, got back--I knew something was wrong--went over to Thailand and Japan, and then went to the doctors, and found out what it was. Within about a month I was in the hospital for open-heart surgery so--

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Wiegel: I have got most of my energy back, but it's not complete.

Swent: I don't think you've mentioned your--and at some point, we must get it on the record--I can't remember whether you mentioned your surgery on the tape?

Wiegel: Maybe not. I've had two major--

Swent: You've had some major health setbacks.

Wiegel: However, except for that, I've been very healthy. I almost never get colds or illnesses of any sort. My daughters say that except for my terrible problems, I'm very healthy. But I had acoustic neuroma, this took over an eight-hour operation in the inner ear. But then a little over two years ago, I had open-heart surgery because they had to replace the aortic valve with--they don't use plastic; they tried plastic, but those don't last--with either a pig valve or a calf valve that they insert. Then, as they say, once they got you open, they do lots of other things. So they took the veins out of my leg and by-passed my arteries and so forth.

Swent: So this is only two years ago?

Wiegel: That was a little over two years ago for that; the other was about eight or nine years ago. I got most of my energy back, but still I'm getting to the age where you don't have as much energy as you used to have anyhow.

Swent: That's an awful lot.

Wiegel: But I'm not worried about flying any more, except that I won't go anyplace that I can't get back on a non-stop flight to San Francisco in case I have to.

Swent: Right.

Wiegel: But that did cause me to make some major changes in what I was doing, of course.

California Advisory Commission on Marine and Coastal Resources

Wiegel: You've alluded to public service a couple of times, and we'd get started a little bit and so forth, but I would like to spend a bit of time on that. The commissions--and these are, I guess, legal type of things--I served on two commissions in California: one was the California Governor's Advisory Commission on Ocean Resources. That was established by Pat Brown when he was governor. That was changed, and it became the California Advisory Commission on Marine and Coastal Resources. I was on the tail end of the first commission and then I served on the second commission throughout its entire existence, which is 1968 to 1974.

Swent: This was by the governor's--

Wiegel: Yes, it was Governor Reagan.

Swent: --appointment.

Wiegel: It was established by act of the state legislature, however. There--by law--on the commission would be three members of the state assembly, and three members of the state senate. I remember [Senator] Dill being one of the senators, from Los Angeles. The assemblyman that I remember was quite a young personable assemblyman from San Diego by name of Pete Wilson.

Swent: We've heard of him.

Wiegel: Who later on became mayor of San Diego, and then United States senator, and then governor. He always was interested in the coast; he was really interested in it. When he was on the commission, he wrote an act, but it wasn't accepted because he was too young. That's when I learned some more about politics in the state assembly. It would have had quite a bit in it which came in the California Coastal Commission Act, which was by proposition--initiative. But Pete Wilson had written something with much of that in it as part of that.

Our commission went out of existence when the California Coastal Commission came on. They were entirely different things: ours was advisory; the California Coastal Commission is regulatory. So there's all the difference in the world there.

Swent: Did you get to the point where you recommended regulations though?

Wiegel: We recommended a number of things. I was chairman of a couple of its committees. One was on power plant citings, for example. You need vast quantities of water to cool power plants, and we thought it's better to use the ocean if you can, rather than fresh water in California.

Swent: You did quite a little work down at Diablo Canyon.

Wiegel: That came later. I guess the reason I was asked to chair that committee is because I was doing work on mixing processes; that's my guess. So I was the one on the committee. Incidentally Jack Bonner was on it. Jack eventually became president of PG&E; he was a Cal graduate back in the early thirties. That was very interesting. The persons that chaired that council were really outstanding people.

The first was Bennie Schaefer, who was professor at Scripps at UC San Diego and he was a fisheries expert. He had been science advisor to Udall when Udall was secretary of the interior. Fisheries people have to be very, very broad. They need all the marine biology, marine chemistry--but they need to know economics and politics. Bennie was one of the ones working on setting up these international fisheries agreements to try to have what you call sustainable natural resources. Bennie was one of the people. He died some years ago, but boy, he was a broad-based person.

Following Bennie was Wib (W.M.) Chapman, also a fisheries expert. He had been dean of the Fisheries School, University of Washington. Then with the U.S. government as their representative to the International Tuna Fisheries Commission, and was director of Marine Resources of Ralston Purina Corporation while chairman. Wib, like Bennie Schaefer, died while serving as the chair.

Another one was Dave Potter, who was a physicist; underwater sound. He was the head of the General Motors defense lab in Santa Barbara at that time, and he eventually became undersecretary of the navy, and then General Motors brought him back as president of Delco, and then he headed up, was group vice president for General Motors for all heavy equipment. He's another extremely bright guy.

These people who chaired the council all had the ability to listen--to many inputs, all day long, and to sort out what were the most important items and then to summarize that. They all had this ability.

Robert "Bud" Krueger, who was in resources type of law and things of that sort, followed Dave Potter as chair. You see, we had two fisheries experts as chairs, then a physicist (underwater sound) as a chair, and then a lawyer as chair. It was good to be on these because you learn so much about these different things, and you remain friends for the rest of your life, too. That was a very, very interesting experience.

Swent: Is that when you got somehow involved with the Native Americans?

Wiegel: The only thing I can remember getting involved with the Native Americans was not with them directly.

Swent: "Indians" as we used to call them.

Wiegel: Yes, certainly. It was indirectly, because I was asked to be a consultant for the redevelopment agency of Crescent City after they had the tsunami, a "tidal wave". This was thirty years ago.

Swent: We really haven't mentioned tsunamis yet; we'll have to do that.

Wiegel: I think we have a lot to cover next time.

Swent: Right.

Wiegel: So anyhow, I was looking into all the stuff that we do: the technical literature in underwater earthquakes and what was happening. We did do a lot of work on the Mendocino escarpment; real good science. Out of that came an absolutely first-rate Ph.D. thesis. One of the things that I ran into--you may have got that from Orville Magoon, because both of us have been interested in tsunamis, as you know. Up in Crescent City, I wanted to find out: did the Indians have any tribal knowledge? That is, oral knowledge that's handed down, and most people like that are good observers. So I found out that there was a person doing a Ph.D. thesis in cultural anthropology at Cal.

So I got a hold of this person, and I said, "Did you find out anything in what you're doing?" One of the things that he told me about was that the Indians up in Crescent City had a belief that if you put a ring of abalone shells around an area, this would protect it from tsunamis, but you must put these around an area up on a bluff. [laughter] This is what I liked. So you had two things going for you: you had one which I could go along with very well, and that's building on a higher elevation. That's where the hospital is up at Crescent City. Maybe that's what he's referring to; I don't know.

Swent: I don't know.

Wiegel: I'd forgotten all about that. So we did a lot of interesting things; we had a lot of meetings. If you look on top of my file cabinet, you'll see two stacks of reports. Each one is an annual report or a report of a meeting. In those, we made many recommendations. A number of them did find their way into the California Coastal Commission and things of this sort. I can't tell you how much, but a number of them did.

Swent: Someone did listen.

Wiegel: Or read. You have to have patience. I'm not politically oriented; I'm just not. That's a different sort of person, that's all there is to it. They're very good in what they do, but I'm science/engineering oriented, and hopefully I'm good in what I do, but it's different.

Swent: It's very different; yes, it is.

Wiegel: Orville Magoon, you see--who you've met--he was one of the first to recognize that people have got to stop fighting with one another and work with one another. That's why he started that Coastal Zone Conference. He did it; other people helped, Billy L. Edge was a major collaborator. He's always been good at getting people to help and he always had had good people to help. The first meeting on the Coastal Zone was in 1978 or something like that in San Francisco.

Swent: That was when he was working for the Corps?

Wiegel: He was still with the Corps.

Swent: Coastal Zone Management I think it was called?

Wiegel: Yes. Because that was the point: people have got to talk to each other, and listen to each other. You don't resolve problems by being antagonistic; they're not the ones that resolve things. It's the other people who get together and say, "This is a good point from you; this is a good point from you. I understand why you want to go that way." Then you start resolving these things and Orville is very good at this. That meeting grew into another and another and another, and again, it became the worldwide meeting on coastal zone management. In one way, this is what he's doing in this one in March in San Diego, but it's a little different, because that's got a lot of science base in it.

I just got a copy of the program with who's giving what paper and there's a lot of scientific-based information going into it as well as the management and the legal stuff. Broad: fish, kelp, wetlands, beaches--you name it. Everything connected with it.

Office of Naval Research Natural Hazards Review Board

Wiegel: Let me mention one little thing first, because I just ran across something when I was going through my notes which is interesting. It was just in existence for one year. It was the Office of Naval Research Natural Hazards Review Board. This was 1973 to 1974, and earthquakes had just caught their attention pretty strongly. Bruce Bolt here at Berkeley was on it, I was on it, Harry Seed was on it. The chair of the committee--I can't remember his name right now, but he was at Caltech and a seismologist.

We wrote a report on earthquakes as far as naval bases were concerned. This included Subic Bay in the Philippines and so forth; overseas bases and bases in the U.S. I had forgotten all about this until I was trying to get some background information. Could I read you two sentences?

Swent: Please, yes, of course.

Wiegel: It's a letter from Admiral Tyrrell to the chief of naval research dated 18 April 1980. It said: "Sometime ago, the Office Of Naval Research undertook a study to help us determine the potential vulnerability of a naval shore establishment to earthquake damage. The results of the study were extremely useful and contributed to a decision to fully investigate the earthquake safety of activities located in areas of high seismic risk. The six-year program of investigation funded at one million dollars per year is under way. This will result in substantial operating of facilities and improve earthquake safety which will enhance the navy's ability to carry out its mission."

You do these things, and you're sometimes never sure whether any action is taken, but here was a case where action was taken. This triggers back what happened to the Port of Oakland, what happened to Kobe--effects of earthquakes on filled areas. This is what we were looking into and we made recommendations about that very subject. This was over twenty years ago.

Swent: And you only worked for one year?

Wiegel: Yes, off and on. You make your recommendations. The next step took money, because they had to do in-depth engineering studies.

Swent: But you recommended those studies?

Wiegel: We put in the recommendation, yes. I'd forgotten.

Swent: It was important.

Wiegel: That was important.

Swent: Yes, it was.

Wiegel: The Marine Board--which I referred to before, is part of the National Research Council--I was on it for two terms of three years each, the maximum you could serve--but I was on its predecessor, Ocean Engineering Committee of the National Academy of Engineering. Later, I also was an ex officio member because I was chairman of something or other. But I was on it for years. Back in the late 1960s, the state department was interested in getting cooperation on ocean engineering/science/resources/operations amongst the countries; not just coastal, I'm talking about oceans. This is marine transportation, fisheries, offshore oil and gas, with Japan and European countries.

Engineering Committee on Ocean Resources

Wiegel: Because of some of the things I had done--people knew me in Japan because I'd been on two of these things with the Japanese and the British and the Dutch knew me. So I was asked if I would look into the establishment of an international organization to be advisory to UNESCO on ocean resources, they called it. There was a scientific SCOR: the Scientific Committee on Ocean Research; which was a standing committee of UNESCO. The state department wanted something similar with engineering, but they talked about resources.

So I started to bounce back and forth to Europe. I'd give lectures. I remember once flying all night and then chairing a meeting at nine in the morning in Paris. I had energy in those days.

Swent: I guess so.

Wiegel: I couldn't do it today.

Swent: You weren't flying on the Concorde, I don't suppose?

Wiegel: No. We flew what they called "sardine class." I did a lot of this, and we eventually established ECOR--Engineering Committee on Ocean Resources. I guess because I had done much of the work in getting it together, they asked me to serve as the founding president; this was other people that requested this. So I served as that, and I said that I wouldn't serve another term. I didn't think it should be done; I thought a person should only serve for a three-year term. It's still in existence. I forget who's the secretariat; and I think it's in the U.K. right now. The secretariat was here in the U.S. for sometime, but I think it's now in the U.K.

Swent: This was also connected with UNESCO?

Wiegel: Yes. For a couple of the UNESCO meetings, I was the state department official--whatever they call it--at meetings.

Swent: Delegate maybe?

Wiegel: No, delegates are voting. Official observer I think they called it. So you could respond if they had questions. But it was interesting.

Swent: It must have been.

Wiegel: I had to sit in over there in Paris and so forth. I got to wondering: how many time have I been to Europe? I started to go through, but I can't count--but I have made certainly more than sixty trips. Of course, I've done all kinds of traveling to Asia and so forth.

Swent: But sixty to Europe alone.

Wiegel: It's been more.

Swent: Oh, my.

Wiegel: But for a while I think it was three or four a year. It was quite interesting to establish something, and to do it by consensus, and get it to go. We did give quite a bit of advice to UNESCO, but it was broader than that.

Swent: What sorts of things?

Wiegel: Offshore oil had a lot to do with it. The only reason why there hadn't been any real problem in the North Sea was that the economic boundaries between the U.K. and Norway and so forth were established before anybody had thought there was any oil or gas there; it was all done for fisheries. When gas and then oil was discovered, they had these international agreements already in existence, and that was very fortunate.

Swent: But there has been trouble about establishing fisheries boundaries.

Wiegel: Fisheries will always be a problem. It's a big problem today, worldwide. There are other problems. One of the big problems between China and Vietnam and the Philippines--the three--has to do with little, tiny islands out there in the South China Sea region. Nobody could care who's sitting on them, but because of the economic zones around them and this is an area that's undoubtedly got oil and gas, they all claim them. These problems exist today and they can trigger wars. I can get into that next time; I've done so much international work because of those kinds of things. Anyhow, it was interesting to be the one that really put it together and it's still operational.

Swent: So you were looking just at the resources of the ocean?

Wiegel: This was mostly it.

Swent: So that would be fisheries, fish, pollution control, and--

Wiegel: --oil.

Swent: --oil.

Wiegel: There's another organization I had to meet with, and it's the one that sets the standards for safety for ships.

Swent: What did you call it?

Wiegel: IMO--International Maritime Organization.

Swent: But you used a word just a moment ago that I didn't quite catch.

Wiegel: I think I said that other organization that I had to then deal with--not deal with, but learn from. That's a very formal international group, and the U.S. member on it is the commandant of the U.S. Coast Guard. That may have been what I said. But that was this other organization that I had to interact with. That got me into the safety of off-shore structures because the coast guard would be in charge with the safety of floating structures. There are a lot of floating structures. The Interior Department with the U.S. Geological Survey is responsible for the safety of fixed structures. That's interesting.

Swent: The Department of the Interior?

Wiegel: The Department of Interior. That came about very interestingly because the so-called Santa Barbara oil spill was not an oil spill at all in my terms. An oil spill is something that spills out of a ship or when a pipeline breaks. In drilling for oil out in the federal waters off Santa Barbara, they went through fracture zones, and apparently there was this one segment that was down maybe two thousand feet below the bottom--I don't know what the distance is, but something like that.

There wasn't a casing installed in a long segment, so oil, by the pressure of the gas, came up through this well, but part of it wasn't cased, and it moved out into a fracture zone, and then that got up into another fracture zone and that eventually got up into the water. So it wasn't a spill; it was a geological problem. So Congress said to the Geological Survey, "You've got to be responsible for it." So here the Geological Survey had no staff whatsoever that knew anything--even their own good geologists--they didn't know anything about the design of an offshore oil platform that was fixed. But they were told to do it. They had to build up technically qualified staff.

Eventually, I chaired the National Research Council committee on the safety for those things, or the recommendations for safety.

Swent: That would be safety not only of personnel--

Wiegel: Well, the Coast Guard was in charge of safety of personnel, but then it became much broader because the Coast Guard was given a lot of oil spill responsibilities--

Swent: --environmental responsibilities.

Wiegel: --environmental, and personal safety. With almost no additional funds. They were told, "Do all this," but not given the funds to build up the staffs. They had an impossible situation. They're good people, but you can't do very much if you don't have the money to build up your specialized staff.

Swent: That was a very explosive political issue too.

Wiegel: The next one, the last one I wanted to talk about was the Coastal Engineering Research Board in the army, but that's going to take some time.

Swent: Should we leave that? Okay.

Wiegel: I was on that for eleven years.

Swent: That was a big one.

Wiegel: That was a big one.

Swent: That's after they had moved to Vicksburg?

Wiegel: No, it was before. I was on it for two five-year terms, but I was on it for eleven years, because that's when they moved to Vicksburg. All the records were misplaced for a while, so I was on for another year. I think that was what it was: that simple. There's no way we could ever figure out how ten years became eleven years. It occurred right when the move was made from Washington, D.C., to Vicksburg. [laughter]

Swent: [laughter] Simple housekeeping mix-up.

Wiegel: I think they can count; I'm not worried about that. Maybe we should do that.

Swent: It's already twelve o'clock.

Wiegel: I'm kind of tired too.

More on the Use of TV in the College of Engineering at Cal

[Interview 6: March 12, 1997] ##

Swent: When we ended last time, we were planning to talk about the research board, but you had some other thoughts before that.

Wiegel: Once you're off, suddenly you think of something, and with respect to the use of TV in the College of Engineering at Cal, I don't remember whether I gave the name of the person who really did all of the work on the hardware--that is, the type of equipment for televising and videotaping and things of this sort. Berkeley had a television group which consisted of two people, but in engineering, we had our own person that worked with them, but he really did all of the planning for us. It was Dr. Jim Cumming.

Swent: I don't believe you have mentioned his name.

Wiegel: He did a great amount of work over several years, and when it became operational, for the first couple of years he was the engineer that really ran the thing. I thought that this was very important.

Swent: Yes, that's good to mention.

Wiegel: I should also mention that I used myself as a guinea pig in one of the early--

Swent: Did you?

Wiegel: --televised classes and gave a full graduate course in the early days. That way, you could get other people to do it if they knew that you had done it yourself. The other is about the Coastal Engineering Research Council [CERC]. Tremendous amount of work was done by Bob Dean, Billy Edge, Orville Magoon, Thorndike Saville, Jr., and Ron Noble. These are always collaborative efforts. These were the U.S. representatives on the Coastal Engineering Research Council. In addition, we had a representative from Japan, Kyoshi Horikawa, who was professor at the University of Tokyo, and became president of the Japan Society of Civil Engineers, and is currently president of a private university in Japan. We had a representative from Australia--Doug Foster. Leonoardo Zeevaert was from Mexico. One from the Netherlands was Professor Eco Bijker, for many years. Although it was an American society of civil engineers, it became international through no planning; it just occurred. When this happened, we made sure to appoint people from other countries. These were all members of the American Society of Civil Engineers.

Swent: Is there also an international organization of civil engineers?

Wiegel: No, there's no international organization.

Swent: Congress of something?

Wiegel: No. That was one of the reasons that I was asked many years ago to get something on ocean engineering--ocean resources. But it's not an organization like the American Society of Civil Engineers or something like that; it's a very small organization with representatives of each of the member countries. It becomes too unwieldy. We have the International Union of Geodesy and Geophysics; they no longer can hold a complete congress because it's too big. They have to break it up into geophysics and oceanography and seismology and other different portions of it. The world's too big now.

Swent: It has gotten too big, hasn't it?

Wiegel: Yes, it has gotten too big.

Swent: You do have, however, this international consortium now on ocean engineering.

Wiegel: It's a conference that's held every two years. We've tried to restrict its size. It's gotten bigger and bigger, and we've worked very hard to not allow it to get too big. But even so at the last conference, which was last year, there were five sessions simultaneously. So you spend your whole time wondering, "Which session is the one I should be in, out of a choice of five."

Swent: That's difficult.

Wiegel: I don't care for those. I know they have to exist, but I prefer smaller ones. The American Shore and Beach Preservation Association, for example--our annual meeting that we held last year was in Chicago and it was concentrated on the Great Lakes; the equivalent of oceanography of the Great Lakes, and the coast, and the geology of the local region and so forth. There, for the two and a half days of conference, you could hear every paper that was given and participate. I prefer that, but I know you can't do it except rarely nowadays unless you have what you call a workshop. I did mention one of the workshops we held: it was on Hurricane Hugo.

Swent: Yes.

Wiegel: If you want to take it with you and look--

Swent: Thank you.

Wiegel: --just to have an idea about it.

Swent: This is the <u>Shore & Beach</u> issue from October, 1990. Thank you.

Wiegel: That gives an idea of what can be done in the workshop. Also, at the last session, you asked about my activities in tsunamis.

Swent: Yes.

Wiegel: I've given a little bit, and it will fit in appropriately in a few minutes from now--

Swent: All right. Just didn't want to skip it.

Wiegel: --when I can discuss it. Then you also asked me about the definition of hindcast--

Swent: Yes.

Wiegel: --just as you were leaving.

Hindcasting Wave Conditions

Swent: I keep running into forecasting and hindcasting; and forecasting I understand, I think, but hindcasting I'm not sure about.

Wiegel: That's a very good question. It's like in any trade or profession you have your own jargon. When I speak of forecasting or hindcasting, I'm talking about waves: water waves, ocean waves, and surf, the waves breaking on beaches. This came about from the World War II amphibious operations, having to forecast what would be the surf conditions as a site for the military to plan on. A great amount of research was done on relating the winds blowing over the ocean to the waves that they generate, or even more fundamentally, the atmospheric pressure distributions over the ocean; from those forecasting what would be the winds generated by them. I shouldn't say forecast--relating what winds would be generated by those pressure distributions.

Then after calculating the winds through numerical models (we call them now), to calculate what would be the waves that would be generated by these in the open ocean. Then with the information and the physics, we were developing on what happened to waves as they moved onto beaches; what would be the surf conditions. Then through other work: how would these surf conditions affect the operations of the landing craft and the personnel. You probably know if you have ever seen anything like the movie, "The Longest Day," great losses of life occurred; not just enemy gunfire and so forth, but people drowning. There was great loss of life.

So, you're trying to forecast the weather in advance, and then from the forecast of the weather calculate what would be the waves and surf caused by the conditions. Hindcast is what we use for engineering purposes. We look at old weather systems--these aren't old forecast weather systems, but real measured weather conditions, and then use the same procedures to calculate what would be the characteristics of the waves and the surf generated by those meteorological conditions. Those we call hindcast.

Swent: Is this an evaluation of a previous record?

Wiegel: Yes.

Swent: You had the winds, but did you have the wave conditions?

Wiegel: But then you see we would be measuring the waves so that we could compare the calculations with the measurements to test the numerical models. That's where the word "hindcast" came from. We were hindcasting the waves, but from real measured meteorological conditions, and then using those calculations to compare them with measurements to check the validity of our theory. We also use it now to build up statistics. For example, when we went to operate in the North Sea, for the design of offshore oil platforms, we made use of twenty or thirty years of previous meteorological measurements in the North Sea to hindcast the waves that were developed by those conditions, and then from that we got the statistics that we used to design the offshore oil platform.

Now, since I did this type of work, they've developed a new term, something called nowcast. We have forecast, hindcast, and nowcast. Nowcast is where you use actually measured meteorological conditions that come in from these weather buoys and satellites and--because of the high speed computing system that we have--when I say "we", the United States Navy has--they predict what will be the waves right now from information coming in all over the world from various measurements. Those are called nowcasts.

Swent: I have here the program for the ocean conference, "California and the World Ocean 1997," that's going to be held later this month. I noticed they gave a whole session on the GIS--

Wiegel: That's really where you are.

Swent: But they're using that on a lot of the coasts.

Wiegel: Yes. To me, it's almost unbelievable what they can do nowadays.

Swent: Think of all those calculations that you made so painstakingly.

Wiegel: I prefer the system today.

Swent: It's certainly much easier, isn't it?

Wiegel: I much prefer today's systems. They've done a marvelous job.

Swent: It's taken a lot of the drudgery out of those kinds of calculations.

Wiegel: And then improved the accuracies too. You can check so many things. There's just no comparison. We don't have any new physics, though; we are still working with the old physics. We still include F=ma; maybe we got into $e=mc^2$ with some things.

So now I guess we can talk about the U.S. Army's Corps of Engineers; the Coastal Engineering Research Board; the Coastal Engineering Research Center. I think I mentioned earlier that the Army Corps of Engineers goes back to George Washington, and military engineers--in fact, the word "civil engineers" came into being a few hundred years ago to distinguish between what was done in the civilian area--not just the United States, but in England and France and every place else as opposed to military. This, of course, was dams; bridge building has traditionally been civil the world over.

The term "civil engineer"--at least in the English language --started to be used only a couple of hundred years ago. It's a relatively new term; less than three hundred years ago. It was at about the same time that Admiral--I have to always tie things together--about the same time that Admiral Nelson was growing up as a boy in England and looking at a harbor down on the southeast coast, Wells in Norfolk. There was a person doing a consulting job in that same harbor, and as far as we know he's the first person to use the title and call himself a civil engineer, John Smeaton. It's a relatively new term.

So in the United States, the only engineers that existed when the country came into existence were military engineers. George Washington was an engineer and surveyor. The military always needed to know where they were and where they were going, and fields of fire--this all required surveying. They were called topographic engineers because they studied the topography.

Swent: West Point was established as an engineering school, wasn't it?

Wiegel: For the first many, many years, the superintendent of West Point was always a military engineer. I like to think of--the first coastal engineering project in the United States--after it became the United States--was under the direction of President George Washington, and it was a lighthouse up in Maine. There was another one at Montauk Point on Long Island a few years later. These were coastal structures for navigation purposes. The military were always closely connected to harbors for defense. In other words, navies historically--in the Mediterranean going back to the Phoenicians, and the Greeks, and the Romans, and so forth--they built navies, harbors, ports, and so forth. So the military have always worked closely with ports and protection and coastal defense works.

In the U.S. Constitution, there's something that people refer to as the Interstate Commerce Clause. Well, there's no clause that's the Interstate Commerce Clause. I don't know where the term came from, but there is a clause in there that can be construed, and has been construed, to give the federal government certain power over the states, for interstate commerce. This is the so-called Interstate Commerce Clause. The physical fact is that a couple of hundred years ago, the only way you really could get most materials from one part of the country to another--for example, from Boston to New York or to Baltimore or to Charleston, South Carolina--was by shipping.

Congress said that it was a function of the federal government to maintain navigation because it was between states; not within states, but between states. You had to have certain regulations as to navigation aids, certain depths of water--because it's no good having a ship with a certain draft that moves in and out of one port that can't go into another port. So somehow you've got to regulate these things. The Corps of Engineers was given this responsibility. Then, at some later date, in the early 1800s--

Swent: I think we should try to move on up to the present a little more.

Wiegel: Yes, but that's how they got into it. Because of the effect of these navigation structures on the contiguous beaches--sometimes beneficial, but usually harmful, to the beach--in the early 1930s, Congress agreed that the Corps of Engineers had some responsibilities and they established by law what was called the Beach Erosion Board.

Swent: That's what later, in 1963, became this--

Wiegel: Correct. That was in existence for about thirty years, and then they expanded it--this is again by law--they disestablished the Beach Erosion Board and established the Coastal Engineering Research Board and at the same time, the Coastal Engineering Research Center. Two separate entities, but very closely connected.

Swent: The board and the center?

Wiegel: Yes. The board consists of four general officers and three civilians.

Civilian Member on the Coastal Engineering Research Board

Swent: Did you have a connection with that?

Wiegel: I spent eleven years as one of the civilian members on the Coastal Research Board.

Swent: How did that come about?

Wiegel: How did I get on it? I had had several small research contracts with the Coastal Engineering Research Center to do research at the University of California, and then when the coastal engineering--and the center--and when by law--I don't know by law or practice, whatever--they had to have a certain turnover of both the military members and the civilian members. One of the civilian members died--Professor Art Ippen of MIT--they asked me if I would be willing to serve as Professor Ippen's replacement. I said, "Yes, of course I would." So I was given a five-year appointment, and then a second five-year appointment.

During that time, they moved from Washington to Vicksburg and the records must have gotten mislaid or something, so I was on for eleven years rather than the ten years. When I was on it, the full board--that is, with the generals--and it was presided over by the deputy chief of engineers--the major general that is the deputy chief of engineers for civil works. We had two full meetings per year of the board, and these would be in different areas of the United States--coastal meetings. It might be the Great Lakes, it might be the Gulf of Mexico, it could be New England, it could be California, Washington. They made sure that they went to different regions.

Then the three civilian members would meet on two other occasions at the request of the full board to study certain technical problems that they would like more attention given to. For example, they might ask us to look at the proposed research program two years in advance for the Coastal Engineering Research Center. So we would spend a lot of time doing that, and of course, prior to one of those meetings of the three civilians, you did a lot of homework.

Swent: I'm sure.

Wiegel: You didn't just go to the meetings. You had many documents; you'd look at the research plans, but you also kept yourself current with the research activities that were being done by the Coastal Engineering Research Center. At first that was at Fort Belvoir, Virginia, but then it later moved--it was in the early 1980s, 1983 or something like that--to the Waterways Experiment Station at Vicksburg, Mississippi, which is a big army research establishment that was formed originally to study the flooding of the Mississippi River. Then during World War II, it expanded into all kinds of military operations--pavements, bridges, geotechnical--all kinds of real military engineering.

At my final meeting of the board, I was presented the army's Outstanding Civilian Service Award. I appreciated this very much and was very pleased to have been of service to them.

Swent: In the early days--well, in the 1930s--Berkeley and MIT seemed to be two of very few places that were doing this type of research, and gradually more and more universities have gotten into this. I wonder why the Vicksburg Station--was there a point at which there was a decision whether to continue this work only at universities rather than setting up this government research facility?

Wiegel: I can't really tell you that. The reasons work was started at Berkeley was because of one person--

Swent: O'Brien.

Wiegel: --Mike O'Brien. It was that simple. There was some work also at that time being done at Caltech.

Swent: And O'Brien, of course, came from MIT here.

Wiegel: Yes, but Caltech, I now said. There was some work being done at Caltech. O'Brien got his engineering degree at MIT. Of course, they've always had good people in any kind of engineering--both MIT and Caltech. But Caltech expanded during World War II into ship motion and mooring of ships; why the ships behave as they do in harbors. Ships, unfortunately, don't just remain stationary in harbors, they move around because of waves, winds, tides, and currents. Some of that was done there; some within the navy, of course, was done. But the military--you have to go back to World War II, and that is the floating breakwaters they were looking at. That's military. So the Corps was doing some of that.

Swent: I'm just wondering why they set up their own research facility instead of continuing to use universities.

Wiegel: Well, when they first set up, it wasn't at Vicksburg. I'm talking about coastal. It was in Washington, D.C., and it was a small wave tank; just a small wave tank built in Washington, D.C. They did some studies there, and O'Brien built a small wave tank at Cal. A small wave tank was built at MIT, and there was a small one built at Caltech. These were just little things for students so they could use it, with one person helping them to do something. These weren't anything elaborate whatsoever. Not much was known about physical hydraulic modeling.

O'Brien had been a Freeman Scholar; I think he was the second Freeman scholar. In those days--that was the late 1920s--he went to Europe and studied under a couple of the professors in Europe; one in Germany and one in Sweden. This is where physical hydraulic modeling was done. None of this was done in the United States at that time. So O'Brien started it here, and as I said, there was one at MIT and one at Caltech. The Army Corps of Engineers built one in Washington, but during the war they--i.e., the army--had to study some of these things which were strictly military operations. I forgot, they also had one at Vicksburg.

Swent: I'm talking about your period on the board now when you got into the scene: were you reviewing applications for research or anything like that?

Wiegel: No.

Swent: How was the decision made to do research here or at Vicksburg or at Fort Belvoir?

Wiegel: I can't tell you that. You mean, why did they decide to do more and more internally, rather than to feed it out like ONR--Office of Naval Research--does--

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Swent: It just seems that there might be more duplication.

Wiegel: Sometime, before it was in Vicksburg, when it was still in Washington, or they may have moved to--

Swent: Some place that began with "d." Dalla--

Wiegel: It was something or other reservoir in Washington, D.C. I never remembered it because I would get in a taxi and--

Swent: --just go.

Wiegel: It was next to the Army Map Service, and they all knew where the Army Map Service was. The building there was the one next to the Army Map Service. At some stage, they moved to Fort Belvoir in Virginia, which is very close to Washington, D.C. The Corps of Engineers School is there and things like that.

Swent: Is this where you had your meetings?

Wiegel: No. When I would go to the one in Washington, D.C., that was before I was on the board. This is when I had research contracts with the Corps of Engineers like I had research contracts with the Office of Naval Research; I had them with the Army Corps of Engineers. (I did not do research for the Corps after I became a member of the board, except to conclude our ongoing contract.) At some stage at that time--it must have been in the 1960s--one of the National Research Council's boards--I don't know the name then, it would be called Ocean Affairs boards now--that's the oceanographers. They had made a strong recommendation that the Army Corps of Engineers research activities in coastal engineering be expanded dramatically.

That was made by a committee deliberating for a year or so. These were the same ones that had been looking at underseas warfare for the navy, or helicopters going over landing areas, and things of this sort. I wasn't on that. But anyhow, they did make recommendations that the research activities of the Army Corps of Engineers be expanded dramatically in the coastal area. At that time, they started to build some of their facilities, but also they built a big wave tank. The first, what we call prototype wave tanks. It was one in which you could generate a water wave six or seven feet high, and two hundred feet long.

That was done to study the protection of the reservoir side of earth-filled dams. Reservoirs--winds blow over them, which generate waves, and they had erosion problems--how to protect the dams? They didn't build it to study breakwaters; they built it to study dams. The same facility--it was obvious that you needed it to study breakwaters. So this was the first of the worldwide prototype-size facility. So it was almost by accident that some of these things occurred. They got bigger and bigger and did less research contracting out.

That is where I think they were wrong; I still think they were wrong. I mentioned this to them when I was on the board. I've mentioned this to them many times; this is no secret. I think they have made a mistake by trying to do too much internally. They need to do a certain amount internally, because there are really military things that have to be done. Those are better done on a military facility.

Swent: Why?

Wiegel: Some of the things you may want to have classified.

Swent: It's easier to classify the information there?

Wiegel: Yes. I think I mentioned just in closing on Monday that since the collapse of the Russian Empire--the U.S.S.R.--the whole concept of strategic planning by the United States, by NATO, and so forth, has been undergoing tremendous changes. People are not really sure what to do.

Swent: No, you had not mentioned this, Bob.

Wiegel: The Fleet Admiral Nimitz lecturer on national security affairs last year--that is, in 1996--was Harry Summers, Jr., a retired Marine Corps officer--he is a member of the International Institute for Strategic Affairs. He has taught at the Marine Corps School, he has taught at the Army War College, and he has written two bestseller books: one on the Gulf War and one on Vietnam. These are considered to be the classic scholarly books. Looking at these from the standpoint of military strategy, he talked about military strategy in the twentieth century and where it might be going in the future.

One of things he mentioned was that the United States Navy currently--this is last year--is giving up its "bluewater" strategy, which traditionally had been the very definition of the navy--that's your aircraft carriers, your submarines, your anti-submarine warfare--for what they now are calling the "brownwater" strategy--that is, operation in the littoral--in the shallow water because you didn't know whether you were going to have land troops in the Arabian Gulf, Somalia, Haiti. You don't know where it's going to be.

There was a little note just recently in one of the issues of the magazine Science, 3 January 1997. They were writing about where the research was going to be done for the navy. If I may quote: "But now, because of things like Somalia and Desert Storm, the emphasis in on littoral, i.e., coastal warfare, and on getting landing craft safely onto hostile shores." I think the person is rather naive; the real problem is supply. You can get the personnel on shore by helicopters and so forth. But still, there is a big military thing, and the army and the navy work closely together on some of these things. Some things you don't want everybody to know about.

Swent: And yet, they were able to farm those things those out to universities.

Wiegel: The basic research always. In the early days, we could do classified work. Now it's close to impossible, the way universities and faculty are today, to do classified research.

Swent: What's the difference?

Wiegel: Engineers don't mind doing it, but professors of history, and English, and the liberal arts don't believe that universities should do these things, and they've got a point. One of the functions of a university is teaching; making knowledge available. If you're working on classified information, you're not making this available to your students, are you? Either that, or you're doing something wrong with your classified results. So there are some good points. There are things that are much better done in a military establishment, and should be done there. With these two things I just mentioned, I think more is being done at the present time along those lines, but I don't know what it is.

Swent: So you were on the Coastal Engineering Research Board for eleven years; mostly in the 1970s, right?

Wiegel: I must have served into the early 1980s.

Swent: Then you were chairman of the Coastal Engineering Research Council.

Wiegel: That's entirely separate. I would like to mention about the army and the Coastal Engineering Research Center and its predecessor, the Beach Erosion Board. They developed two manuals; the second one was called Shore Protection Manual. This book is still in existence. It is used worldwide. This is a distillation of all the information they got together two decades ago on coastal processes. It's two thick big volumes, eight and a half by eleven inches, printed by the US Government Printing Office. Well illustrated, lots of tables, graphs for engineers to use. This has become the worldwide design manual; period. It is also used in teaching.

All over the world, they complain, and they say, "Why don't you update it?" Well, the Corps of Engineers does put out the Coastal Engineering Technical Notes--CETNs. These are small, five- or six-page publications on individual types of subjects and problems which are produced as needed; which provide all of these updates. It is a U.S. government thing and sent to the Army Corps of Engineers' operating districts, and libraries such as the Water Resources Center Archives here at Berkeley. So they're generally available. This keeps these things up to date. It stems from two things: one is the navigation and the military portions; but then the recognition that when you build these things, you can cause damage to the beaches. I assume you've been to Santa Barbara?

Swent: Yes.

Wiegel: Santa Barbara is a completely artificial harbor. The people in Santa Barbara wanted a harbor for commercial fishing and pleasure boats, so they built a breakwater out into the ocean. Because of the wave conditions down there, this immediately caused the trapping of sand on the western side of the breakwater. The waves were still impinging on the beach on the eastern side, and moved the sand to the southeast. That's fine. But now, the sand that normally replaced it coming from the west couldn't get there. It built up against the breakwater and then filled in part of the harbor, and the harbor was becoming non-operational. So this is an example. Then it was decided to dredge the harbor, place the sand to the east on the beach--to replace the sand that was deprived from it.

Swent: Is that bypassing?

Wiegel: That's called sand bypassing. That's an example; you've been there. Oceanside, California, is another one. That was built by the military during World War II at Oceanside for Marine Corps training. It did the same thing as at Santa Barbara. Along the east coast, you don't build these out into the ocean. What they do is make use of the inland waters, and then modify the entrances and build jetties to stabilize entrances. These structures cause beach erosion very often. Beach erosion on one side and sand deposition on the other. Congress recognized this and said, "You have got to do a study." I think going back to the 1940s sometime, you had to have the study of a reach of two miles on either side of the entrances, to predict what would happen. Today we would call it an environmental impact statement. They were doing it then. There were some good reasons for this.

Swent: So there is a historic difference between the way the West Coast and the East Coast have done these things?

Wiegel: It's a geologic, oceanography difference.

Swent: Yes, right.

Wiegel: On the West Coast here, we have a very narrow continental shelf. On the East Coast and the Gulf Coast, we have a very wide continental shelf.

Swent: So that's what makes the difference?

Wiegel: So the geology and everything is very different. The only thing you can say is that there's sand, there's waves, and there's tides, but the mix of these is very different.

Swent: Always different.

Wiegel: Very different, yes. You have to do a lot of what they call site-specific types of studies.

Photographs

Swent: I don't want to interrupt with this, but I was just wondering: when you look at a picture like that--I'm looking at the cover of the "California and World Ocean" conference program--when you look at that, what do you see?

Wiegel: I see a conflict of interest. I see two people with surfboards, and they want the waves for themselves. I also see some people down here on the beach. They probably just want to get out there into the waves and don't want to be hit with a surfboard. I would say this was middle of winter, and I would say that a good deal of this had been covered with sand during the summer; the winter storms had taken it offshore, and a few months later most of it will be coming back on the shore. I could see kelp on the beach, so I think there's been a storm just recently because it had torn the kelp from its rock anchor. I see people with their homes on the coast because they like to build their homes and look at the beach and look at the waves.

Swent: Here are some rocks in the foreground.

Wiegel: That's why I'm saying--those may have been mostly covered in the summer and fall--I think that looks like southern California some place.

Swent: I presume it's around San Diego; I don't know.

Wiegel: I would think so. I can't spot it. I'm sorry, I thought I knew every inch of coast in that region.

Swent: It doesn't say anywhere on the program where it was taken.

Wiegel: No, but I can't spot exactly where it is. It wouldn't be an old picture because of the size of the surfboards; those are current-day standards.

Swent: So you see a lot of things in that picture?

Wiegel: I take all kinds of photographs. I have somewhere in excess of nine thousand 35-millimeter slides in that closet in the corner there. Nearly everyone of them has a notation on it--date, location, and perhaps some detail.

Swent: All pictures of coasts?

Wiegel: Almost all coastal or hydraulic structures. Most of them are mine; that is, I've taken them. There are slides that other people have taken that have been given to me as a duplicate set. If there had been a hurricane and somebody had gone out after and taken a series of photographs, sometimes they were kind enough to give me a set. This will be the project I'll continue right now. I've got about 1,100 of them catalogued. I'm using a professional program called "Papyrus" in order to catalogue them, and I look at each one on this viewer, and I type on the computer the various subject matters that these should be catalogued under. I stopped when I got up to 1,100 because I was at the end of the amount of information I could get on a disk. But now, you can get these new disks and have a hundred megabytes. I just had one put in in January. So I now have a hundred-megabyte storage disk, and starting next week, I will get back to it. It will be about a full year's work to catalogue those. When I'm finished, the whole collection and computer catalog will be turned over to the Water Resources Center Archives.

Swent: What form? Are they 35-millimeter slides?

Wiegel: Thirty-five millimeter. If you don't ask, somebody will ask, and I will look into it again: "Why don't you put them on a disk?" Because of two reasons: the scanning for color and the amount of storage necessary for just one color photograph to have it good is tremendous. The technology is not yet here.

Swent: It will be.

Wiegel: It will be. But that's no problem. If I have it all catalogued, then--and every one of these is being numbered as I go--then, it will be no difficulty whatsoever to scan them. That's what I'm hoping for; not only to catalogue, but it will all be eventually on a disk of some sort, at least a selection of the best ones.

Swent: Wonderful resource.

Wiegel: It's worldwide. I think it's very necessary. I think this is important. Our Professor Einstein--our Albert--was professor here at Berkeley for years, and his main field was sediment transport in rivers, river flows, natural rivers, sediment transport in rivers, and he did some of the early work in coastal sediment transport. He had some wonderful 35-millimeter slides that he had taken in different parts of the world. He labeled where he took them and dates, but he hadn't written why he took them. When he gave a lecture, he used these slides, and he'd tell these wonderful stories about what he saw, but he didn't write on the slides what you should look for.

This is what I hope to be able to do: to make these available so that they can be used technically by other people. In other words, not just because it's a photograph or something, but what it is you see there; what you should tell the students that you see. That's what I'm working on. That will take--as I said, I estimate from the work I've done so far that it will take me at least one full year. By that I mean eight in the morning to about three in the afternoon. It will take about a year's time to finish it.

Swent: Where did your interest in photography come from?

Wiegel: In the early days when I got on this project, people did a lot of photography. John Isaacs and Willard Bascom--we called him Bill Bascom. Bill was a very good photographer just like he was a very good writer. Not many scientists are good writers. Bill was a mining engineer; maybe mining engineers are. But as a mining engineer, his main field was tunnels. But then he got into oceanography by chance. He did all kinds of work over the years. Just working with people like that, you started to take photos, that's all. So I took a lot.

Parker Trask, who was a geologist--I shared an office with him when I first got on the faculty. He was the one who started the study of geologic engineering here at Berkeley in the college. He would always take photographs of things because in geology, there's just no other way; you had to photograph something. That's it; it's just the people I was working with and so I just continued. Then I found out that the secret of getting some good photographs is to take a lot of them. If you take a lot, you get some good ones.

Swent: I just want to get these dates in here; I checked them. You were on the Research Board from 1974 to 1985. Then you were chairman of the Research Council from 1978 to 1992.

Wiegel: That's correct.

Swent: Were you a member before you were the chairman?

Wiegel: I was vice chairman for years. Before that, I was the executive engineer of its predecessor. So I was on it from an operational standpoint from its founding.

Swent: So as chairman of the council, what was your responsibility?

Wiegel: To just get things going. The biggest responsibility, of course, was every two years we had the international conference on coastal engineering which would be in different places. I became chairman at the conference in Hamburg in 1978, and then I was general chairman of the next ones that were in Sydney, Australia, and Cape Town--

Swent: I think we covered that pretty carefully last time, didn't we? We covered that adequately.

Wiegel: We covered all of that heavily. I think so. But also in the ASCE, I was in many other things. At one stage, I was chairman of the executive committee of the--they had major divisions: structural engineering division, geo-tech, and then there was the division on waterways, ports, coastal, and ocean (WPCO). This is pretty broad. I was chairman of the executive committee of that division (WPCO) some place in the mid-seventies I guess. I've done a lot of committee work.

Swent: A lot of that. I guess we covered that pretty well last time.

Wiegel: I think we went into that. Public service is the one that we haven't--maybe this is all public service.

Technical Advisory Council for Tsunami Protection for Hilo, Hawaii

Swent: Everything is, yes. So let's move on to more public service.

Wiegel: On tsunamis. I thought under public service, I would mention doing something for a city or a county or a state or a foreign government. These are all public service. One of the early ones: I was a member of the Technical Advisory Council for Tsunami Protection for Hilo, Hawaii. They were all Japanese professors, except myself on it, but Doak Cox of the University of Hawaii was the person who organized it. I was on it, and we--that is, our council, met there a couple of times in Hawaii, and we came up with the recommendations as to what Hilo should do for long-term planning to protect themselves from tsunamis by set-backs, elevations and modified structures (pile-supported). This is the same sort of thing they talk about for hurricanes and so forth now, but we did this back in 1962. That was thirty-five years ago.

The advice was largely accepted: setbacks, bringing soil in, increase the elevations. I remember one recommendation which was mine which was accepted, and if you go to Hilo you can see it: if you wanted to build something like a hotel close to the ocean at a low elevation, build it with structurally sound columns, but build the first floor (ground floor) so it could just be wiped out. The tsunami could come in one side and go out the other side; open. You have the columns to support the hotel, which is really above it. That's what the hotels are like in Hilo. Also, the insurance companies took it over for other parts of the islands and used the same sort of reasoning, and many small buildings are built on top of piles, with a carport below on ground level. That's a type of public service.

I think I should mention: tsunami is a Japanese word and it simply means translated, "big wave in bay." In the early days we didn't know too much quantitatively about how they were formed. We now understand this very well. In 1946, there was an earthquake up in the Aleutian Islands, and in hindsight, I'm positive what happened: it triggered a massive underwater landslide. Absolutely massive; miles long it had to be, and broad. The motion of all that mass of material, of course, displaces water. It generated the big waves which traveled across the ocean. They travel at a speed which is equal to the square root of gravity times the water depth. Which means in the open ocean of the Pacific, they travel at about four hundred miles an hour. If you were four thousand miles away from it, and it's traveling four hundred miles an hour, that means that ten hours after it was generated in the Aleutians, that tsunami would hit that place. Or if it were eight thousand miles, that would give you twenty hours. So you have some time there that you can do things such as evacuations if you have ways of measuring it, if you know these things--

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Swent: We may have skipped just a tiny bit: Hilo is more vulnerable to these--

Wiegel: The geography of Hilo, the offshore bathimetry--water depth. But the details of the water depths, and the volcanic cliffs are such that they trap and enhance. If you take a lens and put it in the sunlight, you can concentrate the light waves and you can get a very high density, and you can cause a piece of paper to start to burn. If you have the inverse type of lens, you can disperse light. The underwater bathimetry affects water waves in a similar manner, and certain underwater configurations will cause the concentration of waves. So the build-up can become high at some places; at other places they'll become low.

Hilo is a place where the waves are built up. Hilo is very interesting. It's at the confluence of the lava flows from Mauna

Kea and Mauna Lea, so they suffer from lava flows historically from two volcanoes. There's a river that goes down through them; they have local earthquakes there associated with the volcanoes; and then their configuration is such that they get hit by these tsunamis. The one in 1946 was generated up in the Aleutians. The one in 1961 was generated off Chile and caused a lot of damage and loss of life. 1957--I think that one originated up in the Aleutians--didn't cause loss of life, but caused a lot of damage. So Hilo is unfortunate in its situation.

Swent: How did you happen to be invited to be on this?

Wiegel: I had been doing this work going back to the Bikini test. At the start, I said why I happen to be here. In 1960 or 1961, after the Chilean event, I had looked into some of the effects of tsunamis along the coast of California. So this is why I was asked to do it. Then, in 1964 was that really great earthquake up in Alaska. That was much bigger than the 1906 San Francisco quake. If one of that size hit California, we'd have tremendous damage and loss of life here. That was a huge one. The area near Valdez--incidentally, the oil spill at Valdez was--I hate to use the word "trivial," but compared with what happened in 1964, it was trivial. I'll get into that, because it kind of worries me as to how much money is spent in some things and how almost zero money is spent on other things.

Now with the advances in plate tectonics and all this, we understand the relationship; but the reason why those mountains come down right to the ocean--if anybody has ever taken that beautiful boat trip from Valdez across the sound--if you happen to have clear weather, it's magnificent with the glaciers and everything. But the reason it's there, is that it is seismically active, and land is moving up and down and sideways, and when it goes fast, fracturing, you have an earthquake and your seismic waves are set off; the earth quakes. A piece of property roughly fifty miles wide and several hundred miles long moved! It moved with the speed of sound in a fracture in the rock. I forget how fast that is, but that's fast. One end to the other in minutes. It's almost instantaneous. It moved up or down, some as much as fifty feet vertically. This is the land, much of it underwater!

Going on, surveys were made after and compared with surveys that were available from before, the permanent--no, nothing's permanent up there--but quasi-permanent along that several hundred miles, and a lot of it was underwater. It's like moving a paddle rapidly through water: you see waves generated; if that paddle is several hundred miles long and it moves up to fifty feet, you generate a series of big waves. Those hit the Hawaiian Islands too and they hit northern California, Crescent City--and there was quite a bit of loss of life in Crescent City.

So I was asked by the redevelopment agency of Crescent City if I would please give them advice. The tsunami was in 1964, so in 1965, I know that I prepared a report for Crescent City. Then, it was up to the Crescent City redevelopment agency to build or have set-backs, and I came up with estimates of what might happen in the future, and estimate of set-backs and elevations. I described this a little bit, because I mentioned at that time about the Indian legend. In engineering, we would call it a redundant system: we'd have more than one way of doing it. The abalone shells could be one, but the fact that the shells were put up at maybe fifty feet above sea level was another part of it. It's a redundant system which I believe in.

Then, because of that, I was asked to go to Japan where the Pacific Science Conference was being held, to give the general lecture on the protection of cities from tsunamis. Then at a later date, I was on the National Academy of Engineering Committee on it and so forth, and I chaired the part of the work on what research should be done long-term on ports and cities and infrastructures and things of this sort, in regard to tsunamis.

I'd like to go back to the Valdez area because this is important. Many years later, there was an oil spill, and it was a big one, and it caused a lot of damage on a short-term basis. It did clear up. Everybody sued everybody. In fact, many scientists believe with the things you're forced to do in clean-up, you're better off if you wouldn't do them; you're better off just letting nature take care of some of these things. But when you got this tectonic displacement, and in parts of it now, and I've seen miles in which all the tress are dead because the ground had gone down, the salt water penetration--part of it is flooded with salt water. But aside from that, the ground water was displaced by salt water, and so twenty or thirty years later, there are thousands of acres of trees all dead because their roots were down in salt water. These are really long-term effects. So nature--

Swent: You can't sue anybody for it.

Wiegel: Nature has got some big punches.

Swent: Yes, indeed.

Wiegel: So you spend all kinds of money for one thing, and I guess maybe the other's just so massive--still these things worry me that we over-react to some things, and then in my opinion, we under-react in other things.

Swent: Yes.

Wiegel: So those are some of the things on the tsunamis. I've done many kinds of public service work.

Nature's Always Responding to the Last Major Event

Swent: I'd like to ask you a personal question: do you put any of these things in a religious context?

Wiegel: No.

Swent: Do you have any religious motivation?

Wiegel: No. I guess the closest I come to religion is that I'm so impressed with nature; with all the marvelous aspects--I like flowers, I love trees, and waves, of course. I'm just always impressed, that's all. But I don't worry particularly. Part of this goes back to one person I worked with, John Isaacs. I think I mentioned this several times before. He's the only person I know of who I really think of as true genius. He read all kinds of things. He didn't need much sleep--always working or reading--and thinking. But he was able to put all kinds of things together. He became a civil engineer, but he went to Scripps and started his work on marine biology, and was elected to the National Academy of Sciences for his advances in marine biology. I understand he had never had one course in marine biology.

He came up with whole new ideas on fisheries, the food chain, and things of that sort. There's histories of him because he was a remarkable guy. He would shake his head over why people would get so upset by, let's say, a cooling water system put in. He said when he observed the warm water discharge from a cooling plant, the fish all seem to like it; they prefer the warm water. He said that for some reason people think that things in nature are static and they're not. Nature is always changing. the mix of the diversity varies. We're always recuperating from something: the last flood; the last drought; the last hurricane; the last no winds for a year; the last volcanic eruption; the last earthquake; the last El Niño; that last whatever is the inverse of the El Niño: La Niña?--just the opposite. Nature's always doing this. I guess that's the closest I've come: nature's always doing these things, and it's just absolutely remarkable how it responds, and I just thoroughly enjoy observing what nature has been doing. That's all. You might say that's my religion. I've got a lot of confidence in nature. I'm the inverse of the worriers.

Advising on the Coastal Construction Control Line in Florida

Wiegel: A few years ago I was asked to chair a committee in Florida to re-look at what they call the coastal construction control line. This had to do with hurricanes, and in northeastern Florida, equally important for the beach are northeasters; the big northeasters. That's the sort of storm we have here in coastal California, only we wouldn't call them northeasters; they would be our big storms coming from the Gulf of Alaska. So they had established coastal construction control lines; that is setbacks having to do with sand dune erosion and beach erosion and with elevations. This is all based on hard data, but with predictions. Any time you're taking a small data base with a few hurricanes or northeasters and the little observations you have, and then you're trying to estimate the likelihood for a big one that comes in the future. That's getting pretty soft, but it's the best you can do. It's done county by county in Florida.

They had been established in many places, but there were a couple of counties where they hadn't been accepted yet, and one was Palm Beach County. Palm Beach County has got some well-known people who live on the coast: there are the Kennedys, Bush's mother.

Swent: So the government has to protect their beaches.

Wiegel: No, not to protect the beaches--to establish a coastal construction control line, to minimize damage to coastal buildings. Somebody once said that the government hears everybody, but some they hear more clearly than others. So they wanted to re-look at their procedures. There was a group of about five people that were asked to do it. I just expected to be on it, but they asked me if I would chair it, which I did. I think this served a good purpose. I can't go into them, but we made certain recommendations. This hopefully gets into policy; so all we can do is make recommendations based upon what we consider the best science. You have got to update these things. This is something that really worries me about so many of these rules and regulations, whether it be air quality or water quality or set backs. They're based upon conclusions that people reach at a certain time based upon the information available then.

Twenty or thirty years later, you've got--I hope you've got!--better information. Certainly I hope you have got better ways of analyzing this information. There should be ways of changing these things, but it's almost impossible. Once you get into policy and law, and you have got a court decision and a court case, it's terrible. People don't want to change; inertia. Here in Florida, they did, and so we made certain recommendations. That's hopefully in the procedures now, but perhaps not; I don't know much about what's being done. But that's a type of public activity and public service.

Evaluating Orange County, California, Reports and Recommendations

Wiegel: The most recent study I have done--I just wrapped it up--was for the Army Corps of Engineers and the County of Orange in southern California. That was to look at a series of reports that are about two foot high--eight and a half by eleven inch, printed on both sides--of data and measurements and analyses for Orange County in southern California. These will be used by the Department of Beaches and Parks, Public Works of the county, to come up with plans which will then go to the board of supervisors for Orange County for the future. Now we get into conflicts. Some people like a beach to sit on; some people like a beach to walk on; some people like to walk their dogs; other people say, "Get those dogs off my beach;" some people like to surf; some people like to fish; some people like to sail; some like to picnic; some like to play volleyball. There's all kinds of things.

Orange County is highly populated along much of the coast. It's Newport Beach, it's Huntington Beach, it's Laguna Beach. This is expensive property if you're right on the beach. Anyhow, I've gone through that.

Swent: We should add that some people like to drill for oil on it.

Wiegel: They've drilled a lot in the Huntington Beach and Seal Beach area, and this is a problem because there's been a lot of subsidence. They talk about global warming and sea level rise, and they get upset about the sea level rise. I've been pointing out for a long time, that they should look at a place like Long Beach, Huntington Beach, along where there is recent substantial subsidence--the beach and the shore doesn't care whether the sea level went up or the shore went down; it's where the ocean is relative to the land.

There's a big wetlands area in Huntington Beach: Bolsa Chica. The Bolsa Chica wetlands are too wet. They've subsided because that was the Huntington oil field, and it's all down several feet. So they asked the engineers to develop gates, weirs, and pumps and so forth so it wouldn't be as wet. [laughter] I'm not joking. I understand. They wanted to go back to about where it was and have some representative--what I would call a wet wetland; the other which is kind of a dry wetland. There's good reasons. I shouldn't laugh. It's just that it's kind of funny that the wetlands are too wet.

Again, if we didn't do it, nature would be doing something because the only reason those wetlands were there was the Santa Ana River was discharging in the region, and a few miles to the northwest, the San Gabriel River, and then a little further up, the Los Angeles River. These things change. Prior to about 1840, the Los Angeles River was discharging into the ocean at about where the Los Angeles International Airport is now; that was the mouth of the L.A. River. That's why all those sand dunes are there. That's why the property was vacant on which to build Los Angeles Airport; there were sand dunes.

In about 1825, there was a big flood. We don't know much about it, but there was a huge flood. The river jumped its banks --this might not have been the first time, this might have been the hundredth time it did it--and wound its way down and discharged into Wilmington Lagoon, which is where Long Beach is. So the mouth of the Los Angeles River is now down there, whereas 150 years ago, it was some place else. Nature did that. There was another flood in the 1860s or 1870s when the San Gabriel and the L.A. River joined together. It just joined together down in that area. Now they're separated. So the reasons the wetlands are there--all these kinds of things were going on, creating new land at the ocean's edge. Now we have people living every place, and we're going to have more people. People like southern California.

Swent: Yes, they do.

Beach Nourishment Advisor in Barrow, Alaska

Wiegel: So that's the sort of thing. Another one I found interesting--because everybody wants to have their beaches nourished.

Three years ago I was asked to be a member of an advising group and go up to Barrow, Alaska. This was by the North Shore Borough, and Barrow is the largest Eskimo community there is.

Swent: I've been to Barrow; it's really the end of the world.

Wiegel: It's the end of the world. If you've been there, you'll remember up to the north there's a sign that says: "Look out for Polar Bears." The person I was with (he chaired our group) had been the director of the naval research facility for that whole area, and he said, "That's real. That's correct." He said they're a dangerous animal. They will attack for reasons which we don't completely understand; not being provoked.

Anyhow, they were having problems with the beaches and they wanted advice, so we went up and we were on the committee and we made certain recommendations and so forth. I don't know--because that was not long enough ago--whether or not it's being done. They were worried about their beaches and they wanted nourishment. They wanted a dredge to come in and put more sand on the beach. It's not just your Miami Beaches that want beach rehabilitation.

Swent: They're not lolling around in the sun up there.

Wiegel: They're not lolling around in the sun--there were ice floes just offshore, even in late July and early August--but they want their beach. It was interesting because from a geologic standpoint, it's absolutely marvelous to see that area because it's all permafrost. It's entirely different from anything that I was used to. A couple of the people on the board were geologists who had spent years studying the north slope. In other words, every summer they would go up and spend their summer field trip doing the geology of the region. They (the Eskimos) had the money because of the North Slope oil, so there's money to do these things.

Swent: What did you recommend?

Wiegel: We recommended that they dredge, and how far offshore they should go to get the material and then bring that onto the beach. It was tied in very closely with some of the recommendations that we made in that National Research Council report because two of us were on the National Research Council committee; I assumed that's why we had been invited up there. That way they figured they'd get some really independent opinions.

Examining the Nile Delta

Wiegel: I've gone to many different countries. I think I mentioned earlier to Egypt for the Nile Delta erosion problem with the beaches there, between Alexandria and the Suez Canal. There's something interesting about that, because it's just been in the last year that I've seen a couple of articles that appeared--one in Science, one in the American Scientist that mention a major change in what has been a misconception. Prior to this last year in the technical press--you would read about the Nile Delta erosion, blaming it on the high dam at Aswan. A U.S. scientist now blames it on the spreading of water and sediment in the farm lands by irrigation. The high dam has helped modify it, and the water spreading and sedimentation has modified it, but the main reason for it is for years, there have been two river mouths, both now dammed. There are only two branches of the Nile today; going back two thousand years there were seven branches. That's why the Nile Delta is big. But today there's only two branches. I think for the last fourteen hundred years there's only been two branches. A few kilometers upstream from each mouth, they had built--decades ago--what they call barrages. Those are low dams; they use the French word. So the two mouths of the Nile, the Damietta and the Rosetta, were dammed. I'm now quoting the minister of irrigation: what he said when we met with him: "The Egyptians either use, or misuse, or lose all of the water in the Nile." Use: irrigation; misuse: maybe there's better ways; lose: evaporation, percolation through the bottom and sides of the irrigation system. Except for about one or two weeks each year when they open the gates in the barrages to flush out the waste that accumulated in the lower Nile, no water gets to sea; period. It's gone: used, misused, or lost. So the Aswan has nothing directly to do with it. The Aswan dam and the evaporation of the reservoir--and in the long term because you don't have floods, it won't bring additional silt, but it was all done before. That is a public service sort of thing.

Advising on a Port for Papua New Guinea

Wiegel: The Papua New Guinea work was public service I think. I made four trips there in the mid-1970s. That was when Papua New Guinea was to get its independence and the Australian government wanted to make several gifts. They had, over decades, set up some pretty good school systems; good hospitals. The Australians had done a pretty good job. The Indonesians in West Irian had been terrible, but the Australians did a very good job. One of the final gifts was some way the country can earn money to do these things in the future; and, they have tremendous hydroelectric capabilities. One of the ones is the Purari River.

In the highlands of Papua New Guinea I think it just rains. I don't think I ever saw the top of the mountains; I saw clouds. The run of the river is such that you can get tremendous numbers of megawatts of hydroelectric power out of it, but you have got to use the electricity for something. The concept was to develop aluminum mills because just across the Torres Strait is the York

Peninsula of Australia and there are huge bauxite deposits. Possibly the greatest deposits there are. It's easy to barge this across--going from bauxite, you have to first go to alumina, and then the alumina to aluminum requires very high-energy processing. Hydroelectric is the best because it's a batch-type process; you want it to go on and off. So in hydro, you can open the valve, start the turbines, generate the electricity. It's a good system. They needed a port to go with it, and I was doing the third-party advising on the port.

Swent: That must have been very interesting.

Wiegel: I thoroughly enjoyed it. Where we were is out in the boondocks. But we went in and out of Port Moresby. I have to admit, when I went through some notes, I found that I had given a lecture at the noon meeting of the Rotary International in Port Moresby, Papua New Guinea; it was interesting to me to recall that.

Swent: Are you a Rotarian?

Wiegel: No, I'm not a Rotary member but I was asked to speak to them.

Study of Sines, Portugal, Breakwater Disaster Led to Watershed Changes

Swent: I enjoyed my visit with Orville Magoon; he certainly is an admirer of yours.

Wiegel: We've worked together for thirty or forty years. It's been a long time. We invite him regularly to give lectures here on breakwaters and other sorts of coastal things. Our students always enjoy his lectures--his enthusiasm. Going back to 1978, there was a big storm in the north Atlantic off of France and Portugal. The Portuguese had just finished a breakwater at Sines, which is about a hundred kilometers south of Lisbon. This formed a completely artificial harbor at that area, and it was the biggest industrial complex development in Portugal. They had spent huge sums of money because the big ships were to come in; the ore carriers, the oil tankers, deep draft, and building plants and shipyards of the new big industrial complex.

The breakwater--the ocean end of it--the furthest end--was in the deepest water of any breakwater built any place in the world. This sort of thing--you extend yourself always. This storm caused devastating damage. At that time, I think it was estimated at something like eighty million dollars. Remember this is twenty years ago; eighty million dollars of damage done overnight to the breakwater. We were in Hamburg at the Coastal Engineering Conference, and Billy Edge and Orville Magoon had received, from somebody in Portugal--whether it was a government minister I don't remember now--but they would like to have an independent look at it. So we talked about it and Mike O'Brien was just going off as chair of the council, and I think I was on the Marine Board at that time.

So we discussed it, and thought, "This is something that NSF [National Science Foundation] might fund, to go over and, while everything is fresh, look at it". So Billy Edge chaired that group. Joe Caldwell, who had been the technical director of the Beach Erosion Board, but I think at that time had the top civilian position in the Army Corps of Engineers--he had been the top non-general, which means he would have the equivalent of a two-star--and a really bright guy--agreed also to go, and then a couple of others.

So they went and cooperated with the Portuguese, including the Portuguese navy, and did diving. Both Orville Magoon and Billy Edge are scuba divers, and they dove with the Portuguese and collected all kinds of data on what happened. It wasn't to point fingers, it was to collect data while it was still fresh. They prepared a report called "Failure of the Breakwater at Port Sines, Portugal." Three of us at Berkeley were asked by the American Society of Civil Engineers to review the draft because everybody was threatening to sue them if they published it. Not everybody, but they were threatened to be sued if they published it. It was O'Brien, Ben Gerwick, who was a professor of construction here, but also a very well-known construction engineer worldwide--and myself. We reviewed it and said, "All they're doing is putting the facts down."

The ASCE's advice by their lawyers was to publish; so they did. It was a very good thing because that has been read and re-read by everybody in coastal engineering: Failure of the Breakwater at Port Sines, Portugal, by Port Sines Investigating Panel, ASCE, 1982, 278pp. But that was the start of a complete change in our viewpoints worldwide on what was happening. It's hard to believe, but the Americans, the Dutch, the British, the Germans, the Japanese, the French, the Italians--we were all wrong; we missed something. It has to do with what we call scale effect.

Some things behave very differently in small scale--that is, model size--than they do when they're big. One of the things was --let me show you something. [A small model unit was taken off the shelf and displayed.] When you get into the deeper water, the waves that can hit a breakwater are bigger. For a breakwater in shallow water, the biggest wave will have broken in deeper water. So we find because of the characteristics of rock that comes out of different quarries, that you can't get rock big enough in some places to stand up to the size waves you're going to have in deep water. Engineers had been designing breakwaters using cast concrete units as armor. They started with a French design called tetrapod. These were cast out of concrete. This is just an example; there's many kinds of different ones: tetrapods, hetrapods, acropods. Everybody's got their own name.

Swent: Tetrapod would mean four feet.

Wiegel: Probably, I would think so. Yes.

Swent: And it's a four-footed affair.

Wiegel: As long as they were less than a certain size they were quite adequate, because people didn't want to put steel in them for a couple of reasons: one, it cost a lot more; but secondly, any cracks and you get corrosion. Remember you're in the ocean, and it is a very corrosive environment. We asked one of the people here, Bill Godden, who was professor of civil engineering at Berkeley, and had done a lot of work on the earthquake resistance design of structures, both theory and making models--shaking tables--shake highway overpasses and shake bridges. He said, "Well, of course you have got this scale effect as you're making your model out of the same material or approximately the same material as the prototype."

Here is the simplest way of putting it: if something gets bigger, it gets bigger as the cube of the size, and the weight goes up then with the cube of the size. But the ability to withstand the stress goes up as the cross-sectional area, which is the square; it's the ratio of the weight to the area that is important. As it gets bigger and bigger--if you got it big enough, it wouldn't even be able to stand on those four feet, it would just break. That's the static effect. Now dynamic--if it's moving--somebody said, "You don't see an elephant jumping around like a grasshopper." If an elephant tried to jump, an elephant would break the bones in its legs. So you see, there are both static and dynamic effects.

This is hard to believe, but this had been overlooked. Everybody's tests of these big things had been looking at the movement and displacement; not the breaking, because none of the model units would break. Godden said, "If you model this down to have the correct strength characteristics in your model, it would be so delicate you would have to treat it as if you were placing raw eggs in there." Then he came up with a way of modeling a material that would have those characteristics and would break in the model if it was going to break in the prototype. So that group did a worldwide public service, and I wanted to mention because I knew you had talked with Orville Magoon and he was instrumental in much of these changes.

Swent: He used the word "dollos."

Wiegel: Dollos. It is another type of cast concrete armor units, and the one used at Sines; they were 42 metric tons each. I'm afraid my sample of the dollos is over at the library as a part of the Water Resources Center Archives exhibit over there. That was the type that was used at Sines. That happened to be the particular type that was used at Sines, but I can't show it to you because it's in an exhibit.

Swent: That's the one that did not hold up?

Wiegel: None of them would hold up. They are okay in relatively small sizes, but not when they are very big.

Swent: No.

Wiegel: Since then there's been another one that's been developed, but it's not yet been used in prototype. The Army Corps of Engineers developed that and it's called the "cor-loc". That's a trade name patented by the Corps of Engineers. Nowadays with numerical models, they've done very extensive calculations on the computer of the stresses and all of these things. They've done hydraulic model studies. Most people think that this will be the way that we'll go in the future, because now that we understand what has happened, as engineers you can develop better ways of doing these things.

Swent: They are sort of interlocking.

Wiegel: They interlock.

Swent: It's sort of like jacks, is what I was thinking.

Wiegel: Pierre Danel! That was his whole idea: he said it's a child's jack. Exactly; they interlock. That way they have hydraulic stability because they interlock, but the problem was the stress in the material due to the gradually settling of the static load, but also then the dynamic load. If you've ever been in the ocean when a wave hits you--. You ought to be there when they're twenty feet high or thirty feet high and they hit something. They exert tremendous forces. That's what some other people have done. Our input was to encourage it, to help them do it, and then to review the report.

I wonder if that would be enough on the public services? My consulting I barely got into. Both public service and consulting are very useful when you teach; to bring to the students the real problems. We talk about engineering and science, and there's an awful lot of overlap. Certainly engineers use an awful lot of science, and a lot of scientists are very good engineers. It's a spectrum. In addition, you have to deal with the economics, and then you have to deal with political realities of these things.

The O'Shaughnessy Seawall: A Lesson in Good Engineering

Wiegel: If you work in public service and then in consulting, you can bring this to the students' attention. Then you can take photos; you can show them slides of these kinds of things. For example, I was trying to think yesterday of a good example I could give, because I would like the students to go out locally and see something close by. We could go down to the Marina at Berkeley, and you can see a breakwater, you can see small craft moored and this and that. Go over to Ocean Beach, San Francisco, and along one part of it you have the seawall called the O'Shaughnessy Seawall, which was put in back in the middle 1920s and it is still there despite the fact that you read articles that say seawalls don't last, by certain well-known people in the state of California. I tell them, "Will you please go out and just look at the O'Shaughnessy Seawall and then retract your statement?" What don't last are usually things that haven't even been designed, just built, or if they have been, they haven't been designed well. Also, they probably have not been maintained.

But usually what people call seawalls: somebody's contractor has just gone in and put a few posts down and a few rocks or something and doesn't know what they're doing and they don't last. But they can last. Of course, I used the O'Shaughnessy and so forth to show how things change. Mr. O'Shaughnessy was city engineer and he's also the one that built--with Hetch Hetchy--the whole San Francisco water supply system. As you know, the city of San Francisco is very conservationist-oriented except when it was suggested that the dam be taken down and their water supply would disappear, and they suddenly had second thoughts. People aren't always consistent.

Swent: That's right. I should say we're looking at a cover photo of an issue of <u>Shore & Beach</u> which is a beautiful aerial photo of Ocean Beach in San Francisco.

Wiegel: --and O'Shaughnessy Seawall. What I love: the name Great Highway.

Swent: The Great Highway, yes.

Wiegel: I do like it, though, because I like sand dunes.

Swent: Those dunes don't stay in place, do they?

Wiegel: No, dunes move. All the dune grass is natural, but natural to South Africa, I think, not to here. What's wrong with jacaranda trees and African violets? The world is filled with these good plants from other places. Anyhow, the point is that I wanted to show that. Then what I want the students to do is to go just a little bit down the along the street adjacent to the Great Highway down to Taravel Street. In those days there was the Taravel Tunnel, so you parked your car and you could walk under the dunes to the beach. Now it's all redone since they put in the big sewer outfall and you have got to go up and walk over the top of the highway and dunes to go to the beach. I say, "Take a look at the Taravel Seawall. You won't see it." The Taravel wall was built in about the early 1940s, and it's only about one city block long --it consists of a series of sheet piles tied together, and then a concrete cap.

Swent: What kind of piles?

Wiegel: Sheet piles. In other words, they're not circular, they're three inches by eighteen inches, and many feet long and then they interlock so you build a wall out of them. You won't see it because the idea was to build a seawall that if you have a series of severe winter storms--and the sand of Ocean Beach is moved by the waves out into the surf zone during storms--that you'll protect your sand dunes and your highway because the wall will be there. Then when the beach recovers naturally, the wall will be covered; and that's what happens.

It was completely covered up for decades up until the great El Niño winter of 1982-1983, when it was exposed again. Then one year later it was completely covered. If you go there you won't see anything that looks like a wall because it's all covered with sand. There's an article in this same issue of <u>Shore & Beach</u>--

Swent: Which issue is this? What's the date on that?

Wiegel: The date is January, 1985, by Paul Berrigan. It goes into the history of it. It's kind of interesting, because when it was exposed during those storms; but the corporate memory of the Public Works Department of the City of San Francisco--nobody was left that even knew it was there. Fortunately, Professor Joe Johnson at Berkeley said, "I know all about it." He told them about it. I use this because you can do things, and you can protect valuable property--this is one thing I try to teach my students. There are sometimes--like if you have got a big pipeline and you have got to make sure that pipeline--the sewer outfall going along underneath the Great Highway is not exposed somehow. You want to protect something, but you may not want to look at what protects it. You'd like it to be there, but not seen except every twenty, thirty, forty years or so when you need it. So there are things you can do.

I think something that is similar to the Taraval Seawall, on a larger scale--and I hope it will completely cover with sand--it's mostly covered now--is up at Bolinas Bay at Seadrift. They put in the riprap--not riprap, but a very carefully placed stone revetment on a sand slope and geotextile base, built during that same winter in 1982-83 after severe beach erosion. There's been all kinds of problems between the coastal commission and the county and the property owners about this. But fortunately, the wind gradually blows the sand off the beach and it's gradually covering the rock. I think originally, the owners wanted to bull-doze sand up and cover it, but were not prmitted to do so. But it's kind of interesting. Some of the owners are putting in dune grass where the sand now is. One of these days, my guess is that it's going to be a dune--much of it is already. But if they have another El Niño winter like they did in '82-'83, the revetment is there to protect things.

Swent: --when they need it.

Wiegel: Yes. So there are things that can be done. You don't have to have ugly things. You can do things that are better than that. That's why I like my students to go look. From this standpoint, consulting, public service--all, I think, makes for better teaching.

Swent: I know the students feel the same way because when they write things about you, they always comment about that sort of thing. A couple of things that I do want to get into the record: one of the things that you're known for was your dedication to your students, and the fact that although you did all this enormous amount of public service and consulting and so on, that you made sure that you got back for your classes; and at tremendous personal cost at times. I know you were doing a lot of red-eye flying, but you were not just the kind of teacher who went away for weeks and left your classes. You always put your teaching first.

Wiegel: Yes, always. There was one other thing, too: Professor Joe Johnson. On many of the subjects we could jointly lecture.

Swent: You helped each other?

Wiegel: Yes. We would make sure that one of us was always here; always.

Swent: Your students appreciated that; they were aware of that and appreciated it.

Wiegel: Yes. They also were aware of the fact that they wanted to hear about real engineering; not just the theory of engineering. We use all kinds of theory, but there are still many things that are still not what you call theory.

Adjudicating a Dispute over a Breakwater in the Strait of Hormuz

Swent: You have to adjust sometimes.

Wiegel: You have to adjust. So there are many of these things. I was going back through some of them. One was in Bandar Abbas in Iran --that's right on the coast down at the Strait of Hormuz--that's the little, narrow strait between the Arabian Peninsula and Iran.

Swent: We know now where that is.

Wiegel: Yes. I was there. There was a problem with the breakwater they were building. German engineers designed it. There was an Italian contractor; Iranian owners. I flew all the way over there. That's a long flight; I had to go from here to New York, New York to London, London to Teheran, Teheran to Bandar Abbas.

Swent: Half way around the world.

Wiegel: That was a red-eye red-eye. I remember going from New York from London and then on down; they sent me first class and that was a Boeing 747, and my seat was up in the bubble and they had great containers full of the absolutely marvelous nuts--

Swent: Pistachio?

Wiegel: --pistachio nuts. That may have been what caused my heart problem later on, I don't know. I think I read they have the richest fat content of any nut. [laughter]

Swent: [laughter] Oh, really?

Wiegel: But anyhow, I got there and I found that I had really not been told the whole thing. They wanted me there to adjudicate a dispute. I think what I did was correct. I did tell the students about that, too; what sort of thing you get into as an engineer. It had to do with the quality of the rock at the quarry, high percentage of fines. It's the thing that engineers get into, too. You get to a place and you don't have the quality of material available you would like to have; but they still want the project built. You have to build roads, you have got to open a new quarry. You come from a mining background; you can take cores and everything, but you really don't know what the rock is going to be like until you open the quarry because it has to do a lot with the residual stresses it, and when you are now blowing it up and so forth it may all fragment; you may not get big blocks--just all kinds of complications.

But the interesting thing was that almost everyone I talked to said, "You'd better get out of here pretty fast because there's going to be a revolution." I did my work and I got back. I had to get back fast because I was teaching. Two weeks later, or maybe three weeks or maybe a month is when the revolution came. This has always gotten to me. I told people about it. For some reason, our government--with all of their resources--seemed to have been surprised, and yet everybody I was in contact with out there whether they were the Germans, or the Italians, or the Iranians, told me there was a revolution coming. So I've just been really worried about our intelligence ever since; how they could have missed it when everybody there seemed to have known.

Swent: What were you adjudicating?

Wiegel: The contractor wanted to use a lot of sand in the core of the breakwater, and what percentage could they use because it would be a lot cheaper to get, a lot easier to get, but nobody had really tested it; they just wanted to do it. I said you don't just start something untested in a remote place like this, because where once you go you won't be able to come back and do maintenance on it. That was the sort of thing. Of course, nothing happened because the revolution came and the whole project--

Swent: --wasn't even built.

Wiegel: Everybody walked away. I have no idea as to whether it was ever even completed; I don't know. I've never been back.

Kittimat, Canada: Local Tsunami

Wiegel: Kittimat was up in Alaska--I'm sorry, Canada--the Alaskan Project was another one. That's one of those long fjords--deep water. That's where the big aluminum plant is, and that's where the underground hydroelectric power plant is--at Kittimat. It's a really interesting engineering thing. They were putting in some piles to modify the small fishing port there, and it triggered off a landslide in the underwater terminus of one of the creeks and that generated a tsunami.

Ocean-wide, it would be nothing; locally, it caused a lot of damage. Right across the fjord was an Indian village. This is another of my experiences with tsunamis and so forth. The biggest wave went right up on over the top of the bank into the village and caused a lot of damage. No loss of life, fortunately. That's consulting. But again, is that consulting or it public service? You're doing it as a consulting, but really it's combined.

On the Board of Engineers of the Mica Dam, World's Second Biggest Earthfill Dam

Wiegel: Mica Dam, on the Columbia River, is in what they call the Big Bend area of the Columbia. At that time it was the second biggest earth dam in the world. The biggest was some place in the U.S.S.R. This was certainly the highest earth dam. The geology of the region was such that they were worried about the possibility of a high speed landslide coming down in a known area of landslides and generating tsunamis which would over-top the dam. So I worked with them, and then recommended tests to be done at a hydraulic laboratory at Vancouver. I made a number of trips up there and--

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Swent: You said that this was in Vancouver, British Columbia.

Wiegel: That's where the hydraulics laboratory was.

Swent: Where the laboratory was.

Wiegel: They then appointed me to the board of engineers of the dam. Hunter Rouse from Iowa State University--this is what's called a treaty dam, because the Columbia River originates in the United States, flows north into Canada and then makes what they call the Big Bend--180 degree and comes back down to the south. Because it originates in the United States, and then in Canada, and then comes back into the United States, it's called a treaty dam. You don't just build a dam. Years ago, people had the good sense to say the Columbia River has to be looked at as an entire river. This was probably in the 1930s based upon the Tennessee Valley Authority--TVA is my guess. You have got to look at the whole basin.

So this dam was built, not to be used to generate hydroelectric power at that dam at that time, but to adjust the flows down the Columbia River so that it would hold flows and work in conjunction with the Grand Coulee dam. You could make higher use of the hydroelectric generating capacity of the Grand Coulee by holding a lot of this water up at Mica, and discharging it on a scheduled basis. But because of other things that were occurring at that time--in the first place, the tunnels were being built for the hydro thing and the intake so that everything would be ready. So when they wanted to build a hydro, all they would have to do is come up with the generators--I shouldn't say "all." You're talking about a huge engineering project.

It turned out that because of development in Canada and so forth--and then this Pacific electric power grid system that was coming and going all the way down to L.A., San Diego--

Swent: --for the power.

Wiegel: --for the power. They started to put these in almost immediately. They were worried about the possibility of overtopping of the dam and the geologists that were working with it, because this board of engineers were the tops--no question about it. They had worked on dams all over the world. Because of what we found and the possibility, as an insurance, they modified the design and increased the elevation of the dam. Well, you don't just increase the elevation of a dam; you increase everything.

Swent: Everything.

Wiegel: You're talking about huge quantities of rock. That was really very interesting work.

Swent: It was because of your knowledge of overtopping--

Wiegel: --that they had brought me aboard. And I recommended that a hydraulic model be built, which was accepted and the work done in Vancouver. We witnessed some of the tests, and reviewed the reports and findings.

Swent: --that you came in?

Wiegel: Yes. They knew what they were doing, and had other people for most aspects, but they put me on the board of engineers for this portion. I stayed on it for three years.

Swent: Was it particularly because of the overtopping prediction?

Wiegel: Yes. That I was on the board?

Swent: Yes.

Wiegel: Yes. The others were on the board because they were geologists and specialists in soil mechanics, or spillways and tunnels and things of that sort. That was interesting, because it gave me the opportunity to work with the five or six top dam people on a worldwide basis. They were the ones that had been to India, Pakistan--all of these places. I learned much from them.

Swent: And dams were something you had not been particularly involved with?

Wiegel: Except way back in the early days with spillways. When I taught advanced hydraulic structures, I would teach the design of spillways.

Swent: So you had the knowledge?

Wiegel: Yes. I could have designed one of those, functionally; not as well as they did, but at least I knew how to design a spillway.

Designing Steel Platforms in the Forties Field, North Sea

Wiegel: Another one--completely different--the Highland platforms in what they call the Forties Field of the North Sea. That was in British water, and I was consultant for quite a while on the wave-loading part of the design of that steel platform. That was one of the real big platforms--on the wave-loading of it you have got to be careful. There's all kinds of things connected to the design of it, but the loading by waves on the platform--which is the crucial design load.

I remember talking to the chief engineer of it after they launched it, and they fitted it, and they got it up to the site with the flotation and they started to fill it with water so it would settle to the bottom. He said, "That's when you get ulcers." Is every calculation correct? Is it going to roll over, or is it going to down like it's supposed to? It went down like it was supposed to.

Swent: Where was it built?

Wiegel: It was built in the north of England on the North Sea; I can't remember the town--Teeside, I think. They've built a series of them since then. That was the first one they built; I can't remember the name of it.

Helping to Design the Cognac Platform in the Gulf of Mexico

Wiegel: Another oil platform that I did work on was called the Cognac platform in the Gulf of Mexico. Shell Oil Company was the lead and operator, and at that time, it was in the deepest water ever. It was in a little over a thousand feet of water. This is a platform. It was taken to the site by barge during two successive summers, was taken in three different parts and put together at the site. We all got one of these nice, little plastic cubes from the first production.

Swent: This is a plexi-glass cube: "First production, September 14, 1979; water depth, 1025 feet." There's a little capsule in there with a little bit of oil in it.

Wiegel: We used to joke about the Cognac field that Shell Oil must really have expected Cognac because the cost of this platform was so great, we wondered if they could ever get enough oil out to pay for it. Well, it turned out that you could.

Swent: They did.

Wiegel: They did. But nobody up till then thought you could ever do anything in that depth of water. Now, of course, they do it in deeper water. It's unbelievable what they can do now.

Swent: What has made the difference? Why are they able to do it now when they weren't before?

Wiegel: They learn something from each one. The newest--and that will be built shortly; I don't think it's finished--I think it's going to be in the Neptune field in the Gulf of Mexico, but it will be in several thousand feet of water, and it's called a spar buoy platform. A spar buoy is like a long pipe; long, relatively small diameter, compared with its length. If you put a pipe out in the ocean, and you balance it properly by floating tanks positioned inside the spar, it will remain almost vertical. Depending upon its size and the wave characteristics, it's going to move, but it might be normal to the wave surface, or it might be vertical, but you can do it so it's vertical by proper design. It will be in almost two thousand feet water depth, and moored to the bottom. I notice that the offshore technology conference this year--which is always held in May in Houston, Texas--the big technology award is going to be to Ed Horton, whose company developed that concept.

A Tension-Leg Platform, a New Technique

Wiegel: Incidentally, Randy Paulling, a retired professor here in naval architecture, has done a lot of consulting on that. So there are new techniques. I did a little work on what is called a tension-leg platform, and Paulling has done a lot. Tension-leg. What that concept is: you have a big buoyant structure, and it's connected by--the simplest concept is with cables, just moored to the bottom--and the cables are kept in tension because of the large positive buoyancy of the structure. You can expand on this; and get into deep water, but then you don't use cables; you use big risers that are hollow like pipes. Only much thicker walls. So it's kept under high tension, and moored to the ocean bottom, and then you drill down through it.

I remember Bill Bascom and Ed Horton, who did most of the basic development of this concept--they had formed a company [Deep Oil Technology, Inc.] in Long Beach together with a major construction firm [Fluor Corporation and Ocean Science and Engineering, a joint venture]--they were at our house for dinner many years ago. They said that there were about a half dozen oil companies interested in building the second one, but none interested in being first. Eventually Conoco had one designed and built (by others), for the North Sea's Hutton Field, in about 1980. Incidentally, Ed Horton's uncle was the famous Hollywood comedian, Edward Everet Horton.

They've come up with these things. They're marvelous what they do. I remember when we did this Cognac platform, they joked that it was like a box to put the Empire State Building in, to give you an idea. The size of it is such that the Empire State Building would nearly fit inside it; except you have all of the cross braces, but that gives you an idea of the size. These are marvelous; nowadays they're multi-billion dollar structures.

Pipelines: Patagonia/Alaskan North Slope

Wiegel: I was just looking for some interesting projects, and one of them was when a French engineer who phoned me up once, and I had to come back from a trip and meet him because his company, ETAP, had installed a submarine pipeline down at Patagonia from the Tierra del Fuego Gas Field, which is in Argentina going across the--

Swent: Magellan Strait?

Wiegel: Yes, the Straits of Magellan. They were having a problem with the pipeline where it came ashore through a cobble beach. So I worked with him on that as to how they might repair it. I mention that because at the other geographical extreme, I did engineering for Santa Fe International on a pipeline up on the North Slope of Alaska. So I've worked on pipelines across the Magellan Straits and on the North Slope of Alaska. This is a pipeline up in the air, parallel to the ground, with wind blowing across it and possibly causing substantial vibrations and so forth--how energy from the wind might cause it to vibrate and how to dampen it and things of that sort. the purpose was to design it so it wouldn't vibrate enough to cause damage. Those were interesting.

A Breakwater for Manfredonia, Italy

Wiegel: Another one is in Italy on the Adriatic Sea in a city called Manfredonia. Don't ask me to spell Manfredonia. That was rather novel. The engineers, because of the soil conditions and the depth of water, wanted to make a breakwater by putting in piles and then use precast, prestressed concrete slabs and mount them on the piles vertically. So it was just a vertical wall. I did the estimate of the waves and the calculation of wave-induced forces on it. It was built. In 1992, I was in Venice and my wife and I decided we were going to drive down along the Adriatic, so we rented a car for a couple of weeks and went all the way down to the boot.

Anyhow, at Manfredonia--this was in October, after the tourist season--there was a nice hotel there, and my wife went in, and I double-parked the car, and we wanted a room for a night.

She got the manager--"Fine, no problem whatsoever." My wife came out and she said [he had asked], "Why do you want to stay here?" She said, "My husband was a consultant on this harbor twenty-five years ago, and he wants to look at it." Then they could understand, but, "Why are you here?" [laughter]

Swent: How funny.

Wiegel: So I remember that.

Another one was down in Australia, Bass Straits. That's off the southeast coast of Australia down towards--

Swent: Tasmania?

Wiegel: --Tasmania. There are gasfields off there, and one pipeline came to shore and they wanted a second one. This gas was transmitted up to Sydney, and this was all the gas in Australia at that time. I guess another pipeline went over to Melbourne, but they wanted a second one from the platforms to shore, and it had to go through the surf. So I was asked to go down and I spent some time down there working with them to try to get the information and to estimate how deep to bury it; beaches and offshore sand bottoms are variable with time. It's a place called Ninety-Mile Beach and it really is ninety miles long. There's one little, small town. This was out in nowhere. That was for Esso Australia which is part of Exxon Corporation. I just thought I'd mention a number of these kind of projects worldwide--these different places--to give an idea. As I said all of these fit in. I guess another thing that I should mention: I'm a great believer in written information.

You Absolutely Have to Read: the Importance of Archives

Swent: I was going to say we haven't mentioned your reading, and you're known as an avid reader.

Wiegel: I read. I'm almost like Bill Gates in that way. He said he reads everything including the labels on ketchup bottles. This is very important.

Swent: I don't know how you find time to do all that.

Wiegel: You have got to read; you just absolutely have to read. On all of these jobs, whether it's public service, or commissions, or consulting, I get reports and papers. I'm always cataloguing. I turn them over to the Water Resources Center Archives. There are probably on the order of twenty-five thousand of these. These are all available to the students. When I was still teaching, I made the students go in and learn how to use these things, because I said as engineers, the first thing you want to do is find out what data are available, what information is available, and look it up. This is the way you do Ph.D. theses: you do surveys first; then you home in and find out, "This isn't known. This is something about which I, as a graduate student, can learn something new." You'll find out what is known first. It's the same when you're designing something.

So this is all in there, and my students got used to using them. I think they'll tell you in the archives that they still come back and they may work for an oil company or a consulting firm or the California Coastal Commission staff or the state of California or the Corps of Engineers. It's a very good source and it still continues to be built up.

Swent: You have your own cataloguing system, I understand.

Wiegel: I'm cataloguing it. I'm just sorry it's not on computer, but it's cards. I thought about putting it on computer; there's no way. That's for somebody else. This putting on the computer and indexing my 35mm slides I'll do, because nobody else can do that. It's in my mind; I have to look at those slides. I labeled every one when I took them. I don't mean they're a whole bunch of slides with no labels, but I have got to put other information in. This other--the Archives, anybody can go and put that on a computer eventually. I think this pretty well covers the--

Swent: I think your archives are very important. I'm glad we mentioned that.

Wiegel: At the end, I thought something else was coming through my mind.

Swent: I was going to ask you if you wanted to comment on one little thing I saw here. This was about the Shore and Beach Preservation Association; its goals. It seemed to me these are very broad goals, and in a way--

Wiegel: I'm sure it is very broad.

Swent: They may have changed through time. "Coastal preservation, enhancement of coastal recreation opportunities, and wide use and development of the coastline." Some of those, I think, may have changed with time. Have they always had those goals?

Wiegel: Those were the original goals.

Swent: Were the original goals?

Wiegel: Yes. The background of that was--

Swent: That was from 1926. That's seventy years ago.

Wiegel: The state of New Jersey was the first where you really had development of the coast for tourists. This goes back to the Civil War.

Swent: But preservation, enhancement, wise use, and development? That's a big, big order.

Wiegel: That's right. The conflicts get more severe all the time because there's more people. On the New Jersey coast in summer, you had people from the population of New York and the population of Philadelphia. The first big development was at Cape May and people from Philadelphia came out by railroad. The governor of New Jersey was really worried about it, and there was one very perceptive person there, and at the same time in New York--I'm trying to think of the person who's responsible for Central Park and Jones Beach--the famous person of New York with the development of all these public parks and everything in New York City. He was part of it--Robert Moses. These were long-vision people.

Swent: How do you think we can balance those things now?

Wiegel: The numbers of people are huge. I say it about Miami Beach, and I say it about Honolulu, Waikiki. "Thank goodness for these high-rises, thank goodness for all these people here." The more people you get in the few places like this, then you can keep other places; the ones that I like to go to that don't have people. [laughter] But seriously, lots of people like to be with other people. They're people watchers. Everybody's different. I like to walk along a beach, I like it pristine and this and that. That's great. Other people--that's the last thing in the world they want to do. They like to go, and they like to see people.

Swent: Where the crowds are.

Wiegel: Where the crowds are, and that's great.

The Southern Swell

Wiegel: One other thing that I was going to bring up because you had asked, and that is: what is this term "southern swell?" In southern California--it affects the Orange County study, it affects the Bolsa Chica wetlands, it affects--remember the picture with those fellows with their surfboards?

Swent: Yes.

Wiegel: Well, southern swell is swell--not waves within a storm--but the swell that has moved out from a storm, coming from the south. The term was coined in southern California. These waves are generated in the southern ocean. That is the ocean that circumvents Antarctica in the south forty and south fifty latitude. In other words, you might say the south, south Pacific; the south, south, Atlantic; and the south, south Indian Ocean. Some people call it the Southern Ocean.

These are the roaring forties and furious fifties. The storms are immense, they are frequent, and they generate huge waves. These waves travel across the ocean. They move northerly, between New Zealand and Australia, past Tahiti, across the equator, past the Hawaiian Islands, come into southern California, and they go all the way up to the Aleutians. They've been tracked by installing wave recorders. There was a marvelous U.S. study of these waves from where they were generated all the way up to the Aleutians. It's one of the big projects. Frank Snodgrass was the leader of it at Scripps, with Walter Munk. Walter is still active; he has received a National Medal of Science.

They call it the southern swell in southern California. It's from the south. These are the best surfing waves in southern California. Surf's up! That's what they like, and they come in the summer. You can have them any time of the year; I have got to be careful. But in the summer, when you don't have local storms--and especially in July, August, September--that's the height of the southern hemisphere winter. It's when our local waves are minimal, so it's much more noticeable when the southern swell comes. The good thing is it's this marvelous surf. However, another think about is that it changes the direction of sand transport along the coast. Instead of going from north to south, or rather west to east--southern California is twisted, as you know. So I should say instead of coming from the northwest to the southeast, when the southern swell is breaking, it transports sand in the other direction, so the sand kind of moves in many areas back and forth in that part of southern California.

That's the main reason I've been looking at it recently. I did some of the first studies--in fact I wrote a paper on it in 1950, I guess. That's when I was doing that work at the Marine Corps base at Camp Pendleton, and I would go swimming, and everybody "knew" that sand drifts from northwest to southeast there, and I'd go swimming out there in the surf, and I drifted from southeast to northwest. I thought, "Something's wrong here in the general concept." That got me interested in it, so I talked to Walter Munk--he was at Scripps--and Frank Snodgrass. We had a wave recorder in there for the Marine Corps as part of the operational conditions--we were trying to find out what were the wave characteristics, how these affected the landing craft and so forth.

Walter had written a paper on the gradual shift of the mean period of waves with time. These waves, as they were moving across the Pacific, would stretch out; become lower as they stretch out, and they become very long. It's pretty complicated what occurs. It's wave dispersion, but that's essentially what you see: longer and lower swell, and more nearly periodic. By looking at the shift of wave period with time, you can calculate how far they may have come, using the theory in Munk's paper. I did this. The calculation indicated they were coming from about four to five thousand miles away. I wrote a paper--I think this was my third or fourth or whatever--this was probably a landmark paper.

It wasn't until much later that more information became available--especially Frank Snodgrass, Walter Munk, and their colleagues did the big definitive study, and they had a movie made of it. Walter's wife's brother was one of the Hollywood motion picture stars. I can't remember the name now; I'm sorry, I should. So with these kind of contacts, they had somebody that could really do a good job on the movie documentation. I still have a print of it, and I used to show it to my students. It got awfully brittle, but we got it on video. I told Walter about that and he said, "Thank goodness," because it was well done, and is now preserved. It shows all of these stations: the one down south of the south island of New Zealand. Walter was at western Samoa. Then there was Hawaii, with a tripartite station.

Then there was a spar buoy. Remember I told you about a buoy spar? Sometime previous to this study, someone at Scripps had come up with the concept of a floating structure which you could build like a long ship and take it to a site then flood the stern so it would up-end and act as a spar buoy in the ocean. That was a wave recorder station in the middle of the north Pacific and that was part of it. Walter used to joke--he said of course I was the director of this, so I was at Samoa, and Frank

Snodgrass, who had thought of the concept--he was in the Hawaii Islands because that's at the tripartite station and control center, and a recent Ph.D. had the one up in the Aleutians. [laughter] That was one of the great oceanography studies; the one that they did. That was on southern swell.

Swent: But you did a landmark paper?

Wiegel: Early. The one I did--at that time when I did it--it showed, with data and calculations, that the swell came from four to five thousand miles away. That's what made it important. This other one, by Snodgrass et al was the classic study, a great study--Snodgrass and Munk--and Klaus Hasselman, who was over from Germany worked on it; and others. That's about it, I think--

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VIII MENTORING AND ADVISING GRADUATE STUDENTS

Match a Problem with a Student, Encourage, and Wrap it Up

Wiegel: Mentoring is really to get them interested in doing something, I think. I think so. I had better look up that word. I usually look up words in dictionaries.

Swent: Well, it's the current buzzword. A good teacher has always been a mentor.

Wiegel: My hope is show them examples of how you do something. Isn't that it?

Swent: I think so. And be a role model.

Wiegel: We could wrap it up by talking about Ph.D. theses, because this is mentoring.

Swent: Yes.

Wiegel: I told this to students many times: "There's only a few things that a professor does. Every student has a different combination of abilities, and there's all kinds of problems. To solve each problem requires a different set of capabilities. So the first thing the professor does with a student on a Ph.D. thesis, is to match a problem with that student." Is it do-able? I've seen some pretty bad examples where things haven't been do-able. It wasn't the student's fault; it was the professor who had not done a--I've inherited--not from here, but from other universities--I've inherited a couple, and then was able to straighten it out and get the person working on something that could be done. So part of it is to match.

The second thing, of course, is to help in making equipment and things available, and talking to the student and discussing where the student is. To continue to encourage the student; that's the important thing, because boy, it's hard to do a Ph.D thesis. It's hard work. Then the third thing is to get the student to stop, and finish writing up the work, and get the degree! [laughter] If you're really good, you're always going to think of something else that should be done. So wrap it up.

So you do three things: you match, you encourage, and you get them to stop and get his or her degree. Maybe that's mentoring.

Swent: It is.

Wiegel: I'll end with it there.

Swent: You've advised a lot of Ph.D. students in your time--

Wiegel: A lot, yes.

Swent: --and they're all very grateful to you.

Wiegel: It's always been fun. I'm sure you've heard this before. It is fun.

Swent: I've talked to some of your former students and they all just adore you.

Wiegel: I'm glad to hear that, because that's the other thing: I've had a lot of good students, too. That's why I really never worry about the future. [laughter]

Swent: You've seen the best of the best.

Wiegel: You name it, there's good people working on it.

Swent: The world's in good hands with those--

Wiegel: Some good. There's some pretty bad hands around, I'm afraid. But there's a lot of good hands.

Swent: But some of your students--

Wiegel: No, I didn't mean that type. I think of the ones that start wars and things of that sort. There's an awful lot of really highly competent, qualified, ethical people.

Swent: You've worked with the cream of the crop.

Wiegel: They are. Okay, well that should about do it.

Swent: I think so, Bob. We have to know when to stop.

Wiegel: I didn't know it was going to be this much work.

Swent: Yes, it is.

Wiegel: I had to go back and look up stuff.

Swent: Because you've done it so well, that's why. Thank you very much.

Thoughts on the Political Aspects of Coastal Engineering

[Interview 7: March 31, 1997] ##

Swent: We're doing a little supplemental interview here with some things we thought we hadn't talked about enough in the previous interviews. You had a couple of items that you wanted to give more attention to. I thought you might want to just make a few general comments on the politicization of coastal engineering. I suggested that it was something new, but you think it isn't.

Wiegel: No, I think it's been political for a long time.

Swent: Always was political.

Wiegel: I'll tell you why. I'll give you one example. This review of the very large study for Orange County, California, coast by the Corps of Engineers, the County of Orange, and several small consulting firms that worked with them--I was asked to review the study and make comments where there were gaps and so forth--

Swent: When was this?

Wiegel: I handed in my report a few months ago.

Swent: It's very recent.

Wiegel: Essentially, 1996.

Swent: Okay, recently.

Wiegel: But once I get interested in something, I never let go of it. Formally, I have nothing to do with it any more, but informally, I can never stop because I find things so interesting. As a part of it, I wanted to see or learn as much as I could about what is known as a littoral sediment cell, which I have defined earlier and so forth, at San Pedro Bay. Part of it is in Los Angeles

County; part of it is in Orange County. The coastal processes do not know there's a county boundary there. So politically this study is made for a county, but scientifically, in engineering, you have to look at the physical boundaries.

This got me interested in what was happening in that area before the big ports of Long Beach and Los Angeles were developed. Fortunately, because of the Water Resources Center Archives here in the university, I was able to go back and relatively easily find documents that were written a 120, 110 years ago; several of these being reports to Congress. Reading the various documents of the proposed port of Los Angeles--one proposal to be built where it is today at Wilmington, San Pedro next to Long Beach--the other was in Santa Monica Bay--the big fights between Southern Pacific Railroad Company and others, and all of the work that was done in Washington, D.C, and the presentations made and so forth. So that part of coastal engineering was well politicized 120 years ago. So it's not new.

Swent: No.

Wiegel: That had to do with navigation, and ports, and commercial activities. Of course, what has happened in southern California, in Florida, New Jersey, the Carolinas: more and more people have wanted to go and make use of the beaches. A hundred years ago or so, there weren't that many people; there wasn't that much money; there wasn't that much leisure time. But then the numbers of people increased; the amount of money that's available to so many people have increased. For many reasons, people like to look at bodies of waters: ocean, rivers, lakes. This is very often the most expensive property.

I saw this article; it was in the Friday issue of the Wall Street Journal. That was the twenty-eighth of March 1997; the section is on real estate. They have a section of real estate every Friday, and they were talking about this relatively new development in Orange County by the Irvine Company that's down towards Laguna Beach, not in Laguna Beach, north of it. I forget the name of the particular place. These are rather large individual parcels of land. I think you have to buy two or three acres per house. They have no trouble whatsoever of selling the ones for two and three million dollars that have an uninterrupted ocean view over the golf course. There are a few that are going at "bargain basement prices" for around six hundred thousand dollars. This isn't the house, this is the parcel; but without a dedicated view. So from several millions down to six hundred thousand dollars. That's what people place on a view.

It's not just people with lots of money; most people like views. They go and they stay in a hotel or a condominium or a motel. You'll find that the prices that they can charge for ocean views or lake views and so forth--in particular if it's ocean front or lake front or river front--they're high. This is worldwide nowadays. So you have people that just want to see these things, to make use of these things; but other people want to fish, other people want to surf, other people want to sail. What's appropriate and what isn't appropriate? Everyone has their own point of view. This gets into political issues; there's no other way to resolve these things. Partially, you can resolve them by economics. If you want the view, you pay twice as much or three times as much.

Swent: That's what's resolving it.

Wiegel: But the other way is through different sorts of political action, so it is politicized and it should be. I don't know any other way of resolving some of these things. So it's not new.

Swent: No, it isn't.

More on the California Coastal Commission

Wiegel: You mentioned the California Coastal Commission, which is highly political, and there's no alternative to it being highly political. I was on the commission that was in existence prior to the Coastal Commission. I discussed it [previously]; it was called the California Advisory Commission on Marine and Coastal Resources, and I was on it throughout the entire length of its time. I did touch on that, and I'd like just to make a one-sentence quote. This is the fifth annual report, 1972 "A Summary of Marine Commission Activities." In the covering letter, the chairman of the commission, who was Robert B. Krueger, Bud Krueger, a lawyer. He's an attorney in the Los Angeles area. The previous chairs were scientists; Bud Krueger was a lawyer. In his covering letter of the final report, he says: "The fact that CMC's recommendations on the subject during the years 1969-1972 were not acted upon by either of the entities creating it, points up the futility of establishing advisory commissions with political responsibility, and provides some historical background relevant to the enactment of Proposition 20, the California Coastal Zone Conservation Act of 1972." The entities creating it that Bud Krueger was referring to were jointly an act of the state legislature of California and the governor of the state.

Swent: Executive and legislative.

Wiegel: By law, three commissioners were assemblymen, three commissioners were state senators. One--I think I mentioned--of the assemblymen was Pete Wilson. But most people on the commission, except for the political ones, were scientists or engineers and we had study after study with recommendations. You noticed Mr. Krueger said none of them was implemented. I'll leave this with you. I think it's--

Swent: All right. Maybe we could just include this as an appendix.

Wiegel: I think so. So that was the immediate predecessor. I know that Pete Wilson had written an act which he had presented to the state assembly and it got buried. He was too junior at that time. I think this is rather interesting because last week there was this big meeting in San Diego--

Swent: Orville Magoon's conference. Not his, but he was co-chairman of it.

Wiegel: Orville Magoon did an awful lot of the work to get it going. It was a conference that the state of California and the director of resources pushed, and Governor Wilson pushed heavily. You look at Governor Wilson who was assemblyman, but then he became mayor of San Diego. He was very early and very active in the coastal things because the people in the San Diego area--they love their coast. He's always been active in it.

There was a couple of articles in the San Francisco Chronicle last week, and an editorial in the San Francisco Chronicle about this meeting. I have a copy of the program and there's--I believe--over two hundred papers scheduled to be presented, almost all of them technical: that is, scientific or engineering studies. So there will be a lot of good technical information if it's published. I hope that the proceedings will be published, but they didn't mention it in the brochure, but I hope they will be.

The next step is the political. This commission--they don't really have much money. There's also a California Conservancy that does get money each year from the state legislature. They're the ones that can buy pieces of property or help fund the building of a pier, because so many people like to walk on a pier or sit on a pier.

Swent: That's the Coastal Conservancy.

Wiegel: The Coastal Conservancy. These are two independent entities. The Coastal Commission is the political one because they're the ones that finally make decisions as to what can be built or what can't be built. On that commission and other regulatory bodies, I know that we need many of them, but I worry. So many times politicians who have not been reelected--or in California and other places they can't run again because of the term limits--their friends in the various state and federal things appoint them to commissions. You see this happening time after time. So often, regulatory agencies are made up of people who I think are definitely second-rate; obviously, they've lost an election or something.

Swent: Second-rate in power anyway.

Wiegel: Yes. This worries me. Another thing that worries me is the fact that almost none of them have any background at all in science of any type. I remember three or four years ago attending a national meeting, and the person who was chairing a session had been a congresswoman from New England and lost the second or third election--I don't know which--and was appointed to one of these federal regulatory, or some other post in Washington, D.C., after she had lost an election. Something came up and there were questions, and I was asked to comment. I said, "It's really a very complicated issue."

She shut me up and said, "That's the trouble with you engineers and scientists: you're always saying it's complicated."

How do you respond to that? Since then, I have every once in a while thought, "Is there some way we can get some of our findings on two-minute TV bits?"

Thinking about that time, thinking of other times when I was on the Marine Board--I can remember the board had put a lot of time and effort into having the results of one of one of their main studies presented in Washington D.C. We knew there wouldn't be congresspeople attending, but we were hoping to have a good number of staff of congresspeople; but the turn-out was very small. This was a study done partially at the request of Congress. You can't give up, but it's very, very difficult. I don't see any way except by these two-minute video bits; I really don't. But I can't figure out how to do it myself. I'm leaving this for somebody else to do.

Swent: The information is there but...

Wiegel: It has to be presented in a Walt Disney--and that is not derogatory, because I wish I could do it, but somehow it's got to be presented in that manner or people will pay attention to it, and limit it to their attention span. I guess to wrap it up: I think the Coastal Commission is necessary. It is political; I see no other way for it to exist unless it's political.

Swent: But if they had more informed decisions, it would be better.

Wiegel: Part of the problem is they really need more money for more scientific staff, I think. I think that would be helpful.

A Particularly Interesting Trip to Russia in 1971

Swent: Does that lead us into the other topics?

Wiegel: I'd like to go back to something that's much more pleasure to me, and that is the fact that I had the opportunity of having some really interesting trips. As I mentioned, in 1971, I had been invited to give a presentation at the World Petroleum Congress--which in 1971 was held in Russia, in Moscow. The World Petroleum Congress is in two different levels: there's a level that I was in which is the science/engineering level which you make presentations of various technical things; but then there's the political level, because the chairmen of the board of three or four major worldwide oil companies attended. The minister of resources for Iran, the minister of resources for Algeria, the minister of resources for Russia, etc. Because of that, those of us at the working level got to attend things that normally we wouldn't.

They had just built a new ballet theater in the Kremlin. The Bolshoi was giving their premiere performance in the Kremlin, and it was at the time our group was there, and unless you were invited officially, there was no way you could see it. That was one of the nicest things that ever happened to me.

Swent: How exciting.

Wiegel: That was exciting.

Swent: What did they present?

Wiegel: I couldn't tell you.

Swent: But it was good?

Wiegel: It was absolutely magnificent; I know that. Afterwards, I had planned--because it was less expensive to take a full week's tour of Central Asia--or maybe two weeks, I don't know--this was during the summer--it was cheaper to do that than to simply go to the meeting and then back. So the American Petroleum Institute, who was funding me, said, "Go right ahead. We don't care. If you can save us money, book the thing that's the least expensive." So I booked a tour of central Asia and I went to Samarkand and Tashkent and up into Frunze, which is up in the Himalayas, and then into

Kazakhstan and Uzbekistan, and spent several nights at what was then called locally, Amaty. I now know though the Russians all said it was Alma Ata, but nobody called it that, but I noticed now that it's an independent country that is now known as Amalty, which is right at the base of the west side of the Tian Shan Mountains.

Swent: The "roof of the world?"

Wiegel: The "roof of the world." I asked if I could go up into the mountains. They said there were almost no roads, but they were building a dam and they could take me up to see the dam. It was located such that behind it was a large mountain with glaciers on it; spectacular. But the interesting part of the dam is that it's not to store water. They've had problems there with--during certain types of thaws and melting conditions and rains, they have high-speed mudflows, and there have been several disastrous occasions where villages have been buried and loss of life. So this rock-filled earth dam was constructed to prevent the mudflows, not to store water. I lectured about this to my students when I returned, with color slides, so they would think about it when they became practicing engineers.

The other part that was so interesting was that it was not being built in a traditional manner of moving everything by quarrying, and moving the rock, and so forth. It was done with spaced charges in the rock of the canyon walls. They would space the location and timing of the charges so that the rock blown from the walls fell to construct the dam. I don't know if I remember the statistics correctly, but something like 80 percent of the bulk of the rock in the dam was placed by these spaced charges. That shows an extremely high level of knowledge of rock mechanics and explosives and everything else. An extremely high level. That was fascinating. I found it unbelievable that they would take me there and they would show me this and explain. That was interesting.

I got to see a good deal of central Asia. On the other side, the east side of the Tian Shan Mountains, is the segment of China which is really Turkoman, and it's the part between the Tian Shan Mountains and the Gobi Desert. Then it was known as Sin Kiang Province. It's not called that now. I can't remember just what it is, but it's just been in all the newspapers on the last few months. These are all Turkoman--

Swent: At that time it was Sin Kiang?

Wiegel: [spells] Sing Kang. I'm now spelling what I think I heard, I--

Swent: They keep changing the spelling.

Wiegel: They keep changing, yes. It is Xinjiang now, or Uygur Autonomous Region.

Swent: That must have been very interesting.

Wiegel: I had been in China in 1987 as the invited main lecturer at an international conference on coastal engineering and port design for developing countries. I was invited to give the opening at the meeting. My wife went with me, and we had a nice tour of China afterwards. We wanted to go back, so in 1989, we were scheduled--we bought all our tickets and everything--to go on the Silk Route, all the way to Kashgar or Kotan. We were in Beijing in May of 1989, and that's when the Tienanmen Square event occurred. We couldn't see any of the normal things in Beijing, but we'd been there before. We were interested in the other part, but you had to go Beijing first.

So we flew to Xian, which is where those famous terra cotta soldiers are. We were scheduled to go from there into the West, and we got to the airport the second night at about ten at night, and the plane didn't come, and the plane didn't come, and the plane didn't come. Finally, a worried Chinese woman who was a local tour person came and said, "Come back to the hotel; you won't be able to get out tonight and probably not tomorrow." So the next day we're trying to find out what was going on because we didn't get out then; the train--we'd miss it--it wasn't simple. Well, there had been problems. It started to filter down, and we found out a little bit what happened.

The Chinese military--because the airlines are all run by the military--that was the end. Everything was cancelled, all the flights. They were using them to transport troops into Beijing. We were finally able to get a flight out to Shanghai because the whole thing was aborted at that stage obviously. But I wanted to see the other side of the Tian Shan Mountains. Now I realize it's too late. I can't take trips like that anymore. I can't do it. But I've had some very interesting trips: the one to Malaysia and Indonesia; the ones to the Philippines; South Africa and up in the diamond area of Namibia; Iran; several to Egypt--

Swent: You've seen a lot.

Wiegel: I've thoroughly enjoyed it. My wife doesn't care as much for the out-of-the-way ones as I do, but most of them we both thoroughly enjoy. Now, I'm afraid, we more likely go to Hawaii or Europe or something like that.

Swent: That's all right, too.

Wiegel: Yes. I think that's what I wanted to say about that.

The Coastal Engineering Research Center

Swent: You wanted to say a little bit more about the Corps of Engineers Laboratory.

Wiegel: Yes. I won't go into the background again; I already have. But the original Corps of Engineers coastal work was navigation; its major work still is navigation.

Swent: But then you wanted to expand--

Wiegel: But because of the effect of navigation such as harbor entrances, artificial harbors with breakwaters such as Santa Barbara or the ones at Long Beach--these affect the contiguous coast. Although the federal government never has to take responsibility for any damage done to other things, occasionally they, through acts of Congress and so forth, will try to mitigate or rectify--

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Swent: --about a hundred years ago.

Wiegel: Some of the Corps of Engineers--people recognize the fact that these navigation works were sometimes causing deleterious actions to adjacent bits of the coast. I won't go into--because I discussed it--the Beach Erosion Board was formed and so forth.

Swent: That's all on the record.

Wiegel: In order to study these things, you really need to use hydraulic models, and in the 1930s, the use of hydraulic models for rivers and dams was imported to the United States from the work that was being done in Europe, specifically in Germany: the great German fluid mechanics people. We've all heard of Von Karman and Prandtl and these people in aerodynamics, but the people working on rivers--especially the Rhine and so forth--were doing work with large hydraulic models.

O'Brien was one of the first to go over there, in 1928, and studied with these people and learned about it and then came back, was hired at the University of California at Berkeley, and did some of the first studies making use of hydraulic models. He also was appointed to the Beach Erosion Board shortly after it was founded, not at the start. He was hired by them as a consultant in the summer.

Swent: That's all in his oral history.

Wiegel: That's all in. Now they thought they should have a small model at the Beach Erosion Board because you have got to try things out; you don't go moving nature around, but you can try things on a small scale in a laboratory. So that's how it started. Then at the Waterways Experiment Station at Vicksburg where they were studying the Mississippi, they built a few facilities too. Then during World War II, these facilities were used as a part of their testing and planning for some military operations on amphibious landings. So when World War II ended, they had these facilities and they developed some larger ones because it was found that it was a very useful tool, both for research and also for checking plans--proposed plans for a breakwater or a jetty entrance and so forth.

It was quite logical that the Corps of Engineers would have some of these facilities. There were only small facilities at Berkeley, CalTech, and MIT. That's all that existed. The Army Corps of Engineers Beach Erosion Board let research contracts at that time, just like the Office of Naval Research does.

Swent: I think this is all pretty well documented.

Wiegel: It may be. So that kind of started it, but then in the 1960s, one of the committees of the National Academy of Sciences was called the Committee on Oceanography. This was in the 1960s. They made a study of coastal science and what should be done to improve our knowledge. A good deal of this had to do with amphibious operations.

Swent: Were you on this committee?

Wiegel: No, I was not on that committee. One of recommendations was that the Army Corps of Engineers Beach Erosion Board be expanded to do a lot more research. Just at that same time, the federal government decided to disestablish the Beach Erosion Board and establish the Coastal Engineering Research Board--Beach Erosion/Coastal Engineering--much broader--and the Coastal Engineering Research Center. It was no longer just a board with a few small facilities; it was now a board that kind of set policy, and then a center that was going to develop. This developed, and they had much more money. They still gave research contracts to universities, of which we were one. But at some time, mostly after they moved to Vicksburg, which was in 1983--in 1973, they moved to Fort Belvoir and expanded a bit, but in 1983 they moved to Vicksburg, Mississippi, where the big waterways experiment station was.

I don't have any data to document it, but it certainly seems quite evident that a much smaller percentage of the funds that they had available were used for funding research outside of the

Corps. In other words, funding research at universities. More and more and more was done in-house. This was similar, in many ways, to what was happening at the same time in England at the hydraulic research station, which was first in irrigation and water supply, but then into coastal engineering. It was wholly funded by the British government. In the Netherlands, the Delft Hydraulics Laboratory was the same thing. They expanded very dramatically because after the big storms in the North Sea in 1953, and the flooding with thousand of acres inundated, and hundreds and hundreds of people killed, this was a major effort on the behalf of the Dutch government. So they expanded the Delft Hydraulics Laboratory into all kinds of coastal work. In Denmark, the Danish Hydraulic Institute also expanded because they had much coastline at Denmark, but also, they saw an opportunity to get engineering jobs on an international basis, and they could see you needed hydraulic studies at the same time.

So it was in the United State, in England, in the Netherlands, in Denmark, France--in the Electricité de France, because of their work on tidal electric generation mostly--but also in Japan in their Ministry of Transport is Port and Harbour Research Institute, were doing all kinds of government-sponsored research and development. So this was done worldwide, not just the United States. There's some real problems there in the fact that the Corps of Engineers have operating districts: the San Francisco District, the Los Angeles District, the Philadelphia District, the Jacksonville District, and so forth. These are employees of these districts.

So here you have your facility of Vicksburg, Mississippi, but it's pretty expensive for people to go back and forth. People don't want to leave their family and spend a year at Vicksburg and then come back. I think the Corps has been looking at this, but there has to be changes made, I think.

Swent: They also depend on the universities still to train the people that they have.

Wiegel: They always have. That's another real problem: when you cut back on the research, then you don't have the people being trained. The only thing is, counterbalancing that, is the fact that not much engineering can be done any more on the coast in the United States. So there is not much work being done except the maintenance/rehabilitation type of thing. The British privatized their laboratory about a decade ago; the Dutch privatized their laboratory; the Danish privatized their laboratory; the Japanese did not. So those three European laboratories are out on the international market trying to get as much funding as they can get and they work closely with their engineering firms to try to get contracts in Indonesia or Malaysia and so forth, to do engineering, including the hydraulic laboratory studies. In principle, we can do it here in the United States. A private company can contract with the Corps of Engineers, but--and here it's only hearsay, but I understand the overhead cost is so high that the companies tend not to do it.

All of these things have affected our ability to do research here, and at other universities, where hydraulic laboratory facilities are needed.

Swent: So the Corps does its own work.

Wiegel: It does essentially all its own--yes. I attended a meeting of the Coastal Engineering Research Board in July of 1996. It was held in San Diego. At that time, one of the things they were discussing was what is the future for the Corps in coastal engineering, because President Clinton's policy was that they shouldn't do beach work and so forth--not completely, no, but certainly not much, and I forget the details.

If the president tells you not to do that--now, Congress disagreed with the president on many individual issues, so written into law are a number of projects, and they were essentially budgetizing the proof against veto, because it's a big budget. Nobody really knows what this line-item veto that's now in existence is going to do, because again, our president--when he line-item vetoes something--is losing political ground with the local people and in some cases, two senators may be Democrats in that state, like in Virginia. That gets into politics again; but it is politics.

Anyhow, at that meeting, I recommended that the whole way of funding research in coastal engineering should be looked into, and that much more of it should be funded via the National Science Foundation and so forth. I've made my feelings known to the Marine Board. There is some movement. This may be looked into; I don't know. This is all current type of things. That would require a complete look at how this research is done for coastal science and coastal engineering in the United States. But at the same time--and I've mentioned this before--the U.S. Navy is now doing more of their planning on coastal activities because they see they're asked to go in to support the troops in the Gulf, in Bosnia--who knows where next time.

So right now you have got the navy and the army Corps working together on some of these things. I think things are in a state of flux at the present time. I think that's about what I wanted to say there about that.

Swent: It offers lots of new areas of thought, doesn't it?

Wiegel: But it has to be looked at right now. And I am hoping it will be.

Swent: Right. And you're in a position now where you can be the sage advisor from Mount Olympus.

Wiegel: I can stir people up and try to get them to do something.

Swent: Okay, Bob. The only other question I have is: did you say enough about your own archives? Would you like to say any more about your own archives? You mentioned the Water Resources Center Archives a lot, but you have your own personal--

Wiegel: --which are in the Water Resources Center Archives, but completely separate. What I do is: I now make sure the archives have whatever they would like first, and then if they find it isn't sufficiently in their purview, then I put it in my own. To give you an example: when I was preparing for one of the recent interviews, I went back and I found my whole set of reports of the California Advisory Commission on Marine and Coastal Resources, and so I took them into them, and they had many of them but others they didn't. So they took out the ones that would make their set complete--also the Governor's Commission on Marine Resources, which was prior to that.

Over the weekend, I was looking at some old things, and I found a rather large publication that I must have bought in a museum or something some time, that was the works that Leonardo da Vinci had done on different hydraulic things, both water supply and pumps and things of this sort. This morning I gave that to the archives, saying, "I think it is more important that you keep it. The author is well known: Leonardo da Vinci."

Swent: I understand that it's a very valuable resource.

Wiegel: What I'm doing now and will be doing for the next six months to a year is: I've started to catalogue the nine thousand or so 35-millimeter slides that I have.

Swent: You had mentioned that.

Wiegel: I got back and started it again. It's extremely boring to do it, but I know of no other way. Each slide is already labeled as far as date and where it was taken, but what I'm doing is looking at each one to recall: why did I take that?

Swent: Making the annotation.

Wiegel: Then I put in all this on a professional computer program that I bought called Papyrus, and this allows me to put in subject matters and things of this sort. When this is finished, then I will turn over the entire collection to the Water Resources Center Archives so that they will be available.

Swent: It's going to be a very valuable archive. It already is.

Wiegel: I hope so.

Swent: Well, I guess that pretty well wraps it up, Bob.

Wiegel: I think so.

Swent: Thank you very much again.

Wiegel: I'm sorry to be so negative about the political thing because I see no other way; they are political. It's so difficult. You had mentioned something you had just read.

Final Thoughts on Balancing Coastal Uses

Swent: Yes. This was something that I read in an environmental publication and it's talking about a particular area up in Oregon; "A bay and its uplands that still depend primarily on harvesting natural resources from private land rather than using them in a NEW WEST WAY as a scenic back drop for tourists and those fleeing urban life....environmentalists have come in who think they can create ecological sustainability through a different form of economic development." This is pulled out of context, but the unspoken assumption is that harvesting natural resources from private land--that, in this case, means logging--is bad, and the NEW WEST WAY, a "scenic backdrop for tourists and those fleeing urban life"--tourists and vacationers is--that's good. I thought it was interesting.

Wiegel: Yes. I'm a tourist myself very often.

Swent: We all love to be tourists and vacationists.

Wiegel: Dealing with the coastal things and the demands--

Swent: This was a coastal project.

Wiegel: Willapa Bay, I believe you said it was. That's just a little bit south of the--

Swent: --Columbia.

Wiegel: --Columbia River. Willapa Bay.

Swent: That's what it was, yes.

Wiegel: I know that area fairly well. It all drains locally there. When you have many tourists, you put other pressures on your environment. I'm not saying that tourists aren't better than logging, but they're both pressures. This is the multiple use. How do you sort out whether it's better to build a hotel where you have a change over every week or so forth, so many more people can use that place to get a look at the coast and so forth. Or you permit a house there. A house is going to be less intrusive, but on the other hand, many fewer people can take advantage of looking at the coast. So you have got that balance.

I don't play golf, but I'm so appreciative of people who do, because there's so many areas where the coast has been put into golf courses, and it always looks nice, and you could see across it and things of this sort. So there are other ways you can use your coast. I think that this is one of things that the California Coastal Commission tries to sort out: to try to have maximum availability of the coast to people. They recognize these different demands. How do you sort out whether a beach should be completely safe so little children can go in the water and this and that, or can somebody use a surfboard? In southern California, as you may know, there are stretches of beach where you can use surfboards at certain times of the day, but then you can't use them at other times of the day. So they're gradually learning how to sort these things out. But again, it has to be balanced.

So tourism certainly may be better than logging, but it's got its own class of problems associated with it. That's about all I can think.

Swent: I think that's a good place to stop, Bob. So thanks very much.

Wiegel: Thank you very much.

Transcribed by Estevan Sifuentes
Final Typed by Caroline Sears

TAPE GUIDE--Robert Wiegel

APPENDIX

GUEST EDITORIAL

from Shore and Beach,
July 1994

The Coast and Professor Robert L. Wiegel

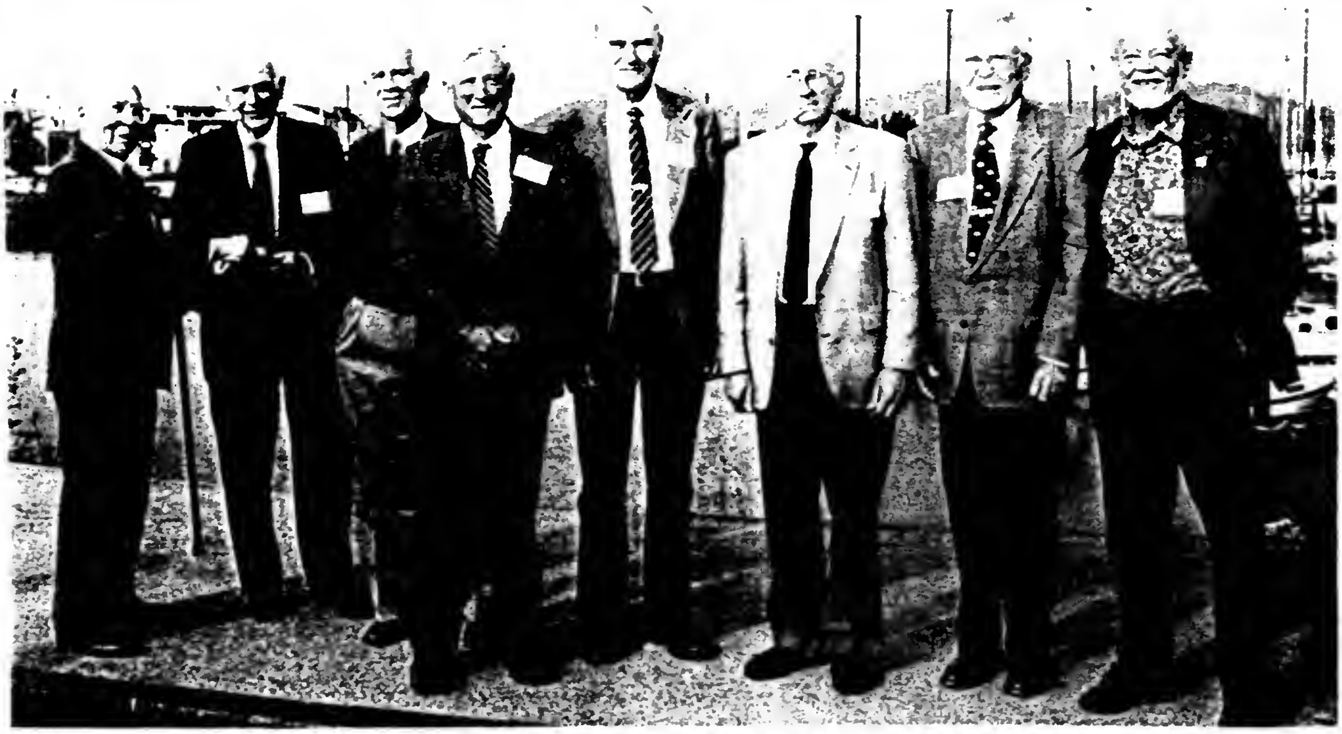
By
Thomas R. Kendall

ACCORDING TO STEPHEN ROBBINS, author of *Organizational Behavior*, the profession with the highest job satisfaction is that of the urban university professor. I would like to discuss a certain professor, from that urban setting known as Berkeley, who normally edits this journal. During his career, this coastal engineering pioneer has demonstrated a kind of job satisfaction that is contagious and has infected many. This issue of *Shore & Beach* is in honor of that Professor, Robert L. Wiegel.

This "Wiegel Issue" is the first of two issues that draw from material presented at the California Shore and Beach Preservation Association (CSBPA) conference held in Professor Wiegel's honor. The conference, entitled "The Coast and Professor Robert L. Wiegel," was held on 9-10 November 1993 at Fort Mason Center in San Francisco.

Among the presenters acknowledging Professor Wiegel's contributions to coastal engineering in California and beyond were several of coastal engineering's senior statesmen, Willard N. Bascom, Richard G. Folsom, Cyril J. Galvin, Ben C. Gerwick, Douglas L. Inman, Joe W. Johnson, Omar J. Lillevang, Orville T. Magoon, Morgan Noble, Fredric Raichlen, and Warren C. Thompson. Incidently, Dr. Folsom's title is President Emeritus of Rensselaer Polytechnic Institute but he also spent much of his academic career at the University of California at Berkeley where his long list of accomplishments included carrying out the first instrumented measurement of waves in the Pacific and hiring a young engineer named Robert L. Wiegel.

The next generation of coastal engineers was also well represented at the conference with presentations being made by such individuals as the present Director of the Coastal Engineering Research Center, Dr. James R. Houston. Professor Wiegel's contributions to industry were also recognized by Barton W. Shackleford, Retired President of Pacific, Gas, and Electric Company. And while the conference tended to have a



"Senior Statesmen" at The Coast and Professor Robert L. Wiegel Conference, held 9-10 November 1993 in San Francisco, (left to right) Omar J. Lillevang, Ben C. Gerwick, Willard N. Bascom, Morgan Noble, Warren C. Thompson, Robert L. Wiegel, Barton W. Shackleford, and Douglas L. Inman. (Photo by Craig B. Leidersdorf)

California focus, a slight international flavor was given to the gathering by Dr. Choule J. Sonu's delivery of a message from Chancellor Horikawa of Japan and by a presentation from A.W. Sam Smith of Australia.

Professor Wiegel was presented with the Professor Joe W. Johnson Outstanding Beach Preservation Award. The award was recently named after this founding father of CSBPA. Professor Johnson himself was physically unable to attend the conference but gave a talk via video tape. This video tape is now archived at the University of California at Berkeley's Water Resource Center Archives. For a good biographical sketch of Joe W. Johnson, the reader is referred to Robert L. Wiegel's article in the October 1988 issue of *Shore& Beach.*

Professor Wiegel has meant so much to so many. To me, he has been instructor, mentor, and employer and he continues to be someone I learn from. Dr. Richard J. Seymour remarked in his conference message that he has always credited Professor Wiegel with single-handedly creating the discipline of ocean engineering with his extraordinary text book (Oceanographical Engineering, 1964). Dr. Sonu recently commented that Professor Wiegel has been carrying the empirical torch for coastal engineering first lit by Morrough P. O'Brien and Joe W. Johnson. Professor Wiegel has never hesitated to acknowledge the many mysteries that still remain in this field, to place a high value on non-synthetic data, and to challenge his pupils and associates to observe coastal processes first hand.

While he recently retired from active teaching and from his role as General Chairman of the International Conference on Coastal Engineering, Professor Wiegel remains active in a variety of ways with his profession, including as an advisor to students. At least that is my perception because I still get calls from students who have been referred by him. One of his more significant ongoing responsibilities is, of course, his role as Editor of *Shore & Beach*, where he not only edits but also writes (witness his thorough treatise on beach nourishment on the U.S.'s Pacific Coast in the January 1994 issue).

This current issue of *Shore & Beach* contains reflections on Professor Wiegel by Ben C. Gerwick (the conference keynote speaker) and Choule J. Sonu, Joe W. Johnson's address on the beginnings of coastal engineering at the University of California or what he likes to refer to as the "Berkeley mafia," and four varied but excellent technical entries. The only common theme in the technical articles is that the topics are all of special interest to our honoree, who's interests are as broad as his enthusiasm is great. Again, this is only the first of two issues that will be derived from presentations made at the Coast and Professor Robert L. Wiegel Conference. With the exception of some observations from the Gold Coast of Australia, the next issue will be more regionally focussed on the shorelines near San Francisco.

Finally, I would like to acknowledge all those who have assisted thus far in the review of manuscripts, Robert G. Dean, George W. Domurat, Craig H. Everts, Reinhand E. Flick, Joe W. Johnson, Craig B. Leidersdorf, Jim McGrath, Robert M. Norris, and last but not least, Robert L. Wiegel.

Some participants at The Coast and Robert L. Wiegel Conference, CSBPA, held 9-10 November 1993 in San Francisco. (Photo by Craig B. Leidersdorf)

Keynote Address

By
Ben C. Gerwick, Jr.
Professor Emeritus,
University of California at Berkeley

THIS SYMPOSIUM HAS BEEN assembled to honor a great Coastal Engineer, Professor Robert Wiegel. It's indeed very fitting that this be carried out at this Coastal Conference and at the time of his retirement from active teaching, although I think we all know that Bob can never retire from his interest and involvement in Coastal Engineering.

The respect with which he is held by his associates and peers is shown by the prestige of the speakers and participants who have gathered to honor him. The titles of the presentations cover the many professional aspects of his career.

Since I don't want to anticipate the material of the other speakers, I will address some facets of his life that may not be known to all of you, which illustrate his remarkable breath of interests and activities.

Although I had met Bob before, I first got to know him well when I joined the University. I initiated a course in the construction of offshore structures. He attended every class, sitting well to the back, to evaluate this newcomer and to judge whether or not he should encourage his students to take my course. Fortunately, I passed his rigorous examination.

Bob is faithful in his support of other teachers, not only his fellow professors, but our guest speakers, especially those in our Ocean Engineering Symposium, in which many of you have participated. He was instrumental in the founding of this graduate seminar in Ocean Engineering and has been by far the best attendee of all of us. This wouldn't be so remarkable except as one tries to imagine how he managed to fit it into his schedule of trips to Washington for the National Academy of Engineering, overseas trips to chair the International Conference on Coastal Engineering, service on the Coastal Engineering Research Board, and a busy teaching and research schedule.

Bob has been active on the Marine Board of the National Academies and the National Research Council for as long as I can remember: an energetic participant, willing worker, but most importantly, one who continually examined and addressed the future needs of our country and society.

In the 1970's Bob participated in the founding of a very idealistic international organization, ECOR, the Engineering Commission for Ocean Resources, to be under the auspices of the UN. It was a noble attempt and for awhile motivated many outstanding ocean engineers in both developed and less developed nations worldwide: several excellent conferences were held throughout the world. I attended two in Argentina at which all the South American coastal nations were well represented. Unfortunately ECOR has diminished as an effective organization, not because of lack of interest at the academic and research level but because it was radicallyupstaged by the development of offshore oil activity in the 1970's and 80's and taken over by the Offshore Conferences, and by ASME's Offshore Mechanics Group and ASCE's Ocean Engineering Section. Everyone wanted to get into the act.

Bob, of course, is known for his pioneering efforts in the quantification of wave energy spectra. When the North Sea offshore concrete platforms first emerged, there was a great divergence as to which spectra to adopt. Bob developed a modification of the Scott spectrum, which was adopted for all the early North Sea platforms and, as far as I know, is still used. Similarly, when interest developed in the Gulf of Alaska, Bob integrated the Japanese data with those of the Naval Oceanographic Center at Monterey to produce spectra applicable to the Gulf of Alaska. He was one of the first to recognize the existence of significant energy in the long period range in the Pacific- 20 to 22 seconds and the distances to which this low frequency energy could propagate.

Bob produced his epic, Oceanographical Engineering, in 1964 and it is still the bible. I can visualize the long nights he put in writing such a comprehensive book and the pains he must have taken to review, evaluate, and reference every credible source.

That brings us to the matter of references. Bob is the terror of doctoral students, although eventually they realize that a great teacher must be demanding. Bob will not tolerate unreferenced statements and opinions unless they are backed by appropriate data and argument. Loose assertions will not pass his scrutiny.

My next venture with Bob was in connection with the effect of earthquakes on massive offshore structures such as the concrete platforms in the North Sea, and later, those proposed for the Gulf of Alaska. Bob recognized the inapplicability of the conventional assumptions as to the added mass coefficient. With one of his brilliant doctoral students, Bob Byrd, and interaction with the rest of the dissertation committee, which included Professor Joe Penzien and myself, he confirmed the previous analytical work by Professor Jim Garrison, which had postulated coefficients approaching 0.5 for large structures, as opposed to the conventional 1.0. The results of this research are still in use in the North Sea and more recently, offshore Newfoundland.

Bob was interested early on in tsunamis and solitary waves formed by landslides into bodies of water, such as those which have caused such catastrophic failures in Italy. The possibility that a massive underwater explosion could generate a tsunami led to Bob's initial employment at the University. The Navy was about to conduct the nuclear explosion test at Bikini atoll. They were worried that a landslide down the underwater slopes could lead to a tsunami such as that which had caused such a loss of life in Hilo in 1946.

This led to his being appointed as one of the members of the Hilo Technical Tsunami Advisory Council in 1965. Bob has been a leader in alerting our profession to the dominant role played by episodic events which impact our coasts: tsunamis, great storms and great floods with their massive discharge of sediments.

His chapter on tsunamis in the volume "Earthquake Engineering" is a classic exposition of the subject. This work led him to the coasts of Canada and Alaska, where the impact of landslides initiated by earthquakes has been more devastating in terms of loss of life and property than those due to earthquakes themselves.

Bob was one of the organizers of the week-long short course on Earthquake Engineering in 1965. He had organized this through University Extension, the first such short course ever held at Berkeley. It was a tremendous success: they had so many enrollees that they gave it twice. At its conclusion, Bob was asked if he would assemble and edit the papers: thus arose the classic volume "Earthquake Engineering".

His enterprise with the University led later to a similar conference, also highly successful, in Ocean Engineering. As a result, Bob was appointed Chairman of the University-wide Committee on University Extension the next year and has continued a strong interest and involvement in this means of continued interaction with the practicing profession.

Bob in yet another activity was Director of the State Technical Services Program from 1965 to 1968, working with the Governor's office and an advisory committee of leading industrialists such as David Packard to investigate means of transferring new ideas from the research community to industry, a pioneering effort in technology transfer that still is a key theme of the National Academy of Engineering.

Have you noticed how many activities occurred in 1965? That must have been a very productive year!

Bob has had a long term love affair with coastal development, especially marinas and breakwaters. His ideal vacation is to drive along 1,000 miles of beaches in Italy or California or Florida with his wife Anne, looking at the efficacy, or more often, lack of efficacy, of breakwaters, jetties and groins and the positive and negative aspects of intensive coastal development.

Often on these trips he is diverted by another interest, that of marine archeology, especially as it relates to ancient harbors in the Mediterranean where so much information concerning the early empires has been uncovered in recent years. This interest extends to more recent history: a few weeks ago he sent me an article on John Smeaton, the designer-builder of the Eddystone Lighthouse on the coast of England. As a result of his several remarkable successes, Smeaton became the first Engineering Consultant, for the article goes on to say, "Thus we can picture Smeaton, travelling around the country to various sites, working on reports and scientific investigations, making designs for and advising on all kinds of projects, and giving evidence to parliamentary committees, proud to call himself an Engineer, conscious of the obligations of his exalted position in the profession."

Does not this also describe Robert Wiegel?

Incidently it was Smeaton who, in 1750, first recognized the tremendous upward forces exerted by waves impacting an overhanging structure, in his case, the Eddystone lighthouse cornice, which had been broken off by the course. Two hundred and thirty five years later, Bob recognized that the initial design of the Fisherman's Wharf breakwater would experience similar excessive upward wave forces on the horizontal overhang, and demonstrated this in the wave tank at Berkeley. The overhanging concrete ledge was modified to give a 10° slope, thus minimizing the momentum change.

Bob's interest in and identification with history never ceases. On his last trip, just a few weeks ago, to Genoa, Italy he discovered that the ancient mole had been totally destroyed in the great storm of February, 1495. The investigation, the first recorded oceanographical study, was carried out by one Leonardo da Vinci.

Addressing yet another aspect of his career, Bob has been intensely devoted to education and the University. He has served many years on committees of the Academic Senate at Berkeley. He continues to devote time to the Committee on ROTC, carrying forward his own career in the ROTC and the Army of almost 50 years ago. He served his country in World War II, returning to work with Dean Morrough O'Brien in perfecting landing craft and techniques for landing in the surf. That's especially dear to me because I spent much of World War II careening in the surf in landing craft and attempting to get off to safety under the exigencies of enemy fire and breaking waves. Needless to say, there was ample room for improvement in equipment and technique, as well as the forecasting of surf conditions.

What constitutes engineering genius? I think it's the gift of intense interest in all aspects of his own discipline, in related disciplines, in all natural phenomena. But most of all, it's the priceless gift of enthusiasm which is contagious and makes us all perform better. So, thank you Bob Wiegel, and don't stop.

Bob and Anne Wiegel on the Great wall of China, 9 Sept. 1985 (Photo by Jerry Eisenberg.)

Robert L. Wiegel in his office just before retirement from the University of California, May 1987. (Photo by Anon.)

Morrough P. O'Brien, Joe W. Johnson and Robert L. Wiegel at the "O'Brien Symposium", 23-24 March 1987. (Photo by Anon.)

Professor Wiegel's Legacy

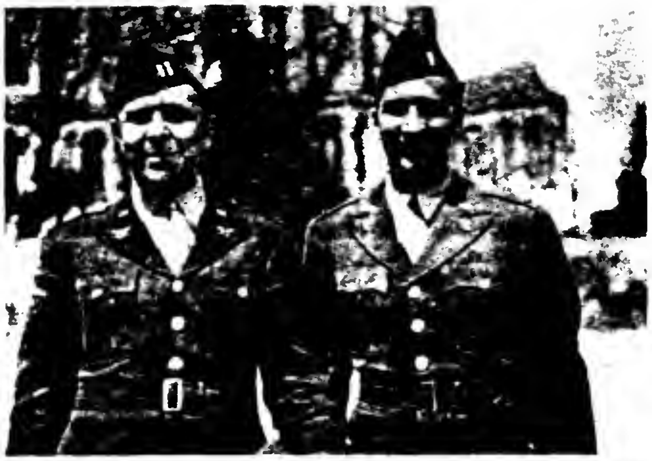
By

Choule J. Sonu

President, Techmarine, Inc.

THE CSBPA MEETING HELD in San Francisco last November in which Professor Bob Wiegel's contributions to coastal engineering were the featured topic of discussion, gave me an occasion to reminisce about the importance of this man to my career of some 30 years as a coastal engineer in this country. When Jim Houston, Director of CERC, confessed at the meeting that in his professional work he relied more heavily on Bob's 1964 book "Oceanographical Engineering" than on the CERC manual, I cried to myself "welcome to the club" and felt that like Jim, I also owe him a confession.

I have not had the fortune to attend Berkeley nor any other U.S. schools, yet I have profited as handsomely by being a loyal student of Bob's writings. Aside from providing practical knowledge, Bob's writings have always reminded me of what the main stream of coastal engineering is all about - the respect of facts and experience, rather than of boldness and deductions. It was always inspiring to learn through his papers how little, rather than how much, we know abut Nature's processes. We would never succeed in outguessing the Mother Nature, yet advancements in coastal engineering will still come in steps with our ability to digest the lessons handed down by her. Bob's work is the hallmark of this basic principle of main stream coastal engineering, and for this reason many of his contributions will stand the test of time, as has his 1964 book.



J.M. Wiegel and Robert L Wiegel in front of the Cathedral in Le Mans, France, about 1 July 1945. (Photo by Anon.)

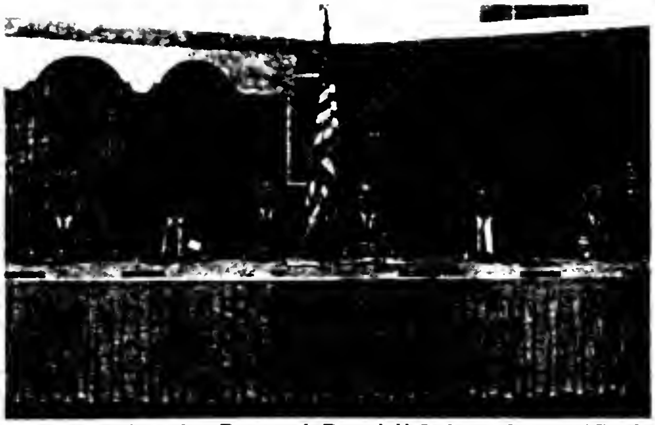
C.M. Snyder, R.L. Wiegel, H.A. Einstein and Joe W. Johnson, in front of Spengers Fish Restaurant, Berkeley, Oct. 1957. (Photo by Anon.)

Robert L Wiegel in Papua New Guinea, 20 July 1975

R. L. Wiegel, R. Adm. Doyle and M.P. O'Brien at Tests of Amphibious Vehicles, Fort Ord, California, February 1950.

Demolition of the Old Hydraulics Laboratory (Mechanics Building), University of California, 1965. (Photo by Anon.)

Billy L. Edge and R.L. Wiegel at the 16th ICCE, Hamburg, Germany, Sept. 1978. (Photo by Anon.)



Coastal Engineering Research Board, U.S. Army Corps of Engineers, about 1980. Left to right: B.G. Robinson, R.L. Wiegel, MG Lewis, MG Heiberg, R.G. Dean, BG Wells (Photo courtesy Corps of Engineers, U.S. Army)

Coastal Engineering Research Council Meeting in New York, NY. Standing, left to right: Dean, Magoon, Battjes. Seated: Bijker, Noble, Saville Jr., Wiegel. 25 Sept. 1989. (Photo by Anon.)

Field tests at Diablo Canyon Nuclear Power Plant, Pacific Gas & Electric Co. 1986. (Photo, courtesy PG&E)

Opening Ceremony, 22 ICCE, Delft, The Netherlands. Left to right. Egbert Prins, unknown, Prince Klaus, R.L. Wiegel, unknown, unknown. 2 July 1990. (Photo courtesy, ICCE Organizing Committee)

Opening day, 23rd ICCE, Venice, Italy, 5 October 1992 Venice flooded. (Photo by Italian Organizing Committee)

11 March 1997

Biodata for:

APPENDIX D

ROBERT L. WIEGEL
Professor of Civil Engineering, Emeritus
University of California
Berkeley, California 94720

Born: October 17, 1922, San Francisco, California

Education:

B.S. Mechanical Engineering, University of California, Berkeley, 1943
M.S. Mechanical Engineering, University of California, Berkeley, 1949

Professional Experience:

1942-46	U.S. Army, Ordnance Corps, to 1st Lieutenant
1946-60	Assistant and Associate Research Engineer, University of California, Berkeley
1957-60	Lecturer in Mechanical Engineering, UC Berkeley
1960-63	Associate Professor of Civil Engineering, UC Berkeley
1963-87	Professor of Civil Engineering, UC Berkeley
1963-72	Assistant Dean, College of Engineering, UC Berkeley
1965-68	Director, State Technical Services Program for California, UC, University-wide
1972-73	Acting Dean, College of Engineering, UC Berkeley
1987-present	Professor Emeritus

Professional Registration:

Registered Professional Engineer in the State of California

Professional and Scientific Societies:

- Member, National Academy of Engineering (Elected 1975)
  - Member, Committee on Membership, 1978-83
- President (Founding), International Engineering Committee on Oceanic Resources (ECOR), 1972-75. Honorary Member, 1987-
- Honorary Member and Fellow, American Society of Civil Engineers
  - Chairman, Executive Committee, Waterways, Harbors and Coastal Engineering Division (1974-75); Member, 1971-76
  - Chairman, Coastal Engineering Research Council, 1978-1992
  - Chairman, Project Steering Committee, U.S./Japan Exchange Program of Eminent Civil Engineers--Underwater, 1977-78
  - Chairman, Task Committee on Wave Forces on Structures, 1963-64
  - Member, Task Committee on Sand Bypassing, 1964-65
  - Member, Task Committee on Water Resources (previously)
  - Member, Committee on Oceanographic and Hydrographic Surveying and Charting (previously)
  - Member, Committee on Reliability of Offshore Structures (previously)
- Member, American Shore and Beach Preservation Association, Vice President and Director, 1988-1995. Director Emeritus 1995-
- Member, Permanent International Association of Navigation Congresses
- Member, Sigma Xi
- Fellow, American Association for the Advancement of Science

Service to Professional Societies and Government Agencies:

- Chairman, First Conference on Coastal Engineering Instruments, Council on Wave Research, The Engineering Foundation, held in Berkeley, CA, 1955
- Hilo Technical Advisory Council, Board of Supervisors, Hawaii County, Hawaii, 1962
- Vice Chairman, Local Organizing Committee, 13th General Assembly, International Union of Geodesy and Geophysics, Berkeley, California, 1963
- Member, Papers Committee and Wave Forces Committee, Permanent International Association of Navigation Congresses (PIANC), 1963-65
- Member, U.S.-Japan Cooperative Science Program: Participant Coastal Engineering Seminar in Japan, March 1964; Tsunami Seminar in Japan, 1965
- Consultant, Redevelopment Agency of Crescent City, CA, protection from tsunamis, 1964-65
- Member, American Society of Engineering Education; member Executive Committee, Pacific Section, 1965-66
- Member, San Francisco Bay Area Relations with Industry Committee, 1963-64
- Visiting Professor, National University of Mexico, Mexico DF, July/August 1965
- Member, Panel on Research Education and Information Dissemination, Committee on Ocean Engineering, National Academy of Engineering, 1966-69
- Commissioner, California Governor's Advisory Commission on Ocean Resources (GACOR), 1967-68
- Member, Committee on Earthquake Engineering, National Academy of Engineering, 1967-69, Chairman, Panel on Coastal Engineering and Inland Waters
- Invited Lecture: "Protection of Populated Areas from Tsunamis," Pacific Science Congress, Tokyo, Japan, 1966
- Commissioner, California Advisory Commission on Marine and Coastal Resources (CMC), 1968-74
  - Chairman, Committee on Marine Transportation, Non-living Resources and Power Plants
  - Chairman, Committee on Transportation and Trade
- Member, Steering Committee: International Decade of Ocean Exploration, National Academy of Science and National Academy of Engineering, 1968-69
- Chairman, Ad Hoc Panel on the International Working Group, Committee on Ocean Engineering (Ex-officio Member, National Academy of Engineering) 1968-70
- Lecture on tsunamis at San Francisco Meeting of Office of Emergency Preparedness, Geological Hazards and Public Problems, Executive Office of the President, May 1969
- Reporter-General, XXII International Navigation Congress, Paris, France, June 1969
- Official U.S. Department of State Observer to Meeting of UNESCO's Intergovernmental Oceanographic Commission, 1969
- U.C. Representative, Consortium Committee, Kanpur Indo-American Program, 1969-71
- Invited Lecture, 8th World Petroleum Congress, Moscow, USSR, 1971
- Member, Editorial Board, Journal of Coastal Marine Science, Academic Press, Inc., Ltd., London, from founding in 1971 until 1988
- Member, Editorial Board, International Association for Hydraulic Research, 1971-75
- Member, Editorial Board, Ocean Citation Journal, Ocean Index, 1971-77
- Ex-Officio Member, Marine Board of National Academy of Engineering, 1971-75
- Member, U.S. Navy, Office of Naval Research, Natural Hazards Review Board, 1973-74
- Member, Honorary Committee for the International Ocean Development Conference, Japan, 1973-74
- Member, Coastal Engineering Research Board, Chief of Engineers, U.S. Army, 1974-85

- Member, Marine Board, National Academy of Engineering, National Research Council 1975-81
  - Chairman, Panel on Harbor/Port Entrance Design, 1980-81
  - Chairman, Panel on Verification Guidelines for Offshore Structures, 1977-78
- Member, Advisory Panel on the International Decade of Ocean Exploration (IDOE), National Science Foundation
- Consultant to US. Committee on International Ocean Affairs (CIOA) and Panel on International Programs and Cooperation in Ocean Affairs (PIPICO), U.S. Department of State, 1976-80
- Lecture Series, Design of Small Harbors, Indian Institute of Technology, Bombay, India, January 1980
- General Chairman, Seventeenth International Conference on Coastal Engineering, Sydney, Australia, 23-28 March 1980
- General Chairman, Symposium on Problems and Opportunities on the Design of Entrances to Ports and Harbors, National Research Council, Washington, D.C., August 1980
- Chairman, International Conference on Directional Wave Spectra Applications, ECOR/Marine Board/ASCE, 1982
- Member, National Research Council, Panel of the Committee on Earthquake Engineering, 1981-82
- U.S. Representative, UNESCO/IOC/ECOR Workshop on Advanced University Curricula in Ocean Engineering and Related Fields, 1982
- General Chairman, Eighteenth International Conference on Coastal Engineering, Cape Town, Republic of South Africa, 14-19 November 1982
- Chairman, Committee on Assessment of the Maritime Administration's Computer-Aided Operations Research Facility (CAORF), National Research Council, 1983
- Member, Advisory Council of the Permanent Secretariat of the International Conferences on Coastal and Port Engineering in Developing Countries, 1984-1995
- Member, Ad Hoc Committee on Natural Hazards and Research Needs in Coastal and Ocean Engineering, Civil and Environmental Engineering Division, National Science Foundation, and the Office of Naval Research, 1984
- General Chairman, Nineteenth International Conference on Coastal Engineering, Houston, Texas, 3-7 September 1984
- Member, National Research Council Committee on Engineering Implications of Changes in Relative Mean Sea Level, 1984-87
- Member, Monterey Bay Shoreline Study: Scientific and Technical Advisory Committee, State of California Resources Agency, Department of Boating and Waterways, 1985-86
- Member (University of California Representative), Marine Division, National Association of State Universities and Land-grant Colleges, 1985-
- Member, Planning Committee on International Conference on Measuring Techniques of Hydraulic Phenomena in Offshore Coastal and Inland Waters, London, England, 9-11 April 1986
- General Chairman, Twentieth International Conference on Coastal Engineering, Taipei, Taiwan, Republic of China, 9-14 November 1986
- General Chairman, Symposium to Honor Morrough P. O'Brien: Working Solutions : Shore and Beach, University of California, Berkeley, California, sponsored jointly by the American Shore and Beach Preservation Association, the ASCE Coastal Engineering Research Council, the ASCE Waterway, Port, Coastal and Ocean Engineering Division, and the University of California at Berkeley College of Engineering, 23-24 March 1987
- Member, Technical Review Committee for Bolsa Chica Bay, City of Huntington Beach, California, 1988-91
- Member, Committee on Coastal Erosion Zone Management, National Research Council, 1988-89

- General Chairman, Twenty-first International Conference on Coastal Engineering, Torremolinos, Spain, 20-25 June 1988
- Editor *Shore & Beach*, The Journal of the American Shore and Beach Preservation Association, 1988-January 1996
- Member, Steering Committee: Hurricane Hugo Workshop, ASBPA, 21-22 May 1990, Folly Beach, North Carolina
- General Chairman, Twenty-second International Conference on Coastal Engineering, Delft, The Netherlands, 2-6 July 1990
- Adviser, City of Redondo Beach, California on Redondo (King) Harbor, 1991
- Adviser, U.S. Army Corps of Engineers, Coastal Engineering Research Center, on Yaquina Jetty, Oregon, 1990-91
- General Chairman, Twenty-third International Conference on Coastal Engineering, Venice, Italy, 4-9 October 1992
- Member, Committee on Beach Nourishment and Protection, National Research Council (Marine Board), 1992-1994
- Reviewer, "Shoreline Erosion Assessment and Atlas of the San Diego Region" for the San Diego Association of Governments (SANDAG), 1993
- Member, Advisory Committee, HYDRO-PORT '94, Yokosuka, Japan, October 1994
- Chairman, Peer Review Group: Assessment of the State of Florida's Coastal Construction Control Line (CCCL) Program, for the University of Florida and the State of Florida's Department of Environmental Protection, 1993-1995
- Member, Review Committee, Barrow Beach Nourishment Project, North Slope Borough, Alaska, Science Advisory Committee, July-August 1994
- Consultant, U.S. Army Corps of Engineers, Los Angeles District, Coast of California Storm and Tidal Waves Study, South Coast Region (Orange County), 1996

Honors and Awards

1962	Research Prize, American Society of Civil Engineers for relating oceanography to the practice of civil engineering
1965	Special Lecture, Strategic Industries Association, Los Angeles, California
1968	Listed in Who's Who
1969	General Lecture, Symposium to Dedicate the Delft Hydraulics Laboratory Facilities, Delft, The Netherlands
1969	U.S. Department of State Observer to UNESCO's Intergovernmental Oceanographic Commission
1971	Participant, White House and NATO sponsored "International Meeting on Earthquakes"
1972	General Lecture, Symposium on Flow-induced Structural Vibrations, IUTAM-IAHR, Karlsruhe, Germany
1973	USC-TRW Distinguished Lecturer
1974	Listed in "Engineers of Distinction: A Who's Who in Engineering," The Engineers Joint Council
1975	Elected Member, National Academy of Engineering
1975-77	Adviser to the Secretary of Minerals and Energy, Papua New Guinea
1976	Theme Lecture, Symposium on Present-Day Challenge of the Sea, Wageningen, The Netherlands
1976	Visiting Professor, Polish Academy of Science
1976	Participant, National Meeting on Science, Technology and Development, Chaired by the Secretary of State
1977	Senior Queens Fellow in Marine Science, Australia

1977	Keynote Lecture, 6th Australasian Conference on Hydrualics and Fluid Mechanics, Adelaide, SA, Australia
1978	Lecture to Directors of the Suez Canal Authority, Egypt
1978	Visiting Professor, University of Cairo, Egypt
1978	Moffatt-Nichol Harbor and Coastal Engineering Award, ASCE
1979	Elected Fellow, American Association for the Advancement of Science
1979	Special Lecture, National Institute of Oceanography, Goa, India, 31 December
1980	General Lecture, Ocean Engineering IV, Tainan, R.O.C.
1981	General Lecture, XIX Congress, IAHR, New Delhi, India
1983	Special Advisor to Egypt and UN Development Program on the Coastal Protection Plan for the Nile Delta, 1983-85
1984	Keynote Address, ECOR International Conference--1984: Ocean Energy, Its Potentialities for Developing Countries, Buenos Aires, Argentina, 2-5 October 1984
1985	Outstanding Civilian Service Medal, U.S. Army Corps of Engineers
1985	International Coastal Engineering Award, American Society of Civil Engineers
1986	Keynote Address, International Conference on Measuring Techniques of Hydraulic Phenomena in Offshore, Coastal and Inland Waters, London, England, 9-11 April 1986
1986	Luncheon Talk, Hawaii Conference on Beach Erosion, Honolulu, 27 June 1986
1987	Berkeley Citation, University of California at Berkeley
1987	Theme Lecture, Second International Conference on Coastal and Port Engineering in Developing Countries, Beijing, China, September 1987
1987	Honorary Member, International Engineering Committee on Oceanic Resources. Elected 19 October 1987, London, England
1988	General Lecture, Giornate Di Studio Sullo Stato Dell'Arte Nell'Ingegneria Della Coste, Bari, Italy, 24-25 March 1988
1988	Keynote Address, Beach Preservation Technology 88: Problems and Advances in Beach Nourishment, Gainesville, Florida 23-25 March 1988
1988	Visiting Professor, Polish Academy of Sciences, 29-June-8 July 1988
1988	Honorary Member, American Society of Civil Engineers
1992	Coastal Engineering 1992. Proceedings of the Twenty-third International Conference, October 4-9, 1992, Venice, Italy. Dedicated to Professor Robert L. Wiegel, ASCE, 3 volumes, 3,516 pages
1993	The Second International Symposium on Ocean Wave Measurement and Analysis. Honoring: Professor Robert L. Wiegel, July 25-28, 1993, New Orleans, Louisiana, USA. ASCE Specialty Conference
1993	Coastal Zone Foundation Award. Professor Robert L. Wiegel - in recognition of his leadership and inspiration in ocean wave measurement, theory and application at WAVES 93, New Orleans, LA, July 26-29, 1993
1993	ASCE Special Award. "In recognition of his pioneering contributions to coastal and ocean engineering research and for inspiration provided to his students and colleagues - presented to Professor Robert L. Wiegel, July 1993
1993	Giornate Italiane di Ingegneria Costera, held in Genoa, Italy, 28-29 October 1993; opened the conference and gave opening remarks, by invitation
1993	The Coast and Professor Robert L. Wiegel, CSBPA Annual Conference, November 9-10, 1993, Fort Mason Center, San Francisco, California American Shore and Beach Preservation Association/California Section
1993	Joe W. Johnson Outstanding Beach Preservation Award, California Shore & Beach Preservation Association
1994	Conference Room at Coastal Engineering Research Center, U.S. Army Corps of Engineers, Vicksburg, MS, named the Robert L. Wiegel Conference Room

1994 Two issues of the journal *Shore & Beach* were dedicated to Robert L. Wiegel, publishing the papers given at the conference held in his honor in November 1993. The issues were: Vol. 62, No. 3, July 1994, pp. 2-52 and Vol. 62, No. 4, October 1994, pp. 1-36.

1995 Morrough P. O'Brien Award, American Shore and Beach Preservation Association

1996 Keynote Address, 25th International Conference on Coastal Engineering, Orlando, FL, 2-6 September 1996

1996 Honorary Member, Japan Society of Civil Engineers

Publications:

More than 155 publications and 100 technical reports on various phases of ocean engineering, including the book *Oceanographical Engineering* and editor of the books *Coastal Engineering Instruments, Earthquake Engineering*, and *Directional Wave Spectra Applications*. Also, a number of consulting reports. In addition, supervised 26 Ph.D. theses.

Note: In addition to the work presented in publications, hydraulic model tests (towing tank) were performed in 1954 as a part of the pre-feasibility studies of the underwater launching of the Polaris missile for the U.S. Navy.

Main Fields of Interest:

Nearly all areas of coastal engineering, including: cooling water systems for power plants, wave forces on offshore structures and pipelines, beach erosion control, harbor arrangements (breakwaters, wave and currents, entrances, etc.), tsunamis, ocean wave spectra.

Major University of California Committee Memberships:

- Office of the President
  - Executive Committee, Institute of Marine Resources, 1966-72
  - Task Committee on the Extended University, 1969-72
  - Chairman, Study Group One, 1970 All-University Faculty Conference, 1969-70
  - U.C. Representative to the Kanpur Indo-American Program, 1969-72
  - Sea Grant Coordinating Council, 1970-74
  - Chairman, Search Committee, Director of IMR, 1970-71
  - Universitywide Council on Engineering Education, 1972-73
  - Coordinating Board, Water Resources Center, 1982-87
  - Chairman, Search Committee, Director of the Water Resources Center, 1986
  - Chairman, Institute of Marine Resources 5-Year Review Committee, 1987

- Chancellor
  - Advisory Committee to the Student and Alumni Placement Center, 1964-70; Chairman, 1968-70
  - Committee for Professional Education, 1969-73
  - Executive Committee, Earthquake Engineering Research Center, 1970-74
  - Chairman, Ad Hoc Review Committee, Center for Research in Management Science, 1976
  - Advisory Committee to the Military Officers' Education Program, 1978- ; Chairman, 1987-88, 1992-93. Member, Committee on Fleet Admiral Nimitz Memorial Lectureship on National Security Affairs, 1985-

- Faculty Club
  - Board of Directors, 1971-73
  - President, 1973

- Academic Senate
    - University Extension, Chairman, 1965-67
    - Chairman, Universitywide Committee on University Extension, 1966-67, 1969-72
    - Chairman, Rules and Jurisdiction, 1968-70, 1976-77, 1978-81
    - Universitywide Committee on Rules and Jurisdiction, 1976-77, 1978-81
    - Parliamentarian, 1978-79
    - Committee on Committees, 1982-84
    - Committee on Privilege and Tenure, 1986-87
    - Secretary, 1988-89

- College of Engineering
    - Dean's Coordinating and Advisory Council, 1964-73
    - Committee on Engineering in the Ocean Environment, 1964-70, 1971-72, 1975- ; Chairman 1984-87
    - Committee on TV, 1973-74
    - Executive Committee, Earthquake Engineering Research Center, 1975-80
    - Interdisciplinary Studies Advisory Committee, 1984-87
    - Collegewide Working Group on Fluid Mechanics, 1987-88

- Department of Civil Engineering
    - Executive Committee, 1975-77
    - Committee on Alumni and Professional Relations, 1978-85
    - Graduate Affairs Committee, 1981-85
    - Numerous other committees

ROBERT L. WIEGEL - PUBLICATIONS AS OF 31 DECEMBER 1996

1 thesis, 157 published books and papers, 97 technical reports

A. Thesis

1. *Some Studies of Surface Waves in Shoaling Water,* M.S. Thesis in Mechanical Engineering, December 1948.

B. Published Papers and Books

1. "Oscillatory Waves: Diagrams and Tables of Relationships Commonly Used in Investigations of Surface Waves," *Bulletin of Beach Erosion Board,* Corps of Engineers, U. S. Army, Special Issue No. 1, July 1948, by R. L. Wiegel.

2. "The Measurement of Wave Heights by Means of a Float in an Open-End Pipe," *Trans. Amer. Geophys. Union* vol. 30, no. 4, pp. 501-506, Aug. 1949, by J. D. Isaacs and R. L. Wiegel.

3. "An Analysis of Data from Wave Recorders on the Pacific Coast of the United States," *Trans. Amer. Geophys. Union,* vol. 30, no. 5, pp. 700-704, Oct. 1949, by R. L. Wiegel.

4. "Experimental Study of Surface Waves in Shoaling Water," *Trans. Amer. Geophys. Union,* vol. 31, no. 3, pp. 377-385, June 1950, by R. L. Wiegel.

5. The Thermopile Wave Meter," *Trans. Amer. Geophys. Union,* vol. 31, no. 5, pp. 71-716, Oct. 1950, by J. D. Isaacs and R. L. Wiegel.

6. "Southern Swell Observed at Oceanside, California," *Trans. Amer. Geophys. Union,* vol. 31, no. 5, pp. 717-722, Oct. 1950, by R. L. Wiegel and H.L. Kimberley.

7. "Elements of Wave Theory," *Proc. First Conf. on Coastal Engineering,* Council on Wave Research, The Engineering Foundation, Ch. 2, pp. 5-21, 1951, by R. L. Wiegel and J. W. Johnson.

8. *Manual of Amphibious Oceanography,* Pentagon Printing Office, 1750 pp., R. L. Wiegel, Project Engineer and Editor.

9. "Near-Coastal Storms and Associated Waves," *Trans. Amer.Geophys. Union,* vol. 33, no. 2, pp. 217-225, April 1952, by D. K. Todd and R. L. Wiegel.

10. *Waves, Tides, Currents and Beaches: Glossary of Terms and List of Standard Symbols,* Council on Wave Research, The Engineering Foundation, 113 pp., July 1953, by R. L. Wiegel.

11. *Gravity Waves, Tables of Functions,* Council on Wave Research, The Engineering Foundation, 30 pp., Feb. 1954, by R. L. Wiegel.

12. "Wave, Longshore Current, and Beach Profile Records for Santa Margarita River Beach, Oceanside, California, 1949," *Trans. Amer. Geophys. Union,* vol. 35, no. 6, pp. 887-896, Dec. 1954, by R. L. Wiegel, D. A. Patrick and H. L. Kimberley.

13. "Amphibian Tractors in the Surf," *Proc. First Conf. on Ships and Waves*, Council on Wave Research, The Engineering Foundation, and the Society of Naval Architects and Marine Engineers, pp. 397-422, 1955, by D. A. Patrick and R. L. Wiegel.

14. "Laboratory Studies of Gravity Waves Generated by the Movement of a Submerged Body," A.I.O.P., *Proc. - verb. no. 6*, pp. 246-247, 1955, (see No. 15 for full paper on subject), by R. L. Wiegel.

15. "Laboratory Studies of Gravity Waves Generated by the Movement of a Submerged Body," *Trans. Amer. Geophy. Union*, vol. 36, no. 5, pp. 759-774, Oct. 1955, by R. L. Wiegel.

16. "Wave Transformation in Shoaling Water," *Trans. Amer. Geophys. Union*, vol. 36, no. 6, pp. 975-984, Dec. 1955, by R. L. Wiegel and R. A. Fuchs.

17. *Proceedings of the First Conference on Coastal Engineering Instruments*, Council on Coastal Engineering Instruments, Council on Wave Research, The Engineering Foundation, 302 pp., 1956. R. L. Wiegel, Conference Organizer and Editor.

18. "Thermopile Water Meter," *Proc. First Conf. on Coastal Engineering Instruments*, Council on Wave Research, The Engineering Foundation, pp. 101-110, 1956, by R. L. Wiegel and J. D. Isaacs.

19. "Parallel Wire Resistance Wave Meter," *Proc. First Conf. on Coastal Engineering Instruments*, Council on Wave Research, The Engineering Foundation, pp. 39-43, 1956, by R. L. Wiegel.

20. "The Design Wave in Shallow Water," *Proc. ASCE*, vol. 82, Rep. no. 910, 21 pp., March 1956, by R. L. Wiegel and K. E. Beebe.

21 "A Laboratory Study of Short-Crested Wind Waves," U. S. Army, Corps of Engineers, *Beach Erosion Board, Tech. Memo. 140*, 81, 28 pp., June 1956, by G. C. Ralls, Jr., and R. L. Wiegel.

22. "Ocean Wave Forces on Circular Cylindrical Piles," *Jour. Hyd. Div.*, ASCE, vol. 83, no. 1199, 35 pp., April 1957, by R. L. Wiegel, K. E. Beebe and James Moon. Also issued in Transactions of the ASCE, vol. 124, pp. 89-116, 1959.

23. "Many Companies Now Considering Moored Ships Rather Than Fixed Platforms," *Offshore Drilling*, vol., 4, no. 4, p. 28, April 1957, by R. L. Wiegel.

24. "Model Study of Floating Drydock Mooring Force," *Proc. Symposium on the Behavior of Ships in a Seaway*, Wagoningen, Netherlands, pp. 563-583, Sept. 1957, by R. L. Wiegel, R. W. Clough, R. A. Dilley, and J. B. Williams. Also published in International Shipbuilding Progress, vol. 6, no. 56, pp. 147-159, April 1959.

25. "Wave Measurements along the California Coast," *Trans. Amer. Geophys. Union*, vol. 38, no. 5, pp. 667-674, Oct. 1957, by R. L. Wiegel and J. Kukk.

26. Discussion of "Properties of Shoaling Waves by Theory and Experiment," *Trans. Amer. Geophys. Union*, vol. 38, no. 5, pp. 760-761, Oct. 1957, by R. L. Wiegel.

27. "Model Study of Wave Refraction," Beach Erosion Board, U. S. Army, Corps of Engineers, *Tech. Memo. No. 103*, 31 pp., Dec. 1957, by R. L. Wiegel and A. L. Arnold.

28. "Breaking Wave Force Prediction," *Jour. Waterways and Harbors Div.*, ASCE, vol. 84, Paper no. 1573, 14 pp, March 1958, by R. L. Wiegel and R. E. Skjei.

29. "Water Gravity Waves Generated by a Moving Low Pressure Area," *Trans. Amer. Geophys. Union*, vol. 39, no. 2, pp. 224-236, April 1958, by R. L. Wiegel, C. M. Snyder and J. B. Williams.

30. "Model Studies on the Dynamics of an LSM Moored in Waves," *Proc. 6th Conf. on Coastal Engineering*, pp. 844-877, June 1958, by R. L. Wiegel, K. E. Beebe and R. A. Dilley.

31. "Laboratory Facilities for Studying Water Gravity Wave Phenomena,"*Proc. 6th Conf. on Coastal Engineering*, pp. 231-251, June 1958, by C. M. Snyder, R. L. Wiegel and K. J. Bermel.

32. "Model Study of Floating Drydock Mooring Forces," International Shipbuilding Progress, Vol. 6, No. 56, April 1959, pp. 147-159, by R. L. Wiegel, R. W. Clough, R. A. Dilley, and J. E. Williams.

33. "Sand Bypassing at Santa Barbara, California," *Jour. Waterways and Harbors Division*, ASCE, vol. 85, no. WW2, Paper no. 2066, pp. 1-30, June 1959, by R. L. Wiegel.

34. "Model Study of Mooring Forces on Docked Ship," *Jour. Waterways and Harbors Divisin*, ASCE, vol. 85, no. WW2, Paper no. 2071, pp. 115-134, June 1959, by R. L. Wiegel, R. A. Dilley and J. B. Williams.

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INDEX--Robert Wiegel

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Attended schools in Lead, South Dakota, Dana Hall School, and Wellesley College, Massachusetts. Phi Beta Kappa. M.A. in English, University of Denver. Assistant to the President, Elmira College, New York. Married to Langan Waterman Swent, mining engineer.

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