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Morrough P. O'Brien: Dean of the College of Engineering, Pioneer in Coastal Engineering, and Consultant to General Electric

With Introductions by Clark Kerr, Robert L. Wiegel, Gerhard Neumann, and Robert G. Dean

An Interview Conducted by
Marilyn Ziebarth
1986-88

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INTRODUCTION I—by Clark Kerr

As a former chancellor of the University of California at Berkeley from 1952 through 1958, I got to know Mike O'Brien and had a chance to work with him closely when he was dean of the College of Engineering.

In 1963, when the first of the great major studies were made rating research universities across the country, engineering at Berkeley was rated along with MIT in the top two. MIT and Berkeley were the only engineering schools which rated in the top four in all of the major areas of engineering; not good in just one or two areas, but good across the board. It was just remarkable. And it was in great competition that this was done—with Stanford, Cal Tech, Illinois, Michigan.

This rating of Berkeley as one of the two best engineering schools across the country holds to this day. I've looked at seven additional studies which all come to the same conclusion. Mike has referred to this 1963 study as "my report card."

Mike not only built a faculty; he also built buildings. A crucial decade for building at Berkeley was 1950 to 1960. And by 1950, some of Mike's plans were coming into fruition. In that decade, the number of assignable square feet for engineering increased by 130,000. There are ten professional schools at Berkeley—engineering being one—and the professional schools altogether are about half of the Berkeley campus, Letters and Science being the other half. All of the other professional schools and Letters and Science together added 140,000 square feet as compared to Mike's 130,000.

How did Mike do it? First of all, he had a vision of engineering rising fast to the top among engineering schools and equal to science at Berkeley. Second, he accumulated very unusual academic and administrative control, which is not the Berkeley pattern, because, in Berkeley, influence and authority are highly dispersed. This great dispersion of authority came somewhat later to engineering, as it had earlier to the rest of the campus. But looking back on it, while the engineering situation was different from the rest of the campus, I have concluded that what happened in engineering could not have happened unless there had been this concentration of administrative and academic authority in Mike's hands. On occasion with Mike, I took the more standard view on dispersion of authority.

Mike also chose some very good assistants. Very great services were performed by Everett Howe, watching out at the undergraduate level, by Howard Eberhart, at the graduate level, and particularly by one other person, Fran Eberhart. A great secret of the administration of the University of California is that it is not really administered by the deans or the department chairmen or the chancellors or the president. It is run by an extremely good group of assistants like Fran Eberhart.

Mike also had an agenda of specific items he wanted. In addition to improving the research potential and graduate work and recruiting the faculty, he also began programs such as the Richmond Field Station and the Institute for Engineering Research. But he also built extremely good relationships within the University of California and within higher education in general beyond any other dean, not only at Berkeley, but anywhere else in the University of California. He set up a council of the deans of engineering within the university. Then he set up relations with the state colleges.

Then, in particular, he developed a very close relationship with the community colleges of the state and particularly in connection with their transfer programs. I really looked at what Mike had done in engineering as a model for the Master Plan for Higher Education in California when I developed it. While I had hoped that what engineering had done with the community colleges in the state might become the universitywide patterns in other fields, the record has been greatly disappointing. No one has ever come close to what was done by Mike in engineering.

As a chancellor, that is what I saw Mike do and that is how I think he did it.

Clark Kerr
President Emeritus
University of California at Berkeley

Berkeley, California
September 1988

INTRODUCTION II—by Robert L. Wiegel

I first met Morrough P. O'Brien in early 1943 when, as a senior at the University of California, I took his course on advanced fluid mechanics. He was often away, and it was not until many years later that I learned of his extensive involvement at that time in the U.S. Navy's war programs. What I learned in his class, together with related classes on heat transfer by L.M.K. Boelter and pumping machinery by R.G. Folsom, started my lifelong interest in fluid mechanics.

My next contact with O'Brien was at the end of World War II. I was on terminal leave from the U.S. Army, talking about graduate studies at Cal with my former adviser, Professor Folsom. He asked me if I had "secret clearance," and if I would be available for the next few weeks. I replied "yes" to both questions and found myself working on a model study of water waves generated by underwater landslides, at the request of O'Brien who was at Bikini at that time. I did the work in the Hydraulic Engineering Laboratory, and when O'Brien returned he had me repeat some of the tests for him to watch, and then described to me the model laws he used to predict prototype action from my model results. He also discussed the limitations on the use of partial models of this type. What I learned from him at that time, and in several future discussions on model laws, has been of great benefit to me throughout my professional career.

The "few weeks" of my employment were extended to work on a series of beach and wave studies, both in the field and in the laboratory. I worked with Joe W. Johnson, R.G. Folsom, John D. Isaacs, W. N. Bascom, and others, under the general supervision of O'Brien. During the following several years I met with him to discuss the work I was doing. He had the remarkable ability of walking out of a discussion, almost at mid-sentence, to attend another meeting and returning a month later to continue the discussion as if no break had occurred.

This remarkable memory of his was evident on many occasions. When I was on field trips with him in the early 1970's as part of the U.S. Army Coastal Engineering Research Board, Mike would state something like this: "I was here at the mouth of the Columbia River in 1931, and the flood tide bar appears to have moved inland and to the north a bit,'" or words to this effect. A study of photographs would show him to be correct.

I was appointed to the engineering faculty at Berkeley about the time that Mike retired. Although he moved away, I worked with him off and on for 42 years on coastal engineering councils, boards, committees, and studies, and sought his sound advice many times. Mike looked at problems from a broad perspective, but considered the technical details as well; he was always willing to discuss them with you and with graduate students.

In the early 1950's, he prepared a list of coastal engineering processes (both the system and components) that he believed required research and gave this list to Joe Johnson, and a copy to me. About once a year Mike would ask Joe or me what had been done on these problems. Some have been solved, and some still remain for future researchers.

Although he retired from the university in 1959, he never retired from teaching in its broad sense. Those of us who had the privilege of working with Mike continued to learn from him each time we met. I still remember a method of teaching he used, which I followed with my graduate students. He once responded to a question by a graduate students on why a research project was required of the student rather than an additional lecture course with this observation: new courses can be developed faster than the students can take them, but in performing research on a project and drawing conclusions from their work, students find that they can develop knowledge themselves, and no longer have to rely only on what others have done.

Mike would often remind us of what engineering was during meetings on specific projects, and this was of great value to us. He summarized his thoughts on engineering in his commencement address to graduates of the class of 1976 at the University of California at Berkeley, and his words remain one of the best descriptions of engineering ever written:

> The activity characteristic of engineering is design—not drafting or routine calculation—but design in the broad sense of (1) isolating a worthwhile technological objective, (2) conceiving a means of achieving this objective, (3), analyzing this concept for physical and economic feasibility, and (4) realizing the design in a physical system and proving its safety and effectiveness. Relatively few individuals have the combination of knowledge, experience, and creativeness for the first two steps in this process; a much larger number, who do most of the engineering measured quantitatively, are qualified for analysis, test, and construction of production. One is not better than the other—only different.

Robert L. Wiegel
Professor Emeritus
University of California at Berkeley

Berkeley, California
October 1988

Morrough P. O'Brien

INTRODUCTION III—by Gerhard Neumann

When I met Mike O'Brien in 1950 for the first time, General Electric was in the process of opening a large jet engine plant hear Cincinnati, Ohio. I was neither sure who M.P.O'Brien was, nor what role he was to play in GE's latest business venture, or to whom he was reporting. At that time, I was a low-level engineer who had just been put in charge of the mechanical design of a nuclear-powered aircraft engine. Shortly thereafter I was asked to set up a development organization to design and build a fundamentally new and advanced type of supersonic military aircraft gas turbine. In both programs, Mike O'Brien spent a great deal of time with me, questioning my reasons—or reporting what he had found out (i.e., liked or disliked)—from interviews with my teammates.

During the next few years, his status began to become clear: O'Brien reported findings and observations he had been making at all levels—especially the lower ones—to the Jet Engine Division's top man. As time progressed and I moved up in the organization, O'Brien and I became close personal friends. We worked together many days and nights, weekdays and holidays—and O'Brien reported his findings resulting from the interviews with my designers directly to me. Interestingly, Mike never once—ever—revealed the name of an individual or group of people who did not seem to carry out what they were supposed to do.

I used Mike as my personal technical sounding board when my staff and I laid out a new engine type; also when we left the plant for an annual three-day conference during which we discussed in great detail our future plans over the next three, five, and twenty years, which in the aircraft engine business are not considered very long periods of time.

Mike was always available when needed. If some mysterious or questionable input reached me, and I needed an absolutely reliable evaluation, I asked Mike for assistance. His sincere and trustworthy personality coupled with his outstanding ability caused engineers at all levels to discuss their problems and confide in "Dean O'Brien," who was probably sitting on the top of their desks taking notes. His personality was ideal for this most important charge of fact-finding.

Mike attended my weekly staff meetings and special project and program reviews over more than twenty-five years, and I let him take whatever action he decided necessary to get to the bottom of a problem. But Mike was more than a troubleshooter: he made his own constructive suggestions during our design reviews, whether the subject was mechanical, thermodynamic, or aerodynamic. When he and I were too busy to get together during the week, Mike never hesitated to spend the weekend with me explaining what he thought might benefit our rapidly growing business. We had started with military aircraft engines, but soon branched out into the field of helicopters, business jets, commercial aircraft, stationary powerplants, and marine propulsion.

My staff and I never felt lost when O'Brien was around: he was a thousand percent reliable, always wise and fair in his judgement, a gentleman, and an engineer! All of us will miss Mike and his many contributions to the success of General Electric's jet engineering business which grew to be number one in the world.

Gerhard Neumann
Vice President and Group Executive (retired)
Aircraft Engine Group
General Electric

Swampscott, Massachusetts
August 1988

INTRODUCTION IV—by Robert G. Dean

I first met Dean Morrough P. O'Brien at the Ocean Engineering Conference held in San Francisco in 1967. During the ensuing twenty-one years, we developed a warm collegial and professional relationship. Dean O'Brien made frequent visits to the University of Florida and shared freely his energetic enthusiasm for life, his keen insight, and his visions and concerns for coastal engineering. A prominent trait was his almost instinctive identification of best decisions or appropriate ways of approaching a problem and his abhorrence for wasted resources, inefficiencies, and bureaucracy. He liked to see projects started, work underway, and results flowing. He had both feet firmly on the ground and did not tolerate unrealistic, pessimistic, or irresponsible assessments of the future for coastal areas and coastal residents.

With his friends and associates, he would question aspects of coastal engineering which were poorly understood. These questions were to be treated as "assignments," and, frequently, "progress" on these assignments would be queried until completion. Appropriately, the origin of a number of the technical problems addressed in the *Shore and Beach* commemorative "Dean O'Brien Volume" were suggested to the respective authors by him.

In the two decades that Dean O'Brien visited the University of Florida, I came to know him as a man with many significant characteristics, two of which I will mention here. The first was his love for visiting and observing beaches. A free weekend would find him on the fifth floor of a coastal motel at the window, with his stopwatch and notebook in hand, recording wave period, height, and direction. Observations made and questions raised during such a trip would provide a research menu for several graduate students over a period of years. Secondly, Dean O'Brien enjoyed discussing research plans with young graduate students. During these enchanges, he would range out beyond the limits previously identified, provide the benefits of his experience and background on related problems, and infuse the lucky student with enthusiasm, motivation, and direction. Former students frequently comment on how much they treasured those opportunities to work with a "great man of coastal engineering."

The field of coastal engineering has lost one of its great contributors. Those of us fortunate enough to have known Dean O'Brien will feel the loss forever. Yet he has provided us with many lasting examples for our own personal personal and professional goals, aspirations, and standards.

Robert G. Dean
Professor
University of Florida at Gainesville

Gainesville, Florida
September 1988

INTERVIEW HISTORY

The preeminence of Berkeley's College of Engineering is widely recognized. The story of how it gained an international reputation is less known.

In large part, Morrough P. O'Brien is responsible for the College's growth from a modest undergraduate academy for mining and civil engineers into a leading twentieth-century training ground for the nation's best engineers and engineering scientists. As dean of the Department of Engineering from 1943 until 1958, O'Brien spearheaded the university's involvement in wartime training programs and led its postwar march to become a renowned institution of graduate education and engineering research.

O'Brien joined the Berkeley faculty in 1928 as an assistant professor of mechanical engineering. He laughed when he recalled how he arrived in mid-September at what he believed was President (and engineer) Herbert Hoover's school to teach civil engineering, only to find that Hoover graduated from Stanford University, that O'Brien was to begin teaching *mechanical* engineering, and that the semester had begun four weeks earlier.

Rapidly expanding his intellectual horizons to include the study of waves and beaches—again by accident of circumstance—the young professor began an extensive survey of the Pacific Coast from Tijuana Slough, Mexico, to the Strait of Juan de Fuca, Washington. He soon became an authority in the field he pioneered, coastal engineering, and he continued to define its directions for the next five decades.

By nature skeptical, O'Brien shared Engineering Dean Charles Derleth's suspicion that "those who can, do, and those who can't, teach." By the time the United States entered World War II, O'Brien was convinced of the need for both engineering practice and scientific research to expand undergraduate and graduate instruction in engineering.

Dean O'Brien's subsequent career as an administrator at the university and his tenacious drive to expand the sphere of engineering—on campus and off—make his oral history a mini-history of the university as well. A close friend of President Gordon Sproul for three decades, O'Brien moved deftly through increasingly complex university channels, headed Engineering's massive war training program, participated in the loyalty oath negotiations as vice-president of the Northern Section of the Academic Senate, and initiated dozens of programs and concepts. Acting with a systems engineer's vision of the complicated relationship between elements of complicated systems, O'Brien also became an active force for systemwide reform of higher education in California. His efforts, former University President Clark Kerr recalls, anticipated the important reorganization of the state educational system in the 1950s.

In 1958, O'Brien retired from the university, no longer content to be an administrator and increasingly frustrated with the bureaucratic aspects of the job he never sought. At age fifty-six, he went on to a new career in private industry.

For the next four decades, O'Brien utilized his scientific and engineering background, administrative savvy, and problem-solving skills as a trusted consultant to the leaders of General Electric Company. Hired as an independent trouble-shooter, he focused his attentions on GE's jet engine and nuclear power divisions. He also served actively on many national policy advisory groups for the armed services, engineering education, and large-scale research and development.

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Impetus for doing an oral history of O'Brien came from Provost Emeritus George J. Maslach, who suggested it as the first step in an oral and written history of the College of Engineering. Dean Karl S. Pister supported the idea and allocated funding for the project. Professor Robert L. Wiegel, a long-time associate and friend of O'Brien, also encouraged the idea. They shared the knowledge that O'Brien had created Berkeley's modern engineering college. When O'Brien agreed to make time for the oral history interviews on his occasional visits to Berkeley from his home in Cuernavaca, Mexico, the project was immediately initiated.

Preparatory to interviewing O'Brien, the interviewer extensively searched various campus archives for relevant documents and correspondence. Most useful were the University Archives' Chancellor's files and President's files, both containing extensive reports from and to O'Brien from President Sproul and his successor Clark Kerr. The Engineering Archives in the attic of McLaughlin Hall also proved informative and useful, particularly the detailed Engineering Advisory Council minutes for the years of O'Brien's deanship. O'Brien's personal papers, some of which were consulted for the interviews, are available to scholars in the Water Resources Center Archives (which itself is one of O'Brien's many lasting accomplishments) in O'Brien Hall.

Interviews with several of O'Brien's colleagues at the university proved informational—and highly entertaining. Professors Earl R. Parker, Robert Wiegel, Joe Johnson, Everett D. Howe, and Administrative Assistant Frances Woertyndyke Eberhart offered insights into activities O'Brien himself might have ignored. The informal interviews also provided a telling sense of the strong personal leadership O'Brien exerted over an exceedingly large number of activities he initiated.

The most difficult aspect of the oral history project was finding the opportunity to conduct the interviews. O'Brien, who died before he was able to complete his review of the final edited transcript, passed through Berkeley only seldomly, usually for an occasion hosted in his honor. The series of nine formal interviews began in May 1986 and continued until June 1987. Some interviews took place in his office in O'Brien Hall, others in convalescent homes in Berkeley while he recovered from illness and surgery. O'Brien's extraordinary memory and keen intellect made the interviewer's work easy.

Throughout the course of the two-hour interviews, O'Brien's phone rang frequently. Callers from around the country wished him well and talked about newsworthy developments such as the explosion of NASA's Challenger spacecraft. Old friends from Berkeley jockeyed for social and business visits.

In March 1987, the College of Engineering held a symposium in O'Brien's honor, arranged by lifelong colleagues in the field of coastal engineering Robert Wiegel and Joe Johnson. Renowned authorities and friends in engineering education gathered on campus to give testimony to O'Brien's considerable intellectual powers and leadership. Several of the papers presented there, later published in *Shore and Beach* (55:3-4), appear in the appendix to this oral history.

During the year of interviewing, Professor Werner Goldsmith also interviewed O'Brien for a history of the College's Department of Mechanical Engineering. (Tapes of his interviews are on file in the Water Resources Center Archives.) The two interviewers traded notes and privately marveled at O'Brien's recall of events and details as much as sixty years past.

At the time of his death on July 28, 1988, O'Brien was conducting yet another project for General Electric Company. He was analyzing how GE had managed, developed, and financed the high-technology research and development that brought the company to its considerable late-century prominence. O'Brien's own career is an important and lasting part of that story.

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The interviewer wishes to thank President Emeritus Clark Kerr, Professor Emeritus Robert L. Wiegel, Professor Robert G. Dean of the University of Florida at Gainesville, and former General Electric Company Vice-president Gerhard Neumann for their introductions to this volume. Dean O'Brien would have been pleased.

Professor Wiegel contributed to this volume in many, many other ways, as well. Engineering Deans Karl S. Pister and George J. Maslach (Emeritus) initiated the oral history on behalf of the College.

Thank you also to Steven A. Goldfield for transcribing the tapes with intelligence and accuracy and to Gerald Giefer of the Water Resources Center Archives in O'Brien Hall.

Marilyn Ziebarth
Interviewer-Editor

Berkeley, California
December 1988

In Memoriam
Morrough Parker O'Brien

I. MORROUGH PARKER O'BRIEN DIED Thursday, the 28th of July, 1988, in Cuernavaca, Mexico. He was 85. Long the leader in coastal engineering, his wisdom and counsel will be greatly missed by all who have been privileged to work with him. He is survived by his wife Mary; a son, Morrough, of Boulder, Colorado; and a daughter, Shiela, of Berkeley, California.

He was born in Hammond, Indiana, on 21 September, 1902. He received a B.S. in Civil Engineering from Massachusetts Institute of Technology in 1925. He did graduate work at Purdue University, 1925-27, and in 1927-28, as a Freeman Scholar, at the Technische Hochschule in Danzig and The Royal College of Engineering in Stockholm. He received three honorary degrees; the D.Sc. from Northwestern University; the D.Eng. from Purdue University; and the LL.D. from the University of California.

O'Brien engaged in three fundamentally different careers. His academic career as Professor and Dean of Engineering at the University of California, Berkeley, spanned the years 1928-59. A second career was his pioneering work in the development of coastal engineering which, while it had some impact on the research programs at Berkeley, was unrelated to his academic pursuits. His third career was his service from 1949 until his death on an annual retainer to General Electric Company.

O'Brien's academic steps were:

Mechanical Engineering:	
Assistant Professor	1928-31
Associate Professor	1931-36
Professor	1936-43
Chairman	1937-43
Department of Engineering: Chairman	1943-59
Professor of Engineering:	1943-59
Dean, College of Engineering:	1943-59
Retired	June 30, 1959

Ernest O. Lawrence and Robert J. Oppenheimer were appointed assistant professors in the same year at O'Brien, and the three became good friends. These associations greatly influenced his views regarding the importance of research in a modern engineering school. During his tenure as Dean of the College of Engineering at Berkeley he led the development of the College to its top-ranked status in many engineering disciplines. He was widely regarded as a powerful and perceptive leader in engineering education, understanding that modern engineering education had to be improved by a systems approach: good students are attracted to good faculty; good faculty are active in research in the outside world; good graduate students are needed to work with the faculty on research. The University furthers the interaction of students, faculty and society by the establishment of an external advisory council, and of such research units as the Institute of Traffic and Transportation Engineering and the University-wide Water Resources Center, both of these, incidentally, begun at UC with O'Brien's help. UC President Emeritus Clark Kerr, who had served as Chancellor when O'Brien was Dean, remembered him as "the mighty Mike" and the "builder of the College of Engineering and builder of Berkeley" during the symposium held in O'Brien's honor in March 1987 (see *Shore & Beach,* July/October 1987).

O'Brien received a number of honors from the University. O'Brien Hall, which houses the Hydraulic Laboratory and the Water Resources Center Archives, was named after him. The portrait of O'Brien reproduced with this article hangs in the entry hall of this building. He was awarded the Doctor of Laws degree. In April 1988 he was awarded the Clark Kerr Award, given by the Academic Senate to "an individual considered to have made an extraordinary and distinguished contribution to the advancement of higher education." This Award, presented by Kerr himself, was particularly notable in that it was given to O'Brien as one of four leaders "who played pivotal roles in building the academic strength of the campus." A photo of the presentation is reproduced below. Dean O'Brien was also awarded the Lamme Award for excellence in teaching by the American Society of Engineering Education.

O'Brien continued to be active in university education after his retirement from the University of California, working with Massachusetts Institute of Technology, Harvard University, and Purdue University. From 1968 on he was a part-time consultant and Professor of Coastal Engineering at the University of Florida, participating in many research projects in collaboration with Professor Robert G. Dean.

O'Brien was the founder of modern coastal engineering. He wrote a number of papers on the subject which have had a lasting influence. He was appointed Civil Engineer for the U.S. Army Board on Sand Movement and Beach Erosion in 1929, and initiated research by this board on coastal engineering. In 1930 he made field studies along the coasts of Washington, Oregon and California, and wrote a detailed seven-volume report on the results of his observations. A landmark paper on the relationship between tidal prism and entrance area was one of the results of these studies. He summarized many of his observations and thoughts on beach processes and the effects of structures on beaches in his paper "The Coast of California as a Beach Erosion Laboratory" (*Shore & Beach,* July 1936). In 1938 he was appointed a member of the Beach Erosion Board, U.S. Army Corps of Engineers, and served on it until it was abolished in 1963. He was then appointed to its successor, the Coastal

Engineering Research Board, serving there from 1963 until 1978, a total of forty years on the two boards.

The years of World War II were extremely busy for O'Brien, serving as Chairman of the Mechanical Engineering Department until 1943 when he was appointed both Dean of the College of Engineering and also Chairman of the Department of Engineering. He was Executive Engineer of the Radiation Laboratory under Professor E.O. Lawrence in 1942-43. O'Brien was asked by Lawrence and General Groves, the Manhattan Project Director, to recruit an engineering team to design the production facilities at Oak Ridge for the electromagnetic system. O'Brien said that probably the most important thing he did in his life was to convince them that there was not time to build a competent staff, that they should hire companies with an established engineering staff to do the job. He was in charge of the Statewide University of California Engineering Science and Management War Training program, 1940-44, when the program registered 46,000 students who worked under 1,800 instructors. He worked for the U.S. Navy Bureau of Ships on underwater sound, on cavitation generated by submarine propellers (the results of this research were immediately implemented by submariners), and on the design and operation of amphibious craft. He also worked with Professor H.U. Sverdrup of the Scripps Institution of Oceanography on the forecasting of waves, and he directed a program of field and laboratory studies of landing craft for the Bureau.

In 1950 he and Professor Joe W. Johnson started what are now known as the International Conferences on Coastal Engineering. The first of these was held in Long Beach, California, under the auspices of the University Engineering Extension, each paper being by invitation. The most recent of these conferences was held in Torremolinos, Spain, in June 1988 when more than five hundred abstracts were submitted for consideration and fewer than half could be accommodated (O'Brien served on the Technical Papers Review Committee).

O'Brien had a strong public service orientation. He was a member of the Army Scientific Advisory Panel, 1954-65, serving as its chairman, 1961-65; a member of the Defense Science Board, 1961-65; member of the Board of the National Science Foundation (a Presidential appointment), 1958-60; and he served on numerous committees of the National Research Council.

He was active in professional societies. He was elected honorary member of the American Society of Civil Engineers (1976), the American Society of Mechanical Engineers (1979), and the Japan Society of Civil Engineers (1988). He was elected to membership in the National Academy of Engineering in 1969.

O'Brien was a leader in several fields of engineering, including pumps and air compressors. The compressor design for the first American axial flow jet engine was laid out exactly in accordance with the method presented in the paper by O'Brien and Folsom entitled "The Design of Propeller Pumps and Fans." It was incorporated in what became the J47 engine with a production run of thousands. He was elected to the General Electric Company Propulsion Hall of Fame in 1984.

In March 1987, the *Symposium to Honor Morrough P. O'Brien—Working Solutions: Shore and Beach* was held at the University of California at Berkeley. The State University of New York at Stony Brook established the M.P. O'Brien Fellowships in 1987. The American Shore and Beach Preservation (O'Brien served as its president from 1978 to 1983) is establishing the *Morrough P. O'Brien Award for Outstanding Achievement in Shore and Beach Preservation.*

Robert L. Wiegel
Joe W. Johnson

Morrough O'Brien

By Bill Eaton
The Tribune 3/2/88

BERKELEY — Morrough P. O'Brien, former dean of the College of Engineering at the University of California, whom UC President Emeritus Clark Kerr called "a builder of Berkeley," died Thursday in Cuernavaca, Mexico. He was 85.

O'Brien founded modern sea coastal engineering—studies of wave action and beach erosion for use in the preservation of beaches. He was born in Hammond, Ind., and was graduated with a bachelor of arts degree from the Massachusetts Institute of Technology in 1925. He accumulated three honorary degrees in the course of his career.

O'Brien joined the mechanical engineering faculty at UC-Berkeley in 1928. He was chairman of the department from 1937 to 1943 and dean of the college from 1943 until he retired in 1958.

He brought the college to top status in a number of engineering fields, established a doctoral program in engineering, and founded the departments of nuclear engineering, naval architecture, industrial engineering, and programs in metallurgy and ceramics.

O'Brien Hall, which houses the Hydraulic Engineering Laboratory and the Water Resources Center archives, is named for him.

O'Brien was widely regarded as one of the powerful leaders at UC-Berkeley. Kerr, in a ceremony early this year honoring four of the university's leaders, called O'Brien by his nickname "The mighty Mike," for his pivotal role in building the academic strength of the Berkeley campus to national and international prominence. Last year's symposium on coastal engineering was held on the Berkeley campus, and Kerr at that time referred to him as "a builder of Berkeley."

In his career O'Brien wrote a landmark paper on air compressors for jet turbine engines for aircraft, a paper used in developmental work by the General Electric Co. O'Brien was a consultant for GE from 1949 until his death, specializing in systems management.

He was made a member of the board of the National Science Foundation by President Harry S. Truman in 1948, and was a member of the Army Scientific Advisory Panel from 1954 to 1965 and its chairman from 1961.

O'Brien's honors include the Lamme Award for excellence in teaching, given by the American Society of Engineering Education. This year he was made an honorary member of the Japan Society of Civil Engineering, and the American Shore and Beach Preservation will this year establish an award in his honor.

He is survived by his wife, Mary, a son, Morrough O'Brien of Boulder, Colo, and a daughter, Sheila O'Brien of Berkeley.

I. FAMILY BACKGROUND AND EDUCATION

[Interview 1: May 9, 1986] ##

Family Background

O'Brien: I was born on September 21, 1902, in Hammond, Indiana, the son of Morrough O'Brien and Lulu Avis Parker O'Brien. My mother went to Hammond when I was about to be born to stay with her mother and father.

Ziebarth: What kind of a town was Hammond?

O'Brien: Hammond is an industrial town just outside of Chicago. I spent many summers there with my grandmother.

Ziebarth: Why did your family settle in this part of the country?

O'Brien: My grandfather O'Brien came from Ireland about 1845 or 1850, and settled in Ohio. He had had enough of Ireland and its struggles with the British. When he came to this country, he had many Irish friends, of course, and he was in the wholesale liquor business. But he never joined the Hibernians or any of the Irish groups and never talked about Ireland. So, as far as I'm concerned, Ireland is about as distant as anything.

Ziebarth: Did you have brothers or sisters?

O'Brien: No. I was an only child. My Grandmother O'Brien had ten children.

The symbol "##" indicates that a tape or a segment of a tape has begun or ended. For a guide to the tapes, see page 197.

In our family, the oldest son has always been named Morrough, but my grandfather said, "That's enough of that nonsense. " When the tenth child was on the way, my grandmother said, "Morrough, if we don't name this one Morrough, we'll go on having children until we do. " So she named my father Morrough, and he was the last child. Only two of them married, however, and I'm the only grandchild. With ten children, only one grandchild.

Ziebarth: How do you explain that?

O'Brien: Three of them became nuns. My Aunt Mary became mother-general of the Sisters of Charity based in Montreal. She served up in the Arctic, and went in by dogsled. One boy was killed by lightning; one died early. Charlie, a bachelor, was the editor of the *Toledo Blade.*

I'm the first one in the whole family ever to have a college degree. You see, the Catholics in Ireland weren't really favored by the British, and they had a pretty bad time. But, apparently, my grandfather's family had connections that were somehow in good standing with the British.

But my aunts hired a couple of maids from this part of Ireland, County Clare, who had known of my grandfather and the family. It was a very substantial family in terms of income.

He was educated in what they called "hedge schools". At that time, it was illegal to have any school other than the public school in Ireland. It was illegal to be a priest or have a priest, and it was illegal to have a private school. Many of the estates had high hedges, however, which would intersect at four corners. The hedge schools would meet in any one of these corners with the priest, who came over from France. If the constabulary came by, they could run through the high hedges, and the constabulary's horses couldn't get over the high hedge and run after them.

My granduncles, Duffins, were builders. They were from Dublin, and my Grandmother O'Brien was from Belfast, Northern Ireland. The Duffins were levy builders down on the Mississippi between New Orleans and Vicksburg. They built levies, railway embankments and so on. The only thing I remember about them is from a newspaper advertisement with letters that high [indicates 2 inches] across the top of the page, "Duffin Brothers offer $20 a month and whiskey, Work on the Mississippi levies". (laughs)

When my granduncles died, my grandmother inherited their money. It was not a great deal, but she lived comfortably. While she was alive, they always took a place down on Put-in Bay on Lake Erie, or somewhere like that, for the summer. I would spend the summer with them.

My mother, father, and I lived in South Bend, Indiana, where I went to grammar school until I was about ten years old. My mother had tuberculosis, and we went to Arizona in 1912 to take care of her health.

Life in Phoenix, Arizona 1912-14

O'Brien: Phoenix at that time was a community in the sense that everybody in town, from the wealthiest to the poorest, went to Riverside Park. The park had a huge pool and a dancehall, and that was it. Phoenix produced quite a number of high divers, professional divers, because they had a great big pool and a tall tower for diving. Until I was in college, which must have been ten years later, if I stretched out on my back for a while, I'd get stiff from having done so much diving off the 30-foot diving platform as a kid.

Phoenix was not air-conditioned then. I came to love the desert. I still do. Once we hiked from Mesa, Arizona, to Roosevelt Dam on the Salt River in July—twenty Boy Scouts, a couple of scoutmasters, and a wagon with a team of horses.

I recall the night in 1912 that Arizona became a state and at the same time went dry [Prohibition]. We were staying in the Adams Hotel in Phoenix; it was New Year's Eve. I think they tried to drink the state dry that night. I was young but I remember the commotion all night long.

I went to grammar school in Phoenix for two years, at which point my mother needed to go to a sanitarium in Scottsdale for better attention. She went to a small sanitarium, or nursing home, and it was agreed that I ought to go and stay with my three maiden aunts in Toledo. So I went East on my own and lived there with my aunts through high school and my first year of college.

My mother improved, by the way, was cured of tuberculosis, and lived to be about seventy-eight in the dry air of Arizona. My father and mother lived in Arizona until their deaths.

Life in Toledo, Ohio with Three Aunts

I finished the seventh grade in Phoenix, and my Aunt Charlotte, Aunt Lottie, wanted me to go to the Jesuit high school in Toledo where they knew the priests. They asked if I could skip the eighth grade, and Father Wyeand said, "Okay. " I went into my first year of high school in Toledo without going to eighth grade.

Living with my aunts in Toledo was a bit of a problem—but also a joy because they were so attentive and I was the apple of their eye. Two of my aunts were school teachers in public school. Charlotte was a principal of several different schools in Toledo. Gertrude taught third grade near where we lived. And the third aunt taught dancing. Lily, Lottie, and Gertrude were their names, Lillian, Charlotte, and Gertrude [O'Brien]. They were very conversational people with lots of friends, and I learned to listen. I'd seldom get a word in edgewise anyway.

St. John's High School and College, Toledo

I lived in Toledo for five years and went to St. John's High School and one year of St. John's College, a very small Jesuit school with the best instruction I ever encountered. The freshman class in high school consisted of about twenty students at most, and my senior class in high school was about five students.

In the course of my first year in high school, the instructor said to me one day, "Morrough, your name is too complicated. I'm going to call you Mike. " And from that day to this, everybody has called me "Mike," including my family.

That schooling included such things five mornings a week of Latin, Greek, and English through our last years of high school and the first year of college.

Ziebarth: Was instruction in language a Jesuit tradition?

O'Brien: Yes. They were to take fifteen years of Novitiate—I think they call it—to become a priest in the Jesuit order. So we were taught mainly by brothers, and it was the best instruction I ever had.

We went to the point of writing short essays in Latin and Greek. When I applied for admission to Massachusetts Institute of Technology (MIT), nothing was mentioned of Latin and Greek. MIT wanted French or German. So I went to night school at Toledo University for one year to earn three years of French credits to qualify. I astonished the registrar of MIT when I showed up with six years of Latin, five years of Greek, and three years of French.

At St. John's, physics, chemistry, and mathematics were the frosting on the cake. You could take them in the afternoon and Saturday morning, if you wished, but the real core of the program was Latin, Greek, and English.

At St. John's College I had a tutorial instructor in mathematics—I was the only student. He put me through practically all the problems in a published booklet of entrance examinations to MIT and other universities. He decided I was to go to MIT. He just said, "You're going to go to MIT." And he told my aunts the same. That's the way it happened.

Ziebarth: Did you have an inclination for mathematics and science?

O'Brien: Yes. I enjoyed working the problems, and I enjoyed working with him. I also took chemistry and physics as electives.

My aunts had a family friend from New Orleans who had gone to MIT. I talked with him when he visited Toledo, and he talked with my aunts. He had gone to Holy Cross College in Worcester, Massachusetts, for four years and then four years to MIT. He seemed to be doing pretty well, and we all decided I would do the same.

So, after one year in Toledo—my first year in college—I went to Holy Cross for my Sophomore year. Again I took more Latin, Greek, and English, but also more formal math and physics.

When I went back for my junior year at Holy Cross, about two weeks before MIT and Harvard began classes, we started scholastic philosophy. I phoned my aunts and said, "I think I've had enough classical education. I'd like to move on. " They said, "It's okay with us, but can you get into MIT?" So I went down and talked to Dr. Tryon, the registrar at MIT, and he was astonished at my qualifications. He admitted me to MIT, and that was the beginning of things. But I do consider my Jesuit education in Toledo as a good start. I didn't realize at the time how good it was and how expert these people were. Their lives were devoted to the business of learning.

Holy Cross College, Worcester, Massachusetts 1920-21

O'Brien: Holy Cross is in Worcester, Massachusetts. It was a great baseball school. Connie Mack and John McGraw and the big moguls of baseball showed up to watch the boys play and see if their players were getting along all right. My roommate was a catcher for the New York Yankees. (laughs) He worked for the big-league farm teams around the country in the summer. He was about 19,a great baseball player. His name was Murphy.

Holy Cross was an Irish Catholic school with very strict discipline. Normally, we couldn't go out in the evening at all, but on Friday and Saturday nights we could stay out until 11 o'clock. If you played on any of the teams, you could have special permission to be out a little later. (laughs) So I went out for track and football in order to be able to stay out late on Friday and Saturday night. Boy, things have certainly changed.

In those days, even at Holy Cross where we lived in the dorms, we wore ties and high detachable collars. All through high school and college, I wore a tie and a collar and a coat. Now I go to the Faculty Club, and the faculty look like bums. (laughs)

Ziebarth: Were the classes at Holy Cross in the classical tradition?

O'Brien: Yes, it was a continuation of Latin, Greek, and English education.

Ziebarth: What did its graduates do?

O'Brien: Many of them became lawyers and quite a number became priests and doctors. I think the whole system has changed a great deal now, but at that time in the junior year, we started scholastic philosophy, that's Aristotle, and so on. And that was more than I wanted to undertake.

At Holy Cross, my class would be 25 or 30 students. We not only had a written exam in every subject, but we had an oral exam. Three professors sat there, and asked us to read Xenophon, Cicero, Demosthenes. You didn't know where they were going to open the book. They just opened it at random and said, "Mr. O'Brien, page so and so, let's go. " That was a real ordeal. Boys stood out in the hall waiting for their turn—and quite a bit depended on the oral exam. (chuckles)

Life in Cambridge, 1921-25

O'Brien: Because I had advanced credits from the two years at Holy Cross and St. Johns, I had some leeway at MIT but took the regular math, physics, engineering sequence with elective courses. In the four years to a bachelor's degree, which is the only degree I have, I had a choice, and I wanted to get a degree in math as well as in civil engineering. But the instructors advised me to take advanced courses in civil engineering, hydraulics and so on, which I finally did, but now somewhat regret. I think I'd have been a little better off the other way.

Ziebarth: How would that have been better?

O'Brien: Mathematics is a subject which should be studied young, when your mind is fresh. I didn't know that then; I just liked it. But nowadays, I feel it would be best for students to go as far as they can in math until they find out that that's about the end of the line for their ability. I think, of course, that the courses I took in hydraulics, graduate courses, were helpful and more or less pointed the way for me in the general field I followed.

Ziebarth: Were there "leading lights" teaching engineering at MIT at the time?

O'Brien: MIT was popularly known as Boston Tech. It was in Boston, originally. But it was certainly the best-known engineering school in the country then. As Father Heitkamp, the math instructor at Toledo, said, "Now you should go to MIT." (laughs) And so that was the choice. I wrote to "Boston Tech" to get the catalog, thinking that was the official name. I was astonished to have it come back reading "Massachusetts Institute of Technology".

Harvard University began fall classes about two weeks ahead of MIT at that time; MIT started about October 1 and Harvard in the middle of September. I had a number of friends who were at Harvard, so in that two-week period after I registered at MIT I was free and went over to see them. The upshot of that was I took a room in Cambridge, but I really lived at Harvard in Randolph Hall for the first two years and then in the middle entry of Grey's Hall in the Harvard Yard. These guys had biddies, mostly Irish, maids. I had a bed and clothes at Harvard, and down the street about three blocks was a room which I went to once in a while to get my mail. (laughs)

I lived at Harvard for three years —these fellows were a year ahead of me. They were mostly athletes. A couple of them were coxswains of the crew; they were little fellows.

Ziebarth: How did you meet them?

O'Brien: A couple of my friends from Toledo were in the group, and it turned out we had three suites, a living room, a bathroom, and a big bedroom. There were about nine of us. One was captain of the golf team. My close friend Lou Gordon was lettered in football, baseball, basketball. Three letters every year.

One of the fellows in this group, for example, who had graduated at Georgia Tech, was Bobby Jones, the golfer, Robert Tyre Jones. Sports and social life was the life at Harvard.

I went to classes, too, with some of them. The fellows would talk about an instructor who was pretty good, and I'd go along with them once or twice to hear the man. But mainly, I took MIT fairly seriously during the day.

When they graduated, I was on my own for senior year. I was taken into the fraternity at MIT, Delta Tau Delta, during my junior year, and so I lived at the fraternity house my senior year at MIT.

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Living at MIT was a good experience. It turned out that my close pal at MIT, Ted Kuss, was a very good analyst, a very thoughtful guy. He became a resident engineer on the [San Francisco] Golden Gate Bridge after he was graduated, and he built that bridge. I got to know him early. We became good friends, and he was a serious student as well as somewhat of a drinker. We talked, and I learned more from him than from the instructor about the fundamentals of the subject of engineering.

I became editor of the *Tech Engineering News*. Because of that I was offered a job by General Electric on the *General Electric Review* when I graduated.

Engineering Education at MIT

Ziebarth: How would you characterize your engineering training at MIT?

O'Brien: It was a traditional engineering education.

The chairman of the department at MIT, Professor Spofford, was a member of the firm of Faye, Spofford & Thorndike, one of the best- known civil engineering firms in the United States. The professor of hydroelectric engineering and water power was also with a well-known firm in Boston. Many times, when we wanted to talk to a professor about our senior thesis, we went to Boston to talk to him in his office. They were not on the MIT campus.

In electrical engineering, the head of the department was Professor Dugald Jackson. The firm of Jackson and Morland was probably the best-known electrical engineers in the country. That was the caliber of the teaching there. Professors were very much oriented toward practice. There wasn't much research or testing going on in engineering at the time. They were mostly practicing engineers who taught part-time. Good instruction, not rote, but without the emphasis that there is now in the sciences on analysis.

The whole field of sanitary engineering, for instance, was developed in New England by the New England Water Works Association. It spread through the world. Boston was the most important center of civil engineering work.

Ziebarth: Was the emphasis on a practical education typical of engineering education in the 1920s?

O'Brien: Anywhere it would have been about the same. We had relatively small classes, about twenty students, and we had much in the way of discussion in class. Professor Spofford, head of the department, seldom lectured. He quizzed the class. You were supposed to study outside and come to class ready. He would say, "Now, Mr. O'Brien, go to the board and do your problem."

Ziebarth: Were there standard textbooks used at the time?

O'Brien: Many of the texts were written by the professors who were teaching. The structural textbook we used was by Spofford. MIT was certainly the leading engineering school in the country and, to some extent, is still today.

Ziebarth: What about editing the campus engineering publication?

O'Brien: *Tech Engineering News* was a magazine which had been published monthly during the academic year, or possibly four, five, or six issues a year. I worked on it from the time I was a freshman, I guess. I went around Boston soliciting ads at various times and became editor when I was a junior.

I got in trouble several times. There was a thing called an MV Automatic Train Control. We were busily soliciting articles, and some people not only wanted to have an article in the magazine but they wanted to buy a couple of thousand reprints. Well, that sounded like pretty good business, and so I sold them the reprints. It turned out they were kind of a shyster outfit, and they were distributing these reprints as evidence that MIT was behind them. Harold Lobdell, the dean of men, happened to hear about it, and I caught heck over that.

Next, there was an organization called the Audit Bureau of Circulation, which certifies the circulation size of magazines. When they started to publish *Tech Engineering News*—it was about ten years before I arrived—the staff had been a little optimistic about the circulation. Year after year, they were a little bit optimistic about the growth. Then one day, Dean Lobdell called the printer and asked, "How many copies are you printing?"

The fat was in the fire. I paid no attention—I didn't even know this was going on—but I was the guy that was editor at the moment, and we caught heck for that. The Audit Bureau of Circulation figure was two or three thousand, and we were only printing five hundred or so.

Ziebarth: Did this magazine contain information about campus projects?

O'Brien: No. Articles by alumni, by anyone. In fact, the first job I got came out of an article that I solicited about the Hudson River Regulating District in New York State. That led to correspondence, and that led to the chief engineer offering me a job, which I took.

Summer Employment

O'Brien: When I graduated from MIT in 1925 I took a job in New York with the Hudson River Regulating District, which was building a dam to regulate the flow of the Hudson River. The dam was on one of its tributaries, the Sacandaga River. There were a number of existing dams on the river which would benefit from the regulated flow and gain more steady power output. One of those dams , Green Island, was owned by Henry Ford.

Henry Ford had had a sad experience with the bankers; they almost got his company away from him on the basis of loans, on which he would not pay interest. When he got his bill for his share of the operating work being done, there was an item of interest. He told his lawyers, "Get busy on that," and he refused to pay interest.

This situation tied up the money of the regulating district. Ford told his lawyers to keep at it, so the regulating district was having money troubles. I had had a field party out making surveys of the boundaries of the proposed reservoir, and I said to the field engineer, "I had an offer of a job at Purdue University. Maybe I better take it. Go out there for the winter, and come back next summer and take the party in the field again." He agreed to that. I called Purdue and asked if the job was still open. They said it was. So I went out there. That was the beginning of my academic career.

In college, when we were talking about what we would do, I was determined I didn't want to work for government. I was determined I'd work with a private consulting firm, like Professor Spofford. So, in the course of my career, I worked for the State of New York, the State of Indiana, the State of California, and the federal government. (laughs)

Beginning of Academic Career at Purdue University, 1925

Ziebarth: Why did Purdue offer you a job?

O'Brien: In those days, we had all kinds of job offers. I had a job offer from Purdue as an assistant. I had one from General Electric Company to work on the *General Electric Review*, the technical magazine they published. I had job offers from a couple of oil companies.

Ziebarth: Was there a shortage of trained engineers at this time?

O'Brien: MIT graduates were in pretty good standing. But it was 1925, and things were going well in the country. Any number of recruiters came to MIT from various companies. We were invited in to talk, or sometimes they had a lecture room and talked to a group.

I went out to Purdue as an assistant in civil engineering—$1,200 for the academic year, nine months. I went back to the Hudson River Regulating District the next summer and the summer after that. I spent two academic years [1925-1927] at Purdue.

Purdue was not quite up to the level of MIT at that time, but not much different either. Most of the engineering schools were undergraduate only, with a little graduate study.

I had decided that I didn't want to be an academic or to work for states or the federal government; I wanted to be in private practice. So, at Purdue, I took two courses with Dean A.A. Potter —that's how I got to know him well.

I also got to know the dean of graduate studies and his daughter. He said to me several times: "Why don't you take this or that so that you can qualify for a master's degree. " I said to him, "I don't want a master's degree; I'm not going to teach. " (chuckles) So that was the end of that. I didn't get a master's degree and never paid any more attention to the matter.

O'Brien: The first year at Purdue, I ran the laboratory. A man died early in the fall, and Professor Greve had to take over his classes, and so I ran the lab for the rest of that year. It was a standardized operation for the mechanical and civil engineers; they had four or five experiments to perform.

The second year I was in the experiment station doing work on flow problems of one kind of another. It was a field I was interested in. As a matter of fact, the first article I ever published was produced jointly with the father of Professor David K. Todd, whose desk I'm sitting at [O'Brien Hall, UC Berkeley]. It was published in *Power Plant Engineering*, on the flow of viscous fluids while I was at Purdue.

Ziebarth: Were you paid for your work as an assistant?

O'Brien: About $1,200 a year. The State of New York paid me about $125 a month as head of a field party.

Purdue was an interesting experience also. A very good friend was a metallurgist, and he had a laboratory with bunsen burners. We had lunch together practically every day because we bought a crate of canned Campbell's soup and had a fine lunch on his bunsen burners.

In the course of that year, I became acquainted with Dean Potter, and with some of the people in the experiment station. The second year, they made me an assistant in the experiment station, and I was associated with some very good people. I remember a man by the name of Hubotter, a very analytical individual, who worked on problems of combustion, and Morris Zucrow, who became prominent later in the field of aeronautics and airplane engines. I did experiments in the laboratory, so I had some introduction there to research.

As a matter of fact, research at Purdue was not much. And we three—I, the youngest and most inexperienced, and Zucrow and

Hubotter—we'd get together to read articles or to hear about it in the classroom or somewhere else. I had my start with them, some introduction to the ideas about how the principles of engineering were developed and applied to science, some sense of how science moved ahead. That was good.

Freeman Scholarship and Europe, 1927-28 ##

O'Brien: During the second year at Purdue, the Freeman scholarships were offered by the American Society of Civil Engineers (ASCE), American Society of Mechanical Engineers (ASME), and the Boston Society of Engineers. The scholarships were paid for by Mr. John R. Freeman, a prominent and active engineer, whose main business was insurance. He'd been president of both the ASME and ASCE.

They advertised a scholarship for the American Society of Civil Engineers. Then the ASME said, "Well, why don't you give us one? You've been president of ASME." So he said okay. Then the Boston Society of Civil Engineers, to which he had belonged for years, said, "How about us?" That made it three. Dean Potter at Purdue said to me, "Why don't you apply?" He had the facts about it, and I hadn't even heard of it.

What happened was that quite a number of people applied, and they sent the applications to Mr. Freeman. He liked it so much he paid for seven scholarships. So seven of us went to Europe the same year.

Mr. Freeman's interest was hydraulics—and he was particularly impressed by the fact that in Germany and in Switzerland there had been a great deal of work in laboratories concerned with river hydraulics, with flood controls, with floods, and with the whole matter of hydraulic phenomena. He wanted to introduce that work into the United States. He had been a consulting engineer on the Yellow River and Yang-Tse River in China and on the Po River in Italy. He felt that the Germans were way ahead of us, and that the first step to catching up was to have some scholars go over there, study, and bring that expertise back.

We went to Germany and met with him. He had arranged for a special summer course at the Technische Hochschule (Technical University, really) of Danzig on the Baltic Sea. We spent about six weeks there.

I was awarded the scholarship, but I'd never studied German. I bought a German grammar and got on the boat. We landed in Rotterdam and went by train across to Danzig.

Danzig was in a peculiar position. It was in a customs union with Poland, surrounded by Poland, but was part of Germany. We had about six weeks there. It was about one month from the time I bought a grammar and started German to the point where I was listening to lectures in German in Danzig.

Ziebarth: Whom did you study with in Danzig?

O'Brien: I studied with Professor Richard Winkle. He was a typical German professor. He lectured on hydraulic models, the theory of models, a subject that I knew quite a bit about. When I say I listened to lectures in German, I knew most of what he had to say. I got along reasonably well. Pretty stuffy, though.

One of the seven Freeman scholarship men was Blake R. Vanleer, who had been assistant professor of mechanical engineering at Berkeley's hydraulics lab. Blake and I decided that it would be a good thing to make a tour of Sweden. We got on a little steamer taking matchwood to Sweden, and made a tour of southern Sweden. Then, he went on to Germany, and I went on a big loop through Norway to Trondheim and northern Sweden, looking at power stations.

I became acquainted with the people in the laboratory in Stockholm, Professor Filenius and Mr. Lindquist, and I wanted to study there, but I wasn't sure that Mr. Freeman would approve. So I got word off to him asking if it was okay to study there, and I made this tour around Sweden while I was waiting for an answer. I spent that year in Sweden.

Exposure to University Research in Europe and United States

O'Brien: In Sweden, I was assistant extraordinaire, which I used to say was an extraordinarily good assistant. This meant that I was irregular in some fashion. I had an appointment in the Hydraulic Laboratory with Mr. Lindquist, who was head of the laboratory, and under Professor Filenius who was a professor of hydraulic engineering. That was a good experience, because Lindquist was writing papers and was quite active in studies and research. He became chief engineer of the City of Stockholm shortly thereafter.

I attended Professor Filenius' course. In those countries, you see, a professor is appointed by the king and can be removed only for a felony. Professor Filenius had written some papers, which were quite good and was qualified, but once he became a professor, he went into the real estate business in Stockholm. He was a rather portly man, and he had a series of cards, and he read from these cards. The students had taken it all down, and I bought a set of their notes in Swedish. I could read the notes and listen to him—and I wasn't proficient in Swedish—and I learned quite a bit from that.

In the same building was the Hydraulic Machinery Laboratory, run by Professor Dahl. He was quite active. I learned something about hydraulic machinery, which had a mechanical side, by talking with him and with his assistant and having lunch with them. I lived in an apartment with a family, just about a block from the university, so I had lots of time there. That was a pretty good way to begin learning about research studies.

Reflections on Tenure System and European Engineering Education

Many years later after the war I went to Europe for the Air Force on the tail end of what they called Paperclip Operation, which was to persuade German professors either to come to the U.S. or at least not go over to the Russians. I could make recommendations to secure equipment and small instruments, things they wanted. Some of these laboratories were so antiquated, it was incredible.

I called on a number of professors at one of the best-known schools in Europe, the ETH (Eigenischische Technische Hochschule) in Zurich. I went to see the dean, and his assistant took me around to visit various places in the school. Professorships there were very independent—they even had someone standing at the doorway to limit entrance to their lecture rooms. A professor might be appointed by the king and by the government of Switzerland and therefore be immune to action short of crime. This man told me that this was the first time he'd ever visited many of these people in his own school. I got a good lesson in how things can go sour in any such tenured system. I became convinced that that was no way to do business in the academic field, and I still think so.

I personally am convinced that tenure is a very bad thing. It never should have come into engineering. Positions get blocked out, and the state goes on paying salaries for years while people aren't doing a damned thing.

I'm not against tenure in general. I believe it is important for people in sensitive areas like philosophy, economics, and so on. But in a professional field like engineering, there are loafers who haven't done anything in years.

In Europe I met some professors who were up-to-date and very active, and some that had gone to nothing. There they were in a little empire with a budget, assistants, and lab facilities but nothing happened. Other places, there were very active young men.

Ziebarth: Engineering education in Europe had a very high reputation in this period.

O'Brien: In the first place, the admission is and was more selective. They have a different cut of IQ. Also, the formal requirements are more severe.

I think the American system went through a very bad period following the war, with the relaxing of requirements to admit Hispanics and Blacks, because an educational system feeds on itself. It takes the graduates in as the teachers. Once you begin to have graduates who aren't very bright, and they apply as teachers and get tenure, then the system goes downhill.

Offer to Teach at UC Berkeley and Return to the United States

I was offered another Freeman scholarship to study in France. I took a job in the American legation in Stockholm in the summer of 1928 between studying in Sweden and going on to France. My job was writing letters to American companies about markets for apples in Sweden and things like that. The legation was a part of the Bureau of Foreign and Domestic Commerce. Herbert Hoover was secretary of commerce, and this Bureau of Foreign and Domestic Commerce was a pet of his. Everybody in it was enthusiastic about Hoover.

Toward the end of the summer, a telegram came which had been in Berlin for about a month and finally got forwarded up there. It was an offer from the University of California. As a matter of fact, it was an offer to take the job that Blake Vanleer had resigned there. My associate said, "You've got to go there, Mike. It's Herbert Hoover's school." That was one surprise when I got there. (laughs)

The offer just said, "The University of California offers you appointment as assistant professor." I don't think it even mentioned money.

Well, it had been in Berlin for a month or so, and it was then August. I cabled to ask if the offer was still open. I'd decided I was fed up with being half-broke all the time.

They said the position was still open. I accepted, thinking that the University of California was on the same schedule as other schools. So I planned to get to Berkeley about the middle of September. I came back by way of France and England. When I got to Berkeley on Admission Day, September 11, a holiday, I learned classes had been in session for a month already.

I went to see William Wallace Campbell, the president, who had signed the cable offering me the job, on the day after Admission Day. He referred me to Walter Morris Hart, who was the dean of the university, who then referred me to Baldwin Woods,[1] who was an engineer and associate dean in the president's office.

William Wallace Campbell had heavy eyebrows, Walter Morris Hart was a real aesthete, and Baldwin Woods was an engineer. They used to refer to them as "eyebrow, highbrow, and lowbrow." (laughs)

When I went to see Dr. Woods, who was serving in the president's office but was also really running the College of Mechanics, I had a shock. The offer was not in civil engineering, which is my field, but mechanical engineering. I got here, and they said, "You're an assistant professor of mechanical engineering." I thought, "They're going to fire me as soon as they find out the fraud that's involved."

When I saw Dr. Woods, he eased the tension about the job. It turned out that they wanted someone whose field was hydraulics, and the Hydraulic Laboratory, which served the Departments of Civil and Mechanical Engineering, was actually administered by the Department of Mechanical Engineering. So I was thrown into the mechanical scheme of things as a civil engineer.

[1] See Baldwin M. Woods, *University of California Extension,* an oral history conducted in 1956 by Corinne L. Gilb, Regional Oral History Office, The Bancroft Library, University of California at Berkeley, 1957.

Beginning of Beach Erosion Studies, 1929 ##

O'Brien: I've never done much planning in my life. (laughs) I told you about the offer from Berkeley, which I thought was for a civil engineering professor and turned out to be in mechanical engineering. Here's how I got started in coastal engineering.

In the spring of my first year at Berkeley [1929], the United States Corps of Engineers officer at San Francisco, Lt. Col. Eliott Bell, came over one day in April and asked me if I would take a leave and go to Washington, D.C., to work for the Corps of Engineers. I asked, "What are they doing?"

At this time there was a lot of agitation over the need for a national hydraulic laboratory. The Corps of Engineers and the National Bureau of Standards were in competition, and he said, "I think they want you to help them design a hydraulic laboratory." After two years in Europe and now being in charge of the lab at Berkeley, I thought this would be my cup of tea.

When I got to Washington, Major Brehon Somervell, the district engineer, was busy at the Senate Office Building. His secretary said, "If you go out there, you can probably get a few minutes with him." I got just that, about two minutes, in which he said to me, "We're going to study beach erosion." I didn't even know beach erosion was a subject. Literally, I'd never heard of it; I didn't know anything about the ocean or about wave action.

I'd taken a leave from Berkeley in the spring of 1929 before the end of the term. The deal with the government was that they couldn't pay my travel from Berkeley to Washington and back, but they could set my salary at a level such that it would compensate me for the expense. But I was off the payroll at Berkeley.

I thought, Oh, my God, as soon as we have a talk, he'll find out I don't know a damned thing about it and send me back to Berkeley. I'll be in a mess. I didn't have many financial resources.

But Somervell, a very active engineer who became Lt. General of Service and Supply in World War II, gave me a bibliography of the subject, and I went to the Library of Congress and spent a lot of time reading about beach erosion.

I was to work for a board called the Board on Sand Movement and

Beach Erosion. At the board's first meeting, it decided to get someone for field work. The second meeting was held at Cape May, New Jersey, which had some serious erosion problems. There I learned that I had perfect qualifications for the job. I had made measurements of hydraulic phenomena, and I knew absolutely nothing about the shoreline. That was exactly what they wanted—someone to come in fresh and start the whole business.

The chief had decided that it was time they stopped writing papers and go into the field, make some measurements, and learn what was going on in a far better fashion. They were looking for someone to start this study that would be the first worldwide systematic investigation of coastal phenomena. There had been papers and books written by practicing engineers with casual connections. But as for studying the phenomena as such, it hadn't been done. In effect, that summer I started the systematic study of coastal engineering in the United States.

In Washington for that summer, I took on a lieutenant in the Corps of Engineers as my assistant, and he took over the work. Then I went back to Berkeley in August for my second year on campus. That explains why I'm sitting here in this office almost sixty years later.

Coastal erosion has been a field of interest since then. I was appointed to the Board of Sand Movement in about 1936 when one of the civilian members died. Congress made this activity a formal organization called the Beach Erosion Board, and I served on it and its successor for about forty years.

In the following years and after World War II, the Corps of Engineers assigned a number of young second lieutenants for graduate study, and they were all more or less told to work with me. That's how Berkeley got started in the laboratory studies, and in this period practically all of the work and publications were from Berkeley.

Ziebarth: Was the university's program research-oriented?

O'Brien: Oh, yes. It had anywhere from three to seven corps lieutenants a year, who had to write a thesis on some phase of coastal problems.

Ziebarth: Was this assignment part of your work for the corps?

O'Brien: No, it was as a professor at Berkeley. I wasn't paid or under contract with them. In those days, faculty were happy to have graduate students as an added duty.

II. UC BERKELEY'S DEPARTMENT OF MECHANICAL ENGINEERING, 1930s

Beginnings of Graduate Engineering Program

O'Brien: When I came to Berkeley in the late 1920s, there were three colleges: the College of Civil Engineering, the College of Mechanics, and the College of Mining. All had deans. In the College of Mechanics was Everett D. Howe, who had a master's degree. He was an assistant professor when I came and helped me in the lab. Professor Carl Vogt in mechanical, who had a master's degree from Berkeley, also was involved. There was just one graduate student, a man by the name of Schmidt, who also had a job working in the department as a helper.

Professor L[lewellyn] M.K. Boelter was the real head of mechanics, although the dean of mechanics was Dean Clarence L. Cory. Cory was ill mentally, gone to pieces with age. The real director was Associate Dean Baldwin Woods in the president's office, who had to approve the budget. But the active head was Associate Professor Boelter, a great man.

In the years that followed, about the only graduate courses—there were a couple advertised in the catalog but not many students—were seminars with Boelter, Howe, and Vogt and maybe one or two students: this man Schmidt and one Fred Dittus.

Then I began this program right away with the civil engineering students in the laboratory. Professor Boelter was a real investigator, a sound man, a very good teacher. But there weren't many students. We had a lot of time to talk about what we'd like to do, and so we really were prepared after the war when we had a lot of students coming in.

Slowly, we got some graduate study going. Standard Oil of California became interested, and I was teaching a course on hydraulic machinery in the department of mechanical engineering by that time. That led Standard of California to give me a fellowship and Boelter a fellowship. A man by the name of James E. Gosline, who later became president of Standard of California, was a graduate student, and we worked on all kinds of problems of machinery, especially different types of pumps, and published papers. I became sold on the academic life during those years and was happy doing it, and I had a small amount of consulting work outside.

Mechanical Engineering Program

Ziebarth: What was Berkeley's mechanical engineering (ME) program like at the time?

O'Brien: Berkeley was fortunate to have Boelter in the faculty. He had long studied the development of chemical engineering in the United States. His interest was in heat transfer, which is between mechanical and chemical engineering. In the years before World War II, when I was getting organized in mechanical, this department was very far in advance of most ME departments in the country on the undergraduate level.

We were getting away from the power-plant, strictly mechanical, side to unit processes like flow: pipe flow, flow in a wind tunnel, a combination of heat transfer and flow. So our ME program was becoming much more like chemical engineering in the rest of the country than it was like mechanical engineering.

At that time we had great interest in the mechanical problems of the automobile and its vibration, its dynamics, and so forth. Professors like Carl Vogt and Everett Howe had an interest in the traditional subjects. But in the seminar I mentioned, which mostly professors attended, we were taking up texts written in German and translating them under the leadership of Boelter who was the spark plug.

That was part of the education, and when I took charge of the senior lecture lab course, it was a really big show: about three lectures a week and an afternoon of lab and a whole lot of teaching assistants, which we had in those days. Practically every experiment had to be written up by us because there was no textbook or lab-book that had our kind of business. This was quite a departure, certainly from mechanical engineering anywhere in the country. It was even somewhat of a departure from chemical engineering. We didn't quite do what they did; we had our own ideas here.

At that time it was more educational to be a teaching assistant in

ME than it was to be a graduate student because the ME assistants were helping us write all these lab instructions.

Ziebarth: Why was Boelter taking this approach?

O'Brien: He was just that kind of person. He got his degree at Berkeley in mechanical engineering, and he was very inquisitive, very thoughtful, very even-tempered. He was also our specialist in dealing with students. Later on, when President Robert Gordon Sproul asked me come down to Los Angeles and offered me the job of dean, about the only thing I said was, "I'll agree if you appoint Boelter associate dean if he'll take it. " There were all sorts of things I should have asked for, but that was the only thing I thought of.

It was because of Boelter's initiative that we had this seminar which ran once a week, all year, every year for three or four years. This new development, our ME 131 and 124 combination of lecture and lab, was miles ahead of any other ME department in the country. It was undergraduate, but it was really far different and much more comprehensive and basic. That was his doing. I got the job of running it after I was here about three or four years, but it was his doing.

One of the things that plagued me while I was dean was the fact that President Sproul, who had studied civil engineering at Berkeley, had taken the ME shop class, a machine tools laboratory. When he took the shop, students planed off a piece of metal to get it smooth. Years later every time I talked to him about money I needed, he'd say, "Mike, Are you still planing metal with a file over there?"(laughs)

Our lab in the machine tool laboratory was equipped with dynamometers. Instead of just cutting metal, we had a dynamometer on the member that was pushing, doing the cutting. We were measuring the force or energy involved We were already busy with the scientific analysis of the machinery. No place else was doing that; that was Boelter's credit.

We talked a lot about graduate study and the standards of graduate study. Charles B. Lipman, graduate dean and a very good friend, talked to me about what graduate studies could accomplish, what the standards should be, what you should try to do. I was already of that mind. But there was a lot of push to think big, think advanced, not to let anybody be better than you were.

When the post-war period came, we hadn't had many graduate students, but we certainly were armed with definite ideas as to what we wanted to do. For example, this is probably the only engineering school in the country where we had a large percentage of faculty who weren't engineers.

In other places, they talk about collaboration between engineering and economics, or engineering and physics, and this and that. But it wasn't evident to me that this collaboration ever came about. Here, however, we brought in an applied mathematician from New York University, a rather high- powered institute, as chairman of mechanical engineering. That was Sam A. Schaaf.

As dean I concluded that the thing to do if we were going to work in applied science, was to have honest-to-god scientists in engineering and collaborate with them on an intimate basis. Professor Samuel Silver, who headed the Spaces Sciences Lab planning, was a physicist. We had three mathematicians; we had Ronald W. Shepherd, who moved into industrial engineering, Sam Schaaf in mechanical, and Alan W. Searcy, a Ph.D. in chemistry. We set out to get real scientists into engineering instead of applying absolute standards, such as you have to be a civil engineer to be in the civil engineering faculty.

In those days, hydraulics was a subject dealing with water: pumps, canals, or water. But the real basic subject is fluid mechanics, the flow of fluids. So one thing I got started on early was the writing of a book on fluid mechanics, and the book came about, published in '37: *Applied Fluid Mechanics.*

When we started on that, it was unique; no one was teaching it, as far as we knew, anywhere in the country. It was part of what I brought back from Europe. They were talking about fluid mechanics and Reynolds number and so forth. I heard about that in the courses I took at MIT before I graduated.

Ziebarth: That was a coauthorship?

O'Brien: Yes, the coauthor was George Hickox, who got a Ph.D. in radiation under Boelter. He was a very industrious and meticulous individual. I had been using these notes in class; I was teaching the course that the book was about. He was the one who talked about publication, and I thought it would be a great chore to get all that junk together and publish it, the illustrations and everything. So he agreed to put it in form, and he did the illustrations. But, what he did with the text was to make it a series of staccato sentences.

I read a draft and thought, "Oh, my God. " So I re-wrote that whole book, just sat down and wrote and wrote and wrote and put it in the form it appeared in. I never would have gotten through all of the mechanics of publication, all those damned illustrations that had to be drawn.

Ziebarth: I understand that the text was a classic which is still consulted.

O'Brien: I meet people occasionally who say it was pretty good, but after they reprinted it a couple of times, it ought to have been revised. The publisher said the introductory chapter particularly was too rough on students. But by that time there were three or four other texts. I said the heck with that; let's cancel the whole thing. Writing textbooks isn't a very rewarding activity.

Ziebarth: What did you do as a teacher?

O'Brien: The classes were very small. Furthermore, I soon had added into the mechanical engineering curriculum such courses as river and harbor hydraulics, which isn't mechanical engineering, but that's where it was.

The army students were here, too, which was rewarding and interesting because they were good students, excellent, top-flight. I was working on this matter of coastal problems and wave motion, and these students were all doing a thesis in the field. So that course was pretty interesting.

Then I taught elementary fluid mechanics, and after about three years I was put in charge of the senior mechanical lab lecture course, a big, five-unit combined course. I spent a lot of time boning up on the subjects that ME people knew about and I didn't: thermodynamics, heat transfer, radiation, etc.

Lack of Advanced Degree

Ziebarth: Do you feel that your career at Berkeley was affected by your lack of advanced degrees?

O'Brien: As a matter of fact, just through chance circumstance, my lack of advanced degrees helped me because Dean Charles Derleth was a man who believed, "Those who can, do. Those who can't, teach. "

Derleth was the consulting engineer on the Bay bridge, the Golden Gate Bridge, and the Carquinez Bridge. He was a good practicing engineer. So far as he was concerned, if you taught as a sideline, students were the better for it. He regarded me as teaching as a sideline. Professor Sidney T.

Harding, Raymond Davis: we had some of the most talented engineers in the state teaching civil engineering here at Berkeley. That was the environment.

I don't think not having a degree has made any difference. Certainly not here at Berkeley. I do have honorary degrees from Berkeley, Northwestern, and Purdue, which I never mention.

Chairman of Mechanical Engineering (ME) Department, 1937-43 ##

O'Brien: I spent three years as an assistant professor of mechanical engineering, 1928-31, and five years as an associate professor, 1931-36. In 1937, I became chairman of the department.

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I only had a bachelor's degree, and Dr. Woods had a doctorate degree. When Dr. Woods took a leave for one term, I was appointed chairman.

I didn't know it. I was on a vacation—we went up to northern California—and I came back just to get my mail and to go on to Carmel. Mrs. Nauta, the secretary of the department, was phoning all around to get me. I went over to the office, and she said, "Where's the budget?" I said, "Why should I know where the budget is?" She said, "You're the chairman of the department. "

So I looked through the pile of junk in my circular file, where I put all the university's publications. Sure enough there was the budget of the department. I hadn't seen that; I never even thought about being chairman.

Similarly, on the matter of becoming dean, I never sought an academic job. As a matter of fact, I retired early because I got so fed up with the administration of things.

Ziebarth: You didn't anticipate the appointment as ME chairman in any way?

O'Brien: No. A complete surprise. I couldn't believe Mrs. Nauta; I thought she was crazy telling me I was chairman of the department. Charles Derleth, dean of the college of engineering, which included the old colleges of mechanics and civil engineering before they were combined, just recommended me as chairman, not acting chairman or acting chairman for a term.

I think I was recommended because I had no advanced degrees. Derleth regarded me as an embryo engineer more than a teacher.

Dr. Woods thought I had been appointed as acting chairman for one year, but old Derleth submitted it as regular chairman for the full year. When Dr. Woods returned, he asked for leave for the second half of the year and after that became head of the extension service.

Baldwin Woods was a really fine man, very able. He had run the aviation ground school which the University of California had in World War I. He had contact with people in the aviation industry. He was a member of the board of directors of the Boeing School of Aeronautics, which was out at Oakland Airport for many years.

Dr. Woods had a Ph.D. from the University of Texas. He was a very skillful administrator. While he was in the president's office when I arrived here, he was also supervising the College of Mechanics, because Dean Cory was non compis mentis or something of the sort.

So Mrs. Nauta asked me, "What are your policies as chair of the department?"—Lord, I hadn't thought anything about policies. I had only worked very hard to learn the kind of technical background that the other faculty in ME had; I'd never got that far, but at least I worked at it and had some qualifications.

I think Boelter was hurt that he wasn't asked to be the chairman then. I didn't know about that either. It came upon me as a complete surprise; I didn't seek it; I didn't want it. President Sproul made that appointment. I met him one day at the door of the faculty club; he knew me and I didn't know him.

About that time, around 1935, my friend Charles Lipman, then dean of the Graduate Division, also proposed me for membership in the Bohemian Club of San Francisco. That was in the depression years when they were losing members, not gaining them. So I got in immediately.

From then on, I saw Sproul at the [Bohemian] Grove in the summer. I got to know him pretty well. I was in a group called Swagatam; it means "welcome" in Sanskrit.

Berkeley Campus's College Avenue Pool ##

O'Brien: The first expansion of the hydraulics lab out of the Mechanics Building, where I had my office, consisted of getting the College Avenue laboratory pool. That was the old men's pool, and they had built the new pool up at Strawberry Canyon. So the old pool was sitting there idle. I wanted to get it as an adjunct to our laboratory, because there was this big pool, a body of water, and there was some area around it. I had to think up some good reason for getting it.

I had been interested in the matter of what's called the virtual mass of objects. If you accelerate a ship, not only do you have to push the ship, but the water around the ship has velocity. That velocity represents energy, and you've got to supply all the energy, both to the ship and to the field around it to get it to move.

When it came to College Avenue laboratory, I thought, "This is just right." In order to study acceleration, you want to have a very steady acceleration. The best drive for such a thing is a falling weight. You have some pulleys. You let a weight fall steadily. Here on the pool is a line attached to the model. You pull the model, measure the motion of the system, and if it has small friction, you get the virtual mass.

So I wrote to the university administration and said we were very much interested in pursuing studies of such subjects, which so far as we knew had not been investigated by anyone experimentally, although it had been written up theoretically in Horace Lamb and elsewhere. We wanted to apply the theories to real ships because we had models of ships, we had naval architecture here, and we had good, expensive models of about the right size.

Our plan worked. They assigned the College Avenue pool to us as the College Avenue laboratory in about 1936. We built the falling- weight drive system, and we did experiments on models. A man by the name of Jim Murphy wrote his master's thesis on it. He became associate director of the Shell Research Laboratory later.

We did that experiment and got control of the area. Then we built the model of the estuary of the Columbia River. That was a big, long, continuous experiment.

Teaching in the Mechanical Engineering Department ##

O'Brien: I think I did reasonably well at teaching. I took it seriously and tried to learn how to do it. You see, they give no instruction around here to people coming in to teach, not a damned thing. But I did talk with Boelter about it. What were the problems of students? What kind of exams should we give? It soon turned out that I was more and more involved in things like the Columbia River model and the salt-water barrier and administration. So my formal teaching dropped off. My teaching was more in seminars and tutorials, the kinds of things you do with graduate students.

For a long time I had a course called "Thesis Substitute. " The B.S. people used to write a thesis, and they put in this course before I came here.

Many of those Army engineering students who came here became high-level officers. Bruce Rindlaub became a lieutenant-general. Weary Wilson became chief of Engineers. McDonald became a lieutenant-general and later was the staff man, as a civilian, for Senator John Stennis, who was chairman of the Armed Forces Committee.

The engineer officers in the Army are the select group, the top five percent. They were the only ones eligible for engineering at that time. They were brilliant students, hard-working guys, very sincere people. They all did well.

Ziebarth: What did you see as your major tasks as chair of the department?

O'Brien: (laughs) To get money! (laughs again) Just to keep things going was the first problem. I hadn't thought about broad problems of engineering education. Certain courses were offered; that was all right with me. Leave it that way, I thought. But once I had the job, I got to talking about the problems.

Curriculum Development ##

Ziebarth: Were you firm in your mind about the directions you wanted mechanical engineering to go?

O'Brien: In mechanical, we began to get ready for moving ahead before World War II when we had only a few graduate students. During the war I had army engineer students, who were really in civil engineering, but they came over and did a thesis with me in mechanical because they were my connection.

When I first reported to the university in 1928, I went to see Baldwin Woods. Dr. Woods said to me, "We must get to the point in graduate work where we are offering courses like Lamb's hydrodynamics. " That's a mathematical approach to engineering and a very useful thing written way back in the preceding century. I hardly knew what it was. I had referred to a parallel book, Barrett's hydrodynamics, nowhere near as useful. It took a long time, but I finally persuaded Dr. Woods, who had a degree in mathematics, to offer hydrodynamics. I said I'll help if you'll give the course.

Because he had all sorts of university duties besides being chairman of the College of Mechanics, he started the course but pretty soon I was giving it. And I certainly wasn't qualified. But there were only two or three students, so we kind of worked along together.

Then I persuaded Professor Hans Levy in math to come over. He was fully qualified. We only had two or three students, but we at least got it going. We got the course on the books and got it listed in the catalog. In effect, I was the reader for Levy in that course. It helped me, too, to learn something about the subject.

Professor Younger, who later became the chairman of mechanical engineering, had a course in dynamics. L.M.K. Boelter and a couple of graduate students were pretty well-known nationally even in heat transfer. Berkeley's been very strong in that subject way back. So we began to have courses.

There was also a seminar composed of Boelter and about four assistant professors and one graduate student. We worked and translated German texts and offered the course. Then I spent a lot of time visiting courses in physics and math—partial differential equations, complex variables, and so forth—that I hadn't studied as a student. We were getting ready in subject matter and thinking.

We also learned from our association with people from Cal Tech at the joint seminar. I used to visit down there pretty frequently.

ME Faculty Recruitment ##

Ziebarth: How did you approach recruitment of faculty?

O'Brien: The first and most thorough job I ever did of recruiting faculty was getting Professor John Dorn, a metallurgist. I had become convinced that mechanical engineers needed to know more about metals. I had a friend down at Lockheed. He had filled me full of stories about what horrible things engineers did when they didn't know anything about metals and specified the wrong material for some service. Thinking that over and finally getting it straight as to the proper definitions took time.

Process metallurgy is beneficiation of ore, taking ore from the mountains and making solid metal out of it. Physical metallurgy has to do with using the metal—copper, aluminum, steel, iron—for manufacturing purposes. Two very different things. Physical metallurgy was properly a branch of mechanical engineering in my opinion, and our people didn't know anything about it.

So I tried that for two or three years and finally got a professorship established. Then I set about finding a professor, and I didn't know much about metallurgy. I didn't know who to consult, and I sure didn't want to talk to the wrong people who gave me all sorts of bum advice. They were process metallurgists, people in mining.

I got in touch with the American Institute for Mining and Metallurgical Engineers, the American Society for Metals and got quite a long list; I must have had a hundred names on that list. Then I got the girls in the office to go through the journals and make up a bibliography for some. We shook it down to twenty-five or thirty. Finally, we decided that John Dorn was the best choice, and I invited him out. He came out on the *Streamliner* in the morning—in those days we only had the *Streamliner*; the airplanes were just beginning. I had lunch with him, showed him around. He got back on the *Streamliner* and went back to Columbus, but he accepted the job. He came the next year.

Once he was here, I had somebody to consult who knew what it was all about. I added ceramics. Finally, I put metallurgy with materials over in mining.

Departmental Strategies

Ziebarth: You said you saw your objective as chair of the department as fundraising.

O'Brien: Fundraising, not outside, but inside. Learn how to state things so that the budget committee and the president agreed that I had a case. (chuckles)

Ziebarth: What sort of process was that for you?

O'Brien: It was just learning what things they asked. I answered them in the first place as best I could, making a case with a subject like metallurgy. The budget committee members were appointed by the Committee on Committees, which was elected by the faculty. The Committee on Budget and Interdepartmental Relations was an important committee. It went over the budgets proposed by all the different departments and passed on their recommendations to the president.

The Committee on Committees was the key committee because they were elected. For quite a number of years, we'd spend a couple of meetings chewing the fat about who would be helpful and had the time and the sense. I used to meet with Alva R. Davis, dean of Letters and Science, and Wendell M. Latimer, dean of Chemistry. We would cook up a slate and pass the word, "Let's get these guys onto the committee. "

Ziebarth: You wanted people from the College of Engineering on the committee?

O'Brien: I didn't care if they were College of Engineering; I wanted to know they were sensible people, not wild men. But usually we tried to get one from Engineering and, of course, Letters and Science, and frequently Chemistry had one. But the main point was to put up a list. There were seven members of the Committee on Committees, and we all agreed. What we wanted to do was pick four good guys that we wanted to push and then tell people, "Vote for those four but don't vote for the others. " If there are seven people running, many people will fill them all out. But if you want anyone elected, you vote for your guy and don't vote for the others.

Influence of Interdisciplinary Perspective ##

Ziebarth: How did your background in civil engineering influence your perception of mechanical engineering and what direction it should be going?

O'Brien: My first years at Berkeley were the busiest years of my life, trying to learn something about thermodynamics and heat transfer, dynamics and elasticity. In the third year I was here, I was already in charge of the senior lab lecture course. Believe me, I spent a lot of hours getting ready for that. Not only preparation of lectures, but we were changing the nature of the lab course to follow the chemical engineers and have in it unit operations as though we were taking up acoustics, control systems, and a number of things that hadn't been touched on before. So there was a lot of writing of instructions for those lab experiments for the students to be done, as well as giving lectures.

It was a team effort. There were a number of professors and teaching assistants involved. But the man in charge had a lot to work on. (chuckles) It certainly smeared my view that civil engineering, mechanical engineering, and electrical engineering were all totally different subjects.

I've been a consultant for General Electric for thirty-eight years reviewing technical programs. I found so many deficiencies in the electronic]electrical engineers who don't know anything about heat transfer, fluid flow, or dynamics. After all, all engineering works on something tangible, a product.

I've spent more time helping figure out how to get around the ineptitude of many electronic engineers who only know electronics and breadboards. They try to put them together in a missile, compactly, and they generate heat. Then they have to get the heat out. They have weight, too, and volume. A missile is very short on space for things. They want to use the space for fuel, nothing else. I helped them solve these things for which they were insufficiently trained.

It was great to be able to add all this mechanical engineering to civil engineering at Berkeley. Civil engineers are stumped if anything moves.

Berkeley is perhaps the only college of engineering in the country that consistently has taken physicists, chemists, economists, and others into the college. Not engineers or applied scientists who took other courses, but people who are so advanced that they were offered full professorships in the math department, say.

Most people in engineering schools think that every member of the civil engineering faculty should be a civil engineer. Well, my early experience here taught me otherwise. I also became convinced of one thing: departments on this campus or other campuses don't collaborate. It's a shame.

Ziebarth: What did you see as the proper mix of research and teaching in the College?

O'Brien: In the early days Carl Vogt worked on engines; Boelter had his interests in heat transfer and thermodynamics; I was interested in hydraulics. That was all that was going on—there was no theorizing about the proper balance. We took a normal teaching load, plus the graduate work. We weren't making allowances for having graduate students; they were fun and we were lucky to have them.

When the defense program came along in about 1939, we began to talk more about what to do after the war. We were really in the doldrums here. We had nice fellows in electric power, not electronics. They were fuddyduds.

When Navy men who had been assigned here for graduate study went back to the electronics branch of the Bureau of Ships, they could recognize the deficiencies in their training here. No schools were very far along in the matter of electronics, but we should have acted and moved that group over and joined them with mechanical engineering, because mechanical engineers use all sorts of electronic equipment and controls. That old-fashioned group—new fashioned, if they'd joined up with mechanical—should have been joined by a whole new department formed for electronics. We should have done that, but I wasn't bright enough for that.

Ziebarth: Could that move have been readily accomplished?

O'Brien: That simply wasn't the thinking. The thinking was that electric power is passe. Forget it. Later, when I was dean, two captains who had been at Berkeley said we should change things. They helped me write the proposal which got the half-million dollars a year for the Electronics Lab. Here's how it happened.

Beginnings of Electronics Research Lab ##

O'Brien: I was working with the Bureau of Ships on wave forecasting and underwater sound. A couple of captains did graduate work at Berkeley as part of the Navy program.

Two of these fellows were in the Buships' [Bureau of Ships] electronics branch. Toward the end of the war they came around one day and said, "Professor O'Brien, when are you going to modernize electrical engineering at Berkeley. " I said, "What would I use for money?" They said, "How would half a million dollars a year do?" I said, "Boy, that'd really work. "

They helped me write the proposal. We not only got the money; we got a whole laboratory moved up here from Point Loma. They had a lab down there that was working on radar. They had glass blowers who were capable of making vacuum tubes. We had a lab down there in a temporary building. The contact really resulted in the Electronics Laboratory, which Professor John Whinnery staffed afterwards. That lab put Berkeley on the map.

Ziebarth: Was it at this time that you began to stress the importance of faculty doing research?

O'Brien: My most important contribution to the engineering college was to put the whole engineering faculty in the university budget, on hard money, state money. This made it possible for those who wanted to do research to have at least a quarter of their time free for that. Working with graduate students was part of the three-quarters time associated with the university. One-quarter was theirs for research. If they wanted to spend it on a Navy project and charge the Navy for it, that was up to them.

We didn't want to get in the position where we were scrambling to get money and then taking any kind of contract we could get to pay the faculty. We are unique today in that respect. The faculty here are quite happy about this.

This was a hard decision. We could have had a lot more money for graduate students even if we'd gone that route. But all these schools are now scrambling to get money to pay the faculty. Stanford went through a terrific crisis after the war which shut off the period of easy money from the Defense Department. When I was a visiting professor at MIT, eighty-five percent of the academic payroll of the electrical engineering department was paid by outside contracts. I thought that was a horrible situation.

III. BERKELEY IN THE 1930s

Great Depression

O'Brien: I lived in the Faculty Club on campus the first three years [1928-31] at Berkeley. That brought me in touch with quite a number of people. Ernest Lawrence, for example, lived in the club at the same time. We became well-acquainted and played tennis together.

Then I was married in 1931 to Roberta Libbey. We had two children: Morrough, not Morrough, Jr., [born 1932], and my daughter Sheila [born 1935].

During those depression years, faculty members were in fortunate circumstances. In fact, they were the best years I've seen financially because we did not pay federal income tax then, and the State of California was in fair financial condition, at least so far as university support was concerned. We suffered a decrease in salary of only about five percent late in the depression. That's all.

Before the bank holiday, I had put $100 in cash in my lockbox at the bank. Then, after the Crash, the banks were closed. You couldn't get money out of your accounts, but you could go to your lockbox. So, Walter Weeks, professor of mining, and his wife, and my wife and I went to Carmel. They had a cottage for us, and I had a Standard Oil credit card. That's all we needed to live on for a couple of weeks while the banks were closed.

I had lived in Toledo, of course, and my aunts still lived there. Toledo was operating on script, which sold for about fifteen percent on its face value, but the stores there were selling things at the price marked. So we bought oriental rugs and all sorts of things we needed at fifteen percent of the original price.

My mother and father lived in Arizona. I went down there at Christmas time because our term started in August and ended before Christmas. Then Berkeley had a long vacation of about six weeks before the second term started. Our second term ended in May, which was also fortunate because our students were then available for jobs ahead of students at other schools.

The second year of the Depression, Ernest Lawrence, Robert Oppenheimer, and I made a tour of Death Valley and that region, including Boulder [Hoover] Dam, which was under construction. Then we crossed the Colorado River and went down to Arizona. It was an expedition of about three to four weeks. I got well-acquainted with them, and that had a certain amount of influence on my thinking regarding the importance of research in the academic curriculum. Lawrence was getting started on the cyclotron at the time, and Oppenheimer was a theoretical physicist later in charge of developing the bomb at Los Alamos Laboratory.

Campus Life, 1930s

O'Brien: President Sproul used to say that this university paid off in climate and prestige, not in money. But we were well off, and things went well for us financially.

Ziebarth: Berkeley must have been a closely-knit community at that time.

O'Brien: Yes. The faculty was a fraction of the size it is now, or what it was when I retired.

From the very beginning, I met people at the Faculty Club like Dean Lipman, who became dean of graduate studies shortly after that, or Armin O. Leuschner, who was the dean of graduate studies, and others on the budget committee, the committee on committees, and the committee on budget and interdepartmental relations. The faculty club was a convenient place for meetings, and many of these committees would eat and do their business in the evening.

I'll never forget my first experience with President Sproul there. He had the most remarkable memory I've ever dealt with. One day, I met a tall, good-looking man coming through the door. He said, "How do you do, Professor O'Brien. " I didn't know who the heck he was. He was the president of the university.

At Berkeley the young faculty used to usher at events. I did that several years. Whenever I took anyone up to meet Sproul, he's say, "Well, Hello, Joe. How's Mary? How are the kids?" He was amazing.

Ziebarth: Did the Depression change life on campus?

O'Brien: California wasn't in much distress. When the 1929 crash came, I had been in the East and knew a lot of young fellows from college who had gone to Wall Street and who lost money. But out here, when I talked with one of our senior professors almost a year after the crash, the stories about food lines, bread lines, selling apples on the street was all news to him. He missed the whole thing and didn't even know there was a depression.

Ziebarth: Were campus people enthusiastic about the big bridge projects of the 1930s?

O'Brien: Oh, yes. Dean Derleth had been the engineer on the Carquinez Bridge and a consultant on the Golden Gate Bridge and the Oakland-Bay Bridge.

At a meeting on the Society for the Advancement of Science, Derleth gave a talk on the principles underlying the design of the Golden Gate Bridge. At that time it was the longest anywhere in the world. He talked about how highly-stressed those cables were, and we were all impressed.

That was the only time I even heard Derleth give a paper or even heard that he wrote a paper. He was a practicing engineer of considerable ability, a very persuasive man. About half the faculty, mostly in civil engineering, were practicing engineers.

IV. PRE-WAR RESEARCH AND CONSULTING, 1928-41

[Interview 2: May 12, 1986] ##

First Projects at Campus Hydraulics Lab

O'Brien: My first graduate student was a man by the name of Whitney M. Borland. After I spent the summer of 1929 in the East getting started on studies of the coastline, Borland carried on many experiments at Berkeley related to such things as the settling velocity of sand in water and constructing a model of some groins at Long Branch, New Jersey. In the laboratory we looked at what was the smallest wave that would break?

I think that Borland's was the first thesis in the field of coastal engineering in this country. Then I took over the position of Blake R. Vanleer in the hydraulics laboratory and a program that they talked about but hadn't done much about.

This was to study all the fundamental methods of pumping fluids. I had a student fellowship, which was filled by James E. Gosline. He and I went at such things as the water jet pump, the hydraulic ram, the air line, gas lift, things of that kind.

In that period before World War II, this general program of studying pumping methods included centrifugal and axial flow pumps. By that time, Richard G. Folsom was here. He had finished his Ph.D. at Cal Tech and came here at the princely salary of $85 a month. Not that that was what he deserved, but that's all the money I had. He survived.

One of the things that we did was to develop a theory for the method of design for propeller pumps and fans, and we published a paper of that name. That, incidentally, was the reason for my connection with General Electric Company.

Ziebarth: What were your objectives with that research?

O'Brien: There was no decent quantitative theory which you could use to predict the performance of a jet pump, high-pressure water pumps, low pressure water. This would come in handy in dealing with things like sand, where you don't want to use rotating machinery.

Folsom and I put together a book. Then the war came along, and by the time the war was over, the book was obsolete so we never did anything with it. That was one research program carried on by Folsom and later on by Harold Iverson.

We had two kinds of work at the hydraulics laboratory. We had the civil engineering kind of hydraulics, where, mainly, we worked on problems of the shoreline or wave action. The other program in the laboratory had to do with hydraulic machinery of all kinds.

First of all, there was the friction pump. Folsom's thesis at Cal Tech had been on ways of increasing friction. You run a disk; if that disk is very rough, it drags liquid with it, and that can be used as the basis for a pump. As a matter of fact, you can get very high heads or pressure for very small pumps, which is a very handy device. Not highly efficient, but a good pumping system.

That pump existed—it had been invented. It had been in production and was available for sale, but there was no theory of it. So we decided to develop a quantitative method of prediction based on Folsom's experiments.

We also worked on the hydraulic rams. There was no theory anywhere for it, just a qualitative description of it. We turned it into a quantitative thing. You could predict that it would pump so much at such head, at such efficiency, and so on.

We also ran centrifugal pumps backward as turbines and all sorts of things like that, much of which Gosline participated in.

Ziebarth: Were these projects funded by Berkeley?

O'Brien: Our lavish budget for the hydraulic laboratory was about $3,000 a year.

Ziebarth: How did you stretch it to meet the costs of these projects?

O'Brien: What we did was to figure out the things that we could do in the laboratory that we had. We didn't spend much time concocting programs requiring more equipment, because we weren't going to have it. We had some equipment, and the shop built things for us. We built flumes for experiments on wave action, for instance.

Salt Water Barrier Research ##

One really big experiment in those days was on the so-called salt-water barrier. The United States and the State of California formed what was known as the Hoover-Young Commission; Hoover was president and Young was the governor of California. They were considering erecting a barrier in the Straits of Carquinez which would keep the water area above the barrier fresh. This large area of fresh water could be used for irrigation. That was the concept.

The problem was that you had to pierce the barrier somehow for navigation. You had to put locks in it. The question then was: Will salt water go through these locks and into the area above and contaminate it?

The U.S. Engineers approached us on an experimental program to investigate that point. A man by the name of John Chernow was assigned to work here, and that investigation must have continued for about a year in about 1932. We published a paper in 1932 called "Model Law for Motion of Salt Water through Fresh. " The point was this: we had a ship going through the locks. The ship moved in from the fresh water side; they closed the lock. Then they opened the lock on the salt water side, and the ship went out. The lock was full of fresh water, and that was lighter than salt water. All the fresh water then went out of the lock, and the lock became filled with salt water. Then a ship went in the other direction. They opened the gates, it went in to the lock full of salt water. When they opened the gates on this side, the salt water ran up into the fresh water.

We did experiments in a flume and developed the general principles involved quantitatively. Then we did it in three dimensions with a basin and a lock. We operated the lock, and this stuff spread out over the bottom.

Ziebarth: Where was that model?

O'Brien: In the hydraulics laboratory in the old Mechanics Building. It was a tiny little model. That was one of the big experiments of that era. We had a motion picture which I showed various places.

Ziebarth: Does it seem feasible that they could keep a salt water barrier at the mouth of the Delta?

O'Brien: We found out that if you had a big basin, you could dig into the bottom, and you could let this heavy salt water go into that container. Then you could pump it back out. So there were actions that could be taken to counteract the effect, but it wasn't automatic. Just putting a barrier there and putting in a lock didn't solve the problem because the number of ships going back and forth to Antioch and elsewhere was considerable, and the amount of salt that would go through was substantial. So that barrier in the straits of Carquinez has never been brought up again.

But they had other ideas: to put the barrier farther up, for example. A promoter named Rieber did a lot of talking, saying that we didn't know what we were doing, our experiments were not valid, and they ought to put the barrier way down here in the Bay. This was the Rieber plan.

Ziebarth: Describe the hydraulics lab set up in the twenties by Joseph LeConte, "Little Joe LeConte".

O'Brien: It was the center courtyard of the Mechanics Building. It was housed over with an arching glass roof. We had pumps and a Pelton turbine, a water wheel that develops power for building high heads in the mountains. We had another turbine and several different pumps. We did a number of experiments on orifices and weirs and various hydraulic devices. It was just ordinary equipment. It was a teaching laboratory; no research had ever been done there. No papers had ever been written about anything in the laboratory.

Santa Barbara Breakwater

My big consulting project at the time was the Santa Barbara breakwater. A man by the name of Max Fleishmann, of Fleishmann's Yeast, lived in Santa Barbara and had a big yacht. The weather was generally pretty calm in Santa Barbara, but when they had a storm there was no protection, and yachts had to take off to sea because there was no harbor.

Because of his yacht and his interest in sailing, he gave the City and County of Santa Barbara the money to build a harbor. They brought in one of the well-known harbor experts: Drydock Smith was his name. He laid out what became the Santa Barbara harbor.

Because of the protection of the Channel Islands, waves come into the channel from the west and strike the shore at an angle. They break and drive sand eastward.

They knew that there was a drift of sand. Drydock Smith left a gap between the breakwater and the shore with the thought that the sand would go on through there. What they missed was that with the breakwater in place, the breakwater cut off the wave motion which was driving the sand, and sand accumulated behind the breakwater.

So they closed that gap. But then the sand began to accumulate on the west side of the breakwater, and erosion started on the east side. The sand side was cut off: accumulation here, erosion there.

When I surveyed the west coast of the U.S. in 1929 for the Beach Erosion Board, I spent several days at Santa Barbara. I got acquainted with a senior lifeguard and others on the beach. This lifeguard said to me, "You know, that breakwater is causing erosion of this shore. You ought to go down to the Miramar Hotel and that region, because it's just beginning to have an effect there. "

What was going on down there became an object of interest to me. Because Mr. Fleishmann had given the money to build the harbor, he gave the Santa Barbara Foundation the money to pay me to be a consultant to follow the work that was going on. The Beach Erosion Board, of which I was not yet a member, made a study of the problem and came up with a report about bypassing sand. Take the sand where it was accumulating on the west side of the breakwater, pump it over to the beach where it would be naturally, and the waves would take it on from there and everything would be happy thereafter.

I talked with the people making the study. Finally, a report came out. I got a copy of the report here at Berkeley the day before. I got on the *Lark*, the overnight train to Los Angeles, and got to Santa Barbara about five in the morning.

A very wealthy man who'd followed all this and who subsidized various politicians to help them get elected, had a big ranch with a huge stable of Palomino horses. He and his driver met me at the train, and we made a tour of the five city commissioners and five county supervisors. It was a rainy day, and in the rain we went from one to the other.

That night the county commissioners had an extraordinary meeting and appropriated their share of the money to go ahead with the transfer project that was recommended by the U.S. Engineers. This all happened the same day: at night we had the meeting; in the morning we'd gone on foot from office to office.

The project went ahead; they pumped sand over to the beach, and it drifted along the shore. That wasn't the whole story— they should have restored the erosion which had occurred and then put sand on the beach to maintain it. But that was the best that could be done then.

For a number of years I went to Santa Barbara regularly to follow the project. We did a number of experiments there. The University of California had a contract with the Beach Erosion Board, and part of that money went to surveyors in Santa Barbara to make regular surveys of accumulating sand and the erosion.

Ziebarth: Part of the work was privately funded and part university-sponsored work?

O'Brien: Yes. For a number of years, I was paid by the Santa Barbara Foundation. Then, after we got a contract from the Beach Erosion Board, I got no pay for it, but we got the money to have surveys made and make trips down there.

Pacific Coast Survey

Ziebarth: The work you did for the Beach Erosion Board in 1930 surveying Pacific harbors must have been an extensive project.

O'Brien: It took all summer. Then I spent the following year writing the report on it.

I had my Ford car. I went to beaches and got samples of sand, profiles, and all the surveys which had been made of various harbors. All the way from the straits of Juan de Fuca up at Canada to the Tijuana sloughs.

When I visited the U.S. Engineers in San Francisco, one of the fellows there had a friend at the *Chronicle*. While I was in the office, he called the *Chronicle*. This guy came over to talk about what I was doing. Then I went down to Los Angeles and San Diego, picked up a copy of the *Los Angeles Times* on a Sunday morning, and went out to the beach at Tijuana Sloughs to have a nice day in the sun. In the middle of the front page of the *Los Angeles Times* was a headline: "Expert Predicts Closure of Golden Gate. " The expert was me.

I had talked about sand movement along the shore closing up harbors; it did close up in Oregon once in a while. It turned out this guy from the *Chronicle* had written a story and the *Los Angeles Times* took the story, turned it around, and said the Golden Gate was going to close up. That was my lesson in talking to newspapermen.

Other Consulting Projects

Ziebarth: Weren't you also involved in projects in Long Beach and Santa Monica?

O'Brien: Those were consulting jobs in connection with the erosion of the beach around Santa Monica harbor and the beach club there in the late thirties. I was asked to be involved as an expert in that affair. The longest sentence I ever heard was written to qualify me as an expert. It was written by a skillful lawyer using a series of semicolons—it took about three pages.

In Long Beach, there was an argument over ownership of the land in the harbor area. They found an oil field under it, and as they pulled the oil out of the ground, the ground sank about twenty feet. Ford Motor Company and Southern California Edison and a whole lot of companies owned the land. Under the law the boundary lines are determined by what existed when the whole area was in a state of nature.

At a late date, after they'd dug around in the channel and grazed over the area and built railroad trestles across the floodplain, which was the oil field, trying to establish what it was like in a state of nature was exceedingly difficult.

My contribution to that debate was that every time we met to talk about it with the opposition, I said, "You guys had better get together and settle this out of court because you could go on forever trying to settle this one. " They finally did.

Ziebarth: Do you feel this kind of consulting work enhanced your ability to teach or your research?

O'Brien: Engineering is a profession that aims at real tangible works: buildings, homes, all sorts of hardware. So it's important to keep in touch with the practice and the problems of practice. Furthermore, practice tends to define problems of investigation.

Ziebarth: You also did consulting for Weyerhaeuser Company in Washington.

O'Brien: Those were just model experiments. They had a problem of debris accumulating in their log pond. The logs come down the river, and they put them in a big pond, and they're fed into the plant. They had trouble with silting-in, and we did an experiment on the caissons.

We also did an experiment on the caissons of the Tacoma Bridge. It was a very deep channel, and they didn't know how to place the caissons. They had to put this big enclosure on the bottom and build inside it. Then they pumped it out, until it was dry, and they built the towers inside.

The engineer on that job was my classmate from MIT, Ted Kuss. He got worried about currents in the channel. So we did an experiment, not a very extensive one, in the hydraulics lab— by that time we had the College Avenue Pool lab. What happened was that the shape of the enclosure was such that as the water went down in a current, it turned into a whirling dervish—any current did this.

As a result, they had to get all ready with it up just below the surface and then put it on the bottom in slack water in a very short time. They did this and sealed it off, and that worked. But to try to put it in place in a current was impossible.

Ziebarth: Was the Tacoma Bridge the one that had the spectacular disaster in the 1950s?

O'Brien: Yes, Galloping Gertie. I did my work before they were building the bridge. Later on they built the bridge, and it turned out to be aerodynamically unstable, and the deck finally broke.

I did various other jobs of one kind or another, including unwatering pumps for the drydocks up at Mare Island— just consulting jobs.

Hetch Hetchy Water Delivery System

One thing I worked on was of some interest politically. The Hetch Hetchy project was being built, but it wasn't finished. Meanwhile, they predicted that San Francisco was going to run short of water. So they designed a temporary system to take the water over the Coast Range, not through it by tunnel as they had planned to do. The point was to pump water from the Hetch Hetchy system, which had been completed to a certain point, over the hills and into the Spring Valley Lakes over on the Peninsula south of San Francisco. It was a purely temporary thing; it wouldn't serve for more than a year or so.

They asked me to appraise the pumps that were offered. The whole point was, on a pipeline you have to provide for variations in pressure. Usually there has to be a standpipe at the high point. This pipeline would bring water up over the mountains, and they had several pumps involved.

Whenever there was no demand for water, the pumps had to keep running and that would put water up this standpipe. It turned out that Byron Jackson Company put in a bid for a pump which exactly met the requirements. They had the pump, and they'd tested it and knew everything was hunky dory.

Union Ironworks didn't have a pump that met the requirements, but they said they would change a pump to do so. I said, "This project is only for a year, and if you really want to meet this emergency, you'd better buy the Byron Jackson pump. "

They had a hearing over at the Public Utilities Commission in San Francisco in about 1936. A man by the name of Timothy Riordan heard my story; he was chairman of the board. Byron Jackson's pump was ready; and it fit the requirements. The other pump had to be modified; and it might take weeks to do that. He listened to my story, and he said, "Gentlemen, the contract must go to the lowest qualified bidder. A qualified bidder is anyone who puts up a bond of $35,000. United Ironworks gets the job. " Quality out the window.

It turned out the Hetch Hetchy project didn't need the pipeline. They built much of it and bought the pumps, which were stored someplace, but the pumps weren't even cut by United Ironworks. They delivered them as they were, which was with about twenty-five percent too much head.

Pump Testing and College of Agriculture

Ziebarth: Did you work with that Byron Jackson Company again on rating curves for ISA standards?

O'Brien: That was on orifices for measuring flow. Around that time we got the money to establish the pump testing laboratory.

In California, a great deal of water is pumped from the earth by deep well pumps. A motor is at the surface, and a long shaft goes to the pumping unit down at the bottom. It rotates and pumps water up to the top.

There was no place that tested these pumps except the manufacturer's laboratory. All they did was to run the pump to see if it runs all right. As to efficiency and power consumption —all the things that are important about a pump—no one did that. We got together with the University's College of Agriculture, and they said, "That's a pretty important problem. An awful lot of power is being wasted in California because these pumps are not very efficient."

The College of Agriculture helped us get the money to establish a facility over at the College Avenue lab for testing pumps. We had to have a dynamometer for that purpose, and that dynamometer had to have very low friction in order to measure the force causing motion.

We also learned a great deal about ball bearings at that time. We thought we could get the friction coefficients of ball bearings from the ball bearing companies. It turned out we had to do all sorts of experiments on our own. We finally designed the dynamometer, and we tested a lot of pumps.

At the lab we would test anybody's pump and inform them of its characteristics, keeping the facts confidential. We wouldn't turn anybody in.

The College of Agriculture told us, "If you inform the companies of what their pumps are doing, they'll then work to improve them." That happened. For a number of years, right up until World War II, we had some money every year, and Agriculture had this little laboratory over at the College Avenue lab. It contributed to the improvement of pumping in California.

This was an example of the extent to which the University of California was riding on the reputation of its College of Agriculture. The majority of the Senate and the Assembly in Sacramento were from farm counties, and they were so impressed with the work done by Agriculture that anything the university asked for, it got. The whole university really rode on the Agriculture College's reputation. They were the ones who got the money for our laboratory.

Ziebarth: The College of Agriculture was a branch of the University of California?

O'Brien: Yes. Agriculture had a lot of things on this campus, which have been moved, and on the Davis campus. There was a branch of agriculture for research on farm equipment. That same group were academically part of the College of Engineering at Berkeley. The students there were legally and formally members of the student body of the College of Engineering at Berkeley.

Water Jet Pump

Ziebarth: You also worked with the Becker Pump Company in Oakland in the 1930s to develop a jet pump design.

O'Brien: We'd been working on jet pumps. We just designed some pumps for them and told them what they should do to have good pumps.

Ziebarth: Was it unusual that a company wouldn't have its own design engineer?

O'Brien: They had engineers, but they were more or less concerned with the installation and operation of the plant. As for designing pumps, they bought pumps from whoever had them. We simply worked to help them get their system working properly.

As far as I know, we were the only ones doing this kind of thing. There were various experiments on pumps but not much theory of any of it. Cal Tech was then starting work in connection with the big pumps that Byron Jackson supplied for that big dam up in Washington, and they were doing work on centrifugal pumps. But we took on pumping in general, all kinds of pumping.

Ziebarth: Did you make any theoretical breakthroughs? How did pumping change?

O'Brien: The experiment on the water jet pump is still the only paper which has been written. As far as I know, nobody ever worked on the quantitative characteristics of the hydraulic ram. We had a device for measuring pressure, and we were able to predict quantitatively the characteristics of that little pump.

We studied not only the characteristics of the jet pump, but we had good advice about how to makes pumps better in our paper, which was published by the University of California Publications in Engineering.

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Low Pressure Research

Ziebarth: What about your work in low-pressure research with R.G. Folsom and George J. Maslach and and Sam Schaaf?

O'Brien: I'd been associated quite closely with physicists like Ernest Lawrence. Victor Lenzen and Bill Williams taught graduate courses that I attended—not as a student, but just to listen in to get background. I got interested in low-pressure flow, where the pressure is so low the molecules are far apart. It isn't a continuum—it's little marbles rolling around.

I thought that'd be a good experiment to work on in the laboratory. I got in touch with a man who was an expert on low-pressure phenomena. Somehow we got some money and got started on what I would call a molecular wind tunnel. Folsom got interested, and he was the one who really herded it along. Maslach, who became chancellor [vice chancellor for research] of this campus, was hired as an engineer on that project. Sam Schaaf, who became a professor and chairman of mechanical engineering, was brought in as a Ph.D. mathematician.

We got some money and built a little wind tunnel. Then it turned out that when the country's space program got into sending vehicles up to very high altitudes where the air was rarified, they were in the kind of atmosphere created in our little tunnel. This resulted in a very long period of investigation, which I only helped get started. Sam Schaaf, Larry Talbot, and a lot of graduate students worked on our low- pressure project which we built at the College Avenue laboratory. There's still a facility out at the Field Station. Our work became very important for a period when people were trying to understand molecular flow as vehicles went up or down through the rarified atmosphere at high velocity.

Compressible Flow

Ziebarth: What about your work with Jack Putnam on compressible flow?

O'Brien: Jim Gosline, who worked with me as a graduate student, was from a petroleum family; his father was superintendent of one of the oil fields in California. Down at Cal Tech, they had done a lot of laboratory work on the physical and thermodynamic properties of oil, liquid, and gas.

We got interested in flow of fluids in porous media and had this peripheral interest in petroleum because of Gosline.

We began working on the flow of liquid-gas mixtures through porous media. What the petroleum people call porous media looks like solid rock to me. The oil fields are developed around that stuff.

We published a couple of papers. The next thing that happened was that the owners of the Kettleman oil field—it was half-owned by Standard of California and half by I-don't-know-how-many different owners— were beginning to think that they'd better worry about reservoir dynamics and the flow of this fluid. If you pull oil out of an oil well, the pressure toward the well drops. If it drops very far, gas comes out of solution, and these fine pores get blocked with gas. A lot of oil is left in the ground in any field. The problem was how to develop the Kettleman field in a manner to get the most oil out. If they did that, the owners of the other half of the field had to be assured that they were going to get their share.

We had had the experience of Signal Hill down at Long Beach. Every lot owner sold his rights to the underground oil to somebody, and they each put a derrick on it. It got to be that each little house lot was trying to pull out the oil there before the guy next door got it through his well.

Standard Oil took it up with these owners, and they formed the Kettleman North Dome Association and decided that they would seek an orderly development of the oil field to maximize production at minimum cost.

Professor Jack Putnam took a leave, went down to Kettleman, and established a laboratory using a lot of the physical data these guys had developed at Cal Tech. Jack was down there for two or three years.

Ziebarth: Were you involved with that project ?

O'Brien: I was involved in the sense that we had a number of meetings with Standard Oil people, and they brought some of the Kettleman North people up to talk about the reason for some systematic study. Should the laboratory be in Berkeley or down at the field? Since they had to work with a lot of samples and people, it seemed sensible to set up a permanent laboratory down there. So Jack took a leave and went there. After it got going, I was out of that one.

Sediment in Steady Flow

Ziebarth: What about your work in suspension of sediment in steady flow, the work associated with Wilhelm Schmidt?

O'Brien: That was an interesting thing. A man by the name of John B. Leighly, a professor of geography here on the campus who was interested in sediment and flow problems, tipped me off to an interesting paper he'd seen written by Wilhelm Schmidt, published in Berlin: "Die Massen Ausschaft in die Freien Luft" [Mass Transport in Free Air].

Wilhelm Schmidt was interested in the movement of seeds and spores in the atmosphere. There was a forest here, and seeds got into the air and traveled hundreds or thousands of miles. He wrote this little brochure about it. The treatment wasn't mathematical in the sense of symbols, but he described the process.

I got this pamphlet and spent some time translating it. We had this course with the army students about river and harbor hydraulics, and we talked about it. The upshot of that was publication of an equation on the diffusion of material and turbulent flow.

$$\left(\frac{\epsilon}{\rho}\right)\frac{dc}{dy}+cw=0$$

Then some graduate students used that analysis with sediment samples to analyze the flow, flow distribution, and characteristics of real streams and real sediment underneath.

Before the war there was quite an interest in it. I presented a paper on it at the American Geophysical Union in Washington around '35 or '36.

It seemed to be an original contribution, but later on we found out that a man by the name of Hunt in India had done practically the same thing. In those days, there weren't many people thinking about fluid flow in terms of fluid mechanics, but one place where that similar thinking was going on was Cal Tech in the aeronautical school. Dr. Theodore Von Karman, a German engineer, a scientist, had been brought to Cal Tech as a professor, and he had a number of people working with him on the faculty, and he was close to Douglas Aircraft.

We had a joint seminar; one year we met at Berkeley and the next year we met at Los Angeles. I got a letter from Wilhelm Schmidt, and he said he had heard Dr. Von Karman talk about this equation. He more or less implied I had plagiarized. Fortunately, right in the first paragraph was a full reference: Dr. Wilhelm Schmidt, "Die Massen Ausschaft in die Freien Luft und verwandte Erscheinungen." So he was happy.

Dr. Von Karman was impressed with our work. He was a practical and theoretical guy. About thirty years later, at the University of Florida, a man I'd known for a long time was going to speak. He went back to his days with Von Karman at Cal Tech and said that Dr. Von Karman came into the lab one day and said, "Mike O'Brien at Berkeley has this equation. Now what can you do with it?" The man's lecture at Gainesville, which I attended, was on just that.

Here's the surface of the water. You measure the concentration and you get a curve like that; the slope of that curve, plus the concentration times the settling velocity is equal to zero. All reasoning of Wilhelm Schmidt shook down to that. He had the right idea, but he wasn't mathematical.

So Professor Christiansen and several people at Berkeley wrote their master's thesis on applications in flow in rivers. One paper we wrote was about the transportation of sand in pipelines. There the principal fact was that as you pushed the water through the pipe, it's turbulent. The sand that it's carrying is distributed across the pipe in accordance with that equation. Also, that paper included the characteristic of centrifugal pumps handling sand in suspension.

Columbia River Estuary Model

O'Brien: I was a consultant to the Corps of Engineers, and there was the problem of creating the best channel through the estuary of the Columbia River. The existing channel was somewhat of a nuisance because it went in a long arc around the south side of the estuary. It was said that the channel ought be cut straight through a shoal and right straight out so ships could come down the river section and then go straight out to the Pacific Ocean.

Also, there was the problem of the erosion of the outer beach at the mouth of the Columbia River. The first part of that experiment was to do what we could about the erosion. The experiments that Bruce Rindlaub did on the movement of sand by wind were part of that program. We proved that the erosion of the outer beach wasn't caused by waves but was caused by wind that had blown the sand from the beach over into the lagoon. So that eliminated that part of the model study.

Then we build an hydraulic model of the estuary of the Columbia River with tides, currents, and everything involved. That was in place for three or four years in the mid-thirties under contract to the Corps of Engineers. They stationed one of their men here full-time. While Rindlaub was here, he worked on it, but he left shortly after we got started. We consulted people in Europe who had worked on models of the estuary of the Mersey and other rivers in England and France.

We had done model work on a very small scale only. Recent studies have indicated that those models are no good—I always thought they were flawed. We made our model much larger than any of those that were built before. As far as I know, it was the first hydraulic model of an estuary or anything involving tides and currents, and almost the only one of a coastal area in this country or anywhere.

Following World War II a whole new generation of people came along, and they didn't know anything about this work. People studied the Columbia River who didn't know we'd built a model and had all these details. They duplicated our work unnecessarily.

When you have an estuary—it was about twenty miles long and five miles wide— the tide wave goes through there [indicates with hands] and high water occurs here, and then here. Each area goes through a different tide. When we made the comparison of field measurements— currents measured in the estuary—with currents measured in our model, we had to carefully mark out the areas and the timing. We got very good agreement.

Ziebarth: It must have been complicated to integrate all those factors into a model.

O'Brien: We did it photographically. We put little circular pieces of paper, confetti, on the surface and photographed them. Each one of these pieces of paper made a mark depending on how fast it was moving. The length of the mark was a measure of the velocity. We got photographs at each phase of the tide and could compare every phase of the tide at every point with all the measurements we had in nature, which were very extensive.

The Columbia River model experiments were done under contract to the U.S. Engineers. Before the war, Berkeley did most of the work done in this country. Now, it's done all over the country. Laboratories at MIT, Texas A&M, University of Florida. Oregon State has a big tank for wave studies. There are too many places, as a matter of fact. There isn't enough research money to support them.

Review of Personal Bibliography

Ziebarth: Which of your published papers do you see as most significant?

O'Brien: [reading comprehensive personal bibliography] "Studies of the Pumping of Viscous Liquids" with M. W. Todd, published in 1926; "Estuary Tidal Prism Related to Entrance Area," published in 1931; "Checks on Model Law for Hydraulic Structures," published in 1932; "Analyzing Hydraulic Models for Effects of Distortion," 1932; "Review of the Theory of Turbulent Flow and Its Relation to Sediment Transportation," 1933; "The Hydraulic Ram" with James E. Gosline, 1933.

[referring to just-cited paper] Where there was an accident and the oil-well pipe was dented, the only way to get any energy down there to pump oil up was to put it down as a gas or a liquid under pressure past the dent. They couldn't put in any other pumping device.

[continuing] "Model Law for Motion of Salt Water through Fresh," with John Chernow, 1932- 34; "The Transportation of Bed-Load by Streams," with Bruce D. Rindlaub, 1934; "The Water-Jet Pump," with James E. Gosline, 1933; "Velocity of Large Bubbles in Vertical Tubes," with J. E. Gosline, 1933; "Models of Estuaries," 1935; "The Transportation of Sand by Wind," with Bruce D. Rindlaub, 1936.

I spent some time with the man who gathered all the data on this last paper. This was the first experiment done in the field or laboratory. Rindlaub became a general of [the Army Corps of] Engineers. He was out getting measurements of sand movement on the beach at Columbia River when the wind was up to 35 knots, and sand blowing in the air, holding his instrument. He was a bulldog.

[continuing] "The Transportation of Sand in Pipe Lines," with R. G. Folsom, 1937; *Applied Fluid Mechanics*, a textbook written with G. H. Hickox, 1937; "The Design of Propeller Pumps and Fans," with R. G. Folsom, 1939; "Some Problems of Horizontal Steady Flow in Porous Media," with J. A. Putnam, 1941. Putnam's paper led to the work down at Kettleman Hills.

"Hydraulic Test of Spillway of Madden Dam," 1937, and "Transportation of Sand and Gravel in a Four-Inch Pipe," 1939. The whole thing about piles and forces came about by way of the work we did at the College Avenue Laboratory pool.

[continuing] "The Shock Produced by a Collapsing Cavity in Water," 1947; that was a bomb test.

[continuing] "Graphical Construction of Wave Refraction Diagrams," 1948; "The Causes of Plunging and Spilling Breakers," 1949; "Lag and Reduction of Range in Tide Gage Wells," 1950; "The Forces Exerted by

Surface Waves on Piles," with J. R. Morison, J. W. Johnson, and S. A. Schaaf, 1950; "Wave Refraction at Long Beach and Santa Barbara, California," 1950. That was the first use of refraction diagrams.

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[continuing] "The Forces Exerted by Waves on Objects," with J. R. Morison, 1952; That's now known as the Morison equation; I have a group report an inch thick on the Morison equation.

[continuing] "The Engineering Profession," 1956; "The Engineering of Large Systems," 1964; that was in a book by John Whinnery that I worked on a lot. There was a lot of talk about systems. Some diddly-flip mathematics involved. John Whinnery was asked to write a book about the practice of engineering, and he asked me to contribute to it. I suggested I take this subject of systems.

A telephone system is a system. An airplane is part of a transportation system. Systems consist of two or more things that work together. A motor drives a pump. You've got to proportion the motor so it has enough power to drive the pump. And it has to drive it at the right speed. So you start out with electric energy, you put it through a motor, and it drives a pump. Out comes water at certain pressures and temperatures. That's a very simple system. You just go on up from there.

So much talk was about systems in a purely theoretical way, which kind of annoyed me, so I took up the subject of systems. One system, what this describes, is the microwave system which AT&T put in this country around thirty years ago. So I visited Murray Hill [AT&T laboratory in New Jersey] and got some of their reports, which I used as an example.

Ziebarth: What's wrong with theoretical analysis?

O'Brien: It's all right. But the impression was that you "operation research" these systems. It wasn't really anything new in many respects, but the people who were pushing this were making a great to-do about it. It's an important subject, and, furthermore, many engineers don't really understand that what they're doing is part of a system.

I spent a lot of time working on jet engines and working with the jet-engine people. You've got a turbine being driven by hot gases. That turbine is driving a compressor which supplies the gases. You can have the most perfect turbine in the world, of very high efficiency, and you can have the very best compressor of highest efficiency, but if the two don't have their best efficiency at the same conditions, you may be very badly off. So, when you talk about systems, you have to have compatibility between the pieces.

Then you have to have some kind of control system. That control system has to have limits of all kinds, too. So it gets to be quite a subject if you follow it.

[continuing publications review] "Equilibrium Flow Areas of Tidal Inlets on Sandy Coasts," 1966; that's a followup of the one in 1931, a much more complete paper. There were thirty-five years between the two papers. [continuing] "The Hydraulic Coefficients of Tidal Inlets I," 1974. That's quite good. Nothing much since then.

Contributions to Hydraulics Studies ##

Ziebarth: What do you see as your most important contributions to the field of hydraulic studies?

O'Brien: We were interested in all sorts of pumping, and I was in mechanical engineering. So I took pumping machinery as a research program, and that resulted in a number of papers on the water jet pump and the design of propeller pumps and fans. Some of those papers are unique; no one else has ever written about them, especially the water jet pump and the hydraulic ram.

So far as my contributions are concerned, the paper on the distribution of sediment in turbulent flow, the relationship between sediment in suspension and turbulence in flowing streams, pipes or streams, seems important. Each one of these were quoted in numbers of papers. Once we formulated this theory, people like Ned Taylor, who went to Los Angeles as a professor, and Christianson, who became a professor at Utah State, and a number of others followed through. On pumping machinery, we didn't have many who carried on the work, but Jim Gosline became the president of Standard Oil of California.

In the field in which I've been a consultant, I've never published anything because my work with General Electric was all confidential, and I have rarely written any reports. I was advising the senior managers of a business. For the most part, I was looking at troubles: What's going wrong here and how can we fix it? It made it simpler if I didn't write anything because then there was no problem of spilling the beans on anyone.

The war training program is one of the main contributions we made because we really had a big show out here helping the aircraft and shipbuilding industries.

Contributions to Coastal Engineering Studies ##

Ziebarth: What about your contributions to the field of coastal engineering? Didn't you do the first quantitative work in coastal engineering?

O'Brien: I started that kind of work in this country in 1929 for the Corps of Engineers back in New Jersey. Then in the summer of 1930 I made a trip along the Pacific Coast from the Strait of Juan de Fuca down to the Tijuana Slough on the Mexican border. In the course of that, I became very much interested in the problem of forecasting—of what causes waves and how big are they.

But the people at Scripps were the ones who did the work on forecasting as a formal objective. I'd thought about waves in general, made various comparisons, and written formulas. But the people at Scripps had a contract, and they were to come up with a quantitative system, if possible, for forecasting. I was the Bureau of Ships' representative dealing with them on that.

On a weekend down at Carmel [California], I got to looking at some wave data. I said, "Well, this is all possible if you put it all together in dimensionless form. " Then I wrote Dr. [Harald U.] Sverdrup [of Scripps] a letter saying I thought if the data were put in this form, it would be simple to use.

The next thing, a publication came out of the Navy Hydrographic Office, I think it was, on the forecasting of waves, in which all the data were plotted the way I'd suggested, and they didn't mention my name at all. I didn't care a damn. Others like [Professor] Wiegel did.

Ziebarth: Professor Wiegel mentioned [at the 1987 symposium in O'Brien's honor] your work with densimetric models.

O'Brien: This had to do with the proposed salt-water barrier in the Strait of Carquinez to keep the ocean water out of the Sacramento Delta. We did experiments on that and came up with a basic relationship, the model laws governing that kind of phenomenon in answer to that question.

Ziebarth: What about your work on a movable-bed model of beaches?

O'Brien: Many models are built with a concrete surface. They model what the survey shows is there. A movable-bed model, on the other hand, is one in which the sand is modeled the way it was when you made the survey, but then the model takes off and changes in some fashion you hope represents nature. Movable-bed models are something we worked on.

We also built one of the Columbia River. There had been models of steady flow, flow over dams. But there'd never been a model anywhere which really represented the tidal conditions of ebb and flow of the tide. That's much more complicated to do. There had been some models way back by Osmond Reynolds, Vernon Harcourt, way back in the eighties and nineties: extremely small models with large-scale ratios, large reductions in size, which they thought were okay. Subsequent studies had indicated their models were no damned good. Furthermore, the scale ratios they used were incredible, representing the whole Mersey River in an area less than half the size of this room.

This model of the Columbia River was the first in this country to represent tides with great accuracy.

Ziebarth: Were there technological advances that made that kind of model possible?

O'Brien: No. We were it. We corresponded with the people who had worked with these older people. I remember I wrote A. H. Gibson and asked about the model of Osmond Reynolds of the Mersey. He told about their experimentation and finding a sand which they thought represented the river.

Ziebarth: What about your work on the settling rates of sand?

O'Brien: Way back when we started studying this subject, I thought the velocity with which a particle settled was the most important characteristic because when waves throw sand into suspension, it settled. While it's in suspension, the wave is moving onshore and offshore.

I didn't go through all this reasoning then, but I thought, "Gee, the settling velocity is really the important characteristic." You can get other measures, diameter and so forth, but what counts from the standpoint of wave action is the settling velocity.

I served on the Beach Erosion Board and its successor, the Coastal Engineering Research Board, for forty years. During that period, they even went to a system, a rapid sand analyzer, in which they measured the sand settling velocity, but then they didn't report it; they converted it to equivalent diameter of quartz sand. So the little experiment I reported on became understandable in terms of the modern criterion of similarity. We could compare them because I'd measured the velocity of the sand.

Ziebarth: Some of your work focused on how erosion affects erosion structures.

O'Brien: Sea walls. A lot of nonsense had been talked about. You see, all these structures on the shore are expensive. If you build them heavy enough to withstand wave action, they're going to cost a lot of money. The simplest costs, say, a hundred dollars a linear foot. Expensive. Others run to a thousand dollars or more per foot to protect the shore.

People going to the shoreline see erosion, and they see a structure, and they say, "Aha, the structure caused the erosion. " Well, not many people are crazy enough to build a structure if there isn't any erosion. You can always be sure if you see a structure, there's been erosion. People try to combat it, hold it with a structure.

The literature is full of statements about how this structure caused that erosion when this structure was really built because that erosion was happening and they wanted to stop it. Some pretty bright people, including a lot of professors, fall for that kind of nonsense.

The whole subject of study of coastal engineering was started when the Corps of Engineers employed me to start these studies in New Jersey. Those were the first systematic studies I think in the world, not only in the U.S. I don't mean observation by geologists, who went out and observed this or observed that. This was the beginning of an orderly study and experimentation set up in 1929.

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Ziebarth: At the commemorative symposium, Professor Joe Johnson recalled that you did some work using Snell's law to plot trace refraction of waves.

O'Brien: Yes. Visualize a map with contours on it. Waves coming in toward shore tend to turn to become parallel to those contours. They don't make it, but they turn that way. That's called refraction.

The first use of such diagrams was a study I made of Santa Barbara in California. There had been sketches, possibly quantitative sketches, in geology texts that mentioned this. But the point Professor Johnson was making was that the first use for engineering purposes by an engineer was the Santa Barbara study in 1937.

Ziebarth: How did it occur to you to do this?

O'Brien: I knew that waves were refracted; the theory of wave motion indicates that they have to turn to become parallel. And I wanted to know what the waves were like when they hit the shore at Santa Barbara. What I found out was that no matter where they started, the angle that they hit the shore was almost the same within a degree or two. I showed that the wave motion at Santa Barbara was almost all the time in such direction as to drive sand eastward.

I was trying to explain why the existing breakwater was causing so much erosion. The breakwater was built to form a harbor, and they left a gap at the shore, thinking that this transfer of sand would go on. But they forgot that when the waves hit the breakwater, they were reflected, and there was nothing to drive the sand through. It collected in the harbor. So they closed the gap at shore, but the sand piled up on the west side, and there was erosion on the east side. I was trying to explain all of that.

Ziebarth: Professor Johnson also mentioned that you did work on wave forces on structures, especially pile structures, which are important to offshore drilling.

O'Brien: Way back, before we worked on structures in the ocean, I got interested in what's called virtual mass: the fact that when you accelerate an object in water or fluid, not only do you have to push to make the object itself move, but the fluid around it has to move. You've got to provide enough energy for all that. In the equation—Newton's equation—force equals mass times acceleration; that's the acceleration term. Then there's another term on drag. You push things along at a steady pace; still there's a drag. Ships at steady speed have a drag. Propellers have to push.

We did some experiments on ships' hulls and found out what the virtual mass was. Then Jack R. Morison, a graduate student, wanted a topic, and I suggested we take the general proposition of bodies in general, mostly spheres. We did a lot of experiments and wrote a paper on that.

But while that was going on, the matter of structures in the ocean came up. In Texas towers were built by the Air Force for observation, but mainly the oil companies were building platforms out in the ocean in the 1940s and 1950s. It turns out that what we had done on virtual mass and the forces on objects was just what was needed.

Morison and I were asked to write a paper for the Institute of

Petroleum Engineers on the forces on objects. Morison, who'd done the lab work, wrote the paper. I'm saying all this because Professor Wiegel has now come up with the notion that the equation—about which there have been books written— is really the O'Brien-Morison equation. (laughs) The first paper was highly theoretical. The second one was practical. I never worried about who got credit for things. So I never said anything about it.

That's it. We did the first work on the forces on objects, which included the virtual mass, the acceleration term.

Ziebarth: You also added a term to the total force equation?

O'Brien: That acceleration term. Waves go back and forth. They go to zero velocity. Then they go to high velocity. Then they go down to zero. So there's both acceleration and deceleration, and that term accounts for the force during that period.

Ziebarth: What about your theory of the relationships of inlets along the coast to bays?

O'Brien: It was just an empirical relationship. Big bays like San Francisco Bay have a big entrance. A little bay has a small entrance. Nobody's ever noticed this before, and I wrote a paper about it way back in 1931.

Ziebarth: Were you the first to use the term, "coastal engineering."

O'Brien: I guess so. Way back.

Ziebarth: At the symposium, John Whinnery and Joe Johnson noted that you organized the first international conference on coastal engineering.

O'Brien: What happened was that the ambassador of the Kerensky government, Boris Bakhmeteff, was a noted hydraulic engineer. When the Kerensky regime fell, Dr. Bakhmeteff became active in American engineering circles, and it turned out he became chairman of the Engineering Foundation.

I'd invited him out to spend a month or so. We had been working on waves for quite a while in the lab, and when he saw all this work going on, he said, "Who do you people talk to?" We said, "Not much of anybody gives a damn about what we're working on. He said, "Why don't you get it recognized by setting up a council under the Engineering Foundation?" Okay. We wrote some papers and put it through, and pretty soon we were a council.

Then the council talked about what to do. Bakhmeteff was interested in the exchange of ideas, a forum for discussion of papers. So we decided to have a meeting. We put it all under university extension and had it at Long Beach, California, because the southern area is where the beaches problems are.

There was a lot more interest than we realized. A hundred and fifty people showed up. That was in about 1950. Then we had meetings at Houston and Chicago, Cambridge, around the country.

One of the men who visited us regularly was a man by the name of Pierre Danelle, professor at the University of Grenoble in France. Danelle said, "This subject is of worldwide interest to some extent. Why don't you have an international conference?" We said, "Okay. How about at Grenoble where you are?"

So they had one there, and now we've met in London, Copenhagen, the Hague, Sydney, Australia, Canada, Vancouver, and around the U.S.

Ziebarth: What kind of work is going on in the field?

O'Brien: All kinds of things. They're building harbors, breakwaters. Nourishing beaches. The literature of the field is immense. It turns out waves are pretty complicated phenomena, to forecast them, what their characteristics are, the velocity, the orbital velocities at the bottom, and all sorts of things are important.

V. DEAN OF ENGINEERING—EARLY YEARS, 1943-45

Appointment as Dean of Engineering, 1942

O'Brien: Just as chance played a role in my becoming interested in coastal enginering and in being appointed chairman of the mechanical engineering department, chance was a big factor in my becoming dean of the College of Engineering. In 1942, they had a committee, as usual, to pick a new dean. Professor Raymond T. Birge of physics was chairman, and Bernard A. Etcheverry from engineering was a member. There were five of them. Those committees are not supposed to leak information, but of course they did, and they told me, "Well now, Mike, you're going to get the job. Now do it properly. " Fatherly advice.

President Sproul invited me over one day to talk about this matter. I thought I knew what he was going to tell me. But what he said was, "We have these two colleges, Mining and Engineering, as separate entities: they ought to be together. But every time I talk about it to the mining people in San Francisco, they just go, 'Oh, we're going to slight mining. Mining is traditional. We have the Hearst Mining School. Old Man Hearst was a mining engineer. '" Oh, boy. They were all very influential people who had protected the mining school.

Sproul said, "If I appointed a miner from the mining school, a graduate, I could combine the colleges, and nobody would object. " I said, "Well, I think you should do it right now. " While I was still there, he called each and every member of the committee and said, "O'Brien is here with me, and I've told him of your decision and recommendations. I said we ought to combine the colleges, and he agrees with me: get a miner to do it. " That kind of surprised them. But they all agreed and said it was OK with them.

Then he called Dr. Donald McLaughlin who was working in the east and offered him the job. Don accepted. He had also heard about it. That's why he came here.

They combined the colleges. He served just about six months as dean of engineering, until he was offered the job of vice president of Cerro de Pasco Company. After he resigned, Sproul offered me the job of dean.

To be frank about it, I don't think McLaughlin knew straight up about engineering. He was a geologist, and he realized he was in over his depth. Furthermore, his mother had been the secretary to Phoebe Apperson Hearst, the wife of George Hearst. The Hearst family owned the Cerro de Pasco mine down in Peru and the Homestake Mine in the Black Hills. Gold and copper. Depression or boom. One or the other was going great.

The appointment as vice-president of Cerro de Pasco meant a considerable increase in salary, I suspect, but it also solved a problem for him. He didn't have any interest in being an administrator; he was a professor at Harvard in geology. He was an awfully nice fellow, very pleasant to talk with, but actually for guiding engineering, he made a great mistake to come.

The headquarters of Homestake Mining is in San Francisco. San Francisco was an important mining center until World War II, but mining in California had essentially ended. Many of the university's older graduates of mining had gone into the mining business and done well. The family of Philip Bradley, a member of our advisory counsel, had been in mining for two or three generations. All those people were jealous when mining was combined with engineering. They suspected that it would be the end of graduates coming out with some mining background, but they knew the College of Mining wasn't much anymore.

When Don resigned and I was appointed, Sproul had a luncheon meeting for me at the Bohemian Club in San Francisco. He invited the mining people. We gave talks, and I promised them I would not forget mining. Oh, boy. They got a real good show over there.

Merger of Engineering Departments into One Department ##

O'Brien: The main thing that I wanted was to combine the engineering departments into one department. I figured it out. You see, in the university, the departments are the main budget units. The dean had only a kind of a vague connection with it. I didn't want to sit around and argue with seven department chairmen about what was going to happen. Combining was really a ten strike for engineering to get things done.

Now they've gone back to the separate departments structure, but the dean has budgetary control. Then the budget went to the department; it was only a courtesy if they told the dean what they were doing.

The simple, direct cure for that was to have engineering be one department, and it was one department all the time I was dean. All the budgets had to go through me. All the appointments had to go through me. Not that I dictated them, but I was in the chain. They couldn't go around me any more.

The need for this structure became apparent to me just by being a department chairman and by having done all sorts of things around the dean.

Ziebarth: Were there objections to your action?

O'Brien: There was tremendous transition, but engineering was a very small affair. The whole postwar development came about primarily through people that I brought in. There weren't any vested interests as chairmen.

Ziebarth: I have heard that when Earl Parker and John Dorn came into the materials science department after the war [in the mining building], they were given offices in a basement janitor's closet.

O'Brien: You know what. As long as the office had a "B" in front of it, it meant basement. One of the great things I did was start numbering from the bottom up. All the basement numbers were eliminated, and everybody forgot their problems.

Ziebarth: Was there resistance to the merger of mining and engineering from the mining school?

O'Brien: No, they were subdued. They knew who was boss. They'd agreed. Most hadn't done any research, except Walter Weeks, a good friend of mine, who wrote papers on mine ventilation.

Ziebarth: How did you get the nickname of "Black Mike"?

O'Brien: When I was a freshman in high school in Toledo, Ohio, the instructor said one day, "Your name is too complicated. From now on I'm going to call you Mike. " From that day to this, I've been Mike, even to my family.

It's an event when somebody calls me Morrough.

I had dark hair and spent a lot of time in the sun and usually had a tan. Some people thought I was a little dictatorial, I guess, autocratic. But I never sought any kind of administrative job at the university. It was a big surprise and nothing I wanted, and I got out at the minimum age I could retire to stop the paper shuffling.

I never gave a hoot about whether I got fired or not. Once we got started headed for a really top-flight graduate school, I was pretty ruthless, I think, in dealing with people, and trying to get them jobs if they didn't fit here. For those who had gotten tenure, I tried to figure out something they could do that was useful. If they didn't fit—well, I still see some of my mistakes around here. But we also got some pretty good people.

Faculty Recruitment ##

O'Brien: Early on I decided I was not going to bring in senior people with reputations who would bring graduate students with them and all sorts of demands for support for that. Furthermore, we didn't have the money. The only salvation was to go after young people like young John Dorn, who was at Battelle Institute as a postgraduate fellow.

There was a man by the name of Professor Stephen Timoshenko, who was with Westinghouse Corporation and a professor at the University of Pittsburgh. He decided he wanted to come to the West Coast. That got noised about, and it turned out to be a competition between Berkeley and Stanford to get him. He was a well-known man who'd published several books.

I put up all the arguments I could to get him here, and they weren't equaled. I think Stanford got him because of the dry climate, but they also had a lot to offer. He went there.

This convinced me I should never again try to get a senior professor who had his position established; the heck with that. From then on, I didn't recruit anybody as a professor. They were all assistant professors, but the key people we got came as engineers on contracts without even an academic appointment.

I made some awful mistakes but also we got some very good ones that way. But we didn't promise them anything but a job.

Earl Parker came here as an engineer on a project, without even an academic appointment. So did Maslach. So did Joseph Pask in ceramics. In all cases, I was looking for young people that we could afford, and I thought they were better anyway. A full professor with standing at Illinois or Harvard or someplace wants all sorts of assurance about research support and he wants graduate students to come with him. We were young and didn't have much reputation.

Ziebarth: Was it hard to sell younger people on the university at that time?

O'Brien: It took some talking, although I didn't promise them much. Dick Folsom came here out of his own initiative.

We had annual meetings jointly with the Cal Tech aeronautical faculty and mechanical engineering here. One year we'd meet there, and the next year we'd meet here. Dick Folsom was an assistant down there; he came up here a couple of times when he was at Cal Tech and liked us. He took the job. There weren't many jobs then. He came at $85 a month. That's all the money I had—half an appointment, half an assistantship.

Ziebarth: Would faculty have been offered an unusual amount of independence here as to make their positions attractive?

O'Brien: Berkeley had a fine reputation in chemistry. There was Lewis and Randall's book on thermodynamics, which was well-known, and the department was becoming outstanding. Ernest O. Lawrence and Robert Oppenheimer were both on the faculty in physics. So other departments and the university as a whole had a pretty good reputation. But engineering didn't share that up until wartime. Graduates did well, but it was an undergraduate program taught by engineers, not much on the applied science side, not much of the modern approach to engineering.

President Sproul used to say we paid off in climate and prestige, not money. That was an appeal. I think this area is the most stimulating I visit, and I've lived all over the place. Massachusetts and Indiana, Mexico. The university prospered because it keeps people at a high pitch of activity. It's a stimulating area. So we got them.

Ziebarth: Did you have problems keeping people because of low salaries?

O'Brien: Dick Folsom, the chair of mechanical, went on to Michigan, but he looked forward to being a college president. I guess he figured it looked as though I'd be dean for a while. We only lost people in the sense that I got jobs for people I didn't think belonged here. I sent them to Colorado and Utah, all over. We had all sorts of requests for suggestions, and I'd suggest them.

(laughs heartily)

Integrating Engineering Design into Curriculum ##

Ziebarth: What were your concerns about engineering design?

O'Brien: The former departments became divisions of the engineering department, but engineering design was never a division. I worked on it, trying to get professors of engineering to teach design, but I certainly wasn't very successful.

Professors have the idea that they ought to know the answers to everything that they talk about. They can give a lecture, and they can at least be sure that they know what they're talking about if they talk only about what they know. But if you take a design objective, any design objective, there are always compromises. You can't have it perfect, and you can't compromise cost, safety, and all sorts of things that come into it.

The resistance was mainly that the professors didn't really know enough about engineering. Most of them hadn't done enough engineering, hadn't been involved in real engineering problems, and they just couldn't visualize what it was about. Most of our engineering faculty aren't engineers as far as I'm concerned. They're specialists in applied sciences, but they aren't really engineers.

In mechanical engineering and in engineering generally, we have specialists who are really applied scientists but who would flounder incredibly if they tried to design anything. They can design experimental equipment with a lot of help, but otherwise they are lost.

In Europe professors aren't appointed until they have practiced engineering. In Europe, the avenue is through practice. When they get to a certain level of competence and prestige and standing, they participate in a competition and they are selected for faculties.

The faculty ought to be rated: these people are honest-to-God engineers, and we should have a certain number of them. The rest of them are applied scientists, and pay scales and so forth would be different if I had anything to do with it.

Professors Etcheverry and Harding said to me many times, "I'm primarily a consulting engineer, and the University of California is my principal client. " They were more helpful to students than any other faculty, and yet they were the busiest people on the outside doing consulting work. But they were available in their offices. Professor Joe Johnson and others who were students then say any time they wanted help, they'd just drift down to Etcheverry's office, and he was usually there. He did his work weekends.

Some people in engineering, Bob Steidel in mechanical, for instance, took to design, worked on it. Recently, there's been quite a move in that direction again. The fact is that engineering aims at practical results: a bridge, a highway, a jet engine, a radar system. It has to be built. It has to be fabricated. People have to turn machines and make parts, and the engineer has to specify the quality of the material, the tolerance and so forth. And it's pretty hard to work out a system that is both effective and cheap, inexpensive anyway.

We never got department called design. I worked on it and tried to get more of it introduced, but I didn't succeed.

The whole idea that the professor knows the answer is a weakness. If the professor's answer is, "I don't know, but let's see what we can do about it," he is the more experienced person guiding the students to come to some result. But instead faculty want to give a lecture and tell a student this is how you do it.

VI. WARTIME RESEARCH AND CONSULTING, 1941-45

[Interview 3: October 27, 1986] ##

Wave Forecasting

O'Brien: During the war, I was the consultant to the research branch of the Bureau of Ships. They knew I had an interest in shoreline phenomena, so I became the representative of the Bureau of Ships in connection with the work of Harald U. Sverdrup and Walter H. Munk at Scripps Institution of Oceanography of the University of California. Roger R. Revelle from Scripps, who was in the Bureau of Ships, knew of my interest and involved me in the project.

I worked with them on wave forecasting. They were doing the actual work, and I was reviewing it as the contact for the Bureau of Ships.

The idea of wave forecasting was to try to get a handle on surf conditions because we were engaged in many amphibious landings, including Normandy, the South Pacific, and so on.

Vehicles for Amphibious Landings

We worked on dukws and the water buffalo. The dukws were wheeled vehicles to operate in the surf. Water buffalos, developed for Food Machinery Corporation, were track vehicles. Tanks. We used tracks to propel them. Up on top, the track was in a housing. At the bottom it was scooping up water and pushing it backwards; then it went forward. Our experiments here had to do with that propulsion scheme.

The first work on the water buffalo was for the Marine Corps and the Navy—they said they wanted an amphibious tank. Food Machinery said, "No, we're too busy. But why don't you go to Berkeley. They know all about tanks. "

I spent some time on this project. We were told they wanted an amphibious tank with certain properties. We thought about that for a while. Then I went down to Fort Knox, Kansas. I met with a group of about four or five colonels who were concerned with Fort Knox, its tanks, and its tank command. I told them what we were trying to build. One of them said, "Professor, did they ask you to make it fly also?"

Later we had a room up at my house closed off, and I had an engineer, a draftsman, making sketches. We built models and ran them up and down.

The Army requirements were fantastic. They just added one thing after another that made it practically impossible.

Ziebarth: You did some work on campus on the water buffalo project?

O'Brien: Then we used the small towing tank at the hydraulic model basin over at the College Avenue Pool. We were experimenting on that grouser system—the drive system. We built models of the whole tank and put a model of the grouser in it to measure its speed and trim. It was quite a long experiment. I used to go down to Los Angeles every Saturday and Sunday to see Jim Haight, who was the Food Machinery man, making a plan for the next week's operation: They were really in a hurry.

He would tell me what they had been doing and what they thought we ought to test next week. Then, I'd get on the train that night and come back to Berkeley. On Monday morning we'd start testing the new scheme. It was fun in a way. It only lasted four or five months, and we'd play tennis on Sunday afternoon.

Then the war ended. We fussed with the buffalo for a while longer. We tried to learn how turn with these tracks. If you put the two tracks too far apart, you can't turn. Put them too close together, you won't turn at all. So there had to be a proper ratio; we learned things like that. We did design something that might have been worth trying in the field, but the war ended.

Underwater Noise Studies

O'Brien: This underwater noise work was also for the Bureau of Ships. The University of California undertook work on underwater sound: the detection of submarines by their sound in water. Buships had a laboratory at San Diego; I was a consultant for them. We did some work at the laboratory in Berkeley about the noise of propellers in a small channel before I was connected with the Bureau of Ships.

After I was connected with the Bureau of Ships I got interested in the noise problem because submarines were noisy, and we had submarines lurking off the Japanese coast. It was very dangerous for them to make any noise. One of the men of the Bureau of Ships, Commander Arthur College, talked to me about it, and we thought it would be possible to study the noise generated by propellers.

Up to that time it had been thought that the noise from propellers came from the collapse of cavities. In water, anything moving fast generates a cavity. The water doesn't keep up because the pressure isn't great enough. Most of the work on the effect of cavitation on propeller noise had to do with the effect on the surface of the propeller, this hammering noise.

For some reason, we got the idea that the noise would really start in the vortex. This screw was going around in the water, and off the tip of it, the pressure on the lower side is greater than on the upper side. At the tip water is flowing around, and it spirals and forms a vortex. If that vortex spins very fast, it will pull apart the cavity.

The Bureau of Ships proposed an experiment to be carried out at the Taylor Model Basin outside Washington, D.C. But the model basin people didn't think anything would come of it. The Taylor Model Basin is a branch of the navy which studies models of ships— they tow ships and do all sorts of experiments.

I went out to the model basin, and they didn't show much interest. But I did meet a man by the name of Bill Sette, who was an expert on underwater sound and acoustics. I told him what I was interested in doing. We then proposed that we do experiments under controlled conditions.

We were told to go to the Naval Research Laboratory and ask them what they thought about it. They thought our ideas were not very valuable, but we finally got to use the facility at the model basin, which is really a water tunnel. It's like a wind tunnel but it circulates water. It's ideal for studying propellers. At the Naval Research Laboratory they said there'd be so much reverberation, so much reflection from the walls of the tunnel, that we wouldn't learn anything. That was exactly the thing we wanted; we wanted all the reverberation possible. So the little bit of cavitation would make a big noise.

The upshot of it was that we did the experiments. We measured the sound with a sound analyzer, an instrument for measuring different energy and different frequencies. We came up with the point at which cavitation of submarines at different speeds and different depths would start. As the pressure increased, the cavitation was delayed. If the ship was fouled by barnacles and such, the drag on the submarine was greater, and they'd have to push harder and that would cause cavitation at lower speeds.

One day as we were finishing up our work, I happened to go to the Bureau of Ships, and I passed the navy captain who had the submarine desk; he was a submariner in from the fleet. I told him, "Gee, we've really learned something. " He said, "Well, you sit right down now and tell me all about it. " I did. He said, "We're going to send a dispatch to the fleet. " I said, "My God, you can't do that. It has to go through Captain Saunders. " He said, "The hell it does. It goes through me to the fleet right now. "

That was practically the end of my association with the model basin. At the time they were mad as can be. A report was written, but I met Sette one day and he said, "We're having trouble getting that through the editorial office. Every time we send it down they find something that has to be changed. I think the fact that we have your name in it as the representative of the Bureau of Ships being in charge of the project is the cause of our problems. " So I said, "Why don't you cure that problem by just taking my name out?" So they did, and it went through immediately. *That* was the Bureau of Ships.

Ziebarth: Was the cavitation principle useful?

O'Brien: Sure. The submarines knew that if the hull was clean, they could go to a certain speed and depth and not make noise. If they went over a certain speed: too noisy. That's what we showed them.

The captain's dispatch to the fleet summarized that in a simple form and said a report will follow shortly. He saw to it that the main facts went to the fleet and they knew all about it. When a submarine captain knew his hull was fouled and had greater drag, the cavitation started earlier. All that was known.

Ziebarth: Weren't there Mormons connected with this project?

O'Brien: Yes, the University of California was led to this project because Mormons were then experts on sound. The Mormon Temple [in Salt Lake City] is an echoing chamber. You can whisper here and hear it over there. Verne Knudsen and two or three other outstanding acoustic people were involved with the Mormons for that very reason. Knudsen was a professor at UCLA who became dean of Letters and Science. A number of Mormons went into acoustics and became professors and practising engineers.

Manhattan Project and Ernest O. Lawrence

Ziebarth: What about the Manhattan Project?

O'Brien: Technically, the project involved the separation of isotopes of uranium to get U-235 in sufficiently pure form to make a bomb. There were several approaches to it. One that was very expensive and very difficult in terms of equipment and so on was the electromagnetic process. This really is a mass spectrograph: a magnetic field, charged particles go into that field, and the turning depends on their mass and charge. They put a certain charge on, and the mass determines what circle. So when you had U-235 and U-238—they're awfully close together—U-235 had a shorter radius than U-238. You could be sure if you built enough mass spectrographs, you could get enough material for a bomb. That was a sort of a backup program.

The diffusion process, for which a plant was built up at Hanford, Washington, was a second approach. If it worked, it looked as though it would produce U-235 at much greater volume and much less expense. But the question was: Would it work? This was in 1941-42.

I was involved here at Berkeley in the beginning of the Manhattan Project. General Leslie R. Groves came to Berkeley, and Professor Ernest Lawrence invited me to have dinner because he thought I'd enjoy meeting him.

We had a fine dinner, steaks and drinks, and Lawrence said, "Let's go up to the lab up the hill. " That was the first I knew that Mr. Groves had any interest in the hill. On the way up to the hill—I was in the back seat—I heard Professor Lawrence say to General Groves, "Mike, here, has agreed to take responsibility for the engineering of the project. " I didn't know what the project was. He hadn't talked to me about it.

So we went up and wandered around. I'd been up there once a long time before, but they'd changed it a lot. Next day, General Groves got me aside and spent an hour or so grilling me. Apparently he decided I was all right, because I became the executive engineer for the radiation lab.

I was already dean of engineering; I was consultant for the Bureau of Ships; and I was running the War Training Program. This was just thrown on as another responsibility.

Then I learned that Lawrence and Groves really had in mind that I would recruit an engineering organization person by person and try to build it up to be able to design equipment for this big plant they were building down at Oak Ridge, Tennessee. An engineering organization requires expertise in certain fields, and it costs a lot of money. At General Electric, for example, in the engine business are people who know how to do almost everything: all kinds of specialists in everything you can think of.

It didn't take long for me to realize that Groves and Lawrence were just as crazy as could be. We could never do it by hiring individuals or one individual company. We had to hire whole companies. So I spent a while, a month or two, just reading. The job immediately was to see that the right companies were brought it.

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We had hired Stone and Webster, mainly a civil engineering firm which builds power plants. They were responsible for the brick and mortar. They'd already been hired for the Manhattan Project deal. But the hardware, the actual physical facilities—the rectifiers, the electrical equipment— had to be designed, and Stone and Webster was not the organization to do it.

When I learned that they were looking to me to pick up people who could do this, I realized this was clearly impossible. After some months we persuaded General Groves that that was the case.

The terminal point as far as I was concerned was the day when General Groves and Ernest Lawrence told Stone and Webster this conclusion. I sat outside, and the three of them were in the room in Lawrence's office. They told Stone and Webster that General Electric, Allis Chalmers, and Westinghouse were going to be brought in and to design the equipment to separate the isotopes of uranium. It was a blow to Stone and Webster, apparently, because they thought they were going to get into this field.

That was my big accomplishment. That's what I did for Lawrence, with some help from Wally Reynolds, who was a university employee.

Ziebarth: Why do you think Lawrence selected you for this task?

O'Brien: Because he didn't know any better. He knew me, and he knew I was a mechanical engineer. We'd talked about engineering, and I used to go to his journal club very frequently. He had an evening club where students reported on papers.

General Groves was a civil engineer; the Corps of Engineers are civil engineers in the main. They really didn't understand what a complicated thing the Manhattan Project was going to be. Neither did I, really; I just knew it was too much. You couldn't find anyone with any knowledge of a mass spectrograph: no engineer that I knew anything about. The few there were were with Westinghouse or GE.

The only thing I had to do after that was to work on who was going to operate this plant. We found out that General Groves was going to hire an operator and then hire some more men. Oh, my God, I said. You've got to find a company that is in a business like this and get them to undertake to operate the plant. We talked to General Groves only once on that score, but we didn't think we'd convinced him. Next time he came back, we had prepared a list of about ten companies we'd looked into which were not very busy with war work and were in the chemical production field, which was more like it than anything else.

He listened to our story. One of the firms we had listed was Tennessee Eastman, a subsidiary of Eastman Kodak down in Tennessee, where they have a chemical operation. Then he said, "Will Tennessee Eastman do?" I said, "Sure, it's on our list. " He said, "I've already hired them. "

On the Hill, at the laboratory's cyclotron, which they'd converted into a mass spectrograph, scientists were already doing experiments, and they'd got certain results. That indicated what this equipment had to be designed for. I was responsible for seeing to it that the proper information went from the physicists to the companies that had been hired to do this job. There had to be drawings and specifications.

In two or three months I got that pretty well-organized. [Charles] Donald Shane, a professor of astronomy, was my assistant. I remember one day we were walking toward the engineering buildings—astronomy was in a temporary wooden building—and I said, "Donald, I've done everything

I can do on this project. You know all about what our office is doing. Goodbye. " And I quit.

Ernest didn't forgive me for that. Everybody that served on the Manhattan Project was given a little button. But they didn't give me one because I walked out on them.

Ziebarth: What kind of man was Lawrence to work with?

O'Brien: He was a real genius. He had more of an engineering attitude than a physicist on this project. One time when they spent a lot of money on an experiment, he went East, talked with some people, thought about it, came back out on a train, and said "Stop it. " About $25 to $40 million was involved. He was convinced that what they were working on wasn't going to succeed, and he said, "Stop it. Let's go this way. " And they did.

I talked to the physicists every once in a while about the changes. One time Frank Oppenheimer, Robert's brother, did some experiments on the Hill and found that if they doubled the voltage, they got a lot better results. He was up there all night— they ran day and night up there on the Hill at that time. But Frank didn't come to us; he went right to Stone and Webster and said, "Double the voltage. "

Stone and Webster didn't say anything to me. They called their office in Boston. The office in Boston called General Groves, and General Groves called Ernest Lawrence, who then called me and said, "Mike, what the hell's going on around here? Do you realize what doubling the voltage is going to cost? That building is already under construction. We don't have space to put in the transformers. " That was the first I'd heard about it.

I would make remarks to the physicists to think about how much this costs. Not long later, General Groves got me aside and he said, "Mike, you're absolutely right. We ought to worry about the cost. But this is the fact. If we're unsuccessful, it doesn't make any difference how careful we were. If we are successful, nobody will give a damn how much it costs. So forget it. Don't pester the physicists. Don't make them nervous. "

At that point on that project, nobody should have been concerned at all about cost. But I'd been working with the Corps of Engineers and others and knew how Congress was inclined to investigate. Groves was absolutely right. If the project was unsuccessful, it wouldn't make a bit of difference how careful we'd been on the money side. If it was successful, nobody was going to ask questions. So the main thing was speed—let's go, go, go. And that's what happened.

I got out of the Manhattan Project because I had these other jobs, and I could see that Donald Shane could do everything that I was doing. So I walked off without telling Lawrence because I knew he would persuade me to stay. They had a professor of philosophy, who became chancellor of this campus, Ed Strong, as practically the head janitor up at the Hill. It was crazy.

Ziebarth: Was the Manhattan Project a prestigious project on which people were eager to work ?

O'Brien: It was extremely secret. General Groves always traveled in civilian clothes. The Manhattan District developed all kind of stories about what they were doing. The main story was that they were developing a new metallurgical material, a new alloy called tuballoy, very special.

Security Problems

O'Brien: After they settled the matter of how to get the U-235, the active stuff, and built the plant at Hanford—and that went ahead in rapid-fire order—then the problem was to make the bomb. That's where Los Alamos National Laboratory came in. Robert Oppenheimer brought all of the best physicists in the country there. This was top flight work.

The work was so secret that nobody in my department even knew what I was doing. This only lasted nine months.

Lawrence and Groves got along very well; Groves was a real genius. He was accused of all sorts of things, of being dictatorial. I traveled with him quite a bit on the *Streamliner* and by air, and I heard him talk about a great many things.

The Manhattan Project was the kind of project that couldn't be carried out now. We couldn't do it the way Congress messes around in everything, but it was wartime, and the president's powers were practically unlimited. They wanted to develop the bomb in secret. They were pretty successful. Very few people learned about it.

Some spies got in and got some information; we had some here in Berkeley. The Russians wanted to learn about it. They got wind of it and had microphones planted in various houses. I remember listening to tapes of conversations after the FBI got them. They wanted to convince us it was a serious problem and to keep it quiet.

Ziebarth: Did you ever suspect you dealt with anyone who might have been a spy?

O'Brien: No. But we knew they were here. As I say, I listened to tapes, not for the purpose of identifying them or doing anything. The people responsible for security wanted the people who were working on the project to be aware of the fact that this was a really secret project, that they didn't want anyone to know about it until they had the bomb ready to deliver, if possible.

Ziebarth: A 1944 memo from President Sproul said he was looking forward to learning what projects were going on in the university. Did he genuinely not know what was happening?

O'Brien: No, he didn't. They cleared some of the regents, and Sproul must have known there was a Manhattan District project. But as to any details, I'm sure he didn't know the first thing about it.

Ziebarth: Wouldn't people have talked among themselves?

O'Brien: Not many were working up on the Hill then. They were all from the Physics Department. When the industrial people came in, however, they had to be sat on to keep them quiet. They'd go to a restaurant and talk about this mass spectrograph and things like that. Much of the information leaked. The Russians had a bomb much earlier than they would have had if they hadn't had this kind of information in advance.

Ziebarth: Were there any security investigations on campus?

O'Brien: Yes. I think Manhattan District had its own security system as well as everything else. But whomever was responsible for security had identified certain people they were suspicious of and had put microphones in their homes and found out it really was so. All I heard ever about was a man named Haakon Chevalier, a professor of French.

Ziebarth: How was he involved?

O'Brien: He was a French professor, I think, as an excuse for being here. It wasn't difficult. Once word of the neutrons and fission was out in the early forties—that was published in papers—physicists worldwide knew about it.

If this is true and you have spontaneous reactions, you make a bomb. They all knew that right away. Then, where would you go to do the work? The great centers of all this kind of work were Berkeley, Princeton, Fermi at Chicago—only three or four. Once you narrowed it down to that, then you put people at each one of these places to find out what they could. It turned out that Haakon Chevalier was one of these professors.

He left, but one of the problems Robert Oppenheimer had later, and one of the main causes for him losing his security clearance, was that he had lunch with Haakon Chevalier. Robert was a very naive and idealistic person.

Ship Cracking Research

O'Brien: Here's how Berkeley was involved in ship cracking. A man by the name of Harry Kennedy had invented a method of welding which was particularly suitable for building ships. He was a graduate of Berkeley, so we knew him. On a cold night in the harbor at Portland, Oregon, a welded ship cracked; it came partly apart. There were some other incidents of similar problems. That scared the War Production Board badly. We were committed to sending enough food, ammunition, and supplies to Europe to stay in the war. There was a big question whether we were going to have enough ships to do it.

This method of welding—union melt welding—looked as though it would provide the means of building the necessary ships. As a matter of fact, President Roosevelt, in a message to Winston Churchill, said an American has invented a method of welding, and we will use it and can build all the ships that will be needed to support the war.

This ship crack-up scared the heck out of everybody. I happened to be in Washington, staying at the Cosmos Club. I met two men from the National Research Council who said, "The University of California's out there in Berkeley, and you have the means, the people, and you're close to Kennedy. We would like to have you undertake a study of this method of welding and what can be done to prevent ships from cracking. " I said, "That sounds interesting. I'll see what I can do about getting somebody interested in it. "

The upshot of that was we undertook a program with a lot of experiments of one kind or another, and it continued even after the end of the war. Lathrop Meriam, a professor at Berkeley, later a dean of engineering at Duke University, instrumented a Victory ship, a type of carrier ship. He put strain gauges all over it and measured temperatures everywhere to study the stresses in the ship.

It came down to something simple. If you make a cut and have a sharp corner, that's where cracks start. If you round that corner, instead of making it sharp, it's much less susceptible to cracking. The result of that whole investigation was: don't have sharp corners in the hatches. You have to have a hatch to let stuff into the ship and out, hatches with perfectly square corners were cracking and causing the ships to fall apart, starting in those corners.

Ziebarth: What was Kennedy's union-melt process ?

O'Brien: It was electric-arc welding. They were able to weld plates that were an inch thick, far thicker than any previous welding method at high speed. You could run this along a seam and weld it in a very short time, which was a tremendous advance. Now it's routine. Practically all the ships built in the world use that process.

Ziebarth: Had Kennedy been a graduate student here?

O'Brien: No. He graduated here, but he was an older man, an inventor by profession. He had a little shop, and he bought a lot of the excess equipment. He had one helper. If he had an idea, he could try it out quickly. If he called New York and said "I think I have an idea," a vice president of Union Carbide would be out here the next day. In fact, at North Tonawanda, New York, they had a whole laboratory doing nothing but putting these inventions into form for production and sale. He worked with Union Carbide almost all his professional life.

Beginnings of Metallurgy at Berkeley

Ziebarth: With what other wartime projects were you associated?

O'Brien: There are a long string of reports on which my name is included with the others, but I was often just the head of the project.

What I did about metallurgy was to find top-flight people for Berkeley. Before the war I got John Dorn, and once he was here, we were really capable of doing a lot of things because Dorn was a good advisor on who to contact.

Ziebarth: What was Earl Parker's project? Was it related to the war effort?

O'Brien: We got going during the war, and then that was continued. We worked with magnesium, quite a number of different metals, and also steel—especially creep. If you have certain alloys of iron, they gradually grow if you put a load on them. On bridges, for example, cables creep, extend. you can imagine what that does to a bridge. If a bridge cable extends or flows, it isn't much help.

Metals get tired. Fatigue is another property that has to be investigated. We did a lot of basic experiments on the properties of metals, including iron. In the field of metallurgy, the contribution was really getting the competent people here and supporting them.

Ziebarth: Did these projects come to you through a contact in the army?

O'Brien: Except for this work on the cracking of welded ships when the navy came to me and asked for help during wartime, we didn't solicit projects. That was the general principle that I followed and the one that our Institute of Engineering Research followed.

Instead, we helped faculty get projects. When we brought Professor Schade here, his job was not to go out and get companies to put projects at Berkeley. The faculty were never on soft money. If they didn't get a project, they still were fed.

Ziebarth: What kind of effect does such a policy have?

O'Brien: Berkeley is practically number one in engineering schools in the country by every appraisal. The quality of the research is high, and part of it is due to the fact that the faculty aren't scrambling around to get something to pay their own salaries.

Prosthetic Devices ##

O'Brien: One day at the Bohemian Club I happened to meet a Navy medical captain, and in general conversation he said, "You know, after the war there's going to be a large number of men coming back with arms and legs that are going to have to be fixed somehow. There's going to be a big demand for prosthetic devices. "

He wasn't trying to sell me anything. I remembered our conversation, and thought of faculty member Howard Eberhart, who had lost a leg in the course of an experiment over in Marin County at Hamilton Field. The accident happened when they were designing the B-52, a heavy bomber, and they wanted to know how thick the runway should be to stand up to these things. They had a big tractor with the wheels and equipment load that would occur on a bomber which went round and round and round and round and round. They had strain gauges in the concrete to measure the stress.

One night Eberhart was sitting on the concrete fixing a strain gauge.

This big tractor thing came by when he had his leg out and it cut it off. We'd had dinner with him that night. I'd been visiting there with Raymond Davis. Since that happened, I thought he ought to be interested in this.

Then we had Eugene Murphy, a professor in mechanical engineering who'd had polio. He walked with a cane. I just mentioned to them the idea of devices and said, "Do you have any thoughts on that?" They said, "Gee, that sounds interesting. " They went to see him and just took it from there. The upshot is that Howard Eberhart is still, forty years later, a great authority on prosthetic devices. It was divided up: Berkeley worked on legs and Los Angeles worked on arms. Quite a show went on around here.

For example, they put rods with balls at the ends in the leg. That brought in the medical school. We had the dean of the medical school on our payroll over here in engineering part-time. The researchers had normal people and people with some problem walk for them to study what was needed. What they found out, which had not been known, was that you can't walk unless your leg rotates. They had these extensions; then they had synchronized cameras above and on both sides and followed all the motion. Professor Al Levins worked on that part of it.

Gene Murphy is now head of that section of the Veterans Administration. Howard Eberhart is a retired professor here and still chairman of the Prosthetic Devices Committee of the National Research Council.

Naval Communications Equipment

O'Brien: The electrical engineering department was old-fashioned before the war; it was 60-cycle power, not electronics. Men in the Bureau of Ships, captains in the radio branch who had taken graduate work here at Berkeley, knew what was here. They came to me and said, "Why don't you step up work in electronics at Berkeley?" I said, "Well, what would I use for money?" And they said, "How would half a million a year do?" I said, "That'd really be wonderful." I can't even remember writing the proposal getting this money; they wrote it for me. We put it into the budget of the Bureau of Ships and—so help me God—we got it.

They had had a lab down at Point Loma in San Diego, which was studying the radar signal that came from ships offshore. What did the signal of a ship radiating look like? What is the effect of fog?

That laboratory was quite well-equipped, and when the war ended, these people said, "That sort of work ought to be up at Berkeley. Let's put the whole lab up there. " So we inherited not only the equipment but the technicians, the glassblowers, who were quite rare. You had to build vacuum tubes. Nowadays, with the transistor you don't have to, but that was the period of vacuum tubes.

There was standard equipment used to study the ships, and we had an observation station over at Golden Gate Park on the cliff. The ships went on down there—submarines and other ships—at different distances, and they studied their antennae and their signals from shore, the whole thing.

This project was kind of a nuisance because we had to have people out there on a routine basis. It was a service. But in return for that service, we got a wonderful set of equipment. Then the electrical engineering building was built, and now it's mostly electronics.

It was a big boost to get this money from Bureau of Ships and the lab equipment from San Diego.

Atom Bomb Testing at Bikini

O'Brien: We were involved in that because we had been studying waves for a long time. Willard Bascomb and John D. Isaacs were employees of our wave project. We had quite a big project after the war doing amphibious work.

Ziebarth: Military work continued after the war?

O'Brien: The Navy had a heck of a lot of people on their hands. If you dumped that many people, where would they get jobs? What would happen?

This was all related. We had a fleet, a 20,000-ton ship, several LSTs, and a flock of smaller boats, water buffalos, dukws, landing craft, and so on. That fleet moved from San Diego, Coronado, where they have their regular operations, up to a point near San Luis Obispo , and then on to Fort Ord and then to the Columbia River. At all these places, we, the University of California, provided the observations of waves or the guidance of the whole thing. Professor Bob Wiegel was one of the fellows. We were interested in and sort of training the Navy meteorologists in wave forecasting, following up on the wave forecasting of Sverdrup and Munk at Scripps.

When the Bikini test of the atom bomb was planned, Roger Revelle from Scripps was on the staff of the director of the tests so it was just kind of natural that we got involved. We set up towers and synchronized cameras taking pictures of the blast as it went off and the waves that came from it.

Ziebarth: Were you there?

O'Brien: I was there for about eight weeks. It turned out that there was a problem. In order to measure the height and speed of the waves, we needed to know exactly where the ships were when the blast took place. The Baker test really was a bomb set off underwater. The ships were anchored, so where they were located depended on the wind. That water was 180-feet deep, requiring about 600 to 800 feet of anchor chain so the ship could swing.

It's amazing, but nobody had thought about the fact of the swinging ships and their exact location after the blast. It turned out, however, that the cameras that we put on three islands triangulated. We had a fix with one set, and then we had a check on it with the other one.

We had a meeting at which someone, possibly Admiral Blandy, said, "My God, we don't know where the ships were. " And John Isaacs—I learned then, never volunteer—said, "Admiral, we know where they are. We took pictures of them every three seconds. We got a picture of every ship in the fleet, where it was, where its masts were. "

So they sent us over to Kwajelein Island on a destroyer. We got on the *Albemarle*, the ship that took the bomb material out there and the guys who made the bomb. It still had plutonium aboard as we learned when we got back to Los Angeles.

We were given an air-conditioned room and these photographs. Look here, take the measurements. I thought I'd lose my sight before we got through. Every once in awhile we'd come up on deck for air. We worked about sixteen hours a day as far as Honolulu. That's where the destroyer came in. The captain saw that we had everything we needed. We produced a tentative map of all the ships in the fleet by the time we got to Honolulu, and he took that back so they could get started on the studies of distance of ships from the blast and so on.

Then we went on to Berkeley and did some more work on it. But the final thing was to calibrate and recalibrate the cameras to be sure we had the proper focal length. It took about six months to turn out the final results. There were an awful lot of ships there.

Ziebarth: The ships had monitoring devices on them to see how they responded?

O'Brien: Many ships did, but that was unrelated to this. We had horizontal sights on a mast, and we'd puzzle over whether this is the mast of this ship or that ship. We had the location of the fleet; the anchorage showed. But when you looked at the fleet, it was just a mass of smokestacks and all sorts of things. But we had to get down to where this ship could be; it couldn't be more than so far from this point. The *Saratoga* and the other big ships were pretty easy to locate. Then as we got to smaller and smaller ships, it got more complicated. There were several hundred ships out there. It was a big fleet.

Ziebarth: Didn't you build a scale model of the Bikini Atoll?

Yes. The first thing we did was to make a flat plate, curved a bit, put weights on it, raised it up and let it drop to hit the water of different depths just to see what kind of waves came out. Then Professor Joe Johnson had a program in which they fired one-pound charges at different elevations above the water and underwater and measured the waves that came out. He had an expert on explosives who handled explosives, and he measured the waves that came. That program was quite interesting, and they finally ended up firing one-thousand-pound charges in Chesapeake Bay and photographing that.

It was a long buildup. They wanted to know what was going to happen. A man by the name of Sir William Penney, an Englishman, was interested in this. He had the most sense about it. It was awfully difficult to forecast what was going to happen, what the scale of the model would be. In other words, this plume went up here. How far would it go? How long would it take to get there and come down? The main problem, though, was this —as this wave was pushed out, there was a line of foam around the wave. That affected the ships. It was a pretty big wave, maybe fifty, sixty feet high, hidden by this cloud of foam in the fullscale.

In the model scale there wasn't so much foam because of surface tension and lower velocity and so forth. They fired these thousand-pound charges down in Chesapeake Bay and rattled the windows, and a senator from Maryland complained they were making too much noise and breaking windows. Joe Johnson had fun on that.

Ziebarth: Was there also a test on the Salton Sea?

O'Brien: There might have been, but we weren't involved. All the work we were involved in was in Marin County, north of the Golden Gate. There's a military installation with some ponds; we used them. That was an isolated and controlled area.

Effect of Wartime Research on Campus

Ziebarth: What effect did all this research have on the campus?

O'Brien: We gained a lot of experience with physical things. I heard a lot about research elsewhere, and the wartime experience gave direction to what we did afterwards.

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Ziebarth: Did the Army and Navy compete for the research assistance of Berkeley faculty during the war?

O'Brien: No. We had only a few research projects here, ship-cracking, for instance. The only other thing I can think of is a postwar civil-defense program that we had for a while. It was pushed on us by the National Research Council. We helped our faculty prepare proposals, and we suggested where they might apply. But they did it. So any competition would be with the individuals. The Navy or others didn't come to us except about electronics.

Ziebarth: Were there any times when you would turn down a request as not appropriate or not possible?

O'Brien: Oh, sure. We wanted to do applied research, research related to the basic practice of engineering, and not to do routine things. But we did not want to compete with the private testing laboratories. When Professor Schade came here, he followed up on that. We got to the point where instead of being our enemies, private facilities were our greatest friends because we would say to someone who wanted a test made, "We believe that there are private facilities available and you should explore that." There had been quite a bit of commotion about the competition of the laboratory doing routine testing jobs.

As a matter of fact, Dean Boelter and I were much involved in the early discussion of what became the Stanford Research Institute. Originally, it was thought of as an organization doing service for southern California. Then it turned that nothing worked out. We always said, "Well, what they're talking about isn't appropriate to the university." It's now operating independently from Stanford. But we didn't want to get saddled with the routine testing kind of thing. Stanford makes lots of studies for industry of markets and all sorts of things.

We didn't want to get into that; we urged them to either put it at Stanford or make it independent. But it was needed; the fact that SRI has done well showed that it was needed and is useful.

VII. ACADEMIC STUDIES IN WARTIME, 1941-45

Special Programs

Ziebarth: I have seen an Engineering Department memo listing priorities on campus during the war. In descending order they were 1) research critical to the war effort, 2) instruction of the students in regular curriculum, 3) army/navy/air force programs, 4) Engineering Science and Management War Training (ESMWT). Does that seem accurate?

O'Brien: Yes, but we carried on ESMWT in our own fashion.

Ziebarth: What were the college training programs for navy students?

O'Brien: During the war we had the V12 program for the Navy, and that worked very well. It was well-planned. It was an undergraduate academic program prescribed by the Navy. We accommodated them and gave it. It was just ordinary engineering courses, the same courses we offered anyway, but somewhat differently packaged. Different numbers of units.

Then the Army pushed in here because the man in charge of the army program nationally, Colonel Blake R. Vanleer, had held my job here at Berkeley. When he got to be a colonel in the office that handled all the military undergraduate training programs, he was determined to put a unit at Berkeley.

We had it briefly, but it was an awful pain. They had two different programs, not very well planned in my opinion. So I hardly realized it was here. We offered it; they sent students; and we assigned instructors and offered the courses. But I think it was only about one year and then they gave it up and sent the students elsewhere. The navy program, V12, continued after the war.

Engineering Science and Management War Training Program (ESMWT) ##

Ziebarth: What about the engineering science and management program?

O'Brien: That was first called the Engineering Defense Training Program. Then they added science and management. Then when we went to war, they dropped "Defense" and called it the War Training Program, but it was the same program.

We had a big job here in California because of the aircraft companies and, to some extent, shipbuilding. In the course of that program, we had 1,800 different instructors—each one had to be approved by us as a competent, qualified teacher for the course he was going to teach. And we had 46,000 different students. President Sproul asked me a couple of times, "Mike, is your university bigger than mine?"

That was a unique program; we did it differently than anybody else. We decided we weren't going to fool around with students who weren't qualified. We weren't going to offer second-rate courses. They were either going to be good courses for the subject matter, or we wouldn't give them. So one thing we did that was unique was to require an examination during the first hour of the course in which they tested the prerequisites. The instructor had told us, and we agreed, that, say, a working knowledge of algebra and trigonometry was prerequisite to understanding the course.

First we wanted to know if it was really required, not just nice to have. We didn't want to have requirements that were just nominal. And secondly, if that was really a prerequisite, then we wanted to be sure the students really had a working knowledge and were ready to go.

Every first meeting of every section of every course was monitored by a regular member of our faculty. They determined who was qualified to take the course. The second hour, the lecturer or teacher of that course, mostly from industry, gave the lecture. By the end of that lecture, our man had evaluated the exams and said these students are in and these are not.

Sometimes we had a backup and changed the character of the course to make up for the lack of prerequisites. It was a very complicated program because it wasn't financed by lump sums to set up a school. We got money section by section. We rented space all over California. We had former showrooms of a Cadillac agency, for instance, which was moving to other quarters— they didn't have any cars to sell anyway in wartime. We rented a whole school, about a six-story building on Wilshire Boulevard in Los Angeles. We gave courses at San Diego State College and UCLA, all over. So it was a fairly big show with 46,000 students. Many took just one course, maybe half of them. But some of them took as many as seven courses.

One of our faculty people was there to start every section to see that it was a decent kind of a course. We had to qualify the instructors. They gave us a statement like many we'd have for appointing a professor—not quite so much but pretty good, and one of our faculty, R.G. Folsom, Clyne F. Garland, Eneas Kane, Everett Howe, would review it. The program started when I was still chairman of mechanical engineering, and mechanical engineering really ran the show. But Professor Carlson of mining had a big program that ran on for years in safety engineering, too; all these plants were expanding and had all sorts of problems with safety.

The courses were good, and as a matter of fact there were a lot of inquiries by men who wanted to get some credit. They were at college somewhere, and we were able to supply information about what courses they took and who the instructor was. Some of the courses were given several times and ultimately became books. Morris Zucrow taught one.

Eneas Kane, an assistant professor of mechanical engineering, went down alone to handle San Diego, thinking maybe a hundred students would show up. Instead a thousand students showed up at San Diego State College that day. So we rushed people down there and got it going. But the thing was really run by the engineering faculty, not by outsiders.

The people taking these courses were often the engineers in the aircraft companies. We had courses at Vultee, Consolidated, Lockheed, Douglas—all the aircraft companies, lots of courses one after another, given at night, with all sorts of arrangements. There was a woman's program, which was under the direction of Bernice Hubbard May. She had been with University Extension, and then she married Professor Sam May. When this came along, we persuaded her to come back into the business and handle it.

She put on courses in drafting and related things such as trigonometry that draftsmen need for approximately 1,200 women. And they all got jobs. Practically everyone who wanted a job got jobs in the aircraft industry. They were badly in need of drafting people. It was a pretty high-level, closely monitored program. Practically no other school in the country used an exam to screen people. People thought we were nuts. Students said: "I came to learn; I didn't come to take an exam. " We said, "We want to know whether you're qualified to take this course. We don't want you to waste your time if you're not really ready to go. You can take another course. "

Ziebarth: Was the ESMWT run through UC Extension?

O'Brien: No. We were under University Extension for about six months. Then we found that their procedures were so ponderous and so slow and so arbitrary that we had a fight. Boyd D. Rakestraw, the head of extension, and I met with Sproul, and I told Sproul what I thought of Rakestraw. President Sproul called me afterward and said, "You certainly told him, Mike, what you thought. "

We got free of that. So it wasn't really part of engineering at all. It was just a separate activity. Every course had to be financed. We put in more proposals item by item. "This is going to be five sections, and two will be at the Lockheed plant, and one will be at the Douglas plant, and two others will be all over the place. Each one will cost $X. We'll pay the instructors so much." All of that was in detail and went into Washington. I used to go there and have a stack of them and say, "Why don't you approve this whole stack? They're all good. You're going to approve them anyway. Why waste time on details."

Ziebarth: How quickly were proposals approved?

O'Brien: With extension, we had to get instructors approved in a hurry. If we didn't get them, I got on the phone and said, "What are you fiddling around for? We've got to start this course. The date is announced, and we want it approved. We're going to start it anyway." It was a lively thing. The key to it was the faculty people involved could see that it was having a considerable effect: the aircraft companies were coming to us to put on courses.

For example, there was a problem of manufacturing methods, and the War Production Board was giving Consolidated Vultee-Convair a bad time because they weren't really designing airplanes in the proper way. You can design in a very costly way or a simple way. And the War Production Board was pretty unhappy with Convair. I got a call from Tom Faulkner, representing Convair dealing with us, and he said, "Are you coming down to San Diego?" I said, "Yes, well, I can. " He said, "How about tomorrow night about 2 o'clock? I'd like to meet you at the Red Lion Inn to tell you something. "

What he wanted to tell me was that they wanted a course for the engineering department offered by the manufacturing department. They wanted me to go to the manufacturing side and not tell them that the engineers asked for it. (This goes back to a discussion I had with their project engineers one night down there. I said, "Don't you need courses in manufacturing methods for your engineers?" "Oh, no, we design the airplanes and they build them." And I said, "Suppose they design them so you can't build them?")

As a matter of fact, Convair's owner, Reuben Fleet, was told by the War Production, "You're dealt out. You sell to these people." Republic Steel sent in a manufacturing team that took over and really straightened them out. I was at a luncheon down there when Reuben Fleet was handed a check for $10 million and more or less told, "Go away. "

We put on that course. We had a bobtail version of it for the seniors, really the top, vice presidents and so on, very bobtailed. And then we had ten lectures, three hours apiece, in which every phase of manufacturing, right out to flight tests, was represented. The manufacturing people not only told the engineers what they did, but they told the engineers what was wrong with what they were doing, what was causing so much delay and so much wasted material. The stuff didn't come out right; it was hard to manufacture. That was an unusual example, but we were really under pressure to offer courses. We put on a lot for Douglas when they were going into the manufacturing side.

Douglas led the way in manufacturing airplanes. Their research department had given its full attention to manufacturing methods, even before the war started, during the defense program. So then when the war came and they had an expansion of business, they had a lot of instruction for the lower-level Douglas people and their methods of production control, etc. Then the other aircraft companies were needled by the War Production Board: "Get busy." So we would put on courses and get instructors from Douglas to teach over at Lockheed.

Civilian Students ##

Ziebarth: What kind of changes did the war bring to campus for regular students?

O'Brien: There had been an agreement that the government wasn't going to draft all the eligible students. Some of them would be sent off to college. So they were taken in, and then they were assigned by the Navy to instruction. The V12 was for people who were in the Navy but were assigned here as students. There weren't very many other students at that time. In that age group, they were practically all in the Army or in the aircraft companies, so the number of regular students dropped down.

Ziebarth: Wasn't the pass-fail system first tried at this time?

O'Brien: Never in engineering. The university had all sorts of nonsense about courses that didn't help the war effort at all. Except what we were doing. In Letters and Sciences, they had all sorts of courses that were supposed to be war-related. I remember a meeting of deans with the president and a list of these courses. Several of them were courses that engineering had been told to drop in the other years. All of a sudden they were important.

We had one on the automobile, for instance. The Committee on Courses of Instruction said that was low-level. It had been the best way we could think of, however, of teaching physics and chemistry and electrical engineering to Letters and Science students and learn something useful. When the war came along, suddenly that was something very important. In wartime we offered instruction for people in Letters and Science. I can't say I paid much attention to it.

Relationship of University Research to Government

Ziebarth: Do you recall a brouhaha around Professor Weeks, who was doing research on alloys of zinc and refused to reveal the name of the company he was working for?

O'Brien: No. But many of the faculty felt that their consulting work was theirs and none of the university's business. I don't agree with that.

Ziebarth: I've seen a 1947 memo attempting to estimate the overhead which came to campus because of wartime research work.

O'Brien: The university acted very unwisely regarding overhead. They took the overhead and put it in a pool, so the departments with the contracts weren't getting any part of it. It was collected to cover the expenses; but when the expenses were paid, they were paid out of the regular contract budgets.

The university got what was coming to them, I felt. The legislature learned about this fund, and they just dropped the budget by that much that year. So all that overhead that was collected came to nothing. They thought it was going to be a bonanza, and the whole thing was wiped out.

Wartime Technology Transfer

Ziebarth: What about spinoff from high technology developed from research during the war?

O'Brien: All the work in metallurgy was transferred. For engineers to use materials, they must know their properties, not just their strength, but creep and fatigue. We had the money to do the work on magnesium, on steel, on aluminum. Magnesium was one that was quite important, a very lightweight material and not dangerous. Lots of airplanes were fabricated of aluminum. So there were great advances made in metallurgy as a result of war research. Dorn and Parker and others came to Berkeley and did a lot of things.

We also made important advances in ship cracking and welding.

VIII. DEAN OF ENGINEERING: POST-WAR YEARS, 1945-53

[Interview 4: October 28, 1986] ##

Faculty Rebellion

Ziebarth: How did engineering handle the great bulge of some 3700 students entering after the war?

O'Brien: When the war was over, we were swamped by the returning GIs who were undergraduates. They all came back to either start or finish up. That worried me very much because I could see that if we didn't have an adequate number of faculty, we were going to be swamped, and we wouldn't ever get started on the graduate program that was really our objective. If we hadn't had good plans that we thought through before and during the war, we never would have made it. They fiddled around for a while; we wrote letters and I went down and talked to the administration. Sproul was the president at that time; Monroe Deutsch was provost of this campus.

I could see that nothing that I'd planned was ever going to come into being if we went on trying to train undergraduates with inadequate facilities. So I drafted a letter which said, "We have reached the point where if you don't give us what we need, you can take all of our resignations at once. We are through. " I meant it and offered it to the chairmen of the engineering departments to sign it or not. They all signed, and we sent it to Dr. Deutsch. I think he never showed it to the president.

But we were serious. We had recommended certain things and we hadn't received them. Classrooms were full to overflowing, and we had instructors with unreasonable assignments. Classes were too large, and all sorts of things were wrong. To have all our wartime planning swamped by numbers was too much for me.

This was in about 1945 or 1946. This bulge came in and we were swamped. After Dr. Deutsch received the letter, they started moving in the dozen or so temporary buildings you still see around here down by the glade [on campus]. Our letter really worked.

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The next thing was to figure out how to get around the rules regarding faculty appointments. It turned out that an appointment of a lecturer was open. I could appoint a lecturer just like that. So we did, and we got some pretty good people. But that caught up with us; they said the lecturers ought to be appraised like everybody else. But by then the problem was solved.

Another thing on our agenda was the matter of money. We worked it out this way. Around here you have various funds. You put down a blanket account for photography— $50 or $100. Then you made charges against that. In a place like engineering as a whole, there were many, many accounts. Furthermore, on the campus as a whole, the amount of those accounts was considerable. You couldn't overspend, so at the end of the year there had to be money left in all of those accounts. They couldn't get it down to the last nickel, and many of them had quite a bit of money.

So along toward the end of the fiscal year, say in May, they began to pull them in. Olaf Lundberg, the controller, tipped me off to this. He said, "You know, there's a lot of money over there in the president's office toward the end of the year." So I started having requests for equipment, anywhere from something costing $50,000 to things that cost a thousand, in the hands of the president's office. I was getting about $65,000 a year. Clark Kerr was president for three years before he learned what was going on.

Those were the days. Now they've got everything so tied up.

Post-War Plans for the Department of Engineering ##

O'Brien: After World War II, after the threatened faculty resignation and after we had taken care of the first onslaught, the college—department of engineering it was then— went through a process of planning which I don't think has ever occurred with equal intensity, duration, and detail in academic affairs. I, with help from Boelter, drafted a long-range plan of what we wanted to do in the college. We worked it out again and again, a second and third version. We put it all out to the whole faculty, and on every item we had a yes or no vote and comment. We went through that and got it ironed out until everybody agreed. This was a long-term plan for graduate study and buildings.

The plan went to the Committee on Budget and Interdepartmental Relations. They said it was too vague and uncertain. But of course we couldn't write a book about each item.

After we had this plan, in 1945 or so, Boelter and I had lunch with President Sproul one day to present it. He said, "Mike, I know what's going to happen. You're going to show up in my office waving this program and say, 'By God, where's my money?'" We said, "No. Each one of these is an objective. We just want to know whether you approve. Because we don't want to be working on something you don't think is any good." He bought that, and we sat there talking for two or three hours about the whole thing. That was the launching of the plan.

Testing Requirements

O'Brien: One thing we fought hard for was a limit of thirty students in a section. Although I was convinced that many subjects would be offered in sections of a hundred or so, the only way I could figure out of having enough faculty was to nail down section sizes and thereby build a big demand for faculty.

We also decided we were going to have a freshman exam. I'm convinced that it's not a kindness to admit a student who isn't qualified. He flounders. He fails. And that's a bad thing psychologically.

Furthermore, we wanted to push students into the junior colleges, if possible. So we readjusted the whole undergraduate lower division requirements so the student could come here from junior college, and he didn't have to have three units of surveying or five units of this. We mentioned only some subjects that he had to have had. Before that, if a student had two units of surveying, and we required three, we'd make him take surveying over again. Crazy.

We would say you had to have algebra and trigonometry. We took the grades of the junior college that they came from and the grades on the exam that we gave them and combined those with a certain weight and got a number. It really was surprising that we began to have almost no dropouts. If we let them in, they were qualified. Those who weren't qualified, who had lower scores, we felt, were much better off to be denied here and go somewhere else where they'd be successful.

We had a fulltime man, Bonham Campbell, who was the contact for the junior colleges and the state colleges. We did everything we could to ease the requirements for the junior year, but then students had to take an exam as junior. The thing that really burned people up at first was that our own juniors had to take the same exam.

Every exam had a few questions that we graded but didn't count in that exam because they weren't validated. Then we compared answers on those questions with their parallel questions, something on the same subject. Bonham Campbell and others studied this kind of testing.

We finally developed an exam that we thought was pretty sound. I remember one year there were about fifteen students from one of the junior colleges that took the exam, applied, and were denied. Monroe Deutsch appealed to me to let them in, and I said, "We've studied this, and everything indicates that they won't make it. " He said, "Well, let them try." You make mistakes, sure, but this is about fifteen students.

Every one of those students was subject to dismissal at the end of the second term. They came in as juniors; at the end of the junior year not a single one had performed. This is unkind.

Around the campus there were people saying this kind of testing was unfair to the minorities. But I think that it's very unkind to admit students and have them fail. If they are borderline, if they work a little harder, where they can make it, that's OK, but that isn't necessarily the case.

Ziebarth: Was your idea of the exam prompted by your experience with the European educational system?

O'Brien: No. The exam came about because we were swamped with students, and we had so much complaints from the instructors. They just weren't prepared to follow through on the kind of program that we were offering.

Ziebarth: Did you personally develop the idea of an exam?

O'Brien: That was my idea entirely. I had to push it on Boelter.

I go too far in delegating. I find somebody who's competent for the job, and he's got it. Then I walk off to something else. One of the things I try to remember is, there's no limit to what you can accomplish if you don't care who gets the credit. You find somebody who can do it. Praise him and let him go. We recruited young people. We had people like Folsom who came at $85 a month and became a professor, chairman of a department, ultimately president of Rensselaer Polytechnic Institute.

Building Plans

O'Brien: I was determined to work out engineering's building plan in detail because I saw that the engineering faculty were not thinking in terms of what the load would be when we had a lot of graduate students. I asked them to give me their plans. Joe Kelly, professor of civil engineering, helped me.

I didn't want to know in general terms that we want this space for this course which will have so many students. First, I asked the faculty to estimate how many students they were going to have, graduate, undergraduate. Then, how many seminar rooms were you going to need? If you're going to have graduate students, you're going to have to have a place for them to talk.

Then we came up with such things as an undeveloped top floor of Cory Hall. One floor undeveloped for later expansion.

I found out that all the planning was on the basis of square feet, floor area. They didn't say anything about how high the ceiling ought to be. Cory has ceilings so high that it has a mezzanine.

That was worked out in detail. When we said that we needed so much space, when the faculty Committee on Buildings and Space went over it, they just couldn't argue. We're going to have so many students. We're going to need classrooms, seminars, faculty offices. It worked. We got Etcheverry Hall. We got the electrical engineering building [Cory Hall] with the fifth floor. It's now full, completely occupied.

We had long arguments with the Buildings and Grounds people about an empty floor. It's an amazing thing that people plan in terms of what you have now. The building committee, when I first talked to them, couldn't visualize having empty space. We got the Richmond Field Station to grow on, too. But we got it because we weren't talking in general terms of so many square feet total.

New Faculty

O'Brien: One of the main factors in post-war planning was a new attitude toward faculty. The first real recruiting job I did before the war for John Dorn, where I finally appointed someone who was not an engineer, more or less settled my mind about the correctness of the approach, because he adapted and worked in. Pretty soon there was all kinds of collaboration between him and the regular engineering faculty. I continued that approach and brought in lots of faculty who were not engineers.

Engineering School in Los Angeles ##

Ziebarth: Was establishment of an engineering school in Los Angeles among your post-war plans?

O'Brien: That's right. During the war we had people at Los Angeles who were not on the UC faculty but from some other schools around there. We had a program in manufacturing under a Mr. Watson, who was on the UCLA faculty, but Los Angeles had no engineering school.

What happened was that the new engineering school under Dean Boelter took over many of the people who'd been with us on the engineering ESMWT program, including the office and clerical staff. In the past there'd been a number of proposals for specialized things like a school for traffic engineering, and every time that was brought up I'd lead the charge if I was on a committee—and I was usually—that they should set up a proper engineering school and then have specialties after that. So there was a lot of discussion of engineering at Los Angeles, and Dean Boelter, who was associate dean here, was eventually appointed as dean.

Graduate Degrees

Ziebarth: In 1947 a master of engineering degree was established which emphasized design over research. What was its intent?

O'Brien: The intent was to produce some engineers, rather than just professors. Engineering design is meant to work from examples. There are all sorts of things we take for granted. Normally, everything is taught by rote, and a professor takes up the subject and tells students how to solve the problem. Then he gives them problems and they solve them.

If I ask you to design the stairway from this floor to the next, you would stumble around and not know how to get started. Once you think about it, you realize certain fundamental principles have to follow. You can't have steps too high, and they have to be broad enough for people's feet. You've got to get from here to there. And you go on from that. That's the process of designing, which is creating something that's different from what's existed before. The degree of innovation may not be very great. There may have been examples of the same design, and you're just changing the size. But any change involves considering principles, such as limits on stress that are necessary for a successful product. Two-thirds of this engineering faculty couldn't design anything except, possibly, lab equipment.

Ziebarth: Does that relate to the purpose of graduate education?

O'Brien: It's one of the purposes. Another purpose is to provide teachers for undergraduate instruction. Some of the engineering schools which have a heavy emphasis on research are concerned by the fact that all of their work on graduates aims them toward research. Here there's a very large demand for teachers to teach the undergraduate programs in many other engineering schools. And so our graduates are the ones who shouldn't be in those more practical-oriented schools, or the undergraduate schools.

Ziebarth: What about the engineering doctoral degree that was begun post-war?

O'Brien: Those who had doctorate degrees at all were Ph.D.'s, and they hesitated to admit that they weren't designers and didn't have much engineering experience. The number of students who choose a doctorate of engineering has been less than those going for a Ph.D. There isn't a tremendous difference, but a doctor of engineering is a more logical degree in an engineering school than a Ph.D. anyway.

Ziebarth: Before this time, wasn't engineering viewed as a poor cousin on campus?

O'Brien: It always was. Before the war it didn't have much standing on the campus.

University-Industry Relations

Ziebarth: As dean, you worked to build good relations between the campus and industry. Why?

O'Brien: Our graduates go into industry. We thought it was important to know the requirements of California industry.

New Focuses

For example, we explored the field of ceramics. I got an expert from the Tennessee Valley Authority in Tennessee, a ceramic engineer, to come out here and make a study of the state. He pointed out that we had a small ceramics industry but absolutely no research, no work, no courses providing engineers for that field.

It happened that I had been in Washington during the war and stayed at the Cosmos Club, and one of the other people who stayed there, who was on the War Production Board, was from North Carolina. I got interested in ceramics when he told me that when he went to the North Carolina State University at Raleigh, the ceramic industry in North Carolina was zero; there was practically no use of the materials in North Carolina. By the time I met him, the Department of Ceramics had existed at North Carolina for maybe twenty years. The sales at that time of North Carolina ceramic products was about $100 million. That had come about because of the school and its graduates.

Then I looked around and got Professor Pask from Ohio State here, and he, of course, wanted to set up a whole program of instruction. I said "Your salary's in the budget, and you aren't going to be fired no matter what you do for a while, anyway. The wise thing for you to do is to concentrate on research and get some work going and have some associates in those projects. Then you can talk about having some instruction." By the time he was here for a while and associated with Parker and the others who were in metallurgy, the idea of a common materials program took hold. And that's what they have. That was my idea from the very beginning.

I wasn't able to sell plastics to them, to get them interested. That is a somewhat different field, but it's a shame that engineering hasn't been engaged in work on plastics for the last twenty years or more. Ceramics has grown, and now our graduates are the principal engineers in those companies.

Co-op Program

Ziebarth: Where did the idea for the co-op program originate?

O'Brien: The University of Cincinnati started that kind of program. I heard about it. The co-op program was started here because we had lots of students who got summer jobs in the aircraft industry. As the buildup came in defense, they could get jobs. The companies said, "If we hire your student in the summer and he really has a job, then the job has to be filled. And when he leaves in the fall and we fill that job with someone else, we can't fire that someone else and put your student in the next summer. We've got to have people all the time."

Secondly, at Cincinnati, engineering dominates the scene, and they can schedule their courses to fit the program of the students. But we couldn't do that. We had to keep courses on the regular term. So we got the coop started and kept it small because I felt there had to be an assistant dean who gave some attention to it. Finding the jobs in the companies to be filled by the co-op students during their periods in industry, and then following up and seeing that the students were prepared to undertake it. It's a very successful program.

If I were doing it again, I would have that as the main feature of the engineering program. I wouldn't require everyone to do co-op because some students might find it impossible to get a job. But nevertheless, to really organize it around a co-op program. Co-op has to have courses in the summer; some of those students are available in the summer. We had to give courses for two students in the summer when we got it going. So there was a limit to our coop fields; we couldn't undertake electrical, mechanical, civil, and everything else. We just had to start it in mechanical.

It took some planning and was still a small program compared to Cincinnati and some other schools, but it is very effective. The companies liked it. The students liked it.

Engineering Advisory Board and Alumni

Ziebarth: Why did you set up the Engineering Advisory Board in 1942?

O'Brien: We wanted contact with industry. I was recently told by Dean Karl S. Pister that the engineering alumni, which is the outgrowth of all that, contribute $1 million a year to this college. They collect private funds at the discretion of the dean. When I became dean, no one had paid attention to the alumni. Dean Derleth didn't want to have them messing around in college affairs. We didn't even have a list of the alumni.

So I thought the first thing we had better do was get an advisory committee which would be a help to the dean. At that period, we were trying to get a handle on who had studied here. Who were the alumni? We didn't know. We'd dig around in old college records. There'd been the College of Mining, the College of Mechanics, the College of Civil Engineering, and nobody kept the records in a central place.

We did our best to get lists of names, and we went to the extent of sending registered letters to those that we didn't get an answer from to nail them down. By the time when it was appropriate to get this engineering alumni society started, we had a pretty good list of names of the graduates.

Before we had any alumni association, however, Professor Kelly helped me send a postcard to the graduates who were out one year, ten years, and twenty years and over. So each year a batch of cards went out but to a different group. That gave us a lot of information about what our students did, the kind of jobs they got.

That began to build up a really solid list of alumni. So when the talk of an alumni association came up, we could hand them a good list of alumni. The advisory board was formed first. It still exists but is not statewide. There's one for each college of engineering. At first, it was just Berkeley. Then when UCLA came along, it covered that and all the campuses for a while. One engineering advisory board, advisory to the president.

Ziebarth: Was it useful to you?

O'Brien: They met about twice a year. Oh, yes, they wrote reports. For example, their report on engineering education—I think Eneas Kane was the chairman—went to hundreds of copies because schools all over the country were asking for copies of it. They were a great help.

The original board included people like Steve Bechtel and John McCone, who's been head of the Atomic Energy Commission and other government jobs. One of the big engineering firms during the war period was Bechtel-McCone-Parsons.

When we set up the council, people were picked because we knew them. Then the question came up, I remember Miss Lane came to me one day and said, "Who is an alumnus?" I said, "Anyone who has a degree from engineering at Berkeley. " She said, "You should know—maybe you do know—four members of your engineering advisory council don't have degrees of any kind, not engineering or any kind of degree." So before I got through with that, I defined an alumnus of engineering as anyone who spent one term here—if they had money. (laughs)

Bechtel spent two years; his father was building a railroad up in northern California, a big job. Steve quit the university and thought he'd learn more by working with his father. He probably did because he turned out to be a billionaire.

Ziebarth: Did you see the board as filling a public relations function?

O'Brien: No, I thought it would be a help to engineering. It helped me. I wanted some engineers who *practiced* engineering around because the faculty were so nonengineering in orientation. They would give us advice about program and also they would provide me with leverage on the administration. That's what happened.

Engineering Extension Division

Ziebarth: What about the expansion of engineering extension?

O'Brien: By that time Dr. Woods was director of extension, and he was an engineer and had been chairman of the mechanical engineering department. Anything the engineering faculty did was through the regular university extension.

Ziebarth: Was it useful in terms of providing for industry?

O'Brien: Oh, yes. We were under pressure to do more for industry in the way of off-campus courses. People like Bill Hewlett of Hewlett-Packard needled me a bit. We did set up some instruction on the [San Francisco] peninsula, in Silicon Valley, a few individual courses. But I said to Hewlett, "Stanford's right there. Let them do it if you want something in the plant."

The feeling was, this is a state university. Give service. But I couldn't see that our engineering faculty would get any recognition in promotion or salary for giving that kind of instruction. We did something, but other schools have extensive programs linked by telephone. Lectures on tv screens.

But we were more interested in basic research by the faculty and graduate study. That's why Berkeley has 1,500 graduate students now—almost as many graduate students as undergraduates.

Regulation Number 4 and
Testing Services for Industry

Ziebarth: After the war, the university published Regulation No. 4 which says research for the benefit of federal, state, and industrial organizations is to be undertaken only under conditions approved in advance by the president.

O'Brien: We were strongly in favor of that. The testing laboratories became our great friends after we got that under control, because if there was a facility available in private industry, we made people go there for testing. We only undertook the jobs that were a service to industry because no one else could do them.

The regents wanted to get testing under control. They didn't want the university to do testing. And we didn't either.

There was a lot of discussion about the university being dictated to by industry or by government. We didn't have much of any problem with that, except for some criticism. The engineering materials laboratory was doing routine testing business over here. Anybody who wanted their sand tested through a Rotap machine to get size distribution, bring it here and they'd do it. Graduate students earned money by doing it. We thought that that kind of work wasn't appropriate for the university.

Characterization of
University-Industry Relationship ##

Ziebarth: Has there been any noticeable change in the College of Engineering's relationship to industry since you were here?

O'Brien: I think there's much more research consulting done by members of the engineering faculty. I'm not sure that some people aren't doing too much of it. I hear that people go off on consulting assignments and have their class taken by someone else without any approval by the department or the college. They wouldn't get away with that with me. I personally took a leave of absence covering every weekday I was not available in Berkeley. I think there's no reason the faculty shouldn't report their consulting engagements to the university for the record.

We had some problems. We had a professor in sanitary engineering, an older man. In the early days of sanitary engineering in California, he was undertaking practically all the sanitary engineering design, the plants, in California. Some of his graduates, one in particular, said, "We could get jobs only by working under him" or words to that effect.

It got to the point where there was a letter from him to the governor on the desk of every member of the state legislature in Sacramento. I thought it was bad that this monopoly existed. The faculty in civil engineering were practicing engineers. This one man just did too much and had too much of a monopoly. He was undertaking to act as the principal, the one who got the contract. Then he would hire these former graduate students to help him. I heard about it before I became dean, but I was in mechanical engineering and it didn't make much difference to us.

There's some background to that. In wartime, some of the academic people objected to the university being involved in wartime research. It didn't make any difference to us what they thought. They were in philosophy and so on.

Proper Role of Outside Research For Faculty

Ziebarth: What was President Sproul's view on the proper balance for faculty research and teaching?

O'Brien: He gave engineering support always, and so did Jim [James H.] Corley. President Sproul graduated in civil engineering. He was quite favorable as far as engineering was concerned; he was certainly no problem in the building up of research, graduate study. He was a big help.

Ziebarth: Is it true that when Professor Folsom arrived on campus, you gave him a list of forty research projects he could work on?

O'Brien: Yes. (laughs) I do a lot of planning. I was a civil engineer fundamentally, but was not in the civil department. When these engineer officers came here to study, they were told to study with me. That meant that other civil engineering students came over to do a thesis with me. So I had a great long civil engineering list, too. When Folsom showed up, I had plenty for him to work on. I gave the present dean a couple of things to work on this morning. (laughs) [aside] I worked with General Electric for 38 years. Once a man was being put in as vice president and general manager. There was a period of four or five days when they had to clear some personnel problems and couldn't announce that he was the man. It was summer, and he lived on the shore at Swamscott. We got in a canoe and he sat in the bottom of the canoe and I paddled and we wrote down all the things he was going to do. He told me just after he retired, the list was still in his desk and he was working on it.

Ziebarth: I understand you and Professor Birge, chairman of physics, had disagreements about contract research?

O'Brien: Engineering has an objective in mind, namely some kind of hardware: a bridge, a road, a jet engine. People who have some contact with engineering, like Lawrence—who had a lot of equipment built and a lot of admiration for engineering—know engineers have a lot more difficult problems than scientists.

Birge was a physicist, a theoretical man. He didn't know much about engineering. We had to combat those comments.

Ziebarth: Were you ever questioned by the Regents about the amount of research being conducted on campus?

O'Brien: No. I didn't have much contact with the Regents. I knew some of them, and met some of them at the summer encampment up at the Bohemian Grove. But they didn't interfere. There wasn't any problem with the Regents; not like there is today.

Balance of Faculty Research and Teaching

Ziebarth: What do you think is the proper balance between research and teaching in practice?

O'Brien: The faculty in civil engineering are quite good in that respect. The two that I feel were really ideal were Etcheverry and Harding, in a small department called irrigation, which existed only for historical, sentimental reasons but was really part of civil.

Each said, "I'm a consulting engineer, and my principal client is the University of California." And they acted that way. They were available to students more than any other professors of engineering. They also were active engineers.

If I were setting up an engineering school now, I'd have two groups of faculty. I'd have the applied scientists, who'd be mixed in but they'd be treated different. But the practicing engineers would not be appointed until they'd practiced some engineering a while. Then I'd take them in under a contract that said they'd be allowed to take outside work, and their salary would reflect that. The people who were full-time without any outside consulting, the scientists, would have little outside consulting and would get a higher scale than the practicing engineer.

For all the years I was dean, the engineering salaries were tied to the general campus salary scale. Now they've broken loose, thank God.

As dean, in my discussions with President Sproul, I came to this: for a man who does nothing else and teaches several sections of the same course, the fifteen hours a week of class assignments was reasonable. That shocked people when I said that.

Then I said the faculty ought to have at least a quarter of their time to engage in research; otherwise they're not going to stay up-to-date and be productive and effective as teachers. Then we have to give weight to those who have graduate students and are the advisors of graduate students.

Until the wartime period, to have a graduate student was a pleasure. You didn't take that as a working assignment; it was something that was fun. Now that we have a school of fifteen hundred graduate students, obviously, not only do you have to give credit for the teaching, the actual course instruction, but when you had Ph.D. candidates, who were requiring a lot of attention, discussions and reviews, that is also worth doing.

As dean, I had authority that if faculty weren't doing research and weren't working with graduate students, I could load them with other things. There were administrative assignments: assistant dean, associate dean.

I tried to see that everybody was doing something that justified their salary. Some older people who'd been here before the emphasis on research still did a job; they were very helpful. There's a lot of routine work, and it's not work that you can assign to nonacademic people.

I tried to assign things that were reasonable and gave strong recommendations to those that were actively engaged in research and the guidance of graduate study and graduate courses. I think fixed rules on things like that are pretty difficult and usually end up by being unfair.

The only thing I stuck to was that all the faculty were in the budget.

There was no soft money. And two, anyone really active in research could count on having a quarter of his load free for research.

Expansion of Facilities and Programs

Institute of Transportation Studies:

Ziebarth: Let's discuss the postwar expansion of facilities and programs. How did the Institute of Traffic and Transportation Engineering, now the Institute of Transportation Studies, come into being?

O'Brien: There was a study of roads and highways by a state senate committee under Senator Randolph Collier and George Hatfield, his associate, who was an older man than Collier; he had been lieutenant governor. He was from Los Baños. Those two had made a study of transportation in the state, and they came up with what you see now: hardware, highways, freeways. Not everything that's now in place, but the principle of limited access highways and the principal thoroughfares were in their plans.

When they made this study, they called on President Sproul and said the University of California should be doing something about education and research in this field. He listened to them politely and then he called me. I'm sure he expected me to say, "We're doing all right and these guys are full of prunes."

Instead of that I said, "They're absolutely right, and we ought to be doing something more about it and let's go." That's about all I heard, and about a week before I was going out to Bikini for the atom-bomb tests [1946], I got a call from the president's office saying, "Collier called. Where's that bill you were going to draft?" I said, "I didn't hear anything about a bill. What's the idea?" "You said that there ought be formal studies in an institute."

Okay. I sat down and drafted a bill which the legislature passed almost exactly as I wrote it. About the only change was that I had had words which nailed it down to Berkeley, and the administration lifted that out so that it became statewide.

At that time it became the only line item in the state budget beside the university itself. The university was one huge lump, and here was the Institute of Traffic and Transportation Engineering as a line item for several years.

Ziebarth: What is its charge?

O'Brien: ITS [Institute of Transportation Studies] studies highways. First, the physical highway; the noise that a tire makes, the physical design of the concrete. It's a physical problem, an engineering problem. Then there are lot of questions about traffic control, safety. The Institute of Transportation was the sponsor of a lot of conferences. It had an advisory counsel representing people nationwide from transportation and government, state and federal.

We looked for a director and finally Harmer Davis, one of our professors who was already here, undertook it and did an excellent job. It's a very effective organization. It was one of our first successes. It professionalized the whole matter of traffic and transportation.

Now it's off into some other thing. The general faculty got messing around in it so it's been diverted from a practical, realistic approach to transportation studies. Now they study any damn thing.

The people running and spending the money on traffic and transportation not only were employing the graduates of Berkeley—all these different agencies, city, county, and others—but also the studies being made were pertinent to the problems of the state. Our connection with the transportation interests and the Transportation Institute advisory council was very close. We also had a kind of an extension service related to the institute. That meant that there was good research but it was definitely oriented toward problems of importance to the state.

In design, for example, we had a truck that went around measuring noise on different kinds of highways and levels. It got to the point where engineers could specify that contractors must come up with a concrete surface that was quiet. Physical things like that.

Also, money. There were studies of off-street parking. There was a lot of controversy over that, and cities began to buy property for parking lots. It helped the merchants; it helped the people who lived there.

There were controversies regarding the location of state highways. The City of Santa Rosa, for instance wanted the main highway north to go right through the town because they thought they'd sell gas that way. As I remember, when they looked into it, it didn't turn out that many people stopped for gas. It was just a nuisance to everybody, both the travelers and those who lived there. Then the limited access freeway came about and became part of the state general plan.

Ziebarth: Was the limited access freeway something that was developed in California?

O'Brien: One of the men we considered as director of the Institute was the state engineer of Connecticut. and he had built the Merritt Parkway. I think that was limited access. In California, none of the highways were limited access until Collier came along in the late 1940s.

Sanitary Engineering and Environmental Health Research Laboratory:

Ziebarth: What about what's now the Sanitary Engineering and Environmental Health Research Laboratory (SEEHRL), set up in 1950?

O'Brien: Then it was the Sanitary Research Laboratory. Professor Harold B. Gotaas, who was chairman of civil engineering, set it up. The idea was to recognize in the formal organization that sanitary engineering is a special thing. It's half chemistry and biology. It's kind of a freak in civil engineering. And it came about because the physical works were civil engineering works: brick and mortar and piping.

The New England Waterworks Association gets the credit for establishing sanitary engineering worldwide. It seemed like a wise thing to set up the research laboratory which people could identify as a continuing activity. Once things like that are set up, they gather momentum. They're in the budget, the words "sanitary research. " When civil engineering says, "We need money for sanitary research," that's different from having a name in the budget.

It was really to nail down the activity, get it recognized. And it is important. If I were setting it up now, I wouldn't put it in civil engineering. These problems are mostly environmental and related to pollution.

Ziebarth: I have read that the Sanitary Research Lab was established in response to a legislative mandate that hogs were no longer to be fed garbage?

O'Brien: I don't know about that. The reclamation of sewage water *is* something that's going to come. People shy away from that because it just sounds terrible to take the sewage and clean it up and use it over again.

I thought of the sanitary engineering facility in the terms that when you have a lab like that, you can justify appointing biologists and chemists to the faculty. The tendency in civil engineering in this university is to call all the professors civil engineers no matter what they are. If you set up a lab, you can get them in as biologists. And then you can give them teaching assignments as lecturers and pretty soon you can get them made professors if they're very good.

The Sanitary Engineering Laboratory was created to recognize that, and, in my thinking to recognize the fact that it was an activity in which the traditional civil engineering really wasn't the main stem any more. It was biology and chemistry related to environment and ecology.

Ziebarth: Did the lab have a public-relations reason for being?

O'Brien: Sure. It has a director who says, "I'm the director of Sanitary Engineering Laboratory." The chairman of civil engineering addressing the same group wouldn't have the same standing as the director of the Sanitary Engineering Research Laboratory. All that's involved.

Forming a lab is a way to bring two people together for research. Professor Putnam and I wrote some papers on flow in porous media and then he set up a lab down at Kettleman Hills.

Richmond Field Station:

O'Brien: Richmond Field Station came about because it was clear that the kind of thing that I thought we ought to be doing involving acids and smoke and smell obviously couldn't be situated on the campus. We looked around but for a long time thought it was practically impossible because we couldn't find a place that was near the campus. I didn't want something an hour's travel away. That would be too much of a nuisance.

It just happened that I had dinner at the French Club one night, and they have a great big table, family-style. Anyone who eats there eats around this one big table. I happened to tell whomever I was with that we were looking for a field station and hadn't been successful. We couldn't find a place and couldn't propose anything to the Regents.

A man I'd never met across the table said, "I think I have a place for you. " It turned out that the Oliver family had owned the California Cap Company that's the site of the field station. The California Cap Company made caps and detonators and fuses for the Union forces in the Civil War.

They had just given him the commission to sell the place. They'd held it all these years. It was out of use. The Engineering Advisory Council got interested; they came out and looked it over, and we all agreed it was a fine thing to do and let's go. We put it up to the Regents, and, so help me, they bought it right off for about $750,000.

The State Highway Department just paid the university $750,000 for a little thin slice of land that they took from us for a highway out at the field station. So the Regents have their money back.

We had a little problem one time because Stouffer Chemical next door generated enough acids in the air to affect the delicate equipment at the field station. Professor Dorn had some metallurgical experiments for a while, and he found that his instruments were being fouled up. We complained to Stouffer. The simple solution was to move his things back on the campus, which could be done immediately.

They've done lots of experiments out there. We have a thousand-foot-long channel for measuring sediment motion by water and sediment transportation by streams. That's typical of what you can't put on campus. It wouldn't justify the space. Furthermore, the kind of thing we built out there we may want to let sit idle. That flume, that channel has been idle for several years. We don't want to tear it down. It's there; it's valuable. We may get it going again in some experiment. The Institute of Transportation was put out there. A big earthquake unit is there. A big testing machine was moved from the campus out there.

I'm told that now there's talk about having it as a general campus, an auxiliary campus. That will foul it up because they'll fill it with things. People will object to smell or smoke or something out there. Even so, we're kind of restricted in what we can do because of the neighbors, but it's an open area and the land at that time wasn't very expensive. Pacific Gas and Electric wants the Diablo Canyon experiment, which has been in the model basin for five years or more, left because they still have questions.

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Ziebarth: Was there a specific project in mind when you acquired the field station?

O'Brien: No. But it was needed.

We had inherited the men's swimming pool over on College Avenue before the war. It became an expansion of the hydraulic laboratory. We thought up an experiment to do there which would require the pool, proposed it to the administration, and we got it. We built the Columbia River model there, which was a tidal model, the first in the U.S., practically the first in the world of anywhere these proportions. This demonstrated the need for getting engineering into the possibility of doing experiments that required being left in place for years. By the end of the war it was evident from the range of things that we were engaged in that this kind of facility was needed. When the cap Company became available, the Engineering Advisory Council got behind us.

They'd look into something and write a report for the president which said this is a good idea. You ought to support engineering in what it has proposed. When the advisory committee includes Steve Bechtel, John McCone, Larry Kett, who was president of Mountain Copper Company, Black, who was president of Pacific Gas and Electric, the chief water engineer for the state, and Orrin Rider, an engineer who was in the movie industry, it had some effect.

Furthermore, the university then operated without so many committees. President Sproul had a lot of authority, and the advisors were senior people. The oath controversy really changed the university.

Institute of Engineering Research:

Ziebarth: What about the Institute of Engineering Research?

O'Brien: The Institute of Engineering Research [established 1950] was not an organization like Stanford Research Institute, which sells its services and looks for jobs. It was a way of supporting engineering professors, helping them get projects. We worked not by going to the sponsor, selling a project, then going to the faculty and saying will you do this, but by helping the faculty prepare their proposals and seek support where there was a chance they'd get the money.

For example, when a man got a research project, the first question we asked him was about the space. Then, although he didn't have much typing, he would still have things typed. The Institute of Engineering Research had people available so that a research project could get started with a minimum of effort. Up to that point, every professor that got a project started spent time worrying about these details rather than doing the work on the job that he got the contract for.

Once we set this up, he had help with all the mechanics of getting a project started. That was the purpose. I explain that because we definitely were not like a Stanford Research Institute or other research organizations looking for jobs and then persuading the faculty to undertake them. We supported the faculty in what they wanted to work on. That was the basic principle.

Ziebarth: Has there been a big change in the amount of record-keeping required for projects?

O'Brien: Oh, yes. All the legal matters and the overhead. The Institute of Engineering Research was the one to handle it. Now it's called the Office of Research Services.

Electronics Research Laboratory:

O'Brien: I told you about the support from the Navy for an electronics research laboratory. After the war electrical engineering was booming. Having a laboratory recognized as a separate research unit helps get additional support. Also, having a lab splits the administrative job, so that one man can be chairman of electrical engineering, and with that, responsible for administration, faculty, and so forth. Another man can have the lab where there are special facilities.

We had inherited a laboratory and technicians from the Navy at Point Loma, and we brought it up here when we definitely decided to go into electronics. There was a reason to have them given special attention to be sure we didn't get fouled up over salary scales.

On the other side, to recognize a lab at Berkeley as the Electronics Research Laboratory, I felt, would be advantageous in dealing with the services. The Defense Department for a long time, and maybe even now, sponsored electronics research for all the services— Army, Navy, Air Force—and then they picked a few schools— Stanford, Berkeley, Brooklyn Poly or MIT, or maybe all four. Those four got lump-sum grants each year.

That meant the books were kept separately. The lab had one set of books; the department had another set of books. So there was reason to set up a separate lab.

Also, we had a whole lot of glass blowers. They were very rare; there aren't many in the country. We could make vacuum tubes here because we had all the facilities. We had the furnaces.

Department of Industrial Engineering:

O'Brien: Industrial Engineering was set up in about 1954. Mechanical engineering was defining its field in terms of certain applied sciences like thermodynamics, heat transfer, dynamics. The industrial engineering side of it wasn't represented adequately. I just felt that field ought to be recognized.

Operations research was a statistical]mathematical approach to scheduling. Quite a lot of activity in it. I wanted Industrial Engineering also to be responsible for manufacturing. That didn't work out.

There were two groups that didn't have a common interest. The man in manufacturing was a metallurgist rather than a manufacturing engineer. The emphasis came to be on the operations research and the statistical approach to scheduling in industrial engineering. There's nothing wrong with that; it was good. But the laboratory work of manufacturing engineering was quite remote from that. The interrelationship between the two that I'd hoped for didn't develop. The physical part, the manufacturing side, is now back in mechanical engineering.

Construction Division of Civil Engineering:

O'Brien: Then I tried the organization of construction as part of an industrial engineering branch of civil engineering. I got interested in it when the roof of the engineering materials laboratory fell. The big testing machine was going to be there, and they had a clear space three stories high. They were pouring the roof of the building, and they had scaffolding running that whole distance.

When you have a rod and you push down on it, it tends to buckle. Dean Derleth told the engineering faculty they were not responsible for that building. They were not building it, and please keep out of it.

But two of our professors, Professor Troxell and Harmer Davis, had looked in a couple of times, and they were worried about this scaffolding being so high. And sure enough, what they thought about it developed. When they were pouring the roof, as the concrete load developed, finally the scaffolding buckled, and the men and concrete were dumped through three floors down to the bottom. Several men were killed. My office was in the corner of what was the Mechanics Building, and I heard the roar of that stuff falling.

I thought about this, and then I talked with engineers. The whole matter of construction of civil engineering works was in the hands of nonengineers. I had a classmate who was the resident engineer on the Golden Gate Bridge, and he and his superiors were tremendously interested in safety. They put nets under everything. You couldn't fall off that bridge. Except a painting contractor who designed his own scaffolding. Three men were dumped into the Golden Gate.

I began thinking about the fact that construction involved a lot of money, being the payoff of civil engineering, and here it seems to be, in general, operated by nonengineers. All the falseworks—this stuff they have to put up before they build the building, forms, scaffolding—is a big economic problem, which is part of engineering. Cost is related to construction.

So I got approval of a professorship in civil engineering for construction. Then the problem was to find a professor. First we got a man who was from the construction-machinery industry who did a good job. We got him free; I didn't have to pay his salary. He was retired, and the industry paid his salary. He was here for only two or three years.

Then I made the mistake with the next appointment—I was told if we don't appoint somebody, we'll lose the position. In fact, it was vacant for a year. That taught me, don't make an appointment on that basis. If he isn't of our caliber, don't do it. We made an appointment that blocked progress for several years because the man appointed just wasn't equal to the challenge.

Next we got a retired engineer-general to take it. He did a fine job, but again, for only two or three years.

After more than twenty years, we finally got an appointment of the caliber we wanted: Professor Ben Gerwick. We ought to have more work in engineering on the economics of engineering. There's a field there to be developed.

We had an economist in the Institute of Transportation who did a good job; the proof that he was effective was that he took a leave every year to help the senate committee on transportation with the legislation.

We need someone with standing such that the Economics Department would want him but who would come to engineering and work on our type of problem.

The division of construction, I felt, was related to civil engineering in the way that industrial engineering was to mechanical. It's finally in good hands, but it took a long time to get there.

Water Resources Center:

O'Brien: The Water Resources Center was an offshoot of the fact that we had a transportation institute, which had been set up with the help of Senator Collier and the legislature. I talked with him several times about the fact that there was a lot of work on water problems in California, and there were a lot of different individuals, even outside of engineering, who were working on water problems. I suggested a water resources center and archives at Berkeley.

A library has printed things which people can borrow. But a lot of important material never gets to be printed. Engineer's reports go to the client. There's a lot of information in them. I thought it was important to have a collection of those materials, so we established the Archives. We say to people who are retiring—we have lots of reports written by men who retire—"If you'll give us your reports, we'll guard them and keep them. They're available to you, and they're available to others but you can get them out of your files."

The archives is not a true library. We don't have that kind of material. The main objective is to get the non-printed ephemeral material and keep it for the future.

In the process of going through the mill, the proposal for a water resources center got broadened to the point that the director of it is at Davis [now, at Riverside] instead of Berkeley. The Archives is in Berkeley, but the center was to be definitely part of engineering at Berkeley. Now that it is statewide it's gone haywire. It isn't worth much. Interesting papers but without much effect. This is sour grapes, I guess.

The university system has turned into an institution in which everybody says, "Me, too. " All these things that I thought up and got started—once there were these other campuses with engineering, they all said "Me, too. I want the same. "

Once we drafted the water resources proposal and I knew it was going to pass the legislature, it got shipped around to see what all the campuses had in water resources, and finally it was a statewide thing instead of a Berkeley thing. What I wanted was a branch of engineering on water resources to which we could attract people from other fields and have some say regarding support.

Ziebarth: Did you write the original legislation?

O'Brien: I had some help. It was definitely a proposal from engineering that got it started. And we had done enough that we knew it would pass if the university would just put it through.

Ziebarth: How did you know it would pass the legislature?

O'Brien: They told me, "Write the bill. We'll put it through." The sanitary engineering people, Percy McGaughey, wrote a paper that every member of the legislature got. I think the title of it was "The Cost and Value of Water." What he found out was that for all the water projects they spread the cost over the whole taxpaying public but the benefits were greatly concentrated in a few. The legislature took great interest in that matter.

That was just one example, but we must have printed several thousand copies of that article. We did all kinds of work related to water, and I felt that it would be wise to try to direct it into channels like we had in transportation and traffic where the graduates of Berkeley were being employed and the studies being done were pertinent to the problems of the state.

Nuclear Engineering Department:

O'Brien: I had some connection with the nuclear field in the mid- and late 1940s through Lawrence and the Radiation Laboratory. But to me, nuclear engineering was a special field which was suitable for engineers and specialists of many kinds. There were people on the campus, however, like my friend Lawrence and Seaborg and others, who said to me , "Well, you ought to have a nuclear engineering department."

I said, "It's a graduate study program, not a speciality. The engineers in that field are electrical engineers or they're mechanical engineers or physicists." By the time this was being discussed, I'd been connected with the aircraft nuclear propulsion project as a consultant. I spent about a year helping them and GE, and also with electric-power development through central station nuclear power for GE. I have learned a lot from that connection with industry.

Aeronautical engineering is a good example. We never did have any section here on aeronautics—for good reason. The man who could design the whole airplane, the engine and the wings and the whole thing was no more. The aircraft industry was manned by people who were specialists in control systems, specialists in aeronautics, specialists in mounting engines, and the engine companies had specialists in mechanical. The aeronautical engineer was a thing of the past. People learned all that, but their basic preparation had to be in the fundamentals of the mechanical]electrical variety with a lot of structural information from civil engineering.

So we never did go into aeronautical. But with so much going on with nuclear power, nuclear energy, the bomb, and Lawrence and Seaborg, I finally gave in.

It's funny how we got going. Jim LaPierre was the vice president of General Electric at that time; he's a man I'd worked with and he got me into the engine business. A man by the name of de Hoffman had a laboratory down in San Diego under General Dynamics. De Hoffman told LaPierre he was having an awful time with a project in which the project engineer demanded that he have every function. He had to have people right down to the finance.

De Hoffman said, "I'm not an engineer, and I don't know whether you have to have that to do engineering work. " LaPierre said, "I have someone working with me, O'Brien; why don't you get in touch with him?" So I met de Hoffman at the American Airlines lounge in Los Angeles. I spent about a half a day with him talking about it.

The upshot of that was I took the man who was causing him so much trouble and brought him here. He was the man who got it going. It was clear that his view of what he should do wasn't going to fit in with the de Hoffman system.

I understand that they have now expanded the program to undergraduate nuclear engineering. Undergraduate nuclear engineering is awful, terrible. They'll have small classes that will be as expensive as heck. You want people to be specialists. If you are going to be on the nuclear side, you'd better be a physicist and really get into the chain reactions and all the low-energy physics. All the people you want in this field had better be well-grounded, not dilettantes in the field.

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What they're going to do in nuclear engineering is teach them a little bit of mechanical, a little bit of civil. They aren't going to be real experts at anything. Furthermore, if you set up an undergraduate program and a graduate program, you can't offer graduate courses that the undergraduate students already have taken.

What they really need to do, if they're going to have graduate work in nuclear, is recruit electrical engineers, mechanical engineers, and physicists and give them the special things that go with nuclear power, nuclear energy. They've done the same thing in marine engineering and naval architecture.

Universities are practically out of control. Faculty do all sorts of things. Down at San Diego they're starting instruction in civil engineering at UCSD when the capacity for engineering instruction in the field of civil engineering is way over what is needed. They're starting work on coastal engineering at Santa Barbara. The campuses are running themselves without any coordination, and it's going to cost this state an awful lot of money.

Naval Architecture Department:

Ziebarth: Was pressure from the navy responsible for the decision to set up the division of naval architecture in 1958?

O'Brien: No. Naval architecture on the campus was the result of a speech given by Benjamin Ide Wheeler after the Spanish-American War. The ship *Oregon* had been built in San Francisco, and it had had a great record in the war.

Did you know that landing on the moon was the result of a speech by President Kennedy at the Air Force Academy? He wanted to say something startling and inspiring, so he said we'll go to the moon. Similarly, Benjamin Ide Wheeler, who didn't know anything about naval architecture, said "The waters that launched the *Oregon* shall be the site of a great school of naval architecture. " He didn't know what he was talking about or what it involved.

During the war we did work on welded ships, and some other things: hydrodynamics, propeller noise for ships. We began to have some connections which convinced me that since there were only about four schools in the country—MIT, Webb in Brooklyn, Michigan, and Berkeley with any vague interest in naval architecture, we ought to expand.

Professor Vogt had been associated with Commodore H.A. Schade, who was director of the Naval Research Laboratory near Washington. Vogt was high on Schade and suggested I talk with him. I went down and had several conversations with Schade. The upshot of that was I got him appointed to a position, and he came out here to be professor of naval architecture and director of the Institute of Engineering Research. Now they have an international student body from everywhere.

Ziebarth: Did the faculty support the idea of a separate naval department?

O'Brien: No. These things came about without much participation of faculty generally. The ones who were interested worked, and the others tended to their business and went ahead. There was no real sentiment for or against so long as people were all busy.

Student Activities:

Ziebarth: I understand that you supported the idea of student societies.

O'Brien: We certainly encouraged them. Undergraduate student programs, their records, the office, and their advising were very interesting to Dean Boelter, the associate dean. So I delegated that to him. His interest was one reason I wanted him to be associate dean when I talked with Sproul.

Boelter was rigorous. He applied the screws on the students to meet the requirements. But the students respected him, and they felt he was equitable. He was hard, but he was fair.

The whole business of students I paid little attention to. One thing that I did do regularly was I had lunch with groups of students, just three, four, or five. Sometimes I astonished the college office by saying I'd like to have a couple of luncheons with the worst students in the college. I wanted to see how bad they are. Or the senior electrical engineers. Or the presidents of the student societies. Different groups. So when I was in town, about two days a week I'd have a group over to the Faculty Club and we'd talk. But as I say, I delegated student affairs and paid little attention to that office except that I was directly in touch with the students.

Every term I'd meet with half of the people who'd been out on the cooperative program. They were back for their studies, and I'd have groups of them for lunch over at the Faculty Club to learn how they were going. That went on all the years I was dean.

Evaluation of Post-war Expansion:

Ziebarth: When you look back at your extensive postwar development program in engineering, were you able to accomplish the postwar planning you had envisioned?

O'Brien: [looking at "Postwar Plan" document] I wanted a joint program of industrial engineering under engineering and business administration— [reading] "Production methods, engineering economics, industrial psychology, the range of subject matter frequently designated as industrial engineering should be given greater emphasis after the war to support the industrialization of the state." That I gave up.

Cooperation can be maintained only under the same man. In other words, if you want to have these two departments or schools to cooperate, you need to look in the table of organization and see that they report to the same man. One of my GE friends, an executive senior vice president at General Electric, agreed exactly. He said, "I could see that two departments or divisions under me collaborated. But I couldn't be sure of what would happen if one of my divisions tried to cooperate with an outfit under another guy at GE." The same here.

So this is one thing that didn't happen. We didn't develop it jointly with business administration because engineering and business administration come together in administrative control only under the president. It may sound dictatorial, but that's what happens. They have different criteria. If I try to promote collaboration between parts of engineering, they know darn well that I'm going to remember how they responded when it comes to promotion. People who work and accomplish things deserve promotion.

Faculty Response to Postwar Expansion:

Ziebarth: How did the faculty respond to your postwar efforts to develop engineering at Berkeley?

O'Brien: We were fortunate that quite a number of the older people were ready to retire. Maybe I was dictorial and they were afraid of me, which is part of it. But I think Associate Dean Boelter and I went through the most democratic process that I know of anywhere in developing our plan.

Since we were planning for having twice as many senior students as freshmen, having a system in which the junior and state colleges fed into the university, and we were going to produce almost as many Ph.D.'s a year as we were graduating seniors at that time, we had to think this all through.

The chairman of the Space Committee on the campus told me one day, "You know, we practically can't comment or argue. Your plans are all so specific. You've got to have so many students and so much space and laboratories." So we got Cory Hall, and Etcheverry Hall came after that. But we were thrown for a loss when the geology building was built where Etcheverry Hall should have been, next to Hesse and O'Brien Hall.

[Interview 5: March 4, 1987] ##

Evaluation of Postwar Plans for Graduate Education

O'Brien: Our planning was the basis for the high caliber of graduate study that we started. If we hadn't done that, if we'd spent our time taking care of the veterans coming back, undergraduate education, I think Berkeley would have ended up as only moderately interested in graduate study and research.

After I left Berkeley, there were three reports on graduate education made. One was the Carter Report, in the early 1960s. Between the time I retired and the Carter Report, there were no changes whatever in engineering, no appointments. It rated civil engineering at Berkeley number one in the country. Mechanical and electrical were two or three. I regard that as a kind of report card for me.

These three studies consisted of questionnaires to several thousand faculty people who were asked to rate the universities, one, two, three, four, five. In the last one that I remember, which was ten or fifteen years after I retired, Harvard and Berkeley came out even-steven. The report said that engineering at Berkeley was very high in standing, and Harvard had almost no engineering activity. So the nod was given to Berkeley; it became number one on graduate study in the United States. And the deciding factor was the fact that engineering at Berkeley was tops.

When Clark Kerr was here as president and I was dean, we had many discussions. He was a little suspicious of me because I was always coming around with a new idea of something I wanted to do. After he retired and was at the Ford Foundation, he spent some years appraising universities. I happened to meet him one night in the Waldorf Astoria—he likes rice pudding and so do I. He said, "When I was at Berkeley, I was a little suspicious of you. But as I went around the country, I came to realize that Berkeley engineering was way out in front, really moving. It was quite a sensation. "

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Ziebarth: Looking back, is there anything you would have done differently as dean?

O'Brien: In engineering, we need faculty people who have practiced engineering. And for the most part, this engineering faculty, like all others, aren't even engineers really. They wouldn't have the faintest idea how to carry out an engineering project.

The purpose of engineering is to produce useful results. Software studies and computer programs and theoretical works should lead to practical, useful, saleable results.

Another regret is that engineering was frozen on the same salary scale as other faculty, and we didn't break out of it in my time. In engineering we should have a contract which specifies there'd be no question about the propriety of a man doing consulting work.

When the administration broke out of the campus scale applied to everybody, engineering got 15 percent higher on the average. Now they have this scheme by which individuals are appraised and go on up indefinitely. According to the newspaper recently, Professor John Whinnery, who was dean following me, earns $108,000 [nine-month salary in 1988].

In the engineering faculty we brought in people who were physicists and chemists. I think we ought to be flexible and bring them in and say, "Well, you're going to be in here because you're a competent physicist. We hope you'll be interested in the engineering problems, but we're going to put you on the scale of the physicists."

Furthermore we should have been able to go to very high salaries. We never broke through that until Professor Ben Gerwick was appointed in civil engineering.

In engineering we still are frozen in personnel relationships. I tried to change them but made not much progress. They haven't gone far enough to reward the engineers who are practicing engineers and teachers as well. They've been giving too much to the engineering scientists in comparison with the other scientists.

Ziebarth: Is that because someone who practices can earn much more?

O'Brien: Yes. It's funny. My standing on this campus in engineering was principally because I had no intention of being a professor and said so. Dean Derleth, who was dean of the college, liked me particularly because his view was those who can do and those who can't teach. I had only a bachelor's degree, no master's, no Ph.D. If anybody asked me about it, I'd say, "I'm here temporarily, but I don't expect to be here long. " I got to be Dean Derleth's fairhaired boy because my view agreed with his.

Process Engineering Controversy

Ziebarth: What about the issue of adding chemical or process engineering to the engineering college in the mid-fifties?

O'Brien: Chemical engineering is the practice of chemistry, and it involves certain fields like heat transfer, combustion, fluid flow applied to chemical processes. Mechanical engineering was moving toward the same kind of approach as chemical engineering. As a matter of fact, Dean Boelter and I had fellowships on this basis.

Long before the war, we were moving in the direction of teaching mechanical engineering engineers, who need the same subject matter but without so much basic chemistry involved. We were moving in that direction and were close to people in chemical engineering at MIT and Michigan and around the country. We thought that chemistry belonged over there in chemistry, and chemical engineering belonged in engineering along with the rest of engineering.

So I proposed this. Furthermore we decided to call the field something different: we called it process engineering, because that was what it really was.

But it turned out that there were very influential people in chemistry like Joel Hildebrand and others who disagreed. The key point was that we were interested in teaching the application of engineering processes and thinking to chemical processes. Furthermore, what they were teaching and talked about teaching over there was engineering chemistry, not chemical engineering. In other words, physical chemistry is really engineering chemistry.

I remember one meeting with Sproul, a final argument. Hildebrand, chair of the Department of Chemistry who lived to be 100 and was an awfully opinionated, conceited man— and a campmate of mine at Bohemian Grove—said any issue which depends on the difference between engineering chemistry and chemical engineering is trivial and should be brushed aside. Hildebrand just had more power and influence, and I lost out.

What they offer in chemistry as "chem engineering" is a big joke. It's well done, there are good people teaching it, but it just isn't chem engineering.

Ziebarth: What was the final decision on your effort it bring it into engineering:

O'Brien: We were allowed to go on with process engineering, but I was about to retire anyway and it was kind of sandbagged. Students didn't register for it because there was a question about it; it wasn't called chem engineering and that was unusual. We plainly lost that argument.

I think it was a great disservice to the university and especially to the students who go through it in the Department of Chemistry. They're good students and so they do well because they're bright people. But they weren't helped toward engineering.

Ziebarth: Was Hildebrand a jealous protector of the chemistry department?

O'Brien: Yes. They thought they knew all about it, and engineering was just a trivial, low-level activity. A lot of people think that in universities.

Relations with Committees and Academic Senate

O'Brien: One thing I always tried to do was to keep ahead of the latest fad of the faculty. When we were fighting to get enough instructors to take care of the postwar influx of veterans, they wanted to have all the procedures of the faculty appointment carried out. I said, "My God, we've got to have people now. "

So I found out that by appointing lecturers, that waived the rules. We could appoint them, and they could teach, and, furthermore, we could have them one-third time or one-half time. We had a lot of people from industry— about 20 or 25 lecturers at one time.

Then the faculty committees, especially the Budget Committee, said, "You shouldn't have people teaching that haven't gone through the process of appraisal." Then appointing lecturers wasn't so easy. But by that time we were through the hump of students.

Ziebarth: You used the power of the committee system.

O'Brien: I had the advantage that when I first came here I lived in the faculty club, was unmarried, and the Committee on Budget and Interdepartmental Relations met once a week at the faculty club for dinner. So I got to know them quite well. Then I got to know the dean of the graduate division, Lipman; he helped a lot in formulating plans of what we wanted to do. In fact, before the war he often needled me about not doing better.

There were several colleges— the College of Mechanics, the College of Civil Engineering, the College of Mining—when I first came here. First, the College of Mechanics and the College of Civil Engineering were combined to become the College of Engineering, leaving the College of Mining separate.

Dean Lipman would give me lectures: "You've got to go over there and get those people in mining to straighten up and do some research, have some graduate students, and act like a university." I'd say, "My God, Charles, I don't even belong; I'm not a member of that group, they're mining." He'd say, "Nevertheless, you ought to do something about it. "

I had the good fortune to get acquainted with all the senior people in the university when I was about twenty-five years old, had first arrived, and was still an assistant professor. Furthermore, Ernest Lawrence lived in the faculty club and we played tennis. I got to know Robert Oppenheimer, who was half-time at Cal Tech. Those two and I arrived here as assistant professors in the same year, 1928. All of those connections helped me in dealing with university committees.

Furthermore, in those days we did a little politicking. The dean of letters and science and the dean of chemistry and I would meet before the election of the Committee on Committees. We talked about whom we'd like to have on the Committee on Committees, which appointed the other committees. We managed to elect a majority of that committee for ten years. We saw to it that our people voted the right way.

There were five members of the committee. The naive person votes for all five. But the thing to do if you want somebody to get on is vote for him and don't vote for the others. Simple things.

Furthermore, the Committee on Committees were then elected by voice vote at a faculty meeting. We went to a meeting and somebody nominated the people. That was a highly political, psychological thing. The nominators were well-known and very influential. That helped.

Ziebarth: How were your relations with the Academic Senate over the years?

O'Brien: I thought then and I think strongly now that universities are run for the benefit of the faculty, and to hell with the state budget, practically. It's time more management was applied. The inmates shouldn't vote on their own salaries.

Reflections on President Robert Gordon Sproul

Ziebarth: What's your sense of Sproul as a leader of the university?

O'Brien: He was the greatest we ever had. He was incredible. In the first place, he was so interested in people. He not only could remember their names, but he liked them. He just liked to talk with people.

I visited the University of Illinois just after he had been there. He had been elected president, and he made a tour around various institutions. They had a dinner for him, attended by about forty people. He met them all beforehand. They had a dinner. When they left, he named every one of those forty people he'd never seen before.

Not being an academic, he had real respect for learning, for people that really were hard at learning. He wasn't impressed with academic politicians, but people who really had standing he could spot and he did.

Loyalty Oath Controversy

O'Brien: Sproul was hurt by the loyalty oath controversy. In particular, he was misled by two alumni whom I knew: George Tenney, who was chairman of the Engineering Advisory Council, and a good friend of his whose name I cannot remember: we'll call him Mr. X.

Jim Corley was the representative of the university at Sacramento. This was the McCarthy Era of hunting communists. Jim Corley became convinced that the criticism of the university would be slackened if the faculty would just sign a loyalty oath.

He talked to Sproul about it. I think Sproul was pretty hesitant; he didn't like it. Two of the people pushing the oath, friends of Corley, alumni prominent in business, were Tenney and Mr. X. I happened to have lunch at the Bohemian Club when those two men met with Sproul there to push him. After that, he agreed and asked the Regents to require a loyalty oath.

The whole history of that affair is a disgrace beyond disgrace to the faculty.

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O'Brien: When the president came to the Regents with the proposal of a loyalty oath, John Francis Neylan said, "Who would believe it anyway? The ones who are communists will sign right up and have absolution and be in the clear. The ones who won't sign are the conscientious ones that you aren't worried about. " That was John Francis Neylan, the regent who was blamed by the faculty for the whole trouble.

Ziebarth: When were you first aware that something was brewing?

O'Brien: I saw those two men. I didn't know what they were up to that day, and I learned what it was shortly afterward. It was Tenney who told me.

After the Regents adopted the oath, which was drafted by an attorney, they said "Sign this." Then all hell broke out.

I told you what John Francis Neylan said. The faculty had a meeting, and Hildebrand was elected as a representative to talk with the Regents. Instead of going to them and saying the faculty really object to this, he said, "Well, it's just a matter of the wording. If you just change the wording a little bit, everything will be all right." Hildebrand had no authority to act for the faculty. He was just to go there and listen and negotiate, but he said that.

Afterward, I read the minutes of the Regents committee and the Regents meeting on this subject so I know how it happened. When Hildebrand said to the Regents that a little change in wording would make it all right, Francis Neylan said, "Well, if the faculty want this, I'm not going to object, but I think it's a crazy idea. "

Furthermore, that same man [Neylan] had written a defense of a communist woman in early years. It was one of the finest statements on human rights and citizens rights that's ever been written. He really meant it.

So, after Hildebrand's bad advice, all hell broke out. The faculties had a meeting and said, "You didn't have any authority to negotiate for us. What the hell." Then they appointed a faculty committee, Malcolm Davidson, chairman, and three others. They met with the Regents committee, and they said—and this I learned later from Mr. Neylan by reading the minutes which nobody knows I ever read— "You know, the faculty doesn't really object to the oath. They object to the Regents policy of not employing communists."

The Regents had had that policy for ten years, and it had been announced to the faculty and there was no question about it. When the faculty committee said it isn't the oath that's causing the trouble, it's because the faculty object to your policy of not employing communists, the Regents really got mad. That's when John Francis Neylan took off. "Do you mean that you're telling us now that the policy we've had for ten years is a subject, and it isn't the oath, that all this commotion about the oath is phony?"

That committee, those four people, never told the faculty they said that. You can imagine. They let the faculty go on thinking that the Regents were insisting on the oath, whereas it was Hildebrand and his crew. The oath would have been dead as a doornail if Hildebrand said, "The faculty really object. Neylan would have carried the day and said, "The hell with it. Get rid of this foolish idea."

But they told the Regents committee that it was really the policy that was the problem and then didn't tell the faculty. That's the most dishonest thing, the disgusting thing. The man [George Stewart] who wrote *The Year of the Oath*, a deliberately distorted, dishonest, lying statement in my opinion, was offered the minutes of the Regents committee of what had been said. He refused to read it. He never looked at anything on the other side.

All of this commotion was going on, and I thought they were all crazy. But then Dick Folsom, who was chairman of mechanical by that time, and several others came to me and said, "Boy, this is getting to be pretty serious now. Mike, you've got to take a hand. You've got to get into it. Do something."

About that time, there was a meeting of chairmen and deans. I heard these guys saying, "We're going to strike." I said, "You'll just ruin the university if you do that and lose the support of the Regents. And how about the legislature and the farmers of California?"

The upshot of that was that they formed a committee of five and put me on it. We had space over in the Durant Hotel, several rooms there as headquarters. I was astonished; it met for two years. I'd been on leave at Air Reduction Company about the time this thing started.

Anyway, one day Olaf Lundberg, the treasurer or controller of the university, called me and said, "You know there's really a misunderstanding here that ought to be straightened out." I said, "What can I do about it?" He said, "If Mr. Neylan gave you the minutes of the Regents committee, would you read them?" I said, "Of course, I would. Furthermore, I'll talk with Mr. Neylan if he wants to talk. I don't care."

That happened. I talked to Neylan on the phone, and he said, "What I hear talked about indicates that the faculty are up in arms about a subject that isn't the subject that we've been told is the subject of discussion." So I read those minutes, and that was a shock.

Finally, we had a big meeting in Wheeler Hall with about twelve hundred people present, and the vote was eleven hundred and something in support of the Regents' policy of not employing communists and seventy-eight against. By that time I was vice chairman of the senate—the chairman is president ex-officio and the vice chairman is elected— so I had to preside. I also made a little speech in which I said that all this misunderstanding had occurred and it was about time we straightened it out. I was pretty shaky about it because I didn't understand faculty viewpoints or why they got so excited about this stuff.

The *New York Times* and other papers had had headlines when any assistant professor at Berkeley made any headline remarks, but when eleven hundred plus faculty voted for the Regents' policy and 78 people showed up and voted against, do you know where that news appeared in the *New York Times*? I think it was on page five or six and about that long [indicates two inches].

That was the end of that. Really the whole thing died and the Korean War came along.

Engineering Faculty Involvement

Ziebarth: How did the engineering faculty relate to the controversy?

O'Brien: They didn't pay any attention to it; they thought it was crazy— most of them. There were a few that got excited. About eighty-five of them wouldn't sign the oath. The system was set up so that the chairman and vice chairman of the senate, the dean of the college, and the chairman of the department would be the committee to decide on each individual case. So I was stuck with all eighty-five. Alva R. Davis was dean of Letters and Science, and then each department chairman. So we had hearings, hearings, hearings.

That's where Mr. X comes in. Among other businesses, Mr. X owned an industrial security business that had been doing work for the federal government, running down biographies and checking on people. Somehow it got to be known that this review committee existed; everything about this was in the paper every day. He learned that I was going to be on the committee that reviewed all of the eighty-five nonsigners who were nonfaculty; they were underprivileged in tenure arrangements.

He called me. "Mike," he said, "you know I own this security agency. Would you be interested in having my people run down the 85 people on your list?" I said, "Milton, only if you'll never tell anybody. "

It turned out that of the eighty-five, there was only one guy that they were even really suspicious of, that had ever made a speech or done anything. When we had the hearing about this man, he more or less taunted us. He had a job somewhere else; he didn't care whether we fired him or not, and he was going to make history.

There were members of our faculty who came to me and said, "I don't care about the oath. But I've talked about it to the point where my graduate students are practically all determined not to sign it. What should I do?" I said, "Why don't you tell them what you think?" "Oh," they said, "We couldn't do that."

I spent half of Christmas Day with Alva Davis and a third person reviewing the arrangements for the faculty who wouldn't sign, who were awarded all their expenses and pay by court order. There was one man who had taught in Italy, made three trips there. The question was should we pay for three trips? We did our job, and the faculty came out pretty well on it.

I was so disgusted at these guys who said, "I didn't really care much about it, but I thought I ought to say no." Then, when they wouldn't tell their graduate students who were now nonsigners—I didn't acquire a great respect for them

As for Davis and his committee, I thought they were scoundrels.

Ziebarth: Wasn't the engineering faculty in Southern California a more militant rump group?

O'Brien: I didn't pay much attention to them. I didn't pay much to the whole thing. I was busy. Then, I got into it so deeply I was meeting with the Regents and everybody else. We had enough trouble up here. They didn't have much of any faculty down there then.

Long-term Effects of Oath Controversy

Ziebarth: What were the long-term effects of the controversy?

O'Brien: It was a disaster. Until that time, the university was supported by the legislature to the extent that practically anything the Regents or President Sproul proposed went through the legislature unchallenged. In the first place what was proposed was usually pretty sensible . What the university wanted was something that was really helpful. But, two, the College of Agriculture at Davis had done such a tremendous job in support of agriculture in California and everything else. The majority of the legislators were farmers from farm areas.

When Jim Corley heard that someone thought we were spending too much money, he'd talk to them, and the legislators would say, "Well, we didn't understand that." Then they voted for it. There was just tremendous support. That's why the university became such a fine institution.

The oath destroyed all this. From that point on, we began to have competition with proponents of UCLA. Then Clark Kerr came along with the idea of campuses on every crossroads. Each one of them acquired a local constituency, a local group in the Senate and the Assembly. So this collaboration or support for the university on the part of the legislature and the unanimity of the legislature that the University of California was really the place to support disappeared.

Ziebarth: What effect did the controversy have on the Academic Senate?

O'Brien: The faculty was reorganized, and I don't know what effect that had. I became vice chairman of chairmen of the statewide system, which didn't last very long, for two years, and then I was vice chairman of the Berkeley section of the Academic Senate. I came in at the time of the change of organization to the statewide system.

Hardly any engineering faculty were nonsigners. The faculty was genuinely in favor of the oath.

I don't believe a communist should be appointed to the faculty right now. I still think that. He's made a commitment to a certain line of thinking and is not open to discussion, not open to debate.

Ziebarth: How did the oath controversy affect Sproul and his image as leader of the university?

O'Brien: It was terrible. The whole atmosphere changed completely. He didn't have the authority anymore. There were Regents who were mavericks. He had strong support among the Regents, a majority, but there were some who were again' him.

[Interview 6: March 6, 1987] ##

Reorganization of Engineering Education

O'Brien: Before the end of the war we spent a lot of time talking about what the system should be and concluded that Berkeley engineering should have a small engineering lower division. It should be large enough to cause us to give instruction like what we expect others to give, but the main emphasis would be on graduate study. Then we'd have a large upper division, junior-senior students, and a large graduate program, perhaps as many graduates as seniors. This was the general idea.

In order to make that work and to have a system which met the requirements of industry and the state for engineers, we spent a lot of time thinking about how we related to the state colleges and the community colleges. I had in my office a man by the name of Bonham Campbell who spent all of his time on that work. I had a fight to get the money to keep him in my office working on this project. After UCLA was started, he and his successor worked with UCLA as well as here. We had common plans. We got down to the bare bones of what we thought a student should have entering engineering and then made our requirements as flexible as possible.

Secondly, we decided that there should be both a freshman entering examination and a junior achievement examination which would be taken both by our own lower-division students and the incoming junior students. In other words, for a student to stay in Berkeley, he had to take the junior examination just like the outsider students coming in.

Testing Requirements

Testing was useful because we could see from the tests that certain junior colleges, like Pasadena Junior College, were excellent, and others were not very good. Furthermore, we were upset about our own lower division and the inadequacy of preparation for an engineering course of study. There was a time when all of the students from engineering and physics were taking math courses taught by people from the engineering department. The mathematics teaching assistants were interested in number theory and topology and all sorts of nonsense. We were interested in analysis. Analysis is not a part of mathematics, it's a part of applied science, mathematical analysis. Pure mathematics is a branch of logic or philosophy.

If I were doing it, I'd put all applied mathematics in the engineering school. I mean, it's bread and butter to an engineer; it's low-level, routine stuff for a mathematician.

Just after I retired, the Academic Senate abolished the engineering examinations. That was a mistake. It was in this era of "everybody's equal", and you shouldn't discourage people.

If a student doesn't have a good understanding of algebra and enters engineering, he's in real trouble right off.

I had a personal experience with that through my son who went to the University of Colorado. At the end of two years he would have been dismissed if the chairman and dean weren't friends of mine. After his second year, he decided and I urged him to take Colorado's summer course in algebra. (Berkeley High School had said he ought to stay another year, but I said, "Oh, no." I was wrong.) We talked about the course, and I emphasized the idea. "Don't memorize any of this. You think it through and know what the principle is and then do it."

I went to Colorado the night before he took his final exam, and I went over the book with him, and he had a better knowledge of algebra than I ever had when I was at that stage. His grades went up from failure to B+.

The point was that when he went to an engineering class, he was no longer puzzling over what the math meant. He could see that right off.

Failed Plan for Practical Approach at State Colleges

O'Brien: In addition to the testing of freshman and juniors, we considered—at length—the question of what kind of education fitted the state colleges. One member of our advisory council was Herb Wheaton, who was a professor at Fresno State. We talked with him a great deal and sent Bonham Campbell around visiting the other state and junior colleges. The idea was that within the engineering field, the requirements for engineers who actually design and develop equipment, buildings, nuclear bombs, etc. , in terms of mathematics, physics, chemistry, and so on are more stringent than for, say, U.S. Steel Corporation. There's a huge number of people who are well-qualified for technical work with a much lesser technical background than the men who will, say, work in the laboratories and the design offices of the jet-engine business that I've been associated with for so long.

We had examples of what we proposed. In Germany, instructors were almost all practicing engineers who were part-time instructors and actual designers and testers of equipment. Secondly, when they took up a design problem, say, heat transfer or combustion, they would study what would be pertinent to this design problem. Their whole effort was kind of focused on design. In other words, it was an industry-oriented program, but every bit equal to the undergraduate program common in the United States accredited by the agencies. This was in the fifties and sixties.

So we pushed the idea that it was absurd to cut out the men who would be excellent contractors, engineers, technical people in the sales, marketing, construction industries. Only a few schools should try to hit for advanced graduate work.

We had spent a lot of time talking to people about the need for this more practical approach. For example, San Luis Obispo at that time was managed by a man who had been in agricultural extension at the university. He instituted what we called the "upside down" program in agriculture. First students started feeding the hogs, and when they were seniors, they learned about the economics. At any point you could stop, and the student had useful knowledge. As a matter of fact, they earned their keep by owning some sheep and pigs.

This man was a friend of Boelter, and Boelter and I went down there several times to see what they were doing. We went with Dean Potter from Purdue, who had been president of the American Society of Mechanical Engineers and president of the American Society for Engineering Education. He was tremendously impressed with this practical approach.

The senior students there in electrical engineering were taking on the problem of designing a repeater station on the mountaintop to pick up signals from TV in Los Angeles and bring them down to San Luis Obispo. That's a practical problem—they were not only going to design it, they were going to build it and put it in service.

One time we took the whole engineering advisory council down there to see what was going on. We had an impressive presentation for them.

The career opportunities are many times as great for technical people. There's no point to their spending many years in graduate study because what they need to do is get a bachelor's degree and go out. Berkeley should have a B.S. degree that was much more severe. We could step up our entrance examinations to get students who were really qualified for the advanced kind of program.

So, our plan for a dual approach to engineering education was going pretty well. We had quite a number of state- college people that we talked with and explained it to. Clark Kerr later told me that we had the best relationship with the state and junior colleges that they've ever had before or since. We had a definition of engineering education that was explainable, defensible, that made sense in terms of the needs of California.

Engineering graduates, you know, do everything. Even in the technical field, there's a tremendous range of activity. The men who are highly analytical, mathematical, theoretical should go on to graduate study. They should become professors, work in research laboratories, and work in high technology. But there's no point in subjecting all the engineering students to that kind of program.

Two-thirds or three-fourths of the students would be better off with a simple four-year program. In the first place, they aren't equal to the more demanding theoretical study. And they don't want it. Today we spend a lot of money pretending these students are doing research when they're not.

In our discussions about the best way to structure the more theoretical engineering education, we found out that we had to think in terms of a five-year program. You can't say you should take twenty units of chemistry, and then put them all in senior year. There's a sequence you have to have. We wouldn't want all the students entering engineering to study, say, complex variables. But the students in electrical engineering had better get into that because that's the way you analyze alternating currents.

Although we had talked about this for years and had written and circulated papers until we thought we had something that made some sense, two things happened to throw us off track. The first thing that happened was Don McLaughlin. As far as I'm concerned he did more damage to the university and the state of California than anybody ever involved.

He was on the Board of Regents. They had a meeting of the educational representatives to talk about engineering education. They didn't invite me or Boelter to it. The head of the state college system and McLaughlin of the university Regents and some legislators had a meeting on what to do. The results were that the University of California would use the term, "Bachelor of Science in Civil Engineering" and the state colleges would use the term, "Bachelor of Science in Civil Engineering *Technology*" to differentiate their programs. This would make it clear that they had different names and different courses of study.

Then McLaughlin said, "Well, what it is is what counts, not what you call it. You don't need to carry the technology term. "

The upshot of McLaughlin's stand at this meeting was that word spread through the system that O'Brien and Boelter had been misleading everyone. They thought that the University of California had shown up and said that there didn't need to be any differentiation between the University of California and the state colleges.

McLaughlin didn't know enough about it to really discuss it. He was a geologist; he should never have had anything to do with engineering.

Right away all the schools tried to become the same as the University of California at Berkeley. All along we had intended that the instructors of engineering in state colleges should have had industrial experience and students wouldn't get Ph.D.'s

The other unfortunate thing that happened to engineering education was that Clark Kerr wanted to balance the men and the girls on all the campuses— Riverside, Santa Cruz, Santa Barbara, everyplace— by having engineering, which had boys, and letters and sciences, which had girls, on every campus. The Regents soon learned that they didn't have enough money to support that many engineering schools and that there weren't any students who wanted to go to second-rate engineering schools like Santa Cruz. They all wanted to go to Los Angeles or most of them to Berkeley.

We believed that with the importance of science and applied science, it was appropriate for the University at Berkeley and Los Angeles to push into high- technology, advanced engineering, Ph.D. stuff. But we also were convinced that even a state as big as California couldn't justify more than two state schools plus Stanford and Cal Tech at that level.

Ziebarth: Did you get involved in these big issues because you were on the Committee for Coordination with the State Colleges, a Committee on Higher Education Problems, and a state Coordinating Committee for Engineering?

O'Brien: Those followed the decision that McLaughlin was involved in. After it was clear the state colleges were going to do what they wanted to do, and they were all going to try to be like Berkeley, Boelter and I lost interest in the whole damned thing.

Reports on Engineering Education

Ziebarth: Do you recall the 1955 McConnell Report which urged statewide coordination of engineering education?

O'Brien: Yes. We brought out McConnell and the man who was dean of architecture at MIT and then dean of social sciences there, John Burchard. He was an instructor when I was a student.

We also called in Philip LeCorbeiller, who had been head of the whole system of posts and telegraph in France and was known for certain theoretical work that he'd done. He became a professor at Harvard and was involved in the Harvard program of undergraduate studies or lower-division studies, which represented some change in emphasis.

He came out and spent a whole term out here. John Burchard was here for a term. Dean Potter came out for about a month [in 1952]. All this had to do with formulating what should the university do.

We asked him to look into the courses which engineering students should take and should elect, to have a good long list but not too many. One thing is that the first course in economics right on up to the Ph.D. is for people who are going to be economists. We wanted a broad view of economics that a student could take in one year and get some background in the subject.

Overall, in these years we gave a lot of attention to the matter of what kind of education engineering students should receive.

Comments on O'Brien's Contributions ##

Ziebarth: At the March 1987 campus symposium in your honor, Clark Kerr said that the work you did with junior colleges was a model for the master plan for higher education.

O'Brien: It didn't quite work out that way, but we tried to take advantage of what other schools did and match it with what we do and make everybody even. We felt that many junior colleges were better than our own lower divisions, as a matter of fact.

Ziebarth: President Kerr also mentioned that humanities and social sciences people were more resistant and reluctant to broadening their program than you were in engineering.

O'Brien: We brought in consultants to get acquainted with the social science departments and figure out what courses our students could take and also to help design a course—if the social scientists would handle it—for engineering students.

What these men said was that the sequence of courses in economics, political science, and so on is for people who are going to go on in those fields. Secondly, engineering students are accustomed to working hard, and they think these courses that they take over there are so easy you read the book and go to the exam. That's all you need to do.

So they came up with a program in social sciences for the engineering students and physics and chem if they wanted which covered this gamut of subjects: a general view of social sciences.

Here's a sore point. You don't get any credit for good teaching in the University of California. Nobody wanted to teach this course because it wasn't going to count in their promotion. They wanted to work with graduate students.

Ziebarth: At the symposium, Professor John Whinnery, who also served as dean of engineering, mentioned that you started a course on engineering as a profession.

O'Brien: We'd had that for a long time. In the early days, 1920s, the undergraduate student getting a B.S. wrote a thesis. That was kind of perfunctory and not very profound addition to a regular program of study.

Ziebarth: John Whinnery also mentioned that you used a systems engineering approach to education.

O'Brien: I was never convinced that any subject you mentioned could take exactly two terms or exactly one term. I had been in contact with the students at Rensselaer Polytechnic Institute, where they had a different schedule of things.

For example, if they thought a subject was going to take ten lectures, they might schedule it for half a term. In other words, they didn't say every course was a term long. They made it the length of the subject matter to be covered, and I couldn't see why we couldn't do that. But that turned out to be too hard.

In general, we were trying to link everything together.

Ziebarth: Clark Kerr said that when he became chancellor in 1952, he felt pressure from Senate committees and science departments to get the College of Engineering up to the level of the scientific departments.

O'Brien: I'd been hearing about that as long as I was here from the dean of the Graduate Division, Charles Lipman, and, before him, Armin Leuschner, dean of graduate studies. They had made remarks that engineering ought to get up-to-date and get into research.

Ziebarth: Kerr said that he pressured you intensely about this.

O'Brien: I'd say he looked a little skeptically at many of the things that I asked for at that time. I think he learned about engineering afterward.

When Bob Wiegel was appointed to the faculty, there was a party at Kerr's residence out in El Cerrito. Bob Wiegel, being an engineer, went on time and others came late. So he was there with Kerr, who was president by then. Kerr said, "What gives rise to all these stories I'm hearing about the quality of engineering being so high. What's going on in engineering?"

He was hearing from other schools that he was in touch with that engineering was going great guns.

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Evaluation of Tiered Education System

Ziebarth: 1956 was the year that the state master plan on education report was released for discussion.

O'Brien: Yes, that was the time when this technology versus nontechnology program in engineering came up. All these engineering schools were set up in part because Kerr had the idea that having engineers on the campus would balance the number of boys and girls. You see, many campuses became crowded with girls in letters and science. Kerr seemed to want to have engineering everywhere. He was thinking about the social welfare of students, and we were thinking about the education of engineers.

Having engineering at Santa Cruz was a mistake. Riverside abandoned engineering. In San Diego, UC is right there in the same town with San Diego State University, which has the same standards of promotion as we have.

Ziebarth: Did you think that the three-tiered system of the junior colleges, the state universities, and the UC campuses is an appropriate way of meeting the education needs of the state?

O'Brien: The standards of education are entirely inappropriate for our society. We must have people who are plumbers, who are carpenters, who are all sorts of things. There's no reason why those people shouldn't take courses in economics and other subjects, four years, if they can work and develop a vocation at the same time. We need far more than engineers. We need technicians who will run a shop that keeps TV equipment and other electronics equipment in order.

I once went to visit George Tenney on Cape Cod. The yacht club was having a party put on by the plumber. It was a wonderful party. The plumber had a boat larger than most of the club members. He drove a Cadillac car. He was in business. Plumbing is as respectable a business as being an engineer.

Another thing that I remember was being at a meeting down in Fort Lauderdale, Florida. The street along the hotel was lined with boat stores selling big boats, 45-footers. They had a waiting list for months to get a big boat.

We got to talking to the people selling them. "Who's buying all these boats?", we asked. Not many of them were professional engineers. They were grocers, pharmacists, and all sorts of people from the Middle

West mainly, who had a business back home and a home and a boat in Florida.

If you make an economic analysis of a college education, say a professional education, and add up all that would be gained and expenses, and compare it to another person of equal mental ability who started after high school, the latter guy would have more money. I believe strongly that the ones who didn't go on to college would be ahead financially. Furthermore, they might be better off socially.

I think that universities have sold the country and the people a gold brick. There's no magic to education. It doesn't assure success.

Ziebarth: Americans believe in education, though.

O'Brien: It's been sold by the universities and what they've set up as "standards". There's no reason why state colleges shouldn't devote at least twenty percent of the program to vocational training. When people leave there, they would be in a position to take a job, in fact they'd have part-time jobs during the program. They'd be on a cooperative basis. The country would be a lot better off, and the individual would be a lot better off.

A college education, a four-year education, should be set up in a practical way. There's no disgrace about learning to be a carpenter. Pretty soon a man with that kind of background will be the contractor, the owner of the business, head of the business that's doing carpentry. Furthermore, the income is pretty good.

I don't pay a bit of attention to these issues nowadays. When I retired they put me on the dean's council. For a time, I got notices of meetings, but I thought it was up to them. They were in the shop, and I would just be continually disapproving of everything that I set up that they'd tear down. So I'd be unhappy. I've been retired for 28 years now.

Reorganization of College, 1952

Ziebarth: What happened in 1952 to bring about the reorganization of the college of engineering? Didn't Chancellor Kerr believe you had an unorthodox accumulation of academic and administrative power because you were dean and chair of the department of engineering?

O'Brien: It was all settled by the time he came in. That was the original agreement with Sproul, the president, when I became dean. I said I think it ought to be one department. Those were the days when the president ran things. He said, "Yes, that sounds pretty good to me. " And he put it through without asking anybody. Zing. I was chairman of everything.

The departments are the units of the university that make up the budgets and so on. I wasn't going to fool around with five or six chairmen, persuading them to do what I wanted on a budget. We could hardly find out what they'd recommend to the president's office. They were all new. The war was over. The circumstances were favorable. The president said, "Yes. " He put it through the Regents and that was it.

Ziebarth: Didn't Kerr suggest that the chairmen of other departments viewed you as rapacious for space and buildings? Is that accurate?

O'Brien: I guess so.

Ziebarth: Did you knock heads with other department chairs?

O'Brien: Not particularly. I didn't pay any attention to them. I just went to the president, or to the chancellor, after there was a chancellor.

At that time, the legislature gave the university everything it asked for because it only asked for sensible things. Having that kind of backing of the legislature gave Corley and Sproul great power within the university.

Ziebarth: I have the impression from Engineering Advisory Board minutes of the early 1952 that you were increasingly frustrated as dean because it was getting more difficult for you to do your job.

O'Brien: Yes. But the oath controversy changed a lot of relationships. First place, it tore down Sproul in large measure. I guess the Regents began to represent the Santa Barbara campus and the Los Angeles campus and so on, not formally, but that's the way they acted. Things changed gradually as Kerr came in, but he and the Regents were at odds all the time. More approvals were required for things, more faculty participation, more review of this and that by committees that didn't quite know what it was all about anyway.

Ziebarth: In 1956 President Kerr suggested that the deanship and the chairmen should be rotated. He said it was a conflict of interest to be dean of the College of Engineering and chair of the department of engineering; you were one and the same.

O'Brien: I think he changed his mind. Then he'd make remarks about it, and I'd tell him it was necessary.

I think engineering would be better off as a separate school, not even part of the university. Engineering benefits little from the general atmosphere of this campus. For example, I concluded early that there's no use talking about collaboration in research with different departments.

Ziebarth: Did you feel Kerr wanted to make engineering like the rest of the university?

O'Brien: They finally broke it up. Now they have designated the dean of engineering as a budgetary dean. So, the budget and all that matter of promotions and appointments and money and everything else goes through the dean's office just as they did through my office. When I came in, Dean Derleth was dean but he didn't want to be bothered with the details of the budget of mechanical, electrical, civil engineering because they just taught undergraduate courses, and they were practicing engineers.

When I came in, I could see that if I was trying to develop a program of graduate study and research, I had to have a handle on the money all around. So that's why I made it that way. But then they broke it down.

Management of Academic Personnel

O'Brien: In general, academic work deserves and needs better management than it gets. People should be realistically and factually appraised.

Most people begin to slow down when they're fifty-five or so, and there are an awful lot of active people around here that have retired really. I can see a lot of satisfying jobs in other universities and small colleges for the man who doesn't quite make it here.

When I was dean I got other jobs for quite a number of people. There are some pretty successful consulting firms in which the principals are people I told, "You'll never make it here." They just weren't inclined to research, and they went out in the field. They were practical and did very well.

Ziebarth: Were you able to remove people?

O'Brien: I went through that several times. There was a man teaching naval architecture because Benjamin Ide Wheeler had said at the time of the Spanish War that "the waters that launched the Oregon, which was built in San Francisco, should be the scene of a great school of naval architecture." When I came here, it looked like there'd be a resurgence of ship building.

There was, later, during the war, but it was an entirely different basis.

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O'Brien: When they asked me what I thought about him, I told them I don't think he knows much about it. He can lay out a ship and put proper machinery in it, but as for knowing about ship resistance, and wave making resistance, and the things that people are talking about, he just doesn't have it.

I was only an assistant professor. He was an associate professor. But I was asked what I thought, and I told Baldwin Woods, I believe. On that basis the professor just resigned after some review.

Another man had a religious slant, and I always tried, of course, to pay no attention to it. But it seemed to affect what he was doing. The war came along. To make it short, many of these people that we'd just made with—they'd say, "Well, I agree with you."

To be a professor here and really do the job requires people who like what they're doing and do it well. Some people slow down, and we make mistakes. But there are all kinds of jobs at other universities. Some people didn't do very well there as they hadn't here. Others go along fine because of a different level of teaching. There are engineering schools that have a lot of people on their faculty who just teach undergraduate courses.

Attempt to Establish Graduate School of Engineering

Ziebarth: In the mid and late fifties, you tried to establish a graduate school of engineering?

O'Brien: Yes. The Graduate Division is just wasted money. We couldn't depend on their records. They don't do anything that's useful except record and make caustic remarks about standards, such as saying that one student whose average turned out to be 2.95 didn't have a 3.0 average. When I inquired, they said they had consulted the math department, and you just strike off the last decimal place. So instead of going from 2.95 to 3.00, they strike off the 5 and make it 2.9. That's the level of what they were doing.

We had a lot of naval officers here for graduate study before the war. We had a naval officer who took extra courses beyond what we would require for the master's degree and even what the Navy had told him to take. To my mind, if he took the courses that were required for a master's degree, he had it, but the Graduate Division said the whole record had to be considered.

Several years after that man had not been given the degree, he was on the train with President Sproul and told Sproul his story. When Sproul got back here, he called me and said, "Mike, what the hell kind of a college are you running, anyway? Making a point of such things." I said, "That's the Graduate Division. I've been fighting with them for ten years since that man graduated to get that recognized." You know, he never got it either. I think that's a disgrace.

I wanted to get the Graduate Division out of the circuit completely and a school isn't under the Graduate Division. That's the only point. Even then we had to keep our own records of the graduate students. They made mistakes and weren't up to date.

What really burned me up was that they would call some of our Ph.D. students over to talk about their thesis—on a subject the division didn't know anything about. They were not engineers. That was annoying, but our candidates going over there thought it was kind of fun—an idiot asking questions in a field he didn't know anything about. (chuckles)

Ziebarth: Wasn't it also a way to get control and shake it up?

O'Brien: I don't think it made a great deal of difference. It just simplified things. We had to keep our own records and then supply the stuff for the division and then be continually hampered by the fact it made mistakes. So, at the end of my service, we had practically no connection with the division except to submit the formal records.

Off-campus Graduate Study and President Clark Kerr

Ziebarth: In 1958 or so, Clark Kerr wanted to start a program of off-campus graduate study in the Sunnyvale area. What was his motivation?

O'Brien: To support industry. In that area, there's Santa Clara University, San Jose State University, and Stanford. I didn't see why they couldn't put on the same courses we could with a lot less travel time for instructors and less commotion. I'm in favor of these postgraduate opportunities for people in practice. It's a good thing for them and for industry. But I don't feel it makes much sense to ask a man really effective in research to spend a day driving down to Palo Alto to teach an evening course.

So I wasn't against the idea. It was simply that I thought they were being unreasonable in thinking that Berkeley should do the job when they couldn't get Stanford to do it.

Ziebarth: Prior to 1956 hadn't you supported the idea of off-campus graduate study?

O'Brien: We backed away from that. They pushed us into putting on some courses down on the peninsula, as a matter of fact, right in Palo Alto. Stanford said, "No, that's not our business." And I said, "That's not my business either."

Ziebarth: Did you feel increasingly called upon to defend the engineering faculty from the university administration?

O'Brien: No. I didn't have much time under Kerr, and Seaborg was chancellor when I retired. During Clark Kerr's tenure, I had things that I wanted to get and needed to persuade him of. He was suspicious of me and of what I wanted—was that really good, he wondered.

Later, he changed his mind and told me that he had come to regard engineering at Berkeley as leading the parade nationally. We were way ahead of most of the good schools that he became familiar with. So he changed his mind after he left.

He wasn't anti-O'Brien; he's a friendly guy and reasonable. It's just that they couldn't see why engineering is different from other departments. Now, apparently, with the salary scale, they agree that engineering needs as much special attention as medicine or law or any of the other major professions.

Ziebarth: Was Kerr sympathetic to engineering?

O'Brien: As I say, he was kind of casual about it when in the practical working out of it, they put engineering on every campus. And I heard him say—maybe he just joked about it, but I think it was what they had in mind—and certainly other people in the administration said— that they ought to have engineering to balance the sex of the campus populations.

Ziebarth: What about Chancellor Glenn Seaborg?

O'Brien: He didn't do anything. He was chairman of the Atomic Energy Commission, and I remember when someone asked a congressman who was very active in the field about the commission, he said, "Seaborg will go along." Seaborg didn't know anything about it.

Decision to Leave University

Ziebarth: Why did you decide to leave the university in 1958?

O'Brien: I took a leave of absence for half the fall term and half of the spring term, which seemed to be the simplest way to continue my administrative duties before I retired. I never asked for or even thought of holding any administrative job. I never said, "Maybe I'll be a dean some day." President Sproul used to say, "A dean is a mouse in training to be a rat."

I came here with a bachelor's degree only, not intending to stay in academic work, not critical of but not greatly impressed with the academic viewpoint on many things. Over the years there were many meetings of the Land Grant College Association and the Society for the Promotion of Engineering Education. Many of them were attended by older people. I just concluded that people who became deans make noises about this and that. But when I'd try to talk to them about engineering problems, it was clear that they'd long since lost touch with what was really going on in the world. They weren't terrible, but half of the groups that met were fuddy-duds.

Ziebarth: Did you feel you just wanted to get away from the university?

O'Brien: Yes. I'd even resigned once. When I went to Air Reduction in 1947, I sent in a letter of resignation. Dr. Woods and George Tenney—Tenney had been out here on the West Coast for McGraw-Hill—said, "Why don't you just change this letter and ask for a leave of absence." I'm glad I did that because Air Reduction was too small an outfit to have anything called research. They only had development of products.

When I was dean, there were a lot of very active people, bright guys, around. We'd have a meeting and say, "Work on this, and do that, and make calculations" and so forth. Pretty soon I'd found that they'd always say, "We'll do it" because they didn't trust me to do it correctly.

Ziebarth: Did you feel you'd go back into research?

O'Brien: Sure. That's what I'm doing now. I just got fed up with paper shuffling. I wasn't going to deal with these academic committees any more. They were just causing delay, causing a whole lot of paper work. The aftermath of the oath was affecting the university by splitting it apart. I couldn't visualize being here as a professor after retiring, either. When I took this leave, I had a chance to think about what I wanted to do. I decided that I'd had enough.

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Ziebarth: Looking back, what did you like best about being dean?

O'Brien: What I enjoyed most was the fact that we were actively promoting research. We only undertook what the faculty wanted to work on. But in fair measure, I was one of the faculty. And that part was interesting: to see the growth of graduate study and the caliber of students who came here.

For example, the group in engineering did a lot of work on what was called the low-pressure project, which was a molecular wind tunnel, a very low-pressure system. I got interested in that simply because it was mentioned in a book, *Introduction to Theoretical Physics* by Page. Somehow I got the money to bring a professor here to help us get started. It turned out that this was important for all the space vehicles, nose cones, missiles.

It was fun to get started. That end of it I enjoyed; the rest of it was drudgery. I hated going to meetings and hated writing letters on end about this and that. That seemed pretty obvious. The problems of these campuses are tremendous. I'm glad I retired. Here it is 28 years later, and I still can't quite get done what I need to do every day.

Ziebarth: Did you ever regret leaving Berkeley? You had many productive years left.

O'Brien: No. I'd done all I could here.

Ziebarth: Did you ever consider moving up to be a vice-chancellor?

O'Brien: No. (contemptuously) In the first place, I never sought or even thought about any advance position like being chairman or dean. But the thing I saw clearly was this. If I was dean of engineering, I was still mixed in with engineers doing engineering things and thinking about engineering work. Anyone who takes a job above that level is nuts. Then you get all mixed up with the campus free-speech movements, and Lord knows what.

So I never had any ambitions, and I felt that sixteen years as dean was enough. And I was much interested in GE. GE was not in the doldrums but had some critical things going on. Then, of course, I was still interested in shorelines and beaches. During the years I was dean, I had to back away from that.

Marriage to Mary Wallner, 1963, and Lifestyle

Ziebarth: When you retired, did you intend to live in Berkeley?

O'Brien: No. I was here only for a short time after I retired. Then I was divorced in 1960 or so [1961].

Ziebarth: Did you keep a permanent address in Berkeley?

O'Brien: No. I stayed at the Golden Bear Motel. For twenty-five or thirty years off and on I've stayed there. That was the simplest thing. Halfway between the campus and the field station in Richmond. Furthermore, after a while, they'd throw people out to make a room for me if necessary.

I tried an apartment in Florida, when I was in Gainesville, but there's just too much responsibility. If I stayed in a motel, they care for my laundry, do the bed, and so on.

Ziebarth: Your wife must have been understanding in terms of your busy travel schedule.

O'Brien: She travels too. We belonged to a country club on the Hudson, the Sleepy Hollow Country Club, and we'd go there and spend a month in the summer. That was the time when I was going to Syracuse, Utica, Binghamton, Pittsfield, Schenectady, all GE locations where I was a consultant.

So I was shuttling in and out of New York, and we could be at the Sleepy Hollow Country Club. Her very great friend, Helen Hayes [actress], lived just across the road over in Nyack. They have been friends for forty years.

My present wife Mary had lived in Mexico for her arthritis some forty-five years. After we were married [in 1963], that settled where I would go after retirement.

Jim LaPierre introduced me to her. He was at GE and so was I. He and his wife had lived next door to Mary. When Mary was divorced, they said, "Come on up. We have a big apartment in New York and know a man we'd like to have you meet." So it went.

That's how I happened to go to Mexico, which turned out pretty well. I personally would choose the Bay Area of all the places to work, because it is more stimulating than elsewhere. I've been all around.

Ziebarth: Does Mary travel with you?

O'Brien: Not very often. She comes to the States three or four times a year. We go to New Ulm, Minnesota, to visit her brothers once a year. She came from a family of six brothers and one girl.

IX. CONSULTING ENGINEER, 1947-1988

[Interview 8: May 28, 1987] ##

First Association with General Electric Company

Ziebarth: Did you become a consulting engineer while you were still employed by the university?

O'Brien: My long association with the General Electric Company began by chance. Following the war, we had a lot of trouble here at Berkeley getting facilities, and I was discouraged. I was offered a job at Air Reduction Company, which makes industrial gases. I decided to resign in 1947 and go there, but Baldwin Woods persuaded me to take a leave rather than resigning.

When I took the job with Air Reduction, I got out the encyclopedia and began reading about industrial gases. My son Mike and my daughter Sheila asked, "Daddy, what are you doing?" I said, "I took this job; I'd better learn something about it." I knew nothing about industrial gases.

I was there for a full year and part of adjacent years. During that period, I met C.W. LaPierre. Jim LaPierre had been at American Machine and Foundry, where he developed the bowling-pin spotter. Earlier he had been at General Electric's General Engineering Laboratory and had done some excellent work as a developer of instruments. He was an engineer from the University of Missouri. The vice president of engineering for the General Electric Company picked him to come back to General Electric and take over the aircraft engine business, which was new to the company.

The Englishman Whittle had developed the jet engine, but the British government asked the U.S. to take over its production and, in particular, asked for General Electric. LaPierre had no experience whatever with aircraft engines or with power equipment of any kind. But he was noted as a developer of new ideas, new products. He was seen as quite a gambler, but he went through an extensive process, deciding whether to bet on it or not, the conditions of the market, the physical problem, the whole.

Some months after LaPierre returned to GE, he called me in Arizona, where I was visiting my family, and asked if I would join him as a consultant. I had known him in New York, where I had been as a director of research and engineering for the Air Reduction Company on leave from the university. LaPierre, although he was not an employee of GE, had been working with General Electric on their nuclear power stations for the nuclear power unit for the Seawolf submarine. He wanted to borrow two Berkeley professors, Martinelli and Johnson, to work with GE on this Seawolf project.

LaPierre said he was going back to General Electric Company to take over their aircraft-engine business. I said, "Jim, I don't know anything about aircraft engines." He said, "Neither do I. So come on. Let's have fun."

I got out the encyclopedia again and started reading about engines. The kids said, "Daddy, not again." Things have been pretty much by chance in my life, and that was the fun of it.

LaPierre's invitation was the beginning of my connection with GE. I learned years later just why this happened. The people in Schenectady who worked on land-based gas turbines concluded that the axial flow engine would be a better, more efficient design than Whittle's centrifugal compressor engine. They had designed an engine called the TG190, which followed the method of design outlined by myself and Folsom in a paper called "The Design of Propeller Pumps and Fans. "

It was our paper which was the basis for the design of the TG190, which became the J47 jet engine, and I was actually taken on as a specialist with compressors. But so far as I knew, I was just a general consultant.

I've always been a consultant with GE on an annual retainer. In all those years I haven't written ten reports, but I've been an advisor to the head man on all sorts of things: technical, personnel, finance, organization. Whatever bothered the head man and he wanted advice on, I tried to help him with. In general, my assignment was to be familiar with the projects they were working on, to be acquainted with the people, and to be prepared to help the general manager with advice when he needed it.

Consulting Projects for General Electric

Engine Design Competition

Ziebarth: In 1952 LaPierre requested that you review the new engine program at GE and make recommendations for the future. GE was experimenting with four advanced engine projects.

O'Brien: They had a design competition. LaPierre decided that General Electric had to come up with something novel in order to break into the business. People such as Mr. Nyrop, who ran Northwest Airlines, said that when he wanted light bulbs, he bought 'em from GE, but when he wanted jet engines, he bought them from Pratt & Whitney. That was kind of an attitude in the industry that General Electric had to break through.

Variable Stator Engine:

Ziebarth: Was the design dispute regarding a variable stator versus a dual rotor engine?

O'Brien: Yes. That was the original novelty that LaPierre adopted back in 1950 or '51. One of the problems related to the fact that as you put pressure on air, it shrinks in volume. If you run a compressor at slow speed and it takes a volume in and it's at slow speed so it isn't being compressed much, the volume that comes in here is too much for the much smaller stages back there in the engine.

As you go to higher and higher pressure ratios—in other words, as the pressure to which you press the gas goes up—these stages at the back end get to be pretty small. The air won't go through. The engine stalls.

Pratt & Whitney and Rolls Royce came up with two different rotors at different speeds that relieved the pressure. The aft end rotated at higher speed and that let more air go through. But GE came up with the idea that the air could be guided into the compressor by guide vanes. The air goes this way, the rotor turns it this way, and so on. GE came up with the idea, why don't we make these variable instead of fixed? So, in the original stages, when we're taking too much air we close some down. And as the engine comes up to speed and the pressure increases, we open 'em up.

They came up with a design which was decidedly novel. The question was: Was it physically possible? Then, was it enough to make a difference. It sounded good, but was it really workable? So there was a lot of discussion of the theory and some experimentation. I spent quite a bit of time talking with people about it and helping LaPierre decide to go ahead with it or not, which he did.

It was very successful. It solved the major problem of jet engines getting good efficiency. Practically all the engines today use variable stators as part of the system. Other people invented it and worked it up, but LaPierre was the one who backed it, and the process was very, very thorough.

There was a great debate even at GE about whether to go to the variable stator or not. There were people of great authority in GE who said, "It won't work. It's theoretically impossible. "

At that point, a man by the name of Cochran was assigned to me to help. We went off interviewing everybody and his brother about all this. And it turned out that while one man was running an experimental compressor and proving that it would work, another guy was telling us that theoretically it was impossible, it couldn't.

Ziebarth: This led up to the J79 engine?

O'Brien: That's right. The J79 engine was the one that first had a variable stator.

Ziebarth: Was that a significant advance and one which was profitable for GE?

O'Brien: Oh! Was it profitable. The J79 engine was built by the tens of thousands. For commercial and military both. The J79 engine is being built by the Israelis right now.

The implementation of the design was by Gerhard Neumann, who wrote the book *Herman the German.* He built the engine, and in the course of doing that, he demonstrated a lot of energy and savvy and common sense. LaPierre came to look upon him as a person who would be his successor, I later learned, because of his work on this project.

O'Brien's Role:

Ziebarth: In *Herman the German*, Neumann says that many advanced programs would not have happened without your help.

O'Brien: A little exaggeration, I think. I had the advantage of not being an employee, not having any level. I could talk to anybody. Way down at the bottom of the scale of authority or at the top. So I did that. Finally it came to be understood that they could talk to me, and I wasn't going to go say, "Well so and so told me so and so." I'd listen, and if I thought it made sense, then when I saw the appropriate manager above that point, I'd simply say, "I've been listening around, and I think you ought to look into so and so."

That worked because I've been a consultant since 1949. I just plain never turned any particular person in. But I've probably been closer to GE than any other non-GE employee. I had access to everything I needed, information of any kind. I was an advisor to anybody that wanted advice up and down the line. Down the line, someone would say, "A certain engine's going to be reviewed. How about it, Mike? Will you be there?" So I'd be there. When it was over, I'd give them my comments on what I thought ought to be done.

That was the engine business. Then Mr. LaPierre, who was head of the engine business, moved up, and he was the vice president in charge of the aerospace and defense group, which included the engine business, but which also included radar and sonar and flight control and the aircraft nuclear propulsion project and the Apollo support department.

Aircraft Nuclear Propulsion Program

Ziebarth: What was your role at GE with developing nuclear engines?

O'Brien: We actually built and ran aircraft-type gas-turbine engines on nuclear power. We had a nuclear reactor that generated hot gas and sent that gas through the turbines and developed power.

Ziebarth: Was that followed up?

O'Brien: In view of the present attitude toward nuclear power, it's obvious that it was a crazy idea. But in the early 1950s, the Air Force was interested in airplanes that had infinite range, could be put in the air and stay forever if the crew could stand it.

Ziebarth: Were you skeptical?

O'Brien: It was kind of an intriguing idea. They did build an engine and reactor out at Idaho and demonstrated that it worked all right. I was skeptical about being able to build a reactor that would be able to stand the strain, however. They never did give it a life test, so maybe I was right after all. A life test means run it for hours. Nowadays, the life of an engine if you keep replacing the parts is almost unlimited. Aircraft engines run for 10,000 to 20,000 hours, and standing up for that long a period is the problem.

Intercontinental Ballistic Missiles

O'Brien: I helped write proposals for what was called the intercontinental ballistic missile. You fired a missile; it went into the stratosphere and it came down somewhere intercontinental. All of these projects start out with a company making a proposal to do something. Sometimes the government says, "We invite proposals on the design and building of this or that."

In this case of the intercontinental ballistic missile, one of the big problems in addition to firing the thing was bringing it down through the atmosphere. It was up in space where the air was so rarified there was practically nothing there, and it came down at very high velocity through the atmosphere. It would burn up because friction got to be so great.

The design of what they called the re-entry vehicle was a big job. One of the things I helped them with was to write the proposal which got them a job. General Electric hadn't had much to do with the space program at that point. A man by the name of Si [Simon] Ramo was a consultant to the Western Defense Command. Ramo-Woolridge was a consulting firm of fine reputation, and the Air Force hired them as a firm to help with the design of the intercontinental ballistic missile.

When the Air Force hired Ramo-Woolridge, they didn't want them working on anything else. They practically cut off their communication with anybody else. Jim LaPierre at General Electric wanted to know if there was any point in GE proposing anything. They'd been kind of left out before. Jim asked me to ferret out Cy Ramo and ask him if they wanted GE in.

I finally got in touch with Ramo and had a conversation with him for an hour or so at the Western Defense Headquarters. He said not only did they want GE, but GE was essential to getting the job done. Get back and tell LaPierre to get going. I did a lot of things like that.

Ziebarth: What year was that?

O'Brien: In 1949 or 1950, the days of the big push on ICBMs. General Bernard A. Schriever was the man in charge.

The command of the Air Force had become concerned that projects became overstaffed with too many people and that it was costing too much. They investigated Convair out at San Diego— with a team of about three hundred people. They asked questions to the point that they were slowing up the main job.

Then they put together a report on efficiency. They had a review, and the Air Force kept pressing General Schriever that he ought to review his command like that.

Schriever was pretty busy getting the job done, and he wasn't much interested in having time wasted on reviews. So LaPierre sold Schriever the idea that I was a neutral. I was not GE; I was neutral, in-between. If I took a finance man and a manufacturing man and had a look, maybe I could help him some.

The three of us prowled around for a few months. By that time GE had the re-entry job. I remember a meeting with General Schriever and his two principal lieutenants one day. Usually a review like that they expect a whole lot of people in a fairly large room. There were only six of us.

I got up and I told General Schriever, "We've gone all over this, and we're sure there are more people involved than are necessary. But everybody has their jobs assigned, and you're in a hurry. You can save people, but if you do, you're going to have to change the assignment of jobs and combine jobs. If you do that, God knows what's going to be forgotten in the process. We think you better just spend the money and go ahead and don't change anything." He said, "Thank God for that." He bought the idea and away we went. That would have been about 1949 or 1950, the peak of the ICBM.

Problems with Military Services

At a later date, General Electric had all these different divisions and departments working on jobs for the Navy, the Air Force, the Marines. Things were moving along pretty fast and pretty free and easy.

There was a point at which the Air Force was annoyed at General Electric for various reasons. In general, few of the complaints were technical. They had to do with schedules and money and things like that.

Once Jim LaPierre took me up to Wright Field at Dayton, Ohio, to see General Anderson, who was the lieutenant general in charge of procurement. He had an assistant by the name of Bozo McKee, a major general.

General Anderson had a list of the projects that were bothering the Air Force. Of the ten projects bothering them, six of them were GE. Again they said, "O'Brien is kind of impartial, halfway between GE and outside. How about having him have a look at all these projects that are bothering you?"

So I spent the next year of this consulting time—not full-time, of course—investigating these items, project by project. What was wrong? For example, one of the units was developing an airborne flight-control system for the Marines, a system which they could fly to the forward area and set up and control their flights from there with radar. The GE people hadn't kept in touch with the Marine Corps requirements or with the people who wrote the requirements.

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The flight-control systems ran over in size and weight. The General Electric design met the performance specifications as to what it would control, but the physical equipment was too big and too heavy to fit the Marine Corps airplane that was to carry it. The thing that burned the Marine Corps up was that if GE had said we're running over in weight, they would have said, "Make it two pieces and we'll send it in two airplanes. " It ended up in one piece, too heavy for any airplane they had. They were mad, and the thing had to be repackaged. That was possible, but it delayed things.

In other cases, they claimed we'd changed too much. I remember one incident. The Navy said once they scheduled a ship to go to drydock to get a sonar device put in, all sorts of things happen. They change the crew. They provision the ship. They change everything else. So by the time the ship goes to drydock for sonar, sonar is just a trivial part of all the things they're going to do to that ship when it's in drydock.

The point was: If GE's equipment sat on a dock for six months waiting for the ship to come in, it didn't hurt anybody except that the interest on that money was lost. But if that equipment was one day late arriving at the dock, there was hell to pay. This was at the time that we had what was called the DEW line across the North Atlantic and Canada.

Across the Atlantic we had destroyers carrying radar to watch the skies. That command was under the Air Force out of Colorado Springs. If a DEW line ship went into drydock to get anything like a sonar and it was late, not only did the Navy channels hear about it, but it went all the way to an Air Force general out at Colorado Springs. They'd say, "What the hell's wrong with the Navy. Can't you get things scheduled properly?"

Anyway, the Navy gave me a thorough lesson in what was involved. I went back to GE's Heavy Military Electronics group, which was the source of much of this stuff for the Navy, and gave them a lecture. I went through it with several people. Finally, I went all the way down the line to the guy that was responsible for delivering the equipment. When I got all through, he said, "Hell, we deliver it and they let it sit there for months." That guy was off the payroll that afternoon. No sense fooling with a guy like that.

O'Brien's Problem-Solving Approach:

O'Brien: Usually, I reviewed technical designs for GE. Not that I know anything fundamentally about sonar or radar or flight control or computers, but because there are certain general things that happen if you are developing something new. If you're going to do this, you've got to have this, this, this, and this. And this is key. It's the tough one. It's the one where if you don't have it, there's no way around it. Anybody can see you've got to have it or you're out of luck.

So I would listen to a discussion of a program. Then I'd say, "Okay, tell me what's your worst problem. Okay, what are you doing about it? What's your chance of success? How about having dinner tonight with the guys who are really in on it?" You bore in, and you finally find out that the key element is this. You'd better pour all the power you've got on that and get it settled. There's no use spending a lot of time on a lot of other things because if this doesn't go, you're out of luck.

Regardless of what the subject was, there was a standard set of questions. I began to learn enough about electronics and things like that to ask the right questions.

On the Apollo support contract—I watched them launch it— GE had to set up a completely new department to do the Apollo support, and, of course, one of the problems was personnel. Who were the key guys we had to put together to make that team to do the job? I talked to quite a number of guys that were thought of as possibilities. It was a key job to be done in quite a hurry.

I did a job with the Navy like the one for the Air Force and spent almost a year looking into the interface between the Bureau of Ships of the Navy and the General Electric Company. The Navy wanted GE equipment, and it was clear that the Navy engineers and the GE engineers were close together.

One thing I found out (chuckles) was that the two groups would meet, would agree on a course of action without much regard to the costs involved. In many cases, the GE people said, "We can make that change without any cost." But they didn't realize that a lot of things that they didn't really have much to do with were also going to be changed by the changes they made. I found that the Navy was determined to have GE equipment, but GE had better be more accurate on its estimate of costs.

The experience taught me that you better not let the engineers settle on the cost of a change. You better have a finance man or some others involved before it was over.

Ziebarth: What kind of change would have those cost ramifications?

O'Brien: Just a simple thing. If you changed the voltage on a system of any kind, you're usually changed the cost. If you change the size of the transformer, you change the cost. But to the engineers, it was practically the same thing.

Planning for Testing Facilities, 1955

Ziebarth: In 1955 you were involved in facilities planning for GE.

O'Brien: GE was building very expensive testing facilities at Evandale, in Cincinnati, Ohio. The Navy had one engine—a medium-sized engine—requiring 14,000 hours of testing at about $3,000 an hour, or $40 million for testing. This is the kind of business in which you try hard to do it right the first time. In the engine business, you test it on the ground or you test it in the air under safe conditions before you ever let it get into an airplane for service.

So, they wanted these test facilities for compressors, combustors, and turbines. They were planning on big engines so it required big facilities. I knew the facilities were under design but hadn't been asked to do anything about them particularly.

One day, I was at Cincinnati airport, and the young man who was the go-between between General Electric at Cincinnati and the designers of the facilities in Boston, Jackson and Morland, was waiting with me. I said, "What are you doing?" He showed me a design, and it was for a great big building, one big building with compressors and combustors and all sorts of test equipment.

I said to the fellow, whose name was Harry Trescott, "Why, Harry, that's the craziest thing I ever saw. Are Jackson and Morland serious?" "Sure," he said, "this is almost final." "Well," I said, "this is crazy." I said, "Why, the compressors ought to be off by themselves in a separate building. They're as noisy as hell. And there's no use having the combustors in a building; they're going to burn things and you'd better have them outside in the open where you can get rid of the heat. You better have the test cells in a separate building, too, because they make noise. Then you'd better have a separate building right alongside but insulated from that for the offices."

Well, Trescott went to see LaPierre and said, "O'Brien thinks this whole thing is nuts, that we should do it differently." So they did it differently; they did it my way. Jim LaPierre said that I saved the company enough money that the interest on the saving would pay my consulting fee for the rest of my life.

Turbo-jet vs. Turbo-fan Engine

Ziebarth: In 1955 or so, the GE jet engine department asked your assistance about whether to begin producing turbo-jet or turbo-fan engines?

O'Brien: Yes. What a jet engine does is to take air in, compress it, heat it up, and squirt it out through a turbine. The energy that you added by heating provides the energy to drive the turbine which in turn drives the compressor.

The other thing is that if you take air in, heat it, and exhaust it, the kinetic exhaust energy of that stream is considerable. The thrust pushes the airplane, but to do so, you have to use a lot of energy.

When an airplane is standing still, no work is being performed; the object isn't moving, isn't doing anything. But you're using an awful lot of energy in the outgoing jet. It turns out that for commercial airliners, the simple turbo jet engine is exhausting the gases at too high a velocity; it's just wasting an awful lot of energy in that stream.

So the idea was, let's take a gulp of air, heat it, use part of the stream directly out of the engine, but put part of the gas through another set of turbines to drive bigger wheels to push more air. That was an idea that had come up in various connections. It was a way of developing a vertical takeoff airplane. But it also turned out that for a subsonic airplane—in other words a commercial airplane, as we know them now—doing that greatly saved fuel. It slowed the air down at discharge. It handled more air at lower velocity, and the upshot of the whole thing was you saved about 30 percent of the fuel.

There was a long discussion of that, and I'd been following the work of Pete Campus and others on the vertical airplane. So when the thought was, let's turn it sideways and have it push the commercial airplane, the technical facts were in favor of the turbo fan. Pratt & Whitney, the competitor, wrote a paper about how it was no good and it would never win. GE moved on the turbo-fan, and that led to the widebody jets. GE got practically all the business for a while with their turbo fan engines.

People on the outside have a tendency to ascribe these decisions to a person. But practically nothing is done as a person. There's a whole group of people that talk about it. You talk about it and you talk about it to the point where it becomes unclear who thought of it first. It gets suggested and people talk about it. And somebody builds a demonstration unit and adds more information. It just kind of evolves.

In all these matters, I was one of many. In many cases, I was one of those who spoke to the man where the buck stopped: the guy who had to make the decision to do it this way or do it that way. I'd be one of those he'd ask: "Well, what do you think?"

Relations between GE and Boeing ##

Ziebarth: In 1957 the Strategic Air Commanded decided that it would no longer carry high-speed air turbines in B-52s and that it would replace the present pneumatic alternator drives with Sundstrand hydraulic drives. Can you explain this event?

O'Brien: Boeing's B-52 plan has an auxiliary power plant to run the auxiliaries. That power plant was designed by the General Engineering Laboratory of GE, which did not have the authority to sell a product.

So the lab went to the aircraft gas turbine division and said, "If you will sponsor this, we will do the manufacture. You be responsible for it as the point of origin, so we can go ahead." So LaPierre was talked into sponsoring it on the basis that it meant nothing to him. It was already done, and it was just a formality of having a sponsor.

But it turned out that the unit had a fundamental flaw, in my opinion. It had to do with the way in which it was controlled. Normally you control by the output of a unit. You feed that output back in to correct the input to hold the output at what you want: this is feedback control. In this case the generator supplier was Westinghouse, and the GE design, rather than taking the output of the generator as a control element, took the output of the turbine driving the generator. This was really the wrong approach, but it avoided using a competitor's generator. The competitor's generator was going to be there in the unit anyway, but GE's control system didn't depend on the generator.

As it turned out, the unit didn't work well, and the Air Force decided to cancel GE and go to Sundstrand to buy their generator. But Sundstrand didn't do as well as GE, and the Air Force still had trouble.

When the Air Force wanted to go back to GE, Jim LaPierre told the manager, "Okay, but when you cancelled us, we lost several million dollars. If you want to come back to us, you can pay us back the several million lost dollars."

Boeing Aircraft Company, which made the B-52's, hit the roof: "It isn't fair." LaPierre said, "Fair or not, if you want us, you pay the several million dollars or go find somebody else." So Boeing Aircraft Company wrote the contract and paid the money back, but they were annoyed from then on.

Because the airplane was using J-47 engines, there was a lot of contact. There was so much tension that when there was a technical meeting, Boeing people would have lawyers present.

This situation just drifted along. Then, I was at a meeting with Welwood Beale of Boeing, and he invited me to come up for the Gold Cup races, which were to be at Seattle that year. Boeing made a big thing out of it, and all of their principal people had their boats out.

When I told Jack Parker, who had taken over the GE division, that I was going out to the Gold Cup races, he said, "Please talk to the Boeing people and see if I'm welcome out there. Find out where do we stand."

It turned out when I talked to George Shearer and Welwood Beale and others, they said, "Of course, Jack's welcome. Tell him to come any time." So things got better from then on.

Supersonic Transport Engines

Ziebarth: Did GE also produce engines for supersonic transports?

O'Brien: General Electric did develop an engine for a supersonic transport. They built many different engines. One is in the Smithsonian Museum; it's the largest jet engine ever built. The supersonic engine was a big project for GE for several years around the time I left the University of California.

A man by the name of Ed Hood was the project manager. I worked on the proposal. If the military put up the money to develop an engine, GE had to send in all the characteristics of that engine for appraisal by the Federal Aviation Agency (FAA?) and the military people assigned to it.

One of those proposals is the size of a truckload. It isn't just a book; it's a whole doggone set of books. It's such a voluminous set of books that no one man knows what's in all of them.

Ed Hood asked me to put together not a summary of the management scheme but a summary of the whole technology involved. It was to be read by busy managers who weren't going to read the several tons of reports.

This I did, taking weekends for quite a while. The summary report was said to be a major factor in the final result. We had a summary that the appraisers could read and get the whole story in a compact form.

Light-water Nuclear Power Plants

Ziebarth: Another project you were involved was the evaluation of light water nuclear power plants for GE in 1977.

O'Brien: There are two types of commercial power plants in this country. One is what they call boiling water, and the difference lies in the pressure of the system. Boiling water has a pressure maximum of about a thousand pounds per square inch. The other system, which Westinghouse called the pressurized water system, involves a pressure of about 2,000 psi.

In that year, a man by the name of Ed Schmidt and I made a worldwide tour. We visited plants under construction— forty-six different power plants in which there were about seventy different reactors. Many of them had two or three reactors. The plants were in Japan, Taiwan, Italy, Switzerland, Sweden, and all over the U.S. and Canada.

There were people in GE who said that GE had picked the wrong type of reactor and that they should make the change. There were others who said that was not the case. We spent a year visiting power plants in these various countries to help them make the decision.

Ziebarth: Were these only GE plants?

O'Brien: We could visit GE plants under construction because GE owned the equipment. But if the plant was built with Combustion Engineering equipment and completed, we could also see it because it didn't belong to Combustion Engineering. It belonged to the power company by that time. During the construction of a Combustion Engineering plant, we couldn't go there.

There were four companies—Westinghouse, Combustion Engineering, Babcock and Wilcox, and GE—that were supplying nuclear power plants. We did the best we could to get dope on the two basic designs of the different suppliers.

We came back with a conclusion that GE shouldn't invest much more money in that business as a whole. But we felt that the boiling water reactor that GE had adopted was the way to do it.

I read recently that the Japanese ordered two big nuclear plants with GE equipment. When we were in Japan, Hitachi and Toshiba and a governmental agency were busily studying the design of reactors. They spent ten years at it. When we were there, they were thinking about going into the business for themselves. This recent order involves a billion dollars worth of GE equipment and seems to confirm that the boiling water reactor is the best way to do it.

Future of Nuclear Power Generation: ##

Ziebarth: Is nuclear power generation something that's here to stay?

O'Brien: When the effect of carbon dioxide in the upper atmosphere begins to have a serious effect, people are going to be screaming about why we didn't go to nuclear power earlier.

Burning of fuel produces CO2 which goes into the atmosphere. This adds to the greenhouse effect which begins to effect the temperature of the world and the rise of sea level etc.

Some seventy percent of power in France is generated by nuclear. The French have demonstrations, and their government pays no attention to demonstrations apparently on the basis that the 100,000 demonstrators are 100,000 people who think that way, and there are millions of people who thought otherwise who elected us to the government. They don't let the minority determine what's going on.

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The thing that's plaguing scientists now is where they're going to bury nuclear waste. That should have been settled way back when Glenn Seaborg was head of the Atomic Energy Commission. He had no appreciation of the issues.

If the man who headed that commission [in the 1950s] had been an engineer, he would have slowed the whole thing down. What happened in this country was that we went ahead much too fast with the construction of power plants, and companies like GE and Westinghouse and the other smaller people gave the power companies the impression that nuclear power was just a new source of heat with no real different problems.

They just didn't know what they were talking about. In Germany, Professor Schulten, whom I visited several times, has a different type of reactor, an air-cooled reactor. They've been running it now for fifteen years. He built—and I think tested— a larger unit. In a methodical way, he has demonstrated its safety, step by step.

In this country, we had a peculiar situation in which the reactor design was developed by Westinghouse, GE, Combustion Engineering, or Babcock and Wilcox. Then a contractor bid on it and built it. The worst situation—you may have read of the Shoreham Plant on Long Island or the Zimmer Plant in Cincinnati— was one where the construction people, Kaiser Engineers from Oakland, were doing their first job at Zimmer.

I visited that project; I've never seen such a chaotic situation. They hadn't made proper provision for contact with the engineering designers, who were in Chicago. Instead of having a resident engineer or several of them there all the time, the man came down from Chicago once a week or so. Here's a big place costing a couple of billion. The whole thing's been a disaster.

I saw a plant in Japan which was in operation and a beautiful job: clean, everything just right. That plant had been ordered from Schenectady, New York. The plant was put in operation in forty-nine months. We take about twelve to fifteen years, and when we get all through, the interest paid is more than the basic cost of the plan. All that delay!

The mistake has been acting as though nuclear power is simply another source of heat— you mix steam and run it through a turbine and generate electricity. Some of the power companies said, "We shouldn't pay extra to people in those plants." What happened was that senior men who were the operators of plants decided they didn't want to be subjected to all this restriction of the nuclear side when they could just exert their seniority and go back to managing the fossel-fuel plants, coal plants. That left junior men running our nuclear plants.

In Taiwan, to the contrary, the most prestigious technical position is to be the operating manager of a nuclear power plant. That man is paid more than the president of the power company. They do a tremendous job of training their workers.

As for the Diablo Canyon reactor in California: some of the things that Pacific Gas and Electric did by not knowing the heck they were doing were incredible.

I've visited Diablo Canyon. I've never seen such heavy construction. It's absurd.

About ten years ago Ed Schmidt and I investigated this question for GE: Did GE pick the wrong system of generating nuclear power? We visited about forty-five different power plants with about sixty-some reactors. We concluded that the boiling water reactor that GE was building was the way to do it.

We went to Japan to find out what was going on and appraise the reactors. We spent time talking with people at Hitachi and Toshiba, which were involved in turbine power.

We went to visit a power plant being built by Toshiba, and, in general, they didn't speak English as fluently as people at Hitachi. We had conferences at which I don't think much information was conveyed. We went to visit a power plant, and someone asked in broken English, "What do we do now?" I said, "We walk up to the top of the plant and see it." So, my God, we walked all the way to the top, and one of those power plants is a ten-story building. When we got to the top, I saw there was an elevator.

Ziebarth: Are the Japanese doing a better job of developing new projects?

O'Brien: They're very thorough, and furthermore, they imposed standards of quality after the war that were severe. You couldn't get an export license unless it passed their tests. They just decided, by God, we're going to produce quality equipment. And they had the ability. It cost them a lot of money and a lot of effort—but lenses and all sorts of things all of a sudden turned out to be the best or as good as any in the world.

In this country, you find all sorts of agitation about nuclear power plants. At Diablo Canyon plant here in California, you hear, "Don't build it. It's hazardous." The Japanese power companies went at it systematically. They went to each community where they were going to build a plant, and they found out what the community needed and was worried about. If the fishermen were afraid about the plant interfering with the fishing, they systematically took care of those needs. Not only polished up the people by talking to them, but they actually did things. They build schools and other things.

Ziebarth: Do you think plants can be made as safe as they need to be and that the waste problem can be solved?

O'Brien: The world is a dangerous place to live in. If you walk near fossil fuel plants— if you stand at the edge of one of those coal piles—you're exposed to more radiation than is the limit of tolerance of radiation for the operators in the nuclear plant. Everything's radiating. We're living in a world of radiation: cosmic rays are whacking through us all the time. It's bad stuff, as [the USSR nuclear power plant explosion at] Chernobyl indicated, but Chernobyl was a disgrace in design. They didn't have any kind of precautions, and they had operators who didn't know what they were doing.

You ask about whether the Japanese are doing it well. They're spotty. We were in some plants where the operating room was separated by bulletproof glass and you couldn't get in near the operator. We were in other plants where visitors were wandering around the control room, where if you pulled a switch, you could shut a reactor down.

I felt that the Taiwanese were the best of the lot. The man in charge of a nuclear station is the best-paid employee, and his is the most prestigious technical job in Taiwan. Furthermore, they are trained. They use simulators which simulate all the operations, and you can put in all sorts of anomalies—failures of this and failures of that. In Taiwan, every single power plant has its simulator, and the operators of that plant are constantly trying out different accidents, different changes. Suppose this pump shuts down, what do we do?

As for the waste, apparently, the Chinese are willing to take a contract to study it. I read some time ago that the Chinese have taken a contract with some of the European power producers to bury all the waste out in the Gobi desert. That's a remote area that doesn't affect many people, and there isn't much groundwater.

Another thing is that everyone is regarding all this spent fuel as waste. But it has heat in it; it's hot, generating heat. Somebody's going to figure out how to use that heat.

There's all sorts of misinformation. You talk about plutonium being poisonous; it is poisonous. But I can pick up a piece of plutonium like that [folded in paper] and be perfectly safe [because although it is a dangerous chemical poison] its radiation is weak. If they really get down to business, they're going to separate out the hot short-life components and use them for some purpose. The long-life components are going to be put in the Gobi Desert and left there because they don't generate much radiation.

Miscellaneous Activities ##

O'Brien: I give continuing advice about engines of all kinds, small, large. There was a pretty long period of connection with the space division, first when they were working on this re-entry vehicle, nose cone project and then when they expanded and built the large facility at Valley Forge. I helped plan that place.

It has all sorts of laboratories. For example, there were to be three big sun simulators. Three huge establishments in which you could put a space vehicle and subject it to what it would experience in space facing the sun. I told them they were crazy. It would have cost an awful lot of money and never been used.

I've had a long connection with the Army Scientific Advisory Panel, and I served as chairman for four years. For a time I was on what they call the air mobility panel, and that put me in touch with Fort Rucker, Alabama, which is the Army aviation center. For a while Army aviation was supposed to be merely for transport, not for fighting. They weren't supposed to have any armament on a helicopter.

But up at Burlington, Vermont, they were collaborating with Fort Rucker in developing equipment for helicopters. There was a GE plant at Burlington, Vermont. GE was developing a rapid-fire gun, that could fire 600 missiles a minute. It was a great gun for a helicopter. It put out a lot of bang.

For a while I used to go up to Burlington and talk with them, and I would see the guys from Rucker and try to speed up of the day when Army aviation was armed. It was and it is.

[Interview 9: June 2, 1987]

Personnel Management Principles ##

O'Brien: Working at GE, I learned a lot about personnel management. One of LaPierre's principles was: Don't ever move a man whom you regard as key, somebody you're looking to as future management material. Don't ever move him until he demonstrates successful completion of some job.

I know of any number of people who are pushed too fast. When all of a sudden a fellow is moved up, there are all sorts of animosities. Besides, this man hasn't learned to operate at that level yet. He's now a vice president, and he was down three or four levels two years before.

My relationship with GE was very intimate. For twenty-five years I was Gerhardt Neumann's confidential adviser, so his book says. If I advise somebody like Neumann and they buy the idea, it's theirs, not mine. There's no limit to what you can accomplish if you don't care who gets the credit. That's why after thirty-eight years I seem still to be in good standing with the General Electric Company.

The other principle that guided me was that I practically never turned anybody in. I wandered around and encountered a problem, and I'd say, "Gerhard, I think you ought to have somebody look into it." Without telling anyone, he would put somebody to work on it, and he wouldn't even mention the fact that he learned from me there was a problem.

I believe that when you find people who aren't doing a good job, it's usually because the job doesn't match them. If they've been in the company for some time and had a pretty good record to begin with and now there's difficulty, maybe the job is beyond their level and technical requirements or they've got too many people reporting to them or it's a subject they aren't interested in.

That's the way it's gone on for thirty-eight years. Neumann, whom I worked for in the main, became executive vice president of the company, not just the engine business. GE's now run by three men, the chairman and two vice chairmen, and with a $24 billion company, that's a pretty big job for each one of the three. I also worked for Ed Hood, a vice chairman, for a long time. Mine wasn't the usual consulting job, by any means.

Now, I'm working on another project for GE. What they asked me to do was to talk with the retired managers and review the facts of how GE has grown to be such a successful business. Since I've been there since practically the beginning, they wanted me to put together not a history, but a manual— how to do high technology. What do you do? What do you look for? What kind of customers do you seek? How do you treat the customers? All the way from technical problems to management problems to finance. I've interviewed about fifty people now.

Approach to Consulting for Industry ##

Ziebarth: What were the most difficult aspects of your consulting work?

O'Brien: The assignment I had from LaPierre, way back, to become familiar with the new engine development projects including the variable stator and dual rotor engines they were working on. What are they doing? What are they trying to do? And to become acquainted with the people up and down the line, from top to bottom. All that in order to be available to him for advice when he had a problem. That was a demanding assignment.

At one point, he asked me, "Shouldn't you write a report on what you do?" I said, "Well, my expense account shows where I've been, and are you sure you want me to write out why I was there and what we talked about when I was there?" He decided he'd just as soon not have me write any reports about what I was doing.

Although he didn't tell me this directly until many years later, he had a theory of management which he developed when he was at the General Engineering Laboratory. He said you always needed a doer, a project manager, who followed through according to a plan that you'd worked out. Then you wanted somebody who watched what was going on to make sure they didn't get diverted into giving different emphasis and ignoring the really tough jobs— sidestepping the tough jobs and doing the easy ones. Then a third position was someone who looked ahead. No matter what project you worked on—and even if somebody else was paying you for it—it was very important to always keep an eye on whether that project

He looked to me to be the kibitzer, to watch what's going on, see if they're still on track, and especially to identify gaps in what they're doing. I was to see that they were working on things that were essential or they're not getting results which were adverse and they are not emphasizing it.

So I developed some skill, I guess, in reviewing projects, no matter what they were. Projects on flight control. Projects on nuclear power. Projects on radar, on sonar. For a year I followed the Apollo Support Department, which provided that huge launch array they used.

It got to the point where no matter what the project was, I had almost an automatic way of probing. Even though I didn't understand the technology very well myself, I could identify an area where they couldn't explain to me what they were going to do next or how they were going to get around the problem. So that's what they looked to me for: to identify problems. I couldn't solve these problems—and they didn't expect me to—but they wanted to be sure the problems were identified early so they were worked on early. And if they were show stoppers, they should stop the show on time.

At first I worked on the engines, and I got into that by way of the technology of compressors. But when LaPierre moved up to become the vice president of the company in charge of all the aerospace and defense, including the Apollo support and the space division and the heavy military electronics and light military electronics and flight control, I just followed right along and wandered through these areas. Nobody worried about whether I was cleared or wasn't cleared. I was really part of GE.

For example, I never wrote a single paper about the GE work, never a paper for publication. By not publishing anything, I wouldn't get in any trouble and wouldn't embarrass anyone and wouldn't reveal any proprietary secrets.

Ziebarth: Did they want you to become an employee of the company?

O'Brien: No. I was dean of engineering for the first ten years of my association with GE. When I retired as dean, they wanted me to maintain my academic connections. So I think I became something like forty-five percent GE and one-quarter free.

I worked on two very delicate missions. One which took about a year of my consulting time was on the relationship between the Air Force and the General Electric Company's Aerospace and Defense Group. The Air Force was commanded by General Anderson, who had on the wall of his office the ten most difficult projects. Of those six were General Electric

Force was commanded by General Anderson, who had on the wall of his office the ten most difficult projects. Of those six were General Electric projects spread all through the aerospace defense group.

Mr. LaPierre alone took me to Wright Field to meet General Anderson and to propose that I look into this interface. What went wrong? Why were things not going well? The problems in many cases were contract terms and money, but they also all involved technical questions. One by one I looked into them, visiting Andrews Base in Washington, D.C., and the electronics system up on Cape Cod. I visited the Air Force establishments in the whole range of electronics, engines, everything.

One serious problem for the Marine Corps was when GE had gone ahead with the development of a forward flight control. The Marine Corps were furious when they learned about the weight and size of this thing. GE wasn't facing up to it. In fact, GE didn't tell them about it until very late. There was no solution except to divide the thing in half and fly it in two pieces. Get going; don't waste any more time.

There were just about twenty projects that I looked into, and in the course of that I looked into the attitude of the military people, including their civilian employees, about GE.

I would often sit down with a brown-bag lunch with a group of six or eight buyers. They were the men who had to sign on the dotted line. Even General Anderson couldn't buy anything if they said it violated the rules.

It was clear these fellows were trying to do their job, and GE was just ignoring them. They had to have certain information. They wanted the equipment, whatever it was, but the GE department would just not deliver the stuff. Furthermore, they had overruns and they had mistakes in the accounting. I learned you'd better get your accountants busy on this because the Air Force is mad.

Then I did the same sort of thing for the Navy. Those were two jobs that required a lot of diplomacy. At least, I didn't go in and antagonize 'em. Furthermore, I was really trying to find out the truth, and both managements were.

Problems of Large-Scale Research and Development ##

Ziebarth: While you were consulting for GE, you wrote widely on topics such as technological planning and misplanning. What were the major problems that faced a big company like GE?

O'Brien: One important thing is to have somebody in charge. And that somebody had better be fired up about that project. I'm convinced that the United States is incapable of another Manhattan Project.

Ziebarth: Why?

O'Brien: The challenging problem is a bureaucratic problem. None of these high-technology, advanced projects ever get done properly unless there is somebody in charge who is competent. When a whole lot of agencies get to put in specifications, a project gets so fouled up nobody knows what they're doing.

I was on a committee of the Army Scientific Advisory Panel, which looked into the process by which equipment was developed for the Army. We found that there in the normal procedure from the idea to the availability and service there were 125 steps. Many of these were perfunctory steps to check money. But a lot of them had to do with specifications. This process got started, and then every branch of the Army had a chance to comment and say, "Well, we ought to change these specs."

I have a theory we would be better off if we took all the development money we have for each service, divided it up by the number of lieutenant generals, gave every general his share, and said, "Go do something useful. " Half of it would be wasted, maybe two-thirds. But one third would produce outstanding results, and you'd be better off than the present system which wastes damned near all of it."

Ziebarth: Why is it wasted?

O'Brien: There are all sorts of factors involved, including the proprietary interests of those who want to supply part of the machinery. I've a friend who looked into the matter of the battle tank, for instance. Many of the battle tanks have diesel engines. But a diesel engine has to be warmed up before it can go anywhere. Secondly, once you get it going, it has to keep running. It still makes noise.

Gas turbine engines, on the other hand, take no time to warm at all. Push the button. Fuel begins to flow. The turbine begins to turn. And you're off on your way. Furthermore, if you want to stop and listen, turn it off.

All of this evidence just got pushed aside. In the final decision about whether to use gas or diesel engines, the diesel engine manufacturers were really on the inside. There was a lot of phony discussion of the fact that the specific fuel consumption of the diesel was less. That's a small example of proprietary interests leading us down the wrong path.

Not much of it is malicious. The man who said the diesel will always win because of its low fuel consumption has no axe to grind whatever. So far as I'm concerned, he simply had the wrong set of facts to think from.

In a bureaucracy you can't do anything but routine things. As for developing high technology, forget it. It's out of the question.

For example, in the civil service of the government, people go ahead by seniority. In the case of the spacecraft *Challenger* disaster [1986], I think that the senior officials as NASA advanced largely by seniority. The most glaring indictment of Morton Thiokol [manufacturer of the O-ring which malfunctioned] was the statement made by the Thiokol official. He said, "In the past, we were forced to prove that it was safe before they would fire. Now we must prove it's unsafe before they [NASA] won't fire." That's terrible.

To me, that's all the difference between a bureaucratic organization and an honest-to-god development group.

Role of Large Companies: ##

Ziebarth: Some of your writings and speeches focused on the distinction between research and development. Does it seem to you that there has been enough emphasis on basic research in this country?

O'Brien: In 1947 I took leave from the university and went to the Air Reduction Company. They wanted someone to set up and carry on research for them. That company was owned and managed by bankers. It was a synthetic company. They bought the rights to their process from the French. All of the growth had been by acquisition; they bought small companies and put them together. So they had no knowledge whatever of the nature, requirements, or anything else about research. All they knew was they wanted products to sell.

When I looked into it, it was clear that if you call research the search for new principles, new processes, new configurations of things, really new, only very large companies can afford it. Of the research projects at GE that went far enough along to be given a number and a budget—a lot of exploratory stuff went on under some general heading but never reached that state— only one in twenty finallly paid the company profits. The other nineteen died.

General Electric was a big company and could afford twenty projects at a time. I was with a small company that could afford one project at a time, and if one in twenty paid off it might be that we spent twenty-five years before we had something. So my conclusion there was that Air Reduction should confine their efforts to development.

By that I mean confining yourself to realizing in hardware a concept which you can define. You know the technology. You know that you can bend metal, you can make alloys, you can do all that's required. The problem is to do it at a cost that you can recover in the sale of the product that's developed. In other words, after you invented transistors or something like that, you can say to another group of people: "Okay, we know this principle. Now you go ahead and develop a way of making them at minimum or reasonable cost.

I told Air Reduction that they should confine their efforts to development work, where they could see a product. You know how to do it. The problem is to do it at a reasonable cost. You can see a market. You appraise that market to see if it's big enough. That's the kind of effort that a small company like Air Reduction should make.

When I came there, Air Reduction was in the business of supplying acetylene for welding, and just because they had acetylene as a product, they had had a group of chemists working on acetylene chemistry for twenty years. But they had never developed or sold a product.

When I looked into that, I found that Allied Chemical had patents on acetylene derivatives that just covered the waterfront. Anything you thought of to do with acetylene, they had a patent on it. I couldn't figure out what to do about that until I learned that the Navy was interested in acetylene explosives. So I got a job for the acetylene research group working for the Navy.

The Air Reduction Company executives were financiers, bankers, and they met people at the Banker's Club. They even met GE people. Somehow they'd been told by GE that if you mix the engineers with the scientists, you have trouble.

Air Reduction took the advice seriously. But GE has thousands involved. Air Reduction had a total force of about 250 including secretaries and mechanics and everybody else in the whole establishment, engineering and research. Furthermore, because they had various companies within Air Reduction, they set up an area for research. Chemical supplies, medical oxygen, which has to be of a different purity than industrial oxygen. Ohio Chemical also has other things such as bedpan sterilizers and God knows what.

In the research lab at Schenectady there was an area for Ohio Chemical to do research. About all they could use it for was storage or something because they didn't have any research.

After I got straight my own thinking about it, I persuaded Air Reduction to forget the research lab in Schenectady and sell it to somebody.

Here's how it happened. Mr. Moorhead Paterson, president of the company, Jim [LaPierre], and myself went there. The building was finished; nobody'd moved in yet. I didn't want any Air Reduction people to move in at all. I wanted to sell it fast. They finally sold it to a distiller and put all of the work at Murray Hill because we couldn't manage any real research.

Murray Hill is in New Jersey, and the point of Murray Hill is that the big Bell Laboratories are there. Bell Labs had made a very thorough study of the environment, the living conditions, everything, and put their plant there. We figured since there was space across the road, we'd just put our lab right there, and we did very well.

Ziebarth: Can research only be done by huge companies?

O'Brien: It depends on what it is. A lot of this work in the biological areas isn't being done by large companies. People have ideas about it, and they get sponsors, and of course that kind of research goes ahead.

But the companies better be well-heeled. You see, the easiest thing to cut out is the future. The managers of a business have two responsibilities: one is to make money now or they aren't going to stay in business. The other is to spend money and make the effort now to assure profit in the future.

You can look great on number one. If you neglect number two, it may go unnoticed. It doesn't show up on the balance sheet. You have trouble finding out what do they really do so far as the future.

You may have a training program for people. You spend money on it. You have assignments that you give. You have people watch those assignments. You have people that watch the people to see who's good, to get rid of some. You can have a personnel system that's expensive. In the long run it pays off because you get good people out of it. You could cut the personnel system out and save all that money, and nobody knows the difference, until later.

GE Successes at Forecasting:

O'Brien: When you get into the products of the future and the money you ought to spend now to get ready for the future, it's hard to know.

There was a period just after the war, a period when the intercontinental ballistic missile came in, when the Air Force felt that the era of air breathing engines—engines like the present jets that breathe air, burn fuel, and so on—was over for the military. Henceforth the military would use guided missiles.

I talked with a lieutenant general of the Air Force. I called on him for the purpose of part of this interface study. He was serious in his prediction. He said he thought the Air Force had trained its last pilot.

GE went right ahead with its study of air-breathing engines as though missiles didn't exist. Another branch of GE took the missile business seriously, but the engine business went right ahead because they believed that air-breathing engines were not going to go out of the picture.

The situation was so bad that the budget of NASA for air-breathing engines was practically zero, almost nothing. The companies that kept on studying air- breathing engines despite indications to the contrary survived, and all the rest of them are dead. There was Westinghouse, AVCO, Fairchild, Curtis Wright, but the only two American survivors really are Pratt & Whitney and GE.

People at GE had a lot of discussions about what's going to happen next. General so-and-so says it's all missiles. At GE there were enough who were convinced that air-breathing engines for military service were going to stay, and they kept right on working on them. When the thing swung back, they had a lot of technology ready to apply, they applied it, and they got the job. I think by all accounts GE's the number one engine builder in the world. They just beat the hell out of Pratt & Whitney.

There's a big company in Europe called Airbus. In this country, there's Boeing and Douglas. Airbus has done a pretty good job of worming its way into the business by way of some subsidy from their governments.

Airbus is owned by the governments of Germany, England, France, Holland, and Italy.

Airbus made some progress, and they had some differences of opinion with GE over this and that. They put GE engines in some of their first units. But when the Pan American Airways directors visited Airbus with a view to buying airplanes, Airbus industry didn't even mention GE engines as an alternative that might be available if Pan Am wanted them. They just cut GE out completely.

The other day Northwest Airlines ordered 325 General Electric engines to be put in Airbus airplanes. That was a real victory; it took some doing, some selling. And you don't sell on promises; you sell on past performance. Did you support the airplane in the past? Did you have spare parts ready? Did you have extra engines staked out around the world so if you had trouble, you had a replacement handy in a hurry?

When it came to the recent award of military contracts, Casper Weinberger, [former] secretary of defense, wanted to give the whole job to GE because the GE engine really was markedly superior to an engine that had been in service, Pratt & Whitney, for a long time. They had had all kinds of trouble with it. The Air Force said, "No. We don't want one manufacturer. " So they gave GE seventy-five percent of the job and Pratt & Whitney twenty-five percent.

The other big victory that we achieved: a GE engine named the 404 engine is a very successful engine with very good characteristics; it has been tested, tested, tested. Pratt & Whitney Company has now been ordered by the Navy to build the GE engine. That means a couple of things. It means they build the GE drawings; they build it exactly the way GE designed it. GE is annoyed because Pratt & Whitney is going to learn a lot about their technology. But when the engine is so good that they order the competitor to build it, it's kind of a bit of satisfaction.

Industrial Research Role of Universities

Ziebarth: What role should universities play in industrial research?

O'Brien: There are a lot of exploratory areas where I think universities are much better prepared than industry. For instance, the high caliber of help: In universities you have bright students who are candidates for degrees working on projects because they need money and that's how they earn their living.

Two, in most cases the cost is much less for two reasons. You get a very high-caliber help for workman's wages and, two, there's no commitment. What I mean is that if you set up a big research laboratory, you have all those people on your payroll. They begin to have seniority and pension rights and so on. But if you have work done at a university, you don't make any commitment at all. You give them some money. They do some work, and when that work is over, your commitment is over.

The general spirit of inquiry is different in the university, too. But a lot of university work is just busy work, getting numbers to put in a learned paper to present at a learned conference, about which nobody really gives a damn. .

Large companies can afford to explore entire areas. They can do the kind of basic research that results sometimes in a Nobel prize. GE's research lab has the distinction of being one of the very few research labs with a Nobel prize winner. You have to be awfully big; to get one Nobel prize winner you have to support quite a number of people and quite a lot of work.

A good deal of industrial work should be development aimed at products—products you can identify. The universities need to be supported in basic studies. We worked here in Berkeley for some time in plastic ceramics. Ceramic materials, porcelain and so forth, are brittle. Their properties change with temperature so that sometimes they're plastics at high temperatures and at low temperatures they're very brittle.

Here's an area of university research that has paid off recently . The zero friction effect. Cooling certain alloys down to liquid-nitrogen temperatures reduces their friction loss to zero. That's a tremendous advance, and people have worked on that for years and years and years and years.

Perhaps I have minimized my basic support for research at the university. The area of social science research is another very important aspect. Such as what we did in setting up the Institute of Transportation Studies.

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Ziebarth: Do universities train adequately people to be researchers?

O'Brien: Universities are run for the benefit of the professors, not the students, who are often taught by teaching assistants. There's no need for the number of graduate students that are being produced. Professors are accepting graduate students in numbers which exceed any reasonable demand, and the thought is not whether there are jobs for such people and what kind of study would help them get jobs: it is mainly to help professors with their research.

Ziebarth: Are engineering faculty scientists or applied scientists?

O'Brien: It depends on who they are. We have people in engineering who are honest-to-god scientists. Allen Searcy is a physical chemist. John Dorn was a physical chemist. Sam Silver was a physicist. Shepherd, Schaaf, Chambre were Ph.D. mathematicians. Richard M. Zettel was an economist. They are scientists who became interested in applications. There's no sharp boundary.

An example: Harry Kennedy, inventor of union melt [welding], was talking with the Union Carbide company about the needs of the company. The man said, "Well, there ought to be more research. " My friend said, "Well, now, what would you regard as research?" The answer: "How to sell more electric welders in the East Bay." (laughs)

Ziebarth: When you were dean, you introduced a doctorate of engineering degree. What was your intent?

O'Brien: Engineering is both a science and an art. There are reasons for advanced studies, and there are reasons for men to be learned about what has been done in the past, the techniques, the methods, and materials. I thought that the Ph.D. was pushing toward too great an emphasis on theory, and there was reason to encourage studies of more practical systems, difficult systems. So we introduced the doctor of engineering.

While I was in the hospital recently, I talked with one of the nurses, a young man. He told me about his problems; he didn't have all the usual credentials and was having trouble getting approval for his work. And one day, he said, "Where did you get your doctor's degree?" I said, "I never did get a doctor's degree." He said, "Where did you get your master's degree?" I said, "I didn't get a master's degree." He said, "Do you mean to tell me that you became dean of engineering at Berkeley with only a bachelor's degree?" I said, "That's what happened." He said, "That makes my day." (Laughs)

The whole system of engineering education in California is badly distorted. Too many schools got in the act. There is a reason to teach students advanced theory, if they're equal to it, if they have that kind of ability.

But there's also a reason to introduce students to the practical side of engineering. You can say that's a narrower view, but the fact is that there are narrower-minded people. Engineers aren't all the same. Some are plodders. Some are doers, in the sense that they like to build things, not draw pictures of them. And there's great importance to this practical kind of approach.

Boelter, Bonham Campbell, and I had convinced the people who were teaching engineering in the state colleges—they didn't offer degrees, only courses, especially for the first two years, and then the students transferred to Berkeley— that there was a niche for them in this practical approach to engineering. Instead of a full program for four years, they should emphasize the practical side, emphasize what was needed by local industry, say in Los Angeles and San Francisco. But that understanding was sabotaged.

Now it's to the point that to get an appointment to the engineering faculty of one of the state colleges, you must have a Ph.D. If I were doing it, my rule would be: no member of the engineering faculty may have a Ph.D. He must have had five years of experience in industry.

That's how I feel is the university being run for the benefit of the professors, not the students or the public. We don't need students with that kind of preparation in those numbers. We do need a lot of students who come out with a bent toward manufacturing, with a feeling that they have the real engineering program.

X. ADVISORY POSTS, 1950-88

Amphibious Operations, National Research Council

Ziebarth: In your career, you were appointed to a number of federal government advisory posts. Between 1950 and 1953 you served on the National Research Council's Committee on Amphibious Operations with Roger Revelle, Jim LaPierre, and Edwin Land. What did your committee do?

O'Brien: We took a look at how they did amphibious operations and spent a lot of time with the Marine Corps, which had the principal amphibious units. We also spent some time with the Army. We witnessed quite a number of amphibious practice operations. But it finally turned out that the real work that we did—we finally defined the job to be done—was to review the equipment policy of the Marine Corps. We went through the whole list of items that they said they wanted and appraised whether we had any knowledge of it or not.

We came up with something that worked. What we did, of course, was go listen to what the people involved wanted. First step, we went down to the Marine Corps at Quantico and had dinner with the senior people involved in R&D for the Marine Corps. I sat with a general commanding Quantico and he said, "You know, these young fellows come up with ideas that scare the hell out of me. But," he said, "it's my job to support them so I do."

At that time the Marine Corps was beginning to think about vertical envelopment. Don't try to go on shore through the surf and the beaches; that's rough going. Go by air. Go from carriers by helicopter to the land.

So we bought that idea and spent a lot of time investigating it. I got a special project set up to study helicopters. Sidney Hiller of Hiller

Helicopters, a local company in San Jose, said we really gave the helicopter industry a big boost because all of a sudden money became available for them to try new ideas.

We pushed vertical envelopment. That has come about. The Marine Corps has carriers with helicopters, and they do go ashore on gunships carrying men, guns, and so forth.

We also became convinced that there were reasons for fuel depots at sea, big plastic tanks barely afloat.

We also said we could see no sense in taking a huge volume of a ship and filling it with small boats and then using the small boats for the short distance ashore. Get rid of all that. Turn them into carriers and use the space that way. That was one of our main ideas.

Admiral Ned Cochran was a member of the committee. He was on it because he was a vice admiral in the Bureau of Ships and a designer of the wartime navy. He designed the ships we built in a heck of a hurry and put to sea.

Another member was Commodore Schade, who was also a professor. He was a naval constructor who'd been director of the Naval Research Laboratory and was on our faculty.

That committee spent about three years and finally undertook review of the Marine Corps equipment policy as a practical contribution which we could make. Of course, on many items we had no comment: ammunition, things like that. But there were a lot of things that the Marine Corps was unhappy about that we looked into. The final report was classified, and I'm told that after it was published, the Marine Corps—you know, the Marine Corps is under the Navy— would scream, "Hey! It says here you're supposed to give us so-and-so. Now where is it and when are we going to get it."

Army Scientific Advisory Panel

Ziebarth: You also were a consultant for the Army Scientific Advisory Panel.

O'Brien: There were originally ten members. It was created really because the Army chief of staff was J. Lawton Collins. At that time, nuclear weapons were thought of as being used in the field. Collins said that, by God, he wasn't going to let the Air Force drop any nuclear bomb near his troops. And he added, "And they can't hit the broadside of a barn at ten-foot range. I want something done."

The upshot of all this was the nuclear cannon that fired nuclear weapons. This committee of ten didn't have too much effect on that, but we did get a very enlightening letter from J. Lawton Collins, each one of us, in which he said these things. He said it a little more politely, but just was damned if he was going to have the Air Force dropping bombs on his troops.

Then it turned out that the Army had all kinds of problems. For example, I was on a committee that investigated the process by which a new idea wended its way through the Army system. We found out that there were 125 checkpoints. Many of those were money checkpoints, but a lot of them were technical.

In other words, the device, whatever it was, might be referred to the electronics committee at that fort out in New Jersey. They'd say, "It ought to have this and it ought to have that." And by the time that they sent it through all these checkpoints, the specifications got to be incredible. The delay: we found an example of a Corps of Engineers tractor for construction work to be airborne that had been twelve years under development and, as far as we could find, hadn't even started.

We also found that although there were all these checkpoints, if the commanding general of the Army Materiel Command wanted something done, they cut through those 125 checkpoints and had it done at a terrific rate. As a matter of fact, when we came up with this diagram, it was a shock to everybody to see how many silly checkpoints there were. That was greatly simplified and shaken down. I hope it was some help.

National Science Foundation ##

Ziebarth: What did you do on the National Science Board of the NSF?

O'Brien: I was on the mobility panel of the Army panel that was concerned with aviation. At that time, the Army could only have helicopters.

Ziebarth: You managed to help the Army secure funds for research and development.

O'Brien: For helicopters. The Army tried out all sorts of ideas. The membership gradually built up from the original ten to about fifty. I was chairman of it for four years.

Ziebarth: Who appointed you to the Army advisory panel?

O'Brien: The secretary of the Army. I filled out the term of George Murk. I wasn't welcomed on the board because as soon as I got on it, I started making comments on spending too much money on education. It turned out that the head of the board, Alan Waterman, was not an awfully strong individual. But the man who had the education program was quite competent and had a lot of influence with Congress.

So they had all sorts of programs, and I made remarks on that. Then there came the change. Eisenhower appointed me. Kennedy followed Eisenhower. The administration switched from Republican to Democratic, and I was not reappointed. I thought more money ought to be spent on real research grants.

Ziebarth: Did your good connections with people in government and business make it possible to become a member of the Cosmos Club?

O'Brien: No. I became a member of the Cosmos Club way back when I was an associate professor. I had a friend who was a member, and he took me there a number of times. Those were depression years, and they were looking for members, too.

Ziebarth: Did you conduct business there?

O'Brien: Sure. All kinds of business. One of the big projects we had at Berkeley for a long time was how to weld ships. The connection with the Cosmos Club is that I happened to go in the club one night and saw two men whom I knew distantly who were with the National Research Council. They were waiting for me. They said, "You know, one of those welded ships, a tanker, split in two at the Portland harbor. Something has to be done. And we think that you at Berkeley ought to undertake a study of this."

So I said, "Okay, I'll look into it and see what we can do." Eventually we wrote a stack of reports so high on welded ships. What they found out was that really it was sharp corners that caused the cracks. When they rounded the hatch corners, the problem was solved.

The other thing that started at the Cosmos Club was Berkeley's involvement in ceramics. I used to see a man there who was with the War Metallurgy Board and was the professor of ceramics at North Carolina State University and he got me interested in starting a ceramics industry at Berkeley.

University of Florida Association

Ziebarth: How did you become associated with the University of Florida at Gainesville?

O'Brien: I had a friend [Thorndyke Saville], who made a study of the graduate study program at Florida. He had been dean of engineering at New York University. He got acquainted with people in coastal engineering, with Robert Dean in particular. I knew something of the place because I was then beginning to visit the Apollo support department at Daytona Beach, Florida. It turned out that airplane schedules and such made it convenient for me to go to Jacksonville, pick up a car, stop by Gainesville for a day or two, and then go on down to Daytona Beach. I could turn the car in there and get a flight out of there to New York or somewhere.

So I began with that kind of connection. We landed on the moon in '69. So it was about '62 or '63 that I first made my connection. Florida had a lot of problems.

Ziebarth: Were you doing consulting work?

O'Brien: First, I just visited. I just dropped by to see what they were doing and get acquainted. Then there was a period of four or five years when I was teaching. Then I had some graduate students, one or two working on problems. Then we decided we ought to offer a general course open to any registered student, no prerequisites whatever. A man by the name of Omar Shemden and I put on that course, which met twice a week for a term. We split up the program so that I'd be there for two lectures. We brought in some specialists because this covered the waterfront, everything about oceanography and beaches, tides. That was pretty successful. That put us in touch with more students and more research projects came up.

Shemden was chairman of the department. There were all sorts of complications and bickering internally. Then Shemden was part of an international study group studying wave action and related problems in the North Sea. So he took a leave for a term and a summer. I was left as acting chairman of the department. I didn't solve many of their problems, which I became convinced were damned near insoluble.

There finally came a day when I decided to hell with this. Why should I worry about their problems? I had a car that I left there. It was on Memorial Day, the holiday weekend. I decided I'd had enough of this, piled everything in the car, and took off for California. I didn't see the Florida group again for about seven years.

Then I got a call one day from my friend, Robert Dean, who said, "Would you return to Florida to help us there?" He said, "I've accepted an appointment." It was a very fancy, high-level special-deal appointment to come back. He said, "If you are, I'll put you onto Professor Wang, who's the new chairman of the department at Florida." And Wang said, "Why don't you come and join us?" So I did. Went back, and I've been going back regularly.

XI. CONCLUDING REFLECTIONS

Ziebarth: Looking back, what do you see as the most satisfactory accomplishments of your career?

O'Brien: The work at Berkeley as dean. The situation at Berkeley was unique. All the schools were going to have to add to their faculty after the war. But the opportunity to change the faculty at Berkeley was greater than any other.

An increase from 80 to 200 gives quite an opportunity for change. That was fun, looking for people. I made some mistakes, and I see them around the campus still. But on the whole it turned out pretty well.

If I were twenty-five starting over again.... Let me tell you a story. Prior to World War II, the faculty of the Guggenheim School of Aeronautics at Cal Tech, of which Dr. Theodore von Karman was the director, held joint seminars once a year. That resulted in a period of about ten years or so when I was fairly close to von Karman.

I went to Los Angeles and usually had dinner at his home with him, talked with his other people, Clark Millikan and others. When I was appointed chairman of mechanical engineering and told him about it, he was thoroughly disgusted. He said, "You'll regret it. Don't become an administrator. Keep clear of administration." Maybe he was right. So many things are matters of chance.

I say all this because I think I would have become very unhappy about the university as dean and wouldn't have been able to do much. I'm glad I left when I did.

The universities of the country have sold the public a bill of goods about the value of education. I'm convinced that all state colleges should have a requirement that every graduate must have a vocation to earn money. Secondly, I'd have curricula in all the state colleges for plumbing, carpentry, electronic technician, right across the board.

There's no reason why a carpenter or a plumber or others shouldn't have a general education. And people who are that smart and are good plumbers—in other words, I'd have a plumbing course that really plumbed things—they aren't going to remain plumbers putting in valves all their lives. They're going to have a plumbing business in no time.

I'm in favor of the general education, but I think it ought to be mixed with vocational education. I think at least half of the people who go to the university shouldn't go. They don't belong there; they belong at the other kind of schools. All this comes about because of the prejudices of professors and the song and dance they give about getting a degree is better than not getting a degree.

The real problem in the United States today is having a much greater supply of engineers with dirty fingernails. The control of industry through all these financial and banking and law conglomerations isn't working.

END

TAPE GUIDE — Morrough P. O'Brien

APPENDIX— MORROUGH P. O'BRIEN

Morrough P. O'Brien

PART I. Selections From Morrough O'Brien Commemorative Issue of Shore & Beach, July-October 1987

Biographical Sketch of Morrough P. O'Brien

BY ROBERT L. WIEGEL
Department of Civil Engineering
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Berkeley, California

MORROUGH PARKER O'BRIEN was born in Hammond, Indiana, on 21 September 1902. He received a B.S. in Civil Engineering from Massachusetts Institute of Technology in 1925, did graduate work at Purdue University, 1925-27, and at the Technische Hochschule in Danzig and the Royal College of Engineering in Stockholm as a Freeman Scholar, 1927-28. He has received three honorary degrees: the D.Sc. from Northwestern University, the D.Eng. from Purdue University, and the LL.D. from the University of California.

O'Brien has engaged in three fundamentally different careers. His academic career as professor and dean at Berkeley spanned the years 1928-59; it is covered by two papers in this symposium, one by University Professor John R. Whinnery, who followed O'Brien as Dean, and the other by President Emeritus Clark Kerr, who served as Chancellor during O'Brien's tenure as Dean. A second career was his pioneering work on the development of coastal engineering which had some impact on the research program at Berkeley but was unrelated to his academic plans; the paper by Professor Johnson recounts his work on coastal processes. His third career has been his service since 1949 to present on an annual retainer to General Electric Company. O'Brien has said little about this work to his academic and coastal engineering associates and has published nothing about it; there is much to indicate that his work was of major importance to this company, especially in the fifteen years following his retirement as Dean.

This biography is supplementary to those mentioned, with emphasis here more on personal relationships than on professional work. For the reader's convenience in relating times along these paths, the academic steps were:

Mechanical Engineering:	
Assistant Professor	1928-31
Associate Professor	1931-36
Professor	1936-43
Chairman	1937-43
Department of Engineering: Chairman	1943-59
Professor of Engineering:	1943-59
Dean, College of Engineering:	1943-59
Retired:	June 30, 1959

O'Brien's schooling was unusual for an aspiring engineer. When he was twelve, his mother, who lived to be 78, was found to have tuberculosis and was sent to a sanitarium in Arizona; he went to Toledo to live for the next ten years of high school and college with three maiden aunts, two of whom were teachers. This arrangement was to permit him to attend a small local high school and college conducted by the Jesuits; the curriculum was Latin, Greek and English, in that order, five mornings a week. Physics, chemistry and mathematics were electives, taught tutorially in the afternoon or on Saturday. The instructors were mature, scholarly men; high school classes numbered ten or less and college classes, five. O'Brien completed high school and one year of college there and then transferred to Holy Cross College with the intention of completing his B.S. degree in humanities before going to MIT. The curriculum at Holy Cross consisted of work on languages in the first two years and the study of philosophy in the third year. O'Brien attended two weeks of lectures on philosophy and decided not to wait longer to begin studying engineering. He then transferred to MIT (in 1921). "Tech" was not yet in session but Harvard was, and O'Brien visited his friends there – with the result that for the next three years he was a resident of a Harvard dorm at night – with a private room nearby as a mail drop – and an MIT student by day. The Harvard "yard cops" and the "biddies" who took care of the dorms were mostly Irish, and they were amused – not annoyed – by his presence. When his friends became seniors and lived in the Harvard Yard, his bedroom was on the second floor of the Middle Entry of Greys.

In addition to his teaching, research and professional engineering career, O'Brien has had a full public service life. He was a member of the Army Scientific Advisory Panel, 1954-1965, Chairman 1961-65; a member of the Defense Science Board, 1961-65; Chairman of the Committee on Amphibious Operations of the National Research Council (1952); Member of the NRC Committee on Engineering Implications of Mean Sea Level Change (1985-87); Member of the Board of the National Science Foundation, 1958-60 (Presidential appointment); Member of the Beach Erosion Board, 1938-63, and the Coastal Engineering Research Board (1963-1978), both in the Office of the Chief of Engineers, U.S. Army.

O'Brien has been active in many professional societies, especially the American Society of Civil Engineers and the American Society of Mechanical Engineers. He was Chairman of the Executive Committee, ASCE Hydraulics Division, 1951-52. He served as Chairman of the Council on Wave Research of the Engineering Foundation from 1950 to 1963 and as Chairman of the ASCE Coastal Engineering Research Council from 1963-1978. He was active in the American Geophysical Union in the 1930's, and was a member of its Executive Committee, 1937-39, Vice President of the Hydrology Section in 1939. He was President of the American Shore and Beach Preservation Association from 1972-1983.

O'Brien has been honored by a number of organizations; namely,

- Society of American Military Engineers, Tasker H. Bliss Medal, 1964
- American Society for Engineering Education, Lamme Medal, 1968
- American Society of Civil Engineers
  International Coastal Engineering Award, 1982
  Honorary Member, 1976
- American Society of Mechanical Engineers
  Honorary Member, 1979
- Department of the Army Distinguished Civilian Service Award (twice), 1963, 1978
- University of California – Morrough P. O'Brien Hall, 1969
- General Electric Company Propulsion Hall of Fame, 1984
- State University of New York at Stony Brook, M.P. O'Brien Fellowships, 1987
- Symposium to Honor Morrough P. O'Brien – Working Solutions: Shore and Beach, March 23-24, 1987
- Elected to National Academy of Engineering, 1969
- Member of the Bohemian Club, San Francisco, since 1934 and of the Cosmos Club, Washington, D.C., since 1936
- Director, McGraw-Hill Publishing Company, 1958-71

In 1928, while in Sweden as a Freeman Scholar, he received an offer from the University of California of an assistant professorship. Upon arrival at Berkeley, he found that he was appointed to teach mechanical engineering, not his field of civil engineering. He has said the most important single event that shaped his professional career was being assigned to teach mechanical engineering, as it forced him to study the applied sciences related to mechanical engineering; namely, fluid mechanics, heat transfer, thermodynamics, dynamics, chemical engineering, and mathematics; his colleague L.M.K. Boelter was an inspiration in this period.

O'Brien lived at the Faculty Club during his first three years at Berkeley, and there he met, usually across the dinner table, many senior faculty who were there for meetings of academic or administrative committees. He came to know Gilbert Lewis, George Lauderback, Armin Leuschner, Charles Lysman and many other faculty leaders, and learned from them much about the complicated mechanisms and procedures of a large university. These were also the exciting times of Ernest O. Lawrence (who also lived at the Faculty Club), the cyclotron, and the ensuing era of high energy physics. Lawrence and Robert J. Oppenheim were appointed assistant professor in the same year as O'Brien. The three became good friends; they spent the long winter break between semesters in 1929 touring Death Valley, Hoover Dam and the Arizona desert; they spent Christmas in Phoenix with O'Brien's parents. These associations greatly influenced his views regarding the importance of research in a modern engineering school.

As a part of his activities during his first few years at the University, O'Brien was in charge of the Hydraulic Engineering Laboratory (1928-37), modernizing and expanding it substantially (for example, adding a water wave channel). He worked with others (E.D. Howe, R.G. Folsom and J.E. Gosline) on the preparation of a laboratory manual. He did much work with pumps (water jet pumps, hydraulic ram, propeller pumps and fans, air lift and gas lift, centrifugal pumps), both theoretical and experimental. He gave courses on elementary hydraulics, water turbines, and pumping machinery.

In 1929, O'Brien was appointed Civil Engineer for the U.S. Army Board on Sand Movement and Beach Erosion, and initiated research by this Board on coastal engineering. He made field studies of coastal processes along the New Jersey coast from Sandy Hook to Cape May. His first practical problem was on the effects on the adjacent beach of a jetty built at Far Rockaway, Long Island, New York. From this work, he developed a lifelong interest in beaches, tidal entrances, jetties, groins, seawalls and the effects of structures on beaches. In 1930, he made field studies along the coasts of Washington, Oregon and California and wrote a detailed seven-volume report on the results of his observations. A "landmark" paper on the relationship between tidal prism and entrance area was a result of these studies.

Starting about 1933, the faculty and graduate students of the Guggenheim School of Aeronautics at the California Institute of Technology and of the Department of Mechanical Engineering at Berkeley began holding joint seminars for two or three days annually at Pasadena or Berkeley. Dr. Theodore von Karman started these meetings to compensate for the infrequency of technical meetings in California in the days of railroad travel. These meetings were held up to the time of the defense build-up before WWII.

In the mid-1930's O'Brien worked on the problem of sand bypassing and beach restoration at Santa Barbara, California, as a consulting engineer for the Board of Harbor Commissioners of the City of Santa Barbara. As a part of this study, he prepared what was probably the first quantitative wave refraction diagrams by an engineer. During this same time, he built the first tidal model in the U.S.A., the estuary of the Columbia River. This was constructed in 1934 for the U.S. Army Engineers in the Hydraulics Model Basin (formerly the Hearst Hall Swimming Pool). Details of its design, construction, operation and results were presented in a paper on models of estuaries at the 1935 annual meeting of the American Geophysical Union. A small towing tank was built adjacent to the outdoor basin for use in ship model towing tests.

O'Brien summarized many of his observations and thoughts on beach processes, and the effects of structures on beaches, in his paper "The Coast of California as a Beach Erosion Laboratory" (*Shore & Beach*, July 1936). In this paper he concludes that the coastline of California is "divided naturally into sections which appear to be integral units as regards beach erosion and related phenomena, ... which differ in exposure to wave and wind action, in planform, in character and supply of beach material, and in the types of structures suitable for beach preservation." He also stated that "... a beach is merely part of a stream of material in process of being transported from the land surfaces to the ocean depths, ... and that extensive debris control works are being constructed along the drainage channels which originally supplied material for the most valuable beaches in the state, ..." He then recommended that studies be made to provide the basic information required to plan to compensate for the reduced supply of stream debris (sand).

In 1938 he was appointed a member of the Beach Erosion Board, U.S. Army Corps of Engineers, and served on it until it was abolished in 1963, when he was appointed to its successor, the Coastal Engineering Research Board, serving from 1963 until 1978, for a total of forty years on the two Boards.

O'Brien published a number of technical papers in the 1930's, several of which are "landmark" papers. A number of these were on pumps, flow in porous media, sediment transport in streams and in pipes, sediment transport by wind, and cavitation. He continues to do research and publish papers on coastal engineering. During the 1930's his only graduate students were Army Corps of Engineers officers, and O'Brien worked with them on laboratory studies of wave phenomena. In 1939 his classis book, *Applied Fluid Mechanics*, written with George H. Hickox, was published. In 1939 he also prepared a long-term program of research on wave action, shoreline phenomena, and shore protection for the U.S. Army Beach Erosion Board laboratory.

Around 1938, the American Society of Mechanical Engineers appointed a Committee on Hydraulic Friction to promote the study of such questions as the relationships of measured surface roughness and surface friction. Dr. Theodore von Karman was chairman and O'Brien was secretary. One of the other members was Professor Louis Moody of Princeton, who developed the Moody diagram for friction loss. This committee held only three meetings before Dr. von Karmam became so much involved in the program of the Army Air Corps that he could no longer participate in this work. O'Brien spent the summer of 1939 at Woods Hole as a member of Dr. von Karman's group studying the future of the Air Force. (Note: In 1962 he toured the NATO nations with a DOD committee reviewing the Military R&D programs.)

The years of World War II were extremely busy for O'Brien, serving as Chairman of the Mechanical Engineering Department until 1943, when he was appointed Dean of the College of Engineering, and also Chairman of the Department of Engineering. He was Executive Engineer of the Radiation Laboratory under Professor E.O. Lawrence, 1942-43. O'Brien was asked by E.O. Lawrence and General Groves, the Manhattan Project Director, to recruit an engineering team to design the production facilities at Oak Ridge for the electromagnetic system. O'Brien has said that probably the most important thing he has done in his life was to convince them that there was not time to build up a competent staff, and that they should hire companies with engineering staff in place to do the job (Allis-Chalmers, General Electric, and Westinghouse). He was in charge of the Statewide University of California Engineering Science and Management War Training program, 1940-44, which registered 46,000 students under 1,800 instructors. For the U.S. Navy Bureau of Ships he worked on underwater sound, on cavitation generated by submarine propellers (the results from which were used by submariners almost immediately), and on the design and operation of amphibious craft. With Professor H.U. Sverdrup of the Scripps Institution of Oceanography, he worked on the forecasting of waves. He directed a program of field and laboratory studies of landing craft for the U.S. Navy Bureau of Ships. R.G. Folsom, J.W. Johnson, J.D. Isaacs, J.A. Putnam, and W.N. Bascom and many others worked with him on this project. O'Brien's intuitive understanding of fluid mechanics and his thorough knowledge of similitude and model laws led to his suggesting to Sverdrup that ocean wave forecasting procedures should be based on the empirically determined relationships between the dimensionless parameters gH/U^2 and gL/U^2 and the independent variables gF/U^2 and gt/U, where g is the acceleration of gravity, F is the fetch, U is the wind speed, H is the wave height, L is the wave length and t is the duration the wind flow over the fetch. This is the classic relationship that has been in use for more than four decades, with credit for their formulation generally given to others. The original formulation is in a memo from O'Brien to H.U. Sverdrup and W.H. Munk, on 31 December 1943, with an expanded version on 5 September 1944; the first use of this nondimensional presentation appears in SIO Wave Report No. 30, 7 December 1944. In 1946 he was a consultant to the Oceanographic Section, Joint Task Force One on Operations Crossroads, and participated in the Atom Bomb Test at Bikini.

During the first decade after WWII O'Brien devoted much of his time to building the modern College of Engineering, emphasizing graduate programs and research. He also actively supported continuing education, and close cooperation with the State's college and community college systems. He worked out with the University President, Robert Gordon Sproul, the policy that engineering faculty should be able to spend up to 25 percent of their time on research and that their entire pay during the academic year should be from the University budget, not from "soft money." Plans were presented to Sproul in his ten-page report "Post-War Plans of the Department and College of Engineering," (22 June 1944). He brought to the Faculty many young active research-oriented professors, often with backgrounds in applied mathematics, chemistry and physics. He was responsible for the establishment of a number of research and teaching laboratories, the formation of the Institute of Engineering Research within the College, to help faculty in the administration of research contracts and grants. Some of the major programs developed were: Electronics Research Laboratory (John R. Whinnery and S. Silver), Prosthetic Research (H.D. Eberhart), Metallurgy and Ceramics (John Dorn, Earl R. Parker, J.A. Pask and A.W. Searcy), Ship Welding Research (Earl R. Parker), Low Pressure Project (R.G. Folsom, Enos Kane, G.J. Maslach, S.A. Schaaf), Naval Architecture and Marine Engineering (Carl J. Vogt, H.A. Schade, J.V. Wehausen and J.R. Paulling, Jr.), Operations Research and Industrial Engineering (Erick Thomsen and Ron Shepherd), and new programs (structural dynamics, soil mechanics and geologic engineering, sediment transport, coastal engineering and construction engineering) in Civil Engineering (R.W. Clough, E.P. Popov, H.B. Seed, P.D. Trask, H.A. Einstein, J.W. Johnson, R.L. Wiegel). This decade was also a time of major building on the Berkeley campus, and the establishment of the Richmond Field Station (1950). Located at the Richmond Field Station were the Institute of Transportation and Traffic Engineering, the Sanitary Engineering Research Laboratory, the Sea Water Conversion Laboratory, portions of the Structural Engineering Materials Laboratory and the Hydraulic Engineering Laboratory, the Naval Architecture Laboratory, the Low Pressures Laboratory, the Propulsion Dynamics Laboratory, and the Office of Research Services. During this time the College of Engineering's reputation grew and was ranked with MIT nationally. This aspect of his career is covered in the papers by John R. Whinnery and Clark Kerr.

He continued to be active in research. An example is the development of what should be known as the O'Brien-Morison Equation for calculating wave-induced forces on bodies, used world-wide in the design of off-shore structures. From 1945 to 1955 was the time of rapidly expanding research on coastal engineering; with field, laboratory and theoretical studies. A number of young researchers worked under his and J.W. Johnson's direction at that time: R.L. Wiegel, C.L. Bretschneider, R.C. Crooke, R.C. MacCamy, R.A. Fuchs, F.A. Snodgrass, J.R. Morison.

In 1952, he and Professor J.W. Johnson started what are now known as the International Conferences on Coastal Engineering (the 20th was held in Taipei, Taiwan, ROC, in November 1986). The first conference was held in Long Beach under the auspices of the University of California Engineering Extension, with each paper being by invitation, and chosen so that the total presentation was to represent the "state of the art" of coastal engineering. This conference was followed by one in Texas, with case studies in the Gulf of Mexico, followed by the third in Cambridge, Massachusetts, on the U.S. East Coast, and by the fourth in Chicago on the Great Lakes. Pierre Danel of France suggested another one was needed to emphasize European works, and this was held in Grenoble, France, internationalizing the conferences. Details of the development of this major activity on a world-wide basis are given in the paper by J.W. Johnson.

During the two postwar decades, he was a member of the Army Scientific Advisory Panel (1954-1965), being Chairman, 1961-65; member of the Defense Science Board (1961-65); a member of the National Science Foundation Board, 1958-60, (a Presidential appointment); Chairman of the Committee on Amphibious Operations, National Research Council (1952); he also served on other NRC committees, including the Committee on Operations Research, 1952, the Ship Study Committee, 1960, a subcommittee of the Maritime Research Advisory Committee, 1958-60.

A new building was constructed on the Berkeley campus in 1957-58 to house the Hydraulic Engineering Laboratory, moved there from the Mechanics Building (constructed in 1893, where the laboratory had been moved from the Mining and Mechanic Arts Building, built in 1879), and the Engineering Library. It is named O'Brien Hall, in honor of Morrough P. O'Brien. It is of historical interest that Hunter Rouse (who was from the same hometown as O'Brien) states in his book, *Hydraulics in the United States, 1776-1976*, that the Laboratory at Berkeley is probably the first hydraulics laboratory for instructional purposes in the United States, with a printed report in June 1883 by some students of Professor Hesse, supplemented by a colleague's description of the efficiency tests on the Pelton unit, which refers to the water wheels, pressure gages, etc., in the Experiment Department of the Mechanical Laboratory.

O'Brien retired from the University of California in 1959 as Professor Emeritus of Engineering and Dean Emeritus, College of Engineering. He has continued to be active in university education. Just prior to retiring, during the academic year 1958-59, while on leave from the University, he was Visiting Institute Professor at Massachusetts Institute of Technology and Visiting Research Fellow at Harvard University. After retiring, he was Visiting Professor of Engineering at Purdue University and at University of Notre Dame, Consultant in Educational Plans at both Rensselaer Polytechnic Institute and at Polytechnic Institute of Brooklyn. He served in an advisory capacity to Duke University and the University of Kentucky in 1967. He has been on the Engineering Advisory Board of the University of California since 1960. Since 1968 he has been part-time consultant and Professor of Coastal Engineering at the University of Florida, participating in many of their research projects, in collaboration with Professor Robert G. Dean; he has been the Principal Investigator for several projects.

O'Brien believed that an engineering faculty should include a number of individuals who had practiced the profession at a high level of responsibility. When Air Reduction Company offered him an appointment as Director of Research and Engineering, he accepted conditionally, on leave from the University for eighteen months. During the years 1958-61, he worked with the Kaiser Aluminum and Chemical Corporation on the formulation of a long-term research program. Otherwise, his work with industry has been limited to the General Electric Company, except for occasional acceptance tests on pumps or other hydraulic devices.

O'Brien's long association with the General Electric Company began about 1949 when a group of engineers at Schenectady designed the compressor for the first American axial flow jet engine. In this compressor design, the flow system was laid out exactly in accordance with the method presented in a paper by O'Brien and Folsom entitled "The Design of Propellor Pumps and Fans." Tests of this compressor showed the predicted characteristics and it was incorporated in what became the J47 engine with a production run of thousands. Early in the development of this engine, the project was moved to Lynn under a different group of engineers who were unaware of the origin of the compressor design. O'Brien did not learn of his tie to the J47 engine until almost fifteen years later when he happened to meet the compressor designer. In spite of this lack of communication, Lynn engineers recommended that O'Brien be a consultant. He has been on an annual retainer with General Electric from 1949 to the present. O'Brien's special skill lies in the identification of key problems in high-technology development programs. Departments and divisions for which he consulted included: Aircraft Engines (Cincinnati and Lynn), Aircraft Nuclear Propulsion (Oak Ridge, Tennessee, and Lockland, Ohio), Re-entry Vehicles (Philadelphia), Space Division (Valley Forge), Light Military Electronics (Utica), Heavy Military Electronics (Syracuse), Ordnance (Pittsfield), Apollo Support (Daytona Beach), Nuclear Power (San Jose), Corporate Research and Development and others. In his autobiography *(Herman the German*, William Morrow and Company, Inc., 1984), Gerhard Neumann, who was Vice President and General Manager of the Aircraft Engine Group from 1963 through 1979 described O'Brien as follows:

> "GE's outstanding success with the GOL-1590, the J79, the nuclear aircraft engine and many more of our advanced programs would likely not have happened without the help of two extraordinary people.
>
> "One was Morrough P. O'Brien, the former dean of engineering of the University of California at Berkeley, a real engineer. His responsibility as a consultant was to earn the confidence of our engineers at all levels, visiting them individually at their desks, or having lunch with them in order to ferret out (not solve) problems which otherwise might not have surfaced. (O'Brien continued to consult for me for twenty-five years.)"

In his capacity as President and then Immediate Past President of the American Shore and Beach Preservation Association, O'Brien has been very active in providing sound advice to engineers, scientists, government officials and others in regard to the use and preservation of beaches, and in encouraging the continued study of beach processes to provide inputs for coastal planning. Perhaps his attitude towards beaches, an integration of nearly six decades of involvement, can be summed up in the title of his October 1983 editorial in *Shore & Beach* – "Beaches are for People." He is still active in this effort. In the editorial on support needs for research on coastal engineering appearing in these proceedings of the symposium held in his honor, he proposes the following program:

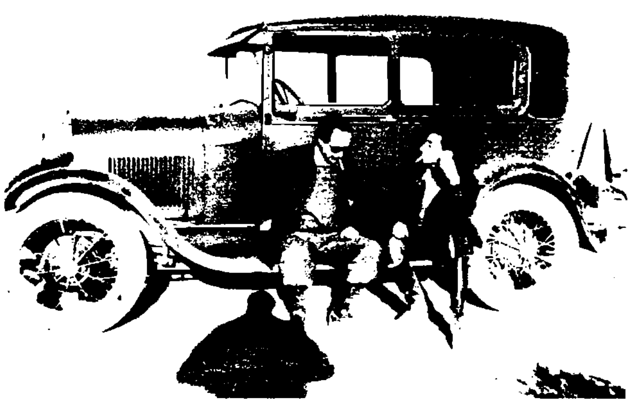
"• NSF program similar to that for Earthquake Engineering. Funding up to $10M/year in third and successive years.
• Field stations on the Pacific and Gulf Coasts like DUCK in North Carolina. Under direction of CERC.
• Large facility for physical models of basic processes administered by a university active in Coastal Engineering Research and Instruction."

O'Brien has thought much about engineering, its relationship to the public welfare, and engineering as a profession. It seems fitting to conclude this brief biographical sketch by capturing, in part, his opinions on these interrelated subjects, using three quotations from the commencement address he gave in Berkeley to graduates of the engineering class of 1976.

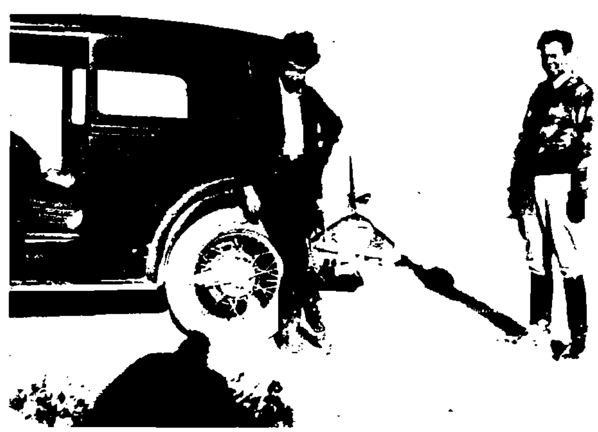
"The activity characteristic of engineering is design – not drafting or routine calculation – but design in the broad sense of (1) isolating a worthwhile technological objective, (2) conceiving a means of achieving this objective, (3) analyzing this concept for physical and economic feasibility, and (4) realizing the design in a physical system and proving its safety and effectiveness. Relatively few individuals have the combination of knowledge, experience, and creativeness for the first two steps in this process; a much larger number, who do most of the engineering, measured quantitatively, are qualified for analysis, test, and construction of production. One is not better than the other – only different."

"Criticisms of the engineering profession imply that the engineer has decision-making authority over development which have a major effect on the public welfare, and this implication is erroneous. The engineering profession has done – and done exceedingly well – what society asked of it. Broadly speaking, the charge was, in the early days of the profession, to replace manual labor by machines; more recently, it was to improve the productivity of labor and capital and thus to permit the application of an increased portion of our resources to the enrichment of our lives. But *society* must decide what it wants – and must decide through the appropriate channels what use it will make of available resources. When public policy came to require that the impact on the environment must be considered in the development of engineering projects, the profession responded; today, a large engineering force nationally is engaged in environmental work."

"At this time of commencement, you should be proud of reaching the goal which you set for yourself when you chose engineering. Now you face another decision regarding the use you will make of the knowledge and the skills which you have acquired. Graduates with an engineering education have open to them a tremendous range of opportunities – and each one of you should appraise your own interests, your capabilities, your criteria of success and match these personal specifications against the opportunities offered. The lucky people in this world are those who find out early what they like to do, what they can do well, and what they will be paid for doing. When these factors coincide, both a successful career and a happy life are almost assured."



Morrough P. O'Brien and Robert J. Oppenheim, Death Valley, California, December 1929. Photo by Ernest O. Lawrence.



Robert J. Oppenheim and Ernest O. Lawrence, Death Valley, California, December 1929. Photo by Morrough P. O'Brien.

A Bit of History on Some Important Contributions to Coastal Engineering by Morrough P. O'Brien

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INTRODUCTION

STARTING MANY CENTURIES B.C., shoreline structures, such as harbors, lighthouses, etc., have been built primarily for the navigator. Around the turn of this century the recreation features of the shorelines started to become of importance and continued to increase sharply as the means of transportation from the cities to the beaches continued to improve. It was in the 1920's that a conflict started to appear between the interests of the navigator and the users of the beaches for recreation. Although not fully understood at the time, the structures built to stabilize entrance channels to harbors also served as barriers to the longshore transport of beach sand by wave action. Since waves have a predominant direction, depending on the locality, sand will deposit on one side of the entrance structures and erosion of the beaches occurs on the downcoast side – sometimes for many miles. Without full knowledge of the littoral processes involved, many and varied types of devices were constructed at many localities in an attempt to stabilize the shoreline. Since a source of sand might not be generally available to replace the sand moved downcoast by wave action, many of the structures which were poorly designed and maintained failed, with the result that many beaches were hazardous to the user. A good example was the popular New Jersey shoreline which could easily be reached by public transportation from such population centers as New York and Philadelphia.

With the rapidly increasing use of the New Jersey shoreline, the State of New Jersey took action by establishing the Engineering Advisory Board on Coastal Erosion of the State of New Jersey in 1922 to study their critical problem.[24] Also, the New Jersey State of Commerce and Navigation was very much aware of the erosion problem and issued a report in 1922 and a second report in 1924. In the meantime, D.W. Johnson of Columbia University was pursuing his interest in coastal matters and was instrumental in the formation of a Committee on Shoreline Studies of the National Research Council in Washington, D.C. As part of this Committee's investigation, it conducted a survey in 1926 of some form of organized effort to deal with coastal problems.

Although response to this survey was favorable, it was officials from the State of New Jersey who took action. Mr. J. Spencer Smith, President of the New Jersey State Board of Commerce and Navigation, discussed the project with the New Jersey Governor – the outcome of which the Governors of the Atlantic and Gulf Coast States were appointed as delegates to a meeting to consider the subject.[24] A group of 85 delegates, representing 16 coastal states, met first at Asbury Park on October 14 and 15, 1926, with later meetings at Norfolk, Virginia, and Washington, D.C. From these meetings emerged an organization known as the American Shore and Beach Preservation Association (ASBPA). This Association was incorporated in the State of New Jersey on December 8, 1926, with Mr. J. Spencer Smith as president for the first year, but he continued as president of the ASBPA until his death in 1953. Later, Professor O'Brien served as the fourth President of the ASBPA from 1972 to 1983. For a while the ASBPA adhered to the policy that the states were responsible for shore protection, but, in time, pursued a different policy – mainly that the Federal Government should be responsible for the study of coastal erosion problems.

In January 1929 the Chief of Engineers set up a board composed of four officers of the U.S. Army Corps of Engineers. The purpose of this board was to investigate and report on the subjects of sand movement and beach erosion at such localities as designated by the Chief of Engineers; hence, it became known as the Board on Sand Movement and Beach Erosion. The Board authorized a number of field studies. Two field sites were set up on the New Jersey coast – one at Long Branch and the other at Seaside Heights. Morrough P. O'Brien, Assistant Professor of Mechanical Engineering at the University of California, Berkeley, and 1st Lt. Leland H. Hewitt were placed in charge of these projects and work was started in May 1929. This program included measurements of waves, winds, currents, tides, beach profiles, sand samples, and tracer studies. Also, a catalog of groins and other similar structures between Sandy Hook and Cape May was started. Surveys were made around a number of these structures to provide a base line for relating subsequent shoreline changes to waves and winds.

It is of importance to note that the Corps' Board on Sand Movement and Beach Erosion was succeeded on September 18, 1930, by the Beach Erosion Board (BEB). Professor O'Brien served on the Board from 1936 until his retirement in 1978. The name of the Board was changed to the Coastal Engineering Research Board (CERB) in 1963. In addition to Professor O'Brien's early work on coastal sediment problems it is important to review some of his substantial contributions to the solution of other coastal engineering problems in the following section.

COASTAL ENGINEERING RESEARCH

Wave Refraction

The early work of D.W. Johnson[4] suggested to Professor O'Brien that a quantitative estimate of the relative distribution of wave height along an irregular shoreline should be possible. Applying Snell's Law, Professor O'Brien developed a procedure for graphically plotting the advance of a wave to a curving shoreline over a mildly irregular bottom.[19] In 1936 the earliest known refraction diagram was prepared by Professor O'Brien and was used in appraising the serious problem of beach erosion at Santa Barbara, California. The wave refraction technique developed by Professor O'Brien is used world-wide today in a wide variety of shoreline problems.[7]

Shoreline Models

The experience with various shoreline structures suggested to Professor O'Brien that a systematic series of laboratory experiments might well provide a better understanding of the basic processes involved and thereby lead to a more rational approach to the design of coastal structures. Several graduate students at the University under the direction of Professor O'Brien conducted experiments on (a) a model of the beach at Long Branch, New Jersey, to determine the extent of the similarity between model and prototype where the transportation of sand by longshore currents and wave motion was studied;[2] (b) a flat ocean bottom of sand with a cliff which was subject to erosive action of waves (groins were placed similar to those in the prototype to evaluate their effect on the beach);[2] (c) the erosive action of waves on a beach located at the base of a bulkhead (the beach slope was varied to determine the effect on filling and scouring); (d) the refraction of waves entering a bay with an opening about one-fifth of the width of the bay (the equilibrium shape of the shoreline was established);[2] (e)seasonal changes in beach profiles;[10] (f) equilibrium slope of beaches;[28] (g) wave refraction around an island;[29] (h) wave refraction in a submarine valley;[11] (i) wave refraction around a breakwater;[9] and (j) equilibrium slopes in front of seawalls,[3] and sand transport along a beach by wave action.[25]

Wave Forces on Structures

Pile Structures. Offshore drilling for oil was first practiced at Summerland, California, by "whipstocking" from shore in 1894. Later there were other wells located in this area on relatively lightweight piers extending into the ocean. The first well in heavy seas was drilled a short distance off the Louisiana coast in 1938. The first well ever drilled out of sight of land was in 1946, also off the Louisiana coast.[1]

With the advent of extensive drilling from platforms located many miles offshore in the Gulf of Mexico, the problems of the design of pile structures to resist heavy wave action began to receive considerable attention in the late 1940's. The methods used in designing the early offshore platforms is not known to the writer, but what appears to be the first published paper on wave forces on piling was that of Munk.[13] This method, however, only considered the drag force resulting from the oscillatory flow of waves past the pile. In 1949, Professor O'Brien with his experience on shiptowing model studies reviewed the theory of wave forces on piling and suggested that an additional term should be included in the force equation to represent the inertia force on the displaced volume of the fluid. In this equation, the total force is obtained by adding linearly the drag and inertia terms.[12] The theoretical studies were followed by both laboratory and field studies to determine the experimental coefficients which appeared in the force equation. This relationship is now used world-wide in computing the wave forces on piling.[30]

Tidal Inlets

The engineering problems encountered in tidal lagoons and estuaries are numerous and complex and involve such phenomena as tidal currents, mixing, stratified flow, sedimentation at both the entrance and within the tidal basin, etc. Many of the early problems in estuaries were concerned with the difficulties of maintaining adequate depths at the entrances for navigational purposes.

In 1930-31 Professor O'Brien made an exhaustive study of inlets and beaches of the Pacific Coast of the United States;[14,17] one result of this study was the elucidation of the relationship between the inlet area below mean sea level and the tidal prism.[15] Later a study of the Sacramento-San Joaquin River Delta showed that the flow areas of the interior channels depended upon the upstream tidal volume. The same study revealed the fact that the minimum flow area of the inlet connecting the San Francisco Bay with the ocean and the distance from the inlet throat to the crest of the outer bar were approximately linear functions of the tidal prism. Later Professor O'Brien reanalyzed all available data on areas of tidal inlets and their corresponding tidal prisms, on the East, Gulf, and Pacific coasts of the United States.[20,22]

An important aspect of flow in many estuaries is that of the interaction between the fresh water flow from upland drainage and the salt water from the ocean. One of the early theoretical treatments of density flow was that of G.I. Taylor[27] who showed that a form of Froude number governed the scaling of models involving this flow phenomenon. Perhaps the first engineering application in the United States to modeling a density flow was that of O'Brien and Cherno[16] in connection with the investigations of a proposed salt-water barrier in San Francisco Bay. In many present-day estuarine model studies, the form of Froude number used by O'Brien and Cherno is referred to as the densimetric Froude number.

Tidal Estuaries

A model of the estuary of the Columbia River was built and tested at the University of California, Berkeley, during the years 1932 to 1936, to study the effects of proposed changes in the navigation channels on the currents and sediment movement.[21] The project was sponsored by the North Pacific Division of the Corps of Engineers, and the engineering results were reported at that time in internal memoranda.[5,6] The basis for the selection of the scale ratios and other factors affecting the design of the model were reported in some detail,[18] but only a brief note was published regarding the operation and the accuracy of the model.[5] In some respects this model is still unique, and the published description of it should be of interest to the coastal engineers.

COASTAL ENGINEERING CONFERENCES

Design procedures for coastal projects prior to about 1930 were based mostly on "cut-and-try" experience and some relatively limited relationships based on field observations. For example, the Stevenson formula[26] for wave height was developed in the middle of the last century but was still in use in engineering books as late as 1961.[23] In general, development was gradual up to about World War II when research efforts accelerated rapidly, and the increase in knowledge of the fundamental mechanics of processes in the nearshore zone increased many-fold.[8] Postwar research in various coastal engineering areas has continued at a relatively high rate, but much of the data of value to the engineer were unpublished or scattered in various journals. This condition led to Professor O'Brien's belief that a conference should be held with invited authorities to present papers covering various areas of value to the engineer. It appeared that to accomplish this objective expeditiously would be to conduct a local meeting by the University of California for engineers and scientists interested in shoreline problems. A list of areas to be presented and the most knowledgeable researchers to discuss the special fields was prepared. The response was enthusiastic, and arrangements were made to hold the meeting entitled The Institute on Coastal Engineering at Long Beach, California, October, 1950, under the joint auspices of the University Extension at Berkeley and Los Angeles. Each author provided mimeographed copies of their papers – and there the course was expected to be completed – but not so. In March, 1949, Professor B.A. Bakhmeteff of Columbia University, who also served as Director of Research for the Engineering Foundation, visited Berkeley to present a public lecture on The Engineer in the World of Today. In a discussion among Professor O'Brien, Professor Bakhmeteff, and others of the hydraulic engineering staff at Berkeley, it was noted by Professor Bakhmeteff that the Engineering Foundation supported various Councils, such as column research, etc., but nothing on water waves and related fields. He urged Professor O'Brien to submit a proposal to The Engineering Foundation for a Council on Wave Research. This was done, and in due time the Council was approved by the Directors of The Engineering Foundation.

The first act of the Council was to request funds from The Engineering Foundation to transfer the mimeographed material from the Long Beach Conference to a book, which after some discussion, was titled, *Proceedings of the First Conference on Coastal Engineering.* Sales from each Proceedings paid for the publication of the following conferences, i.e., Houston (1951), MIT (1952), Chicago (1953), Grenoble, France (1954) – thus becoming an international organization. The 20th and most recent conference was held in Taipei, Taiwan, in November 1986. The 21st Conference will be held in 1988 near Malaga, Spain.

So much for history, but in summary it is desirable to reprint, as follows, a part of the preface of Professor O'Brien from the *Proceedings of the First Conference on Coastal Engineering.*

> "A word about the term "Coastal Engineering" is perhaps in order here. It is not a new or separate branch of engineering and there is no implication intended that a new breed of engineer, and a new society is in the making. Coastal Engineering is primarily a branch of Civil Engineering

which leans heavily on the sciences of oceanography, meteorology, fluid mechanics, electronics, structural mechanics, and others. However, it is also true that the design of coastal works does involve many criteria which are foreign to other phases of civil engineering and the novices in this field should proceed with caution. Along the coastlines of the world, numerous engineering works in various stages of disintegration testify to the futility and wastefulness of disregarding the tremendous destructive forces of the sea. Far worse than the destruction of insubstantial coastal works has been the damage to adjacent shorelines caused by structures planned in ignorance of, and occasionally in disregard of, the shoreline processes operative in the area."

It is of importance to note that Professor O'Brien served as Chairman of the Council on Wave Research from its establishment in 1949 to his retirement in 1978 following the 16th conference in Hamburg, Germany.

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The Waves Project

An illustrated letter to Morrough P. O'Brien

BY WILLARD N. BASCOM
Long Beach, California

ON THE OCCASION of this special symposium in your honor I would like to thank you publicly for the privilege of being a member of the Waves Project at Berkeley in the 1940's. Even though I often got cold and wet, and came pretty close to drowning a lot of times, it was a helluva lotta fun. Those of us on the beach survey party lived in long johns, soggy with rain and sea water; we got used to sand gusting in our faces as we huddled around driftwood fires eating C-rations; and often we worked well into the night digging and winching stuck dukws out of the mud. But it was a great education in the ways of beaches and you were responsible.

Now that 40 years have passed I guess it's safe to tell you some things you may not have heard about at the time.

Did you know that I joined the Waves Project by accident? Well, in October 1945, I had dinner at Spenger's Seafood Restaurant in Berkeley with John D. Isaacs, his wife, sister and infant daughter Cathy. I had just met John and we found much to talk about. He spoke about commercial fishing and forestry in Oregon; I about the life of a mining engineer in the Rocky Mountains. We soon discovered many common interests. Presently, a waiter arrived with a tray holding a bottle of wine and some stemmed glasses. Young Cathy, in a high-chair at my left, flailed about and knocked a wine glass off the tray. I reached out and caught it in mid-air. Immediately John said, "How would you like to work for the Waves Project? The beach party is headed north next week to measure waves and survey beaches and we need an engineer."

I had never even seen the Pacific Ocean but John's improbable stories made it seem adventurous, a matter of greater interest to me than engineering, science, or making a living.

It was not my style to ask foolish questions like: Why would anyone want to do those things? or How much does it pay? Being fancy free, I said, "Sure. Let's go."

The next morning I was formally interviewed by Professor Richard G. Folsom who, after exposing my ignorance of fluid mechanics, reluctantly hired me for $250 a month. This generous offer, he said, was because it would be a short job. I agreed with the short part, expecting to move on to some more useful kind of work after a look at the Pacific Coast.

Before the party left Berkeley, I became aware that the hierarchy reached above Isaacs and Folsom to an unseen grey eminence named Dean O'Brien. Just before we left to survey the coast north of Cape Mendocino his first pronouncement descended to my level: "If you can work in the north coast surf in the winter you can work anywhere." Now, having spent some rough years around the Pacific, off Tasmania and along South Africa, and had a chance to compare, I think you were right, Mike.

The Project's principal equipment was two dukws; six-wheeled trucks with a thin metal skin around them that served as surf boats. Isaacs and I each drove one up the old coast highway until we reached Humbolt Bay. There we stood on a dukw deck and looked out across the bay and the sand spit at a white froth of churning water a half-mile wide. "Some of those breakers must be 30 feet high," said John in a conversational tone. "That's where we'll work." To a guy from Colorado that seemed to be a reasonable size for waves on an ocean as big as the Pacific.

In a couple of days we had set up camera stations at Table Bluff and staked out a 4000 foot long base line well above the winter berm (Fig. 1). Then John

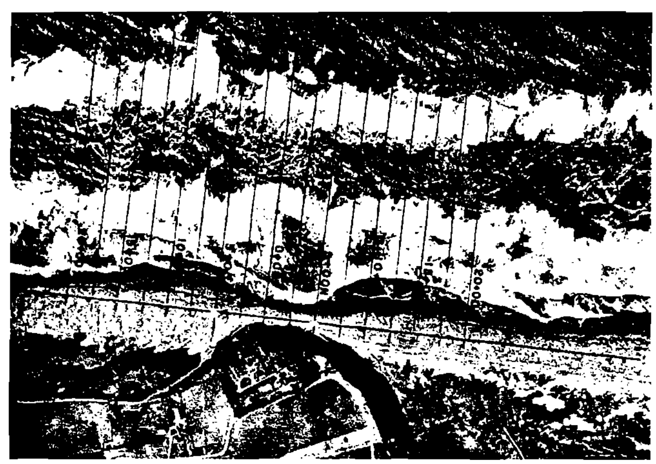


Fig. 1 Beach profile locations at Table Bluff, California, near Eureka.

explained the rest of the system. *"You* stand on the bridge of the dukw, heave the lead and call the water depth into the microphone. *I'll* read the angle on the transit and ..." Soon it dawned that my job on the beach party was to go in and out through at least two sets of breaking waves in a tin truck with a foot of freeboard, where it would be normal to encounter breakers 12 to 15 feet high (Fig. 2). John, as party leader, stayed on the beach and ducked sympathetically when he saw a breaker go over the dukw.

Mike, I was led to believe this was your idea of on-the-job training for coastal engineers. It was certainly an effective method of getting us to concentrate on waves; luckily we all survived.

As the dukw bucked out across the bars to the starting point of each run (Fig. 2), each encounter with a breaker would force a fire-hose-like jet of cold water through the corners of the windows, or sometimes rip the canvas over the driver's seat.

Returning through the breakers was more exciting. After lining up on a pair of striped range boards on the beach and checking the radios, we would select a lower-than-average wave for the ride across the outer bar and in we would go. I would call "Mark," heave the lead in the wave trough, estimate the height of the breaker hovering above us, and add one third that height to the sounded depth. As I hollered the corrected depth into the microphone, the stern of the dukw would rise and it would start to surfboard; the driver would gun the engine, fighting to keep us from broaching, and the crest would crash down on us. Our speed would increase to about 12 knots and the dukw's down-tilted bow would be shoved up to the windshield in the trough ahead; often we expected it to flip. Then, as the wave crest passed under us, the dukw would float up to the top of the broken wave front, its deck eight to 10 feet above the flat water ahead.

When it finally rolled up on the beach, the driver would threaten to quit, and I would remind him that this was really a lot of fun. "Where else could you get a job like this?"

Mike, you sure were lucky we didn't drown – especially since we never wore life jackets (Fig. 3).

One day Dick Folsom arrived at Table Bluff with his new wave recorder. The pressure sensing head was mounted on a tripod, connected to shore by a half-inch neoprene-covered cable. He had brought a couple of thousand feet of the cable, enough to reach just beyond the inner bar. There we dropped the tripod and laid the cable ashore; Folsom connected it to a recorder set up on the beach, and happily began recording waves. Nothing to it! But the next morning when we returned we found the entire cable was now concentrated in one huge knot on the beach and the tripod irretrievably buried in the sand. Scratch that system.

Eventually it occurred to me to ask John why we didn't survey the northern beaches in summer and the southern ones in winter. "Dean O'Brien wants it this way," was the answer.

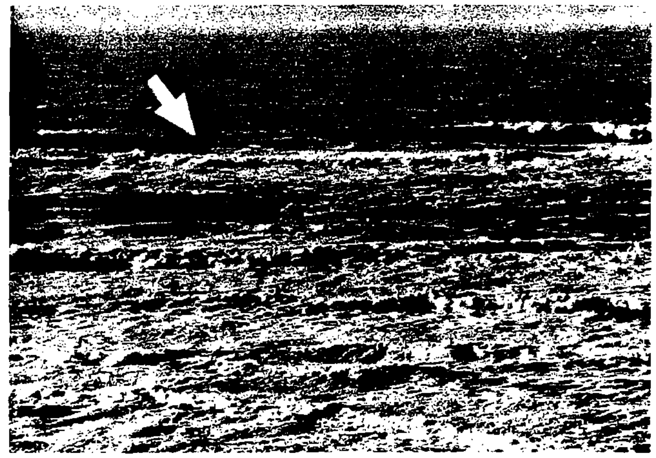


Fig. 2 Dukw in surf zone at Table Bluff (photo taken with 20-inch focal length camera).

Fig. 3 Willard Bascom (left) and Dukw driver Wilson (right).

"You sure you got that straight?" I said. Did he, Mike?

Early in 1946 we pushed on north into the gloom and drear, surveying beaches that became flatter as we went northward with wider surf zones. These included Point Lookout, Kinchaloe, Clatsop spit, Oysterville, Grey's Harbor, Copalis, and Cape Grenville (Fig. 4). In Astoria, Oregon, in the midst of hundreds of beach profiles, I discovered Rhoda. That was forty-one years ago this month. I thank you for that too, Mike. She's here with us today.

About that time President Truman decided to test nuclear weapons against a fleet of old warships at Bikini. You were, of course, involved and sent Isaacs ahead to Bikini leaving me in charge of the beach party. By now the worst of the winter was over, so you allowed the party to come south again. We arrived back in Berkeley on April 1, 1946, the same day a great *"tidal wave"* from the Aleutians swept across the Pacific. On that occasion, Mike, you and I met and talked for the first time. You asked me to make a reconaissance of the California coast with a man from the Corps of Engineers to see what damage the wave had done. So we did, reporting back that commercial fish boats had been heaved onto the coastal road at Half Moon Bay and noting that the wave appeared to be most damaging in bays that faced away form the epicenter.

Isaacs, stalled in Hawaii by a delay in the Crossroads tests, was able to report on the considerable destruction at Hilo Bay, but his best story was the eyewitness account of a young sailor at the Navy's Kaneohe Bay station. This fellow, on guard duty early in the morning, was driving a jeep along a shoreline road when sea level rose around him and began to cover the road. Naturally, he drove up on the nearest high ground which happened to be a storage bunker mounded over with earth. The water surrounding his tiny island was still rising when a dense white cloud reduced his visibility to a foot or two. He had taken refuge above a stockpile of water-activated smoke screen chemicals, and hours after the water had subsided he still waited to be rescued.

I was left behind to survey beaches in the Monterey area while you and Isaacs went off to watch the Crossroads tests. Although the waves from Baker shot were only eight feet high at Bikini island, the photos taken to measure them permitted Isaacs to make precise measurements of ship positions that greatly increased the accuracy of blast and radiation measurements made by others.

Mike, you had lived in the Weeks' cottage on Carmel beach for a number of summers and had often paced off the beach width and observed sand changes. Moreover, you were of the opinion that Carmel Bay is a "closed system," meaning that the total amount of sand remained the same but shifted off-and-on shore and from the north end to the south end, in accordance with wave action (Fig. 5). So the beach party began a detailed survey at Carmel. At the end of that summer you brought the whole family along as we drove from Monterey to Santa Cruz on the sand aboard the dukws (Fig. 6).

A few months later a pair of jettys made of piling was under construction at Moss Landing. They were half completed when the beach party drove past them, and I was startled enough by the flimsy construction to phone you to say they would not survive the next storm. You passed that on to the the Corps of Engineers, but they were not impressed by that unsolicited opinion. However, in a couple of weeks the storm came and the jetty failed as forecast.

For awhile we studied the water-table in beaches by digging holes and plotting heights during changes in tide. Puzzled by the data, I showed it to you, and after due consideration, you settled the matter with an unshakable statement. You said, "Water always runs downhill." I've never forgotten that.

Our closest collaboration in the 1940's was on the growth of the Santa Barbara harbor sand spit – and the consequent denudation of the beaches to its east. We surveyed that spit once a week for a year or more, (concurrently measuring winds and waves) in order to get the causes and rates of littoral transport along that section of the coast.

Dick Folsom sent his new model wave meter down, with Bud Hughes and Frank Snodgrass, to be installed at the approach to Monterey Harbor. The placement was easy and the meter recorded waves from the first moment but, as before, it was out of action by the next morning with a broken cable.

It was pretty clear that real submarine cable was needed if wave recorders were to work. There were insufficient funds to buy any cable, but while passing through the Mare Island Naval Base I spotted a couple of dozen spools of 10 conductor, 1.25 inch armored cable that had been declared surplus.

Now, with confidence in the cable we undertook to install a recorder off Heceta Head on the Oregon coast. We ran a cable out from Cape Creek beach to about 50 feet of water, and the next morning a train of large storm waves about 16 feet high gave us the first really good record of big waves. A couple of weeks later the buoy that marked our tripod was blasted out of the water by the Coast Guard who thought it was a Japanese mine. The pressure sensing head worked for several months until successive storms buried it under sand.

Then we went on to another installation at the Quillayute River (Fig. 7) where the failure was more exciting. The beaches of northern Washington are covered with large logs and stumps. With storm flags flying we placed the pressure head seaward of James Island, ran the cable ashore and under the driftwood into the Coast Guard station. By the next morning the storm had arrived with high winds, pouring rain, and large waves. The new wave meter worked wonderfully, recording large waves with sweeping motions of the pen. Warm, dry and confident we relaxed inside the station. Then Rex Goodwin, who was peering out the window, shouted, "Look at the cable!" We did. A large log rolling about on the beach had snagged it and our heavy cable, with a breaking strength of over 10 tons, stretched through the air above the shoreline, vibrating like a fiddle string. Then it parted and fell back.

Subsequently, we installed wave recorders at Cabrillo Head, Point Sur (Fig. 8), Asilomar and Point Arguello (Fig. 9), the last being an all-out attempt to get a permanent installation. Arguello is a pockmarked point of land rimmed by sheer cliffs about 50 feet high. We decided to set up the cable spool atop the cliff and pull a mile of cable off by hand, figure-eighting it in the two dukws that were lashed together. Then the dukws dragged off enough additional cable to get clear of the rocks, after which they ran out to sea, laying the mile of cable that was aboard.

In order to get the dukws in the water I borrowed a bulldozer from the Coast Guard and built a short road to the beach. Lucky for you it did not roll over and squash me when I drove it off a low cliff. Anyhow, we soon had two pressure heads (one a thermopile) and John Isaac's wave direction indicator working beautifully.

The system should have lasted for years, but the gods were against us. A couple of months after we returned to Berkeley the Coast Guard people began to dump trash in the cove where our cable came up the cliff. One night it caught fire, buring the jute and melting all the insulation, leaving a charred set of wires dangling.

Have you learned anything new yet, Mike?

Between wave recorders we continued the beach surveys and had one incident that I am sure you will remember. That was when we lost a dukw off Neahkahnie Mountain in Oregon. Driverless, it rolled off the road, across a field of flowers and off a 200 foot high vertical cliff into churning white water. But Joe Johnson lost one in the surf at Fort Ord, and you forgave him.

Then there were shipwrecks. One of these, the *Kulukundis* went up on the outer bar just north of Point Arguello while we were working there. The Coast Guard surf boats were unwilling to brave the large breakers so, with your permission, we started a taxi service for the Coast Guard and would-be salvors between the ship and the beach. Greek sailors, eager to get off the doomed ship, tossed down a couple of half-empty bottles of brandy to the dukw drivers and this started a rumor we were smuggling booze. Then, at the request of the Coast Guard, we took the freighter's crew ashore and were accused of helping illegal immigration.

Not even a headache, Mike?

One day an unexpected contract arrived from ONR; the Project was asked to look into the question of why torpedo nets in Alaskan waters failed in wave action. None of us knew anything about torpedo nets but $40,000 was a good reason to learn, so you sent me to the Tiburon Net Depot to find out something. Well, I do remember standing on a ten-acre patch of concrete in an icy wind watching two old guys weaving a huge net, one grommet at a time, from number nine wire. It took several hot-buttered-rums to get the main brace spliced that day.

I returned with the primer on nets intended for seamen second class. The first illustration in it showed clearly what the problem was with nets. So Isaacs made a model net out of a couple of dozen slinky toys with a test ring that could be moved about to measure strain at various points when the net was vibrated. Those slinkys, the lingerie clips to hold them, and the condoms (used to seal wet storage batteries) certainly raised eyebrows in UC's purchasing department.

Fig. 10 John D. Isaacs at Camp Pendleton, California, 1947.

Once you and I had a consulting job for the city of San Francisco. The City wanted to know if Baker's Beach was suitable for public swimming. We couldn't take their money without actually swimming there ourselves, so one dark winter morning about 6 a.m. we ripped off our clothes, flung ourselves in, and swam out about a hundred feet. Our total time in the water was about 30 seconds, but that was enough to certify the beach for any idiot who wanted to swim there for pleasure.

When Project interest expanded to include coastal processes we needed good quality oblique aerial photos. It was impossible to explain to Navy photographers what was wanted, so I became an aerial photographer with Navy planes and cameras, eventually photographing our Pacific coast several times and filling many filing cabinets with film and prints. Let us hope they are safely kept, because some day that collection will be valuable.*

* *Editor – The film and prints are safely stored in the Water Resources Center, O'Brien Hall, University of California, Berkeley, California.*

I won't go into details about one photo plane almost flying into Mount Olympus, or making the shortest flight on record from Astoria airport (in a gale), or standing a blimp on its tail over Monterey Harbor. You have got enough grey hairs! But we got pictures enough to fill the three large volumes of the "Shoreline Atlas of the Pacific Coast," which you later persuaded the Beach Erosion Board to publish.

The work we were doing led inevitably to more direct involvement with amphibious operations. Probably because of your friendship with Admiral Doyle, then Comphibpac, we were invited to study and comment on practice landing operations at Oceanside. Isaacs (Fig. 10) and I set up camera stations on the 40 foot high bluff behind the landing beach and took photos with clocks in the corner that showed quite clearly what happened and when. Despite low surf, a hard beach, and no enemy, these affairs invariably screwed up so that by mid-afternoon the beach looked like a junkyard. But we learned a lot about amphibious operations. Soon afterwards, Bob Wiegel got some LVTs from the Marine Corps and went into a research project to study their behavior in breakers at Fort Ord and Clatsop spit.

Eventually, during Operation Miki on Oahu, Bob Wiegel and Joe Johnson took photos on the ground and I from an airplane circling over the landing beaches (Fig. 11). There we recorded what would have been a wartime disaster – all boats in the first two waves were lost on a steep soft beach, because the Navy's beach reconnaissance group failed to correctly appraise the characteristics of the beaches. That same year, partly as a result of Waves Project findings, you were appointed chairman of the National Academy of Science's Committee on Amphibious Operations – and took me along as its executive secretary.

Fig. 11 Miki Amphibious landing operation, Oahu, Hawaii, 1949.

So much for some of the high spots. The Waves Project made many important findings in what was then the little known world of waves and beaches. You gave us the opportunity to join you in the investigation of that world, and we had the time of our lives.

Thank you very much for that.

Sincerely,
Bill Bascom

Morrough P. O'Brien's Impact on Engineering Education

BY JOHN R. WHINNERY
University Professor
University of California
Berkeley, California

MOST OF THIS SEMINAR has been concerned with Mike's key contributions to coastal engineering, but it is also important to place on the record his critical contributions to engineering education in general, and to Berkeley Engineering in particular. As has been noted, he was a faculty member here starting in 1928, and was Dean of the College from 1943 to 1959. I was on the faculty for 13 of his 16 years as Dean, served under him as a Department Chairman and followed him as Dean, so had an opportunity to see much of his phenomenal contribution to the College. I am delighted and honored to be able to tell you something about those contributions. The theme in everything he did, of course, was improvement of quality, and he had the vision and courage to approach the problem in a multi-faceted way – faculty, students, research, curriculum and organization to effect the changes. He is a true systems engineer and understands that all of these interact, and must be done together.

If there is one key to quality, it clearly lies in the faculty, and although Berkeley Engineering had always had a dedicated and hard-working faculty, there is no question but that its stature was vastly improved during Mike's tenure. Mike took an active role in recruiting such persons as Henry Schade, John Dorn, Earl Parker and Sam Schaaf, looking for persons with established records of accomplishment, or young persons with enough breadth to move into new areas. Recommendations for appointment to the College often had a rough time at the Budget Committee level when he started, so he established a confidential review committee within the College to review all of the appointments and promotions, advising the Department Chairmen when there were real or apparent weaknesses in the cases presented.

A distinguished faculty must be matched to distinguished students. Although the B-average then required for admission to Berkeley provided some selection, there were many failures and drop-outs in the College from students with no aptitude for the rigorous quantitative engineering approach. Mike instituted engineering entrance exams at Freshmen and Junior transfer level, given throughout the state. Although later declared inappropriate by a Senate Committee, it is significant that it has again been recognized that some selection is necessary, with grades, and the SAT scores providing the measure. Perhaps even more important to a good faculty is the quality of graduate students. An Assistant Dean for graduate matters was appointed with an office in the College. The Professional degrees, Master of Engineering and Doctor of Engineering, were established to recognize that design projects which did not fit the typical model of Ph.D. research were also important, deserving study and degree recognition at a high level. But the most important factor in building up the stature of graduate study was in the amount and quality of research in the College.

Good research comes from a good faculty, and in turn attracts other good faculty, so we already see the interactions mentioned. But institutional policies and structures can help. Mike established the Institute of Engineering Research to help faculty with proposals, and the administration of grants when obtained. With persons such as Henry Schade, Earl Parker and George Maslach, this was particularly effective in helping young faculty find sources of support, and effective techniques of proposal preparation. Mike also recognized that some projects appropriate to engineering did not fit on the campus, so worked for the Engineering Field Station at Richmond. This was established in 1950 and has made possible such things as the distinguished Sanitary Engineering research programs, Sea Water Conversion, the towing tank for Naval Architecture and the large shake table for earthquake engineering studies.

The theme of engineering in its interaction with society became popular in the 60's, and in the end all good engineering should benefit society. But there are specific societal problems which are a mix of technology, economics and politics and these are often the most difficult to attack. Mike recognized from the beginning that the College had an obligation to study some of these – especially those critical to the State of California. Thus the Institute of Transportation and

Traffic was instituted, the Sanitary Engineering Research Institute and Sea-Water Conversion Project mentioned earlier, and he also played a key role in the University-wide Water Resources Institute. The shore and beach studies discussed in this symposium provide an excellent example of such research. But it should be made clear that these did not displace the scientifically based research of the College. Low-pressure aerodynamics, analytical studies of materials, operations research, high-frequency electronics and many other research areas expanded and prospered during this period.

As with research, the curriculum is set by the faculty, but stimulation and questioning by a Dean or other administrative officer can help. Mike was very good at this. He believed strongly in a common lower division so that students would not have to make a choice until the junior year, but more importantly so that they would see more than their specialty. With the common base in mathematics, physics, chemistry and English, this amounted to only one engineering course per term, and he hoped that each major Department would give one of these. We spent many hours in faculty meetings hammering these out, with some very imaginative courses resulting. Although the common lower division has been modified, several of our present courses are direct outgrowths of these innovations.

In addition to concern for the technical courses, Mike was deeply concerned that we use effectively the humanities and social sciences part of the curriculum. He invited persons such as Dean Burchard, Dean of Humanities at MIT, and Philip Le Corbeiller of Harvard as guests of the College to advise us. A comprehensive social sciences course at the lower division level was initiated and was very successful for many years. Better advising systems for the elective humanistic-social courses were also established.

Still other innovations during Mike's tenure include the Engineering Science curricular and the Cooperative Work-Study program. The Engineering Science programs, starting with Engineering Physics and later expanded to Engineering Mathematics and Statistics, Bioengineering and Engineering Geo-science are honors programs, designed so that students can go on for graduate work either in engineering or the science of that program. This has worked beautifully; there are now 171 students in the four programs. The cooperative work-study program is another great success, providing experience, motivation and financial support to students who select this program. When I was Dean, I sometimes got calls from parents concerned about the interest of a son or daughter in the Co-op program. They felt it would interfere with the students' academic scholarship. Fortunately we had records to show that the scholarship of students generally improved upon entry into the co-op program because of the increased motivation.

The senior course "The Engineer and his Profession" had been in existence before Mike's period as Dean, but Mike recognized its potential for getting across to seniors some points not covered in other courses – professional practice, professional ethics and experience with oral and written reports. He taught some sections and selected other instructors who believed in the course and would take advantage of this opportunity for high-level exchange with senior students in the small sections in which the course was organized.

There were organizational innovations. I have mentioned the Institute of Engineering Research and the several subject-area research institutes, the office for graduate matters and that from the Co-op program. The most revolutionary, however, was the establishment of the College as one Department with the several specialities as Divisions of the Department of Engineering. Although eventually found unnecessary, much of the early building we have described could not have been done without the unity of this special organization. New programs in Naval Architecture and Nuclear Engineering were established during his term.

Mike established advisory comittees, both within the College and without. Internally there was a Policy and Budget Committee, later becoming the Dean's Coordinating Advisory Committee. Externally there was the very influential Engineering Advisory Council with top executives from major engineering organizations nation-wide. This Council played a major role in the establishment of the Engineering Field Station, and in other important matters. The Engineering Alumni Society, now so valuable to the College, was also established during that period.

Mike also kept strong liaison with Dean Boelter at UCLA, and the other University of California engineering programs as they developed. Similar liaison was maintained with the junior colleges, recognizing that junior transfers to the College were at that point one of our strong assets. (As a JC transfer myself, I greatly appreciated that aspect of the University.) He was active in the American Society for Engineering Education and the engineering arm of the Land-Grant College Association. Many of his innovations were described there and had a major influence on engineering education throughout the country.

We have seen the strong role that science played in the building of the College, but Mike always recognized that engineering was not just science and needed other things too. I would like to quote from his writings[1] a definition of engineering and the importance of science:

> "The activity characteristic of professional engineering is the design of structures, machines, circuits, processes, or combinations of these elements into systems or plants and the analysis and prediction of their performance and costs

under specified working conditions. Professional engineers are persons qualified to engage in this activity."

"Analysis and prediction of performance requires a knowledge of both basic science and of the related engineering sciences such as fluid flow, heat transfer, mass transfer, metallurgy, ceramics and others. One must understand science to apply it and, because these subjects are best studied when young, the principal emphasis is given to them in college."

Another quote[2] makes clear the distinctions:

"There is a superficial similarity between the educational preparation and the work of scientists and engineers which tends to obscure a very fundamental difference in viewpoint and objective. The scientist is interested primarily in discovering new facts and relationships in the physical world and he is not concerned, as a scientist, with the utility of his work. In fact, it is important to the advancement of science, and ultimately to the advancement of engineering, that the scientist not be concerned with the application and that he work on the problems he wants to work on and thinks he can solve. On the other hand, the engineer must develop an absorbing interest in the solution of practical problems and he must come to regard the problem which must be solved as the most interesting one he can imagine. He cannot rest content until his solution is represented by acceptable "hardware." It is the development of this viewpoint, which justifies the maintenance of engineering schools rather than the expanding of science departments to teach the applied sciences now generally taught by the engineering faculty."

And finally, this quote[3] makes clear that he understood the goals we have discussed from the beginning.

"1. Revision of the undergraduate program to give greater emphasis to mathematics and science, to the engineering sciences, and to the humanities; and to prepare the students for at least one year of graduate professional studies.

2. Expansion of the cooperative work-study program to provide a broader basis for professional practice.

3. Strengthening of the faculty in engineering by the addition of applied scientists on the one hand and of experienced professional practitioners on the other.

4. Addition of research facilities both on campus and at off-campus field stations, and expansion of these facilities and of graduate study primarily through research projects sponsored by government and industry."

Finally let's look at the changes in the College during these 16 years: The faculty increased from 72 to 172, undergraduates from 913 to 1771 and most impressively, graduate students from 44 to 550. More importantly, however, was the change in stature. The first ACE rating, following shortly after Mike left the deanship, placed Berkeley as one of the top two engineering schools of the country. Although a good school before, it could not possibly have aspired to that ranking. All of us who have benefited from this enhanced quality and reputation thank you sincerely, Mike!

1. Morrough P. O'Brien, "Educating Engineers," *California Monthly,* Feb. 1952, p 56.
2. Ibid, p 57.
3. Morrough P. O'Brien, "Accent on Creative Engineering," *California Monthly,* Feb. 1955, p 29.

The Beach at a Seawall: Completion of a 1940 Experiment

BY MORROUGH P. O'BRIEN
Dean Emeritus, College of Engineering
University of California
Berkeley, California

MANY YEARS AGO, Dorland[2] experimented in the laboratory on the profile of a sandy beach in front of a vertical seawall. Earlier experiments[3,4] on the equilibrium profiles of beaches had employed the same sand mixture in the same flume; in this experiment, the initial profiles seaward of the wall duplicated the appropriate position of the equilibrium profile found earlier.

At the time of this experiment, it was thought that a wave steepness of about 0.03 marked the boundary between normal and storm profiles. On this assumption, this series was planned to represent storm conditions and erosion of the beach was expected. However, in every run sand was moved towards the seawall and had to be removed to obtain the initial depth for the next run. The data are shown in the Table.

From the beginning of laboratory studies in Coastal Engineering at Berkeley, the settling velocity, as well as the size distribution, was measured for every sand used in the experiments. The sand used by Dorland[2] had a settling velocity of 0.46 ft/sec, a diameter of 0.5 mm and a specific gravity of 2.65.

The fact that Dorland[2] had measured the settling velocity of the sand he used permits interpretation now of his results on the basis of concepts advanced in recent years.

The work of Dean[1] on the dynamical similarity of coastal processes and the application of his criterion of similarity to beach profiles, has shown that the wave steepness separating normal and storm profiles is not a constant (e.g., 0.03). As a function of the criterion of dynamical similarity, ($\pi W/gT$), this dividing line followed the relationship

$$H/L = 1.7\ \pi W/gT.$$

Here, H, L, and T are the height, length, and period of the wave, W is the settling velocity of sand and g is the gravity constant.

The accompanying figure shows that the test data all lie below this line, in the region of summer profile. Sand should have been moved on shore and did.

This series of experiments, long buried in the Water Resource Center Archives at Berkeley, has some significance regarding present day probelems but, it was selected for this symposium for another reason and a personal one. For almost half a century, I have stressed the importance of measuring and reporting the settling velocity of the sand in any laboratory or field study – but with little effect until recently – and this on only a limited scale.

The evidence is clear that the settling velocity of beach sand is its most important dynamical characteristic.

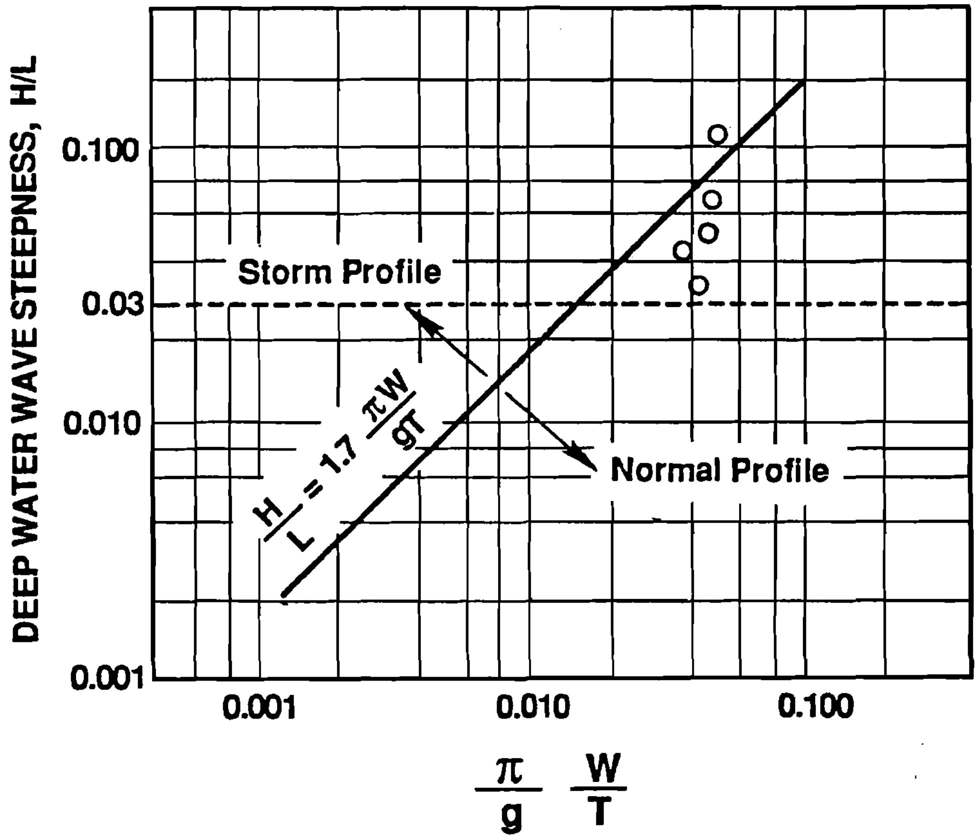
DATA

Run	Period(sec.)	H(ft.)	L(ft.)	H/L	$\pi W/gT$
1	1.07	0.20	5.85	0.034	0.042
2	0.815	0.36	3.44	0.106	0.055
3A1	1.15	0.28	6.50	0.043	0.039
3A2	1.03	0.28	5.545	0.051	0.044
3A3	0.89	0.29	4.20	0.069	0.050

Sand: Diam 0.50mm
Spec. Gravity 2.65
Settling Velocity 0.45 ft/sec.

REFERENCES

1. Dean, R.G. (1985) Physical Modeling of Littoral Processes. A.A. Balkema Rotterdam: Boston.
2. Dorland, G.M. (1940) Equilibrium Sand Slopes in Front of Seawalls. MS Thesis, University of California, Berkeley (Unpublished).
3. Meyer, B.D. (1936) A Model Study of Wave Action on Beaches. MS Thesis, University of California, Berkeley (Unpublished).
4. Waters, C.N. (1939) Equilibrium Slopes of Sea Beaches. MS Thesis, University of California, Berkeley (Unpublished).



Dorland Data and Dean's Boundary
between Normal and Storm Profiles

Support for Research in Coastal Engineering

BY MORROUGH P. O'BRIEN
Dean Emeritus, College of Engineering
University of California
Berkeley, California

INTRODUCTION

IN 1984, a group of coastal engineers, geologists and others interested in the physical processes of the coastal zone met under the sponsorship of the National Science Foundation (NSF) and the Office of Naval Research (ONR) at Oregon State University and generated a 62-page report to NSF and ONR entitled "Natural Hazards and Research Needs in Coastal and Ocean Engineering" (John H. Nath and Robert G. Dean, Editors.).

This report proposes that the National Science Foundation recognize coastal engineering as a unique phase of professional engineering practice by establishing a separate category in its organization and funding for support of research in this field.

The feature which makes coastal engineering unique is the dominant importance of wind-generated ocean surface waves in the planning, design, construction and maintenance of works in the coastal zone.

For the purposes of this report, the coastal zone extends from the outer edge of the continental shelf to the limits of tide water. Within this zone, three areas should be differentiated, namely, offshore, outer coast, and inland waters.

The nature of coastal engineering is not generally understood and a brief review of it is in order before discussing research needs.

COASTAL ENVIRONMENT

Surface waves are generated by winds at sea and move in the direction of the generating winds. Waves which arrive at a particular point were generated by winds acting over fetches directed at this point; the wind pattern is moving and the generating fetch is continually changing in position, length and strength of wind. Several high pressure and low pressure centers may exist concurrently over the ocean surfaces and waves from two or more directions are often observed. The height, length, celerity, energy and other characteristics of the generated waves depend upon the constantly changing strength, fetch and duration of the generating winds. Winds also generate currents with velocities at a small fraction of the wind speed.

As waves move over a shoaling bottom towards the shore, their energy is reduced by bottom friction and other mechanisms to be finally dissipated in the breakers and surf zone. Some energy may be reflected, particularly along rocky shores or at structures. Waves are as variable as the winds. Consequently, each location offshore or along the outer coast experiences variations in wave conditions which constitutes a local "wave climate." Inland waters respond to winds, but the waves generated are limited in height and length by the depth and fetch.

In developing a design for the coastal zone, the engineer must construct the local "wave climate" including seasonal variations in a normal year, variations from year to year, and especially, the probability of extreme events such as hurricanes.

The shores of greatest interest at the present time are the sandy beaches of the outer coast, which are composed primarily of quartz sand, in a relatively narrow range of size; in moving over geologic time from the ultimate source, the boulders and gravel have been either decomposed or trapped inland while the very fine fractions have been carried to deepwater offshore.

Offshore the depth of water is usually such that surface waves have little effect on the bottom directly, because wave orbits decrease exponentially with depth. Waves exert forces on structures resting on, or anchored in, the bottom and the bottom materials provide the means of resisting these forces.

Along the outer coast, waves induce bed load and suspension of the materials and also generate currents. The breakers and surfzone are the scene of the most vigorous such activity. On the offshore at tidal inlets, tidal currents as well as surface waves are active in moving the bed materials.

Estuaries and other inland waters are affected by waves generated by local winds but currents are usually the dominant factor. For example, the flow-conditions in most estuaries are more like those of rivers than those of the outer coast.

COASTAL ENGINEERING

Coastal engineering deals with the planning, design, construction and maintenance of works exposed to ocean wave attack. Coastal works may fail by physical destruction, by failure to produce the intended effect, or by initial or maintenance costs in excess of planned; coastal engineers normally work in an atmosphere of uncertainty due to major gaps in essential information such as incomplete historical records of shore changes, sketchy information regarding wave climate, conflicting opinions and observations indicative of shore processes, and other deficiencies. There will always be uncertainty regarding the frequency and intensity of storms, but in other respects uncertainty can be substantially reduced by systematic field and laboratory studies.

There is no well defined and coherent industry associated uniquely with coastal engineering. Offshore petroleum exploration is carried out by organizations and ships dedicated to this service; fixed structures for petroleum production are fabricated and placed by contractors, many of whom are active primarily onshore. Data recorded in the course of petroleum operations would be of value but is usually classified as proprietary.

Dredging is another coastal industry. Ocean dredging at tidal entrances is generally done by hopper dredges owned and operated by the Corps of Engineers; privately owned pipeline dredges are beginning to undertake beach replenishment but for the most part this equipment works in inland waters. There are many small contractors and consulting firms which specialize in coastal work but large contractors and consulting firms do not. Clearly there is no General Motors or General Electric with resources or interest to support research and development in the coastal field.

The Corps of Engineers through the Coastal Engineering Research Center is the largest agency engaged in coastal work; once the primary agency and source of support for coastal research, its role has become more mission oriented in support of projects of the District Offices of the Corps.

Coastal engineering practice depends upon techniques, theories and data produced by other scientific and technical disciplines including applied mathematics and statistics, geology and geophysics, meteorology, oceanography, materials science, computers, biology and some others. The biennial International Conferences on Coastal Engineering and similar specialty conferences being together interested representatives of these fields.

COASTAL ENGINEERING RESEARCH

Prior to World War II, a small program of research and development was carried on by the Beach Erosion Board (now the Coastal Engineering Research Center) of the Corps of Engineers and by a small group of professors with BEB support. During and following the war, amphibious military operations gave impetus to a broad program of research on wave forecasting, dynamics of the surfzone, landing craft characteristics and so forth. Offshore petroleum exploitation and production gave emphasis to forces on structures and pipelines and bottom response. A third factor has been the growing interest in environmental and ecological considerations as factors in coastal planning and development.

The fact is that the past sources of support for research on coastal engineering are drying up and, what is even more important, there is now no agency with the charter and standing to influence the scope and character of a national program.

EXAMPLES OF TOPICS FOR RESEARCH

Scaling Laws for Physical Models

Physical scale models of coastal locations provide an opportunity to control the wave and tide environment and to shorten greatly the time and reduce cost as compared with making observations in nature. However, an unfortunate aspect of models involving surface waves is that the dynamic nature of surface waves rules out distortion – the use of different scales for the horizontal dimensions and vertical dimensions – which would permit reduction of the required surface area of the model. Very large models may be required to achieve dynamic similarity of model and prototype.

Physical models planned in accordance with established scaling laws would permit not only the use of models to represent specific locations but, more importantly, would justify the study of basic shore processes. A few examples of processes whcih might be studied in model scale are:

By-passing the littoral transport at tidal entrances. Waves, tidal currents, and the inherently complicated hydrography render this problem almost insolvable by field studies alone.

- Is the process selective as to sand size, fractions following different paths?
- What is the effect on dredged cuts?
- What is the effect of different rates of approaching littoral transport?
- Etc.

Littoral Transport. Littoral transport rates reported in the literature until recently have not included sand size as a variable. Recently, R.G. Dean has inferred this effect indirectly from the Shields diagram of critical tractive force and other considerations, but direct measurements are limited. Much field and laboratory work is needed to establish the rate of movement of different features of the sand, the relative rates of motion, and different positions in the surf zone, etc.

Onshore-offshore Sand Movement. The annual cycle of storm erosion followed by recovery under swell conditions, should be related to sand size, wave climate, and tide range.

MONITORING OF COMPLETED PROJECTS

A potential major source of basic information regarding processes on sandy shores is the long-term measurement of both the changes completed with continuing observations of the incident wave climate. Presently there is little funding for this purpose despite the fact that studies of selected projects – replenishment, offshore breakwaters, groins, etc. – would certainly provide extremely valuable data for analysis and guidance of design. Numerical modeling would benefit from such inputs.

For other examples, see report of the NSF.

PROPOSED PROGRAM

- NSF program similar to that for Earthquake Engineering. Funding up to $10 M/year in third and successive years.

- Field stations on the Pacific and Gulf Coasts like DUCK in North Carolina. Under direction of CERC.

- Large facility for physical models of basic processes administered by a university active in Coastal Engineering Research and Instruction.

The Marine Sciences
Research Center (MSRC)
of the
State University of New York
at Stony Brook
is pleased to announce creation
of the
M.P. O'BRIEN
FELLOWSHIP PROGRAM

The M.P. O'Brien Fellowships acknowledge the manifold contributions of Professor O'Brien and are intended to stimulate interest in graduate studies at MSRC in beach and nearshore processes and coastal engineering.

Fellowships are awarded for two years and may be renewed. Each fellowship carries a stipend of $8,500 for the academic year and a full tuition award. The stipend may be supplemented by up to $3,000 for the summer.

Applications should be sent to:
Graduate Program Director
Marine Sciences Research Center
State University of New York
Stony Brook, New York 11794-5000

PART II. Morrough O'Brien Biographical and Bibliographical Data

(March 1969)

BIOGRAPHICAL DATA

MORROUGH PARKER O'BRIEN B. S. (MIT)
D.Sc. (Northwestern 1959)
D.Eng. (Purdue 1961)
LL.D. (California 1968)

Social Security No. 555-38-6542
General Electric Social Secruity No. GE 14-0689340
Born: September 21, 1902 Hammond, Lake County, Indiana
Identification: American; white; 5'10", 195 lbs. brown eyes, black hair, head size 7 1/4; blood group A-Rh factor positive; identifying marks: scar over left eye, scar on left shin.

Regis. #10012 ME (California) 1947-
Regis. #6166 CE (California) 1944-
Regis. #23453 Professional Engineer (New York) 1948-
Chartered Mechanical Engineer, Great Britain

Listed in: Who's Who in America; Who's Who in American Education; Who's Who in Engineering; American Men in Science; Who's Who in California; Leaders in American Science.

Education:

Grammar school in South Bend, Indiana and Phoenix, Arizona
St. John's High School, 4 years, Toledo, Ohio
St. John's College, Toledo, Ohio - 1 year 1919-20
Holy Cross College, Worcester, Massachusetts - 1 year 1920-21
Massachusetts Institute of Technology, Cambridge, Massachusetts - 4 years 1921-25
  Specialized in hydroelectric engineering and hydraulics
  Received B.S. degree in 1925 (June) in Civil Engineering
Purdue University, Lafayette, Indiana 1925-27
  Graduate courses in water power, hydraulics, structural engineering, and mathematics.
Technische Hochschule, Danzig, 1927 (Summer Program)
  Special course under Professor Richard Winkel on theory of hydraulic models
Royal College of Engineering, Stockholm, Sweden, 1927-28
  Advanced study and research in fluid mechanics

Academic:

Purdue University

1925-26 Graduate Assistant in Civil Engineering; instruction in hydraulic laboratory and orientation lecture to freshmen

1926-27 Research Assistant, Engineering Experiment Station engaged in hydraulic research

Academic: (Continued)

	University of California
1928-31	Assistant Professor - in charge of work in hydraulic laboratory and lecturing on elementary hydraulics, water turbines, and pumping machinery
1931-36	Associate Professor
1936-59	Professor
1937-43	Chairman, Department of Mechanical Engineering
1941-44	Institutional Representative, University of California ESMWT Program
1943-59	Chairman, Department of Engineering Dean, College of Engineering (July 1, 1943) Professor of Engineering
1947-48	Industrial Leave - (Air Reduction Company)
1953	Industrial Leave - (Aircraft Nuclear Propulsion Project, General Electric, Evendale)(Spring 1953)
1958-59	Visiting Institute Professor, Massachusetts Institute of Technology (Oct. 1, 1958 - March 31, 1959)
1958-59	Research Fellow, Harvard
1959-63	Research Associate, Graduate School of Public Administration, Harvard University
1959	Professor of Engineering, Emeritus and Dean, College of Engineering, Emeritus, University of California, Berkeley
1960-61	Visiting Professor of Engineering, University of Notre Dame
1959-66	Visiting Professor, Purdue University
1959-	Visiting Professor, Renssalaer Polytechnic Institute
1959-66	Visiting Professor, Polytechnic Institute of Brooklyn
1967-	Member, University of Kentucky, College of Engineering Advisory Council
1967-	Member, Duke University, School of Engineering, Board ov Visitors.

Research and Development:

1927-28	John R. Freeman Scholar (ASCE) in Germany and Sweden. Appointed assistant research engineer in Hydraulic Structure Laboratory at Royal College of Engineering (Kungliga Tekniska Hogskolan) from October 1927 to June 1928. Resigned second scholarship in 1928 to accept appointment at University of California.
1928-37	In charge of Hydraulic Laboratorv, University of California, Berkeley.

MORROUGH P. O'BRIEN - RESEARCH

1929	~~Appointed~~ Civil Engineer for Board on Sand Movement and Beach Erosion (predecessor of U. S. Beach Erosion Board); initiated research by this Board on coastal engineering.
1930	(During Summer) Civil Engineer for U.S. Beach Erosion Board. Prepared preliminary report on beaches and harbors on Pacific Coast.
1934-37	Consulting Engineer, U.S. Engineer Office, Pacific Division on shore line changes at mouth of Columbia River; designed and built Hydraulic Model Basin at Berkeley for studying estuary of Columbia River; field studies, Astoria, Oregon.
1939	Consulting Engineer, Office of Chief of Engineers, U.S. Army, Washington, D.C.; prepared long-term program of research on wave action, shoreline phenomena, and shore protection for Beach Erosion Board laboratory.
1939-41	Collaborator, California Forest and Ranges Experiment Station, U.S. Forest Service.
1942-43	Executive Engineer, Radiation Laboratory, University of California, November 1942 - May 1943. Responsible under Professor E. O. Lawrence for coordination of the design of the electromagnetic plant.
1942-46	Consultant, Research Section, Bureau of Ships, problems of underwater sound, propeller noise, landing craft design, breakers and surf in connection with amphibious landings, etc.
1946	Consultant, Oceanographic Section, Joint Task Force One on Operation Crossroads, 1946 (Atom Bomb Test).
1951-65	Chairman, Council on Wave Research, Engineering Foundation.
1965-	Chairman, Research Council on Coastal Engineering, American Society of Civil Engineers

Professional Practice:

1922-23	Inspector of bridges and culverts, Maricopa County Highway Commission; Summer Employment.
1924	Surveyor . Lucas County, Ohio - Summer
1925-27 Summers	Chief of field party for Hudson River Regulating District on Sacondaga Reservoir Project - June to September.
1928	During summer, while in Stockholm Sweden, spent 2 months as assistant to the U.S. Commercial Attache writing reports on Swed industry and commerce.

Professional Practice: (Continued)

1931-32	Consulting Mechanical Engineer, Hetch Hetchy Water Supply; selection of pumps for proposed Corral Hollow emergency pipe line.
1933-36	Consulting Engineer, U.S. Engineer Department on beach erosion and estuary regulation at Columbia River.
1935	Consulting Engineer, Weyerhauser Timber Company (on silting of log pond at mill at Longview, Washington.)
1936	Consulting Engineer, Byron Jackson Company - Development of rating curves for ISA standard orifices.
1937	Becker Pump Company, Oakland (Developed method of jet pump design for condensate systems; revised the condensate system.)
1937-40	Consulting Engineer, Board of Harbor Commissioners, City of Santa Barbara, California, on beach restoration projects.
1938	Consulting Engineer, Los Angeles County Flood Control District.
1938-39	Consulting Engineer, Board of Harbor Commissioners, City of Long Beach, California.
1939	Consulting Engineer, City of Santa Monica, California, (Litigation regarding erosion caused by breakwater).
1939	Consulting Engineer on placing of caissons for Pier 5, Tacoma Narrows Bridge.
1939	Consulting Engineer, Morris Machine Works. (Acceptance tests and revision of design of unwatering pumps and check valves for drydocks at Mare Island Navy Yard.)
1941	Consulting Engineer, Food Machinery Corporation. Hydraulic design of "Water Buffalo" amphibious tank.
1943-45	Consultant, Office of Production Research and Development, War Production Board.
1946-49	Consultant, Office of Technical Service, Department of Commerce.
1947-48	Director of Research and Engineering, Air Reduction Co., Inc. In charge of chemical research, liquefaction research, apparatus research, process engineering, general engineering, and all research development, design, and construction for company. February 1947-September 1948.

Professional Practice: Continued)

1950 Long Beach Oil Development Company. Protection of wells in Long Beach field against shearing due to horizontal earth movement.

1950 Park and Recreation Commission, City of San Francisco. Appraisal of Baker Beach, S.F., for recreational development.

1950 Consulting Engineer, General Engineering Laboratory, General Electric Company; torpedo design. (Schenectady)

1950- Consulting Engineer, General Electric Co.
1950-51 Aircraft Gas Turbine Division

1951-53 Aircraft Nuclear Propulsion Project

1953-61 Flight Propulsion Division

1957-61 Defense Electronics Division

1961- Aerospace and Defense Group

1958- Member of Technical Advisory Board of the Missiles-Jets & Automation Fund.

1958-63 Consultant, Kaiser Aluminum & Chemical Corporation.

1959- Member, Board of Directors, McGraw-Hill Publishing Co., New York.

Public Service:

1938-1963 Member, Beach Erosion Board, Office of Chief of Engineers, U.S. Army.

1950-53 National Research Council
Committee on Amphibious Operations, Chairman - 1952
Committee on Operations Research - 1952

1951-54 Bay Area Council - San Francisco
Air Pollution Committee, Chairman, 1951-53

1952-53 National Science Foundation - Panel of Engineering Consultants, M.P.E. Division

1952-53 Engineers' Joint Council, Task Committee 6, Water Conservation, Use and Development with respect to recreation, fish and wild life, Chairman

Public Service: (Continued)

1952-53	Water Policy Panel
1951-	Member, Army Scientific Advisory Panel; Chairman, 1961- Subpanel on Mobility - 1954-57 Subpanel on Air Mobility - 1957-62
1954-57	Member, Personnel Security Board, Atomic Energy Commission, San Francisco Operations Office.
1956-	Engineers' Joint Council, Joint ECPD-EJC Committee on A Survey of the Engineering Profession.
1958-60	Member, National Science Board, National Science Foundation (Appointed by President Eisenhower with Senate approval).
1958-	San Francisco Chamber of Commerce, Special Projects Technical Section
1958-60	Member, Maritime Research Advisory Committee of the National Academy of Sciences, National Research Council; Chairman, subcommittee on Naval Architecture and Marine Engineering.
1960-65	Member, Committee on New Frontiers of Technology, U. S. Chamber of Commerce, and subcommittee of above on Science and Technology.
1959-	Member - University of California's Engineering Advisory Council.
1960	Navy project "Poseidon" - Jacksonville, Florida.
1961-65	Member (ex officio) Defense Science Board.
1963-	Coastal Engineering Research Board-Corps of Engineers (formerly Beach Erosion Board of U.S. Army Corps of Engineers

Membership and Activities:

American Society of Mechanical Engineers; Fellow

In National Society:

Special Research Committee on Fluid Meters - 1937-42
Medals Committee - 1950-52
Hydraulic Division, Executive Committee of - 1936
Education Committee (Advisory Member) - 1940

In Local Section:

Chairman, San Francisco Section - 1947

Membership and Activities: (Continued)

American Society of Civil Engineers - Fellow

Chairman, Research Council on Coastal Engineering 1965-
Executive Committee of Hydraulic Division: Vice-Chairman 1950,
Chairman, 1951-52
Chairman, Freeman Fund Committee - 1951-53
Subcommittee of Symbols for Theoretical Hydraulics - 1946
Local Section: Chairman, Committee on Advanced Study (S.F.) - 1940

Engineering Foundation
Council on Waves Research, Chairman, 1951-1964

American Society for Engineering Education
Southwest Region Committee on Atomic Energy Education, Chairman - 1951-53
Advisory Committee on Costs of Higher Education - 1950-53
Executive Committee, ECAC - 1950-51
ECRC, Vice Chairman - 1951-53
Committee on Honorary Membership - 1954-present
Advisory Committee on Relations with Industry Division - 1954-present
Committee on Evaluation of Engineering Education - 1953-55

Svenska Teknologforeigen - 1928-32

Association of Land Grant Colleges and State Universities
Executive Committee of Engineering Division, Secretary - 1951-52
"Engineering Division RECORD", Editor - 1951
Engineering Division, Vice Chairman - 1952

Association of American Military Engineers to 1965

American Geophysical Union
Executive Committee - 1937-39
Section of Hydrology: Committee on Flood Waves - 1937
Committee on Dynamics of Streams - 1937
South Pacific Regional Committee, Chairman - 1937-39
Vice President, Section on Hydrology - 1939

American Rocket Society: 1954; elected senior member 1961

San Francisco Engineering Societies
Engineers Week Committee, Chairman - 1956

American Nuclear Society - 1957-1963

California Society of Professional Engineers (Golden Gate Chapter) to 1967

National Society of Professional Engineers to 1967 - Awards Committee - 1963, 1964

International Association for Hydraulics Structures Research

National Academy of Engineering

Memberships and Activities: (Continued)

Institute of Aeronautical Sciences - 1953-present

Society of Limnology and Oceanography

American Institute of Chemical Engineers
Charter Member, Nuclear Engineering Division - 1954-1967

American Association for the Advancement of Science
Vice-President; Chairman, Section M, 1950-51

American Association of University Professors - 1937-52

American Meterological Society - 1947-50

History of Science Society

Permanent International Association of Navigation Congresses - through 1954

Committee of Consultants to Administer the "Boris Bakmeteff Research Fellowship in Mechanics Fluids," The Humanities Fund, Inc.

Society for the History of Technology - Member, Membership Committee 1959-
Advisory Council 1961-65

The Newcomen Society for the Study of the History of Engineering and Technology, England

Institute of Mechanical Engineers (London) - 1962-present

Sigma Xi

Pi Delta Epsilon (Journalistic Honor Society)

Pi Tau Sigma (Mechanical Engineering Honor Society)

Tau Beta Pi

Delta Tau Delta (Social)

Chi Epsilon (Civil Engineering Honor Society)

Cosmos Club - Washington, D.C.

Bohemian Club - San Francisco

Business-Industry Political Action Committee - 1963-64

Athenian-Nile Club - Oakland

Heresy Club - Berkeley (defunct)

Memberships and Activities: (Continued)

Engineers Club - New York

Faculty Club - U.C. Berkeley

Club de Golf - Cuernavaca, Mexico 1963-

Sleepy Hollow Country Club - Scarborough-on-Hudson, New York

Kenwood Country Club - Cincinnati, Ohio

University Club - Mexico City, D. F.

University of California: (Prior to retirement)

Member or Chairman of many committees; following are representative examples of such assignments:

Berkeley Campus:
Vice-Chairman, Northern Section of the Academic Senate - 1950-52
Chairman, Academic Council, Academic Senate
Chairman, Coordinating Committee, Academic Senate, Northern Section
Member, Academic Senate Advisory Committee to the President - 1950-52
Ex-Officio member, Committee on Educational Policy
The Faculty Committee (Oath Controversy) - 1950
Committee on Honorary Degrees, Academic Senate, Northern Section - 1954-59
Member, Advisory and Policy-Recommending Committee of Bureau of Occupations
Member, Chancellor's Administrative Advisory Committee

Statewide:
Member, Committee on Coordination with State Colleges
University Extension Advisory Board - 1941
Committee on Higher Education Problems in California
Ex-Officio member, Engineering Advisory Council (non-faculty)
Member, Statewide Coordination Committee for Engineering
Chairman, Institute of Traffic and Transportation Engineering-Advisory Committee (non-faculty) and Executive Committee member
President's Special Committee to investigate proposed new campus at San Diego

Honors:
Honorary degrees; Northwestern, Purdue, California
Army-Navy Certificate of Appreciation (World War II-December 10, 1948)
Certificate of Appreciation - Furthering the Objectivies of the Society and Division of American Society of Mechanical Engineers, 1955-56
Certificate of Appreciation - Chairmanship of Bay Area Engineers Week-1956
Citation, San Francisco Section, American Society of Civil Engineers, 1959
Selected to be the First Distinguished Lecturer, Foundation for Instrument Education and Research, September 24, 1959
Distinguished Civilian Service Award, Department of the Army, 1964
Bliss Medal, Soc. of American Military Engineering, 1965

MORROUGH P. O'BRIEN

Honors: (Continued)
Lamme Award, American Society for Engineering Education, 1968
Building named Morrough P. O'Brien Hall, University of California, 1969
Japan Society of Civil Engineers, honorary member, 1988
Clark Kerr Award, University of California, Berkeley, 1988

Miscellaneous

Parents: Morrough O'Brien - U. S. Citizen-Address: 538 W. Wilshire, Phoenix, Ariz.
Lulu Parker O'Brien - U.S. Citizen-deceased (1951)

Wife: Mary Wallner O'Brien, P. O. Box 265
604 Calle Galeano
Cuernavaca, Morelas, Mexico

Born: November 23, 1902

Ex-Wife: Roberta Libbey O'Brien-Born: Watsonville, California
Parents: William G. Libbey - Illinois
Carolyn Murray - Nova Scotia
Married: May 16, 1931, in Santa Cruz, California; Divorced March 6, 1963.

Children: Sheila O'Brien - August 10, 1932
Morrough O'Brien - July 11, 1935

Addresses:	1959-	Faculty Club, U.C. Berkeley and
	1963-	604 Calle Galeano, Cuernavaca, Morelos, Mexico
	1952-59	1570 La Vereda Road, Berkeley, California
	1948-52	2340 Vine Street, Berkeley, California
	1947-48	Raymond Street, Darien, Connecticut
	1945-47	1409 Scenic Avenue, Berkeley, California
	1942-45	Faculty Club, U.C., Berkeley, Calif. and Carmel, Calif.
	1934-42	1345 Queens Road, Berkeley, California

Resident of California 1928 to 1966

No military service; not registered by draft board; has no draft number

Foreign countries visited:

1927-28	--Sweden, Finland, Norway, England, France, Holland, Belgium, Germany; for Scholastic reasons
1953	--Liberia, Gambia, F.W. Africa, Portugal, France, Germany, Austria
1961	--Holland, Belgium, Germany
1962	--England, France, Germany, Belgium,Holland, Norway.
1963	--Frequent visits to Mexico.
1964	--Spain, Portugal, Switzerland, France, Germany, Denmark, Sweden
1966	--Hong Kong, Japan, Taiwan, Macao
1968	--Ireland, Scotland, England
1969	--South Africa

Lived in Indiana from birth date, 1902, until 1913 when moved to Phoenix, Arizona with parents; 1915 to 1925, 602 Tennyson Place, Toledo, Ohio.

U.C. Personnel #66302.

Identification scars: Scar over left eye; scar on left shin.

Published Writings:

1. European High Specific Speed Hydraulic Turbines (with M. J. Zwrow), Power Plant Engineering, November 1, 1926.

2. Studies of the Pumping of Viscous Liquids (with M. W. Todd), Power Plant Engineering, Dec. 1, 1926.

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23. The Hydraulic Ram (with James E. Gosline), University of California Publications in Engineering, Vol. 3, No. 1, 1933.

24. Applications of the Jet Pump to Oil-Well Pumping (with J. E. Gosline), Third mid-year meeting, American Petroleum Institute Proceedings, Vol. 14 M. Sec. 4, Division Production, Tulsa, Oklahoma, May 19, 1933; Oil & Gas Journal, Vol. 32, No. 1, pp. 57 and 63, May 25, 1933.

25. Model Law for Motion of Salt Water Through Fresh (with John Cherno), ASCE reprint, December 1932, Vol. 58, No. 10; ASCE Proc., Vol. 59, No. 3, March 1933, pp. 531-4 (4 parts); ASCE Transactions, Vol. 99, 1934, pp. 576-94.

26. The Transportation of Bed-Load by Streams (with Bruce D. Rindlaub), Transactions, American Geophysical Union, 15th Annual Meeting, Vol. 15, 1934.

27. Reversed Flow Through Centrifugal Pumps (with J. E. Gosline), Power Plant Engineering, Vol. 38, No. 2, February 1934, pp. 100-2.

28. Velocity Head Correction for Hydraulic Flow (with J. W. Johnson), Engineering News-Record, Vol. 113, p. 214, August 16, 1934; Engineering News-Record, Vol. 121, N 3, p. 71, July 1938.

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A letter from James Forrestal, former Secretary of the Navy, to Morrough O'Brien reads as follows:

Dear Mr. O'Brien,

I am addressing this letter to you in appreciation of your services to Commander Joint Task Force One in connection with Operation CROSSROADS.

As technical consultant you provided an unusually accurate analysis of the many complex factors involved in the prediction of the phenomena to be produced by the Bikini (atomic bomb test) explosions, making an outstanding contribution to the successful accomplishment of the objects of the Force. As a basis for these predictions, you organized and directed a valuable series of model studies of explosions in shallow water. The results of your thoroughgoing work were indispensible to proper instrumentation and planning. Your ability and effectiveness in this difficult scientific field were amply demonstrated by the remarkable accuracy by which the behavior of the water in the test was predicted. Your studies and knowledge constitute a contribution of the first maganitude to the CROSSROAD Operation and to future work on the phemonology of atomic bomb explosions at sea.

For this outstanding contribution to the success of the whole operation, the Navy sends you its thanks.

Well done.

Sincerely,

James Forrestal
Secretary of the Navy
[1946]

[A copy of this letter was sent by O'Brien to the interviewer for inclusion in this volume. The original letter hung on O'Brien's study wall.]

C. W. LA PIERRE, GENERAL MANAGER
AIRCRAFT GAS TURBINE DIVISION

August 4, 1952

M. P. O'Brien
Dean, Department of Engineering
University of California
Berkeley, California

Dear Mike:

Pursuant to our discussion Friday, we would appreciate a review by you of our new engine program. A copy of Neil Burgess' proposal is attached as a starting point. The review should comprise a summarization of the salient factors and your recommendations for the future program of this Division in connection with advanced J-73, X-24a and X-50 engines, or your recommended alternatives.

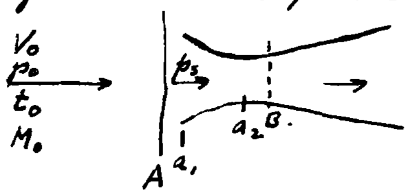
It is hoped that this review can be completed before your return to California in September. To this end, Dave Cochran is hereby assigned to assist in this work. Until further notice, you and he are requested to report to Neil Burgess who will make available the facilities and information which you require.

Sincerely,

Jim

CWLaPierre:feh
Enclosure a/s

Starting conditions in a supersonic diffuser.

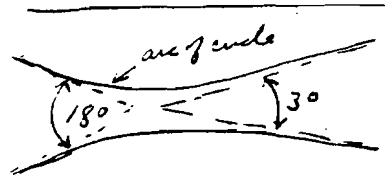


As flow velocity V_0 is increased, normal shock forms at A - and a pressure p_3, etc exists for each value of M_0 - In order for supersonic diffusion to be initiated, the pressure p_3 and the area ratio must be such that the flow at the throat is sonic - The normal shock then moves from A to position B just beyond the throat —

→ After flow is established, the shock at B may be reduced and pulled further towards the throat by reducing the area a_2 or increasing the area a_1 —

Kantrowitz Fig 3 -

Maximum theoretical contraction ratio that permits supersonic flow to start		Entrance Mach numbers M_0
1.0		1.0
1.05.	Experimental	1.34.
1.1	results closely	1.52.
1.2	confirmed this	1.93.
1.3	ratio	2.4
1.35		2.7



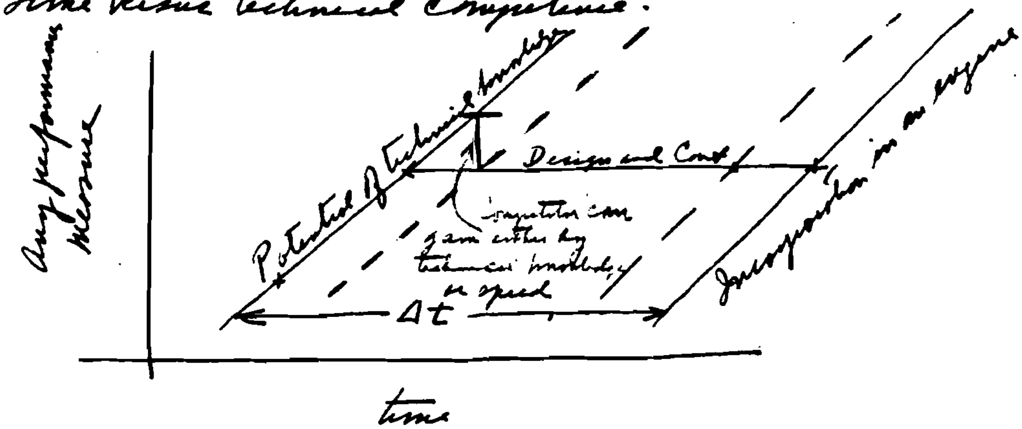
Best design found.

PRC staff -
S. Waters, machine design (English), J. Sullivan (Northrop Institute), S. Huntington x G.E., Soloby, B. Foster, Stanford; H. Roberson (Cal), T. Martin (water pumps), O. Conroy (Cal), R. Oswald Cal 45, E. Beder Cal Tech. Andy McGregor NACA

Experienced AGT men who are no longer contributing directly to design - Dilley, Fischer, Storch, DeWane, Alford, Streid, Auger

Quality of engineering work has dropped

Time versus technical competence.



BOSTON TRAVELER, WEDNESDAY, JULY 18, 1951

bley

"If you don't believe that, take a look at some of the issues of his publication, Air Line Pilot. In one issue alone there are eight pictures of Dave—two of them on the front page."

Behncke has many friends and defenders in the organization and they'll always insist he did a tireless job for the betterment of the pilots' lot.

A summary would seem to be that the sentiment for a change at the time finally took over.

LOCKHEED TURBOPROP—A design for a turboprop medium transport airplane, submitted by the Lockheed Aircraft Corporation, has been selected by the Air Force for development, which probably means one or more will be built. Under present plans, it will be powered by four Allison jet engines turning specially designed propellers. The design includes a cargo floor only 45 inches above the ground making straight-in loading possible through two doors without special loading equipment.

MORE SABRES—Australia has been licensed by North American Aviation, Inc., to manufacture the F-86 sweptwing Sabre. This fighter is now being produced at Los Angeles, Columbus, O., and in Canada. The Australian version will be powered by a British jet engine, probably the Sapphire.

WHO'S WHERE — Currently taking instructors' courses at Maxwell Air Force Base in Alabama are Maj. James P. Goss of Boston and Capts. Melvin R. Lynn of Medford, John R. Galt of Mt. Vernon streets, Boston, and Robert A. Dalrymple of Park drive, Boston.

NEW PAA SPOT—Pan American World Airways is opening new Boston offices tomorrow at 100 Federal street.

Unrest in Ranks of Airline Pilots Led to Ousting of Association Boss

"God, Send Us Peace"

By AGNES CARR

God, send us peace; too many wars have come
To call our sons from home's security
And bid them answer to the beat of drum
That carried them across a distant sea
To fight, perhaps to die on lonely shore,
God, send us peace, let war drums sound no more.

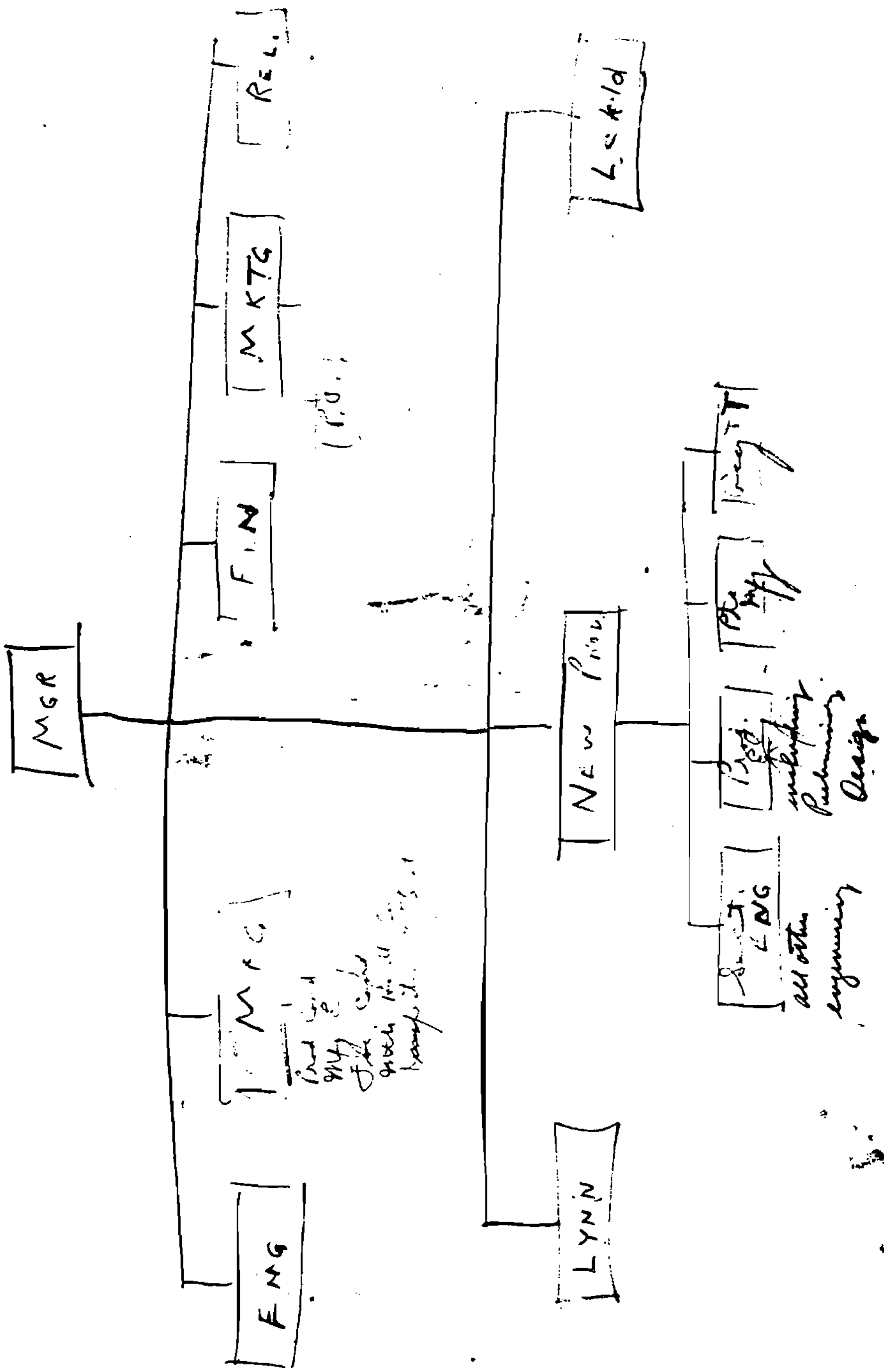
God, send us peace; too many homes there are
Who mourn a son or father in this hour;
Too many homes can show a golden star
For loved ones sacrificed to war's [illegible] power.
Our land is great, we're proud our [illegible] to fly,
God, send us peace, Thy gift of gifts for aye.

(Copyright, 1951, [illegible])

Your H[illegible]
[illegible]om
By CAF

ARIES: (Mar. 21 to Ap[illegible] Surprising conditions oc[illegible] occur at home, or with friend personally brin[illegible] chance to realize an desire.

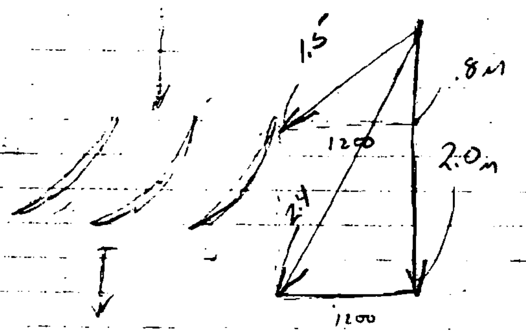
TAURUS: (Apr[illegible] —Communica[illegible] sions take there's "r[illegible] Private[illegible] prestic[illegible] GEM[illegible] Re[illegible]


MGR
ENG
FIN
MKTG
REL.
LYNN
NEW PROD
ENG
all other engineering
Proj.
Preliminary
Design

NACA - supersonic compressor better at subsonic tip velocities

dividing line on work for GE and for Air Force based on diffusion conditions at inlet to compressor

rotating diffuser ahead of 1st stage



Hydrodynamics of all-supersonic flow -

Suggestions from PRC -

engine weight as a function of size -

$\frac{W}{T} \propto D^{0.4}$

$W \propto D^{2.4}$ | $T \propto D^{2}$

GE weights have been high - effort needed - stress design of parts not now stress designed - Many parts designed by eye - especially stationary parts -

Choking of turbine and after burner

Variable area convergent divergent exhaust nozzle

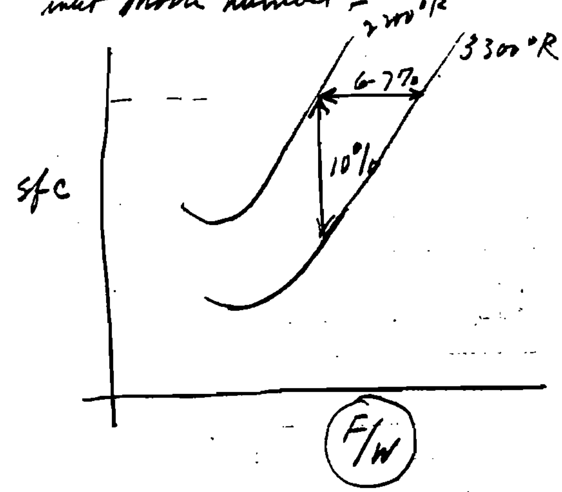
36

6/20/52 Co[illegible]

Turbine inlet temperature –

going from 2700°R to 3300°R at

inlet Mach number = 3.0? Pressure ratio 4:1

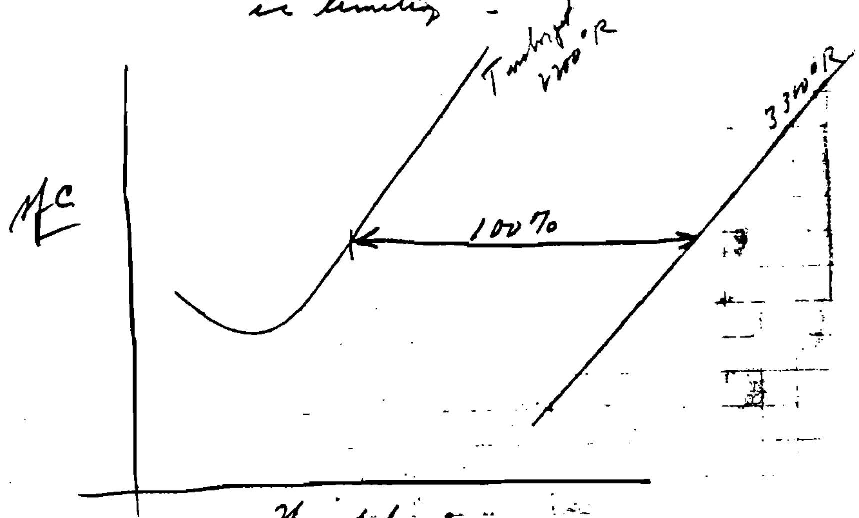


Advantage lies in maintaining engine

operation at high Mach No –

Thrust per frontal area when turbine

is limiting –



Substantial advantage in higher the inlet (turbine) temperature of the flow per frontal area are important

Neumann 6/2

Alford – Wley

La Pierre – 3

6/20/52 Conference on dual rotor vs variable stator compressor 137

Cruise at Mach 0.9 is a basic specification of Air Force -

Two designs in layout designed to the same standards of stress, etc.

4:1

Unresolved -

Complexity, weight, etc of control systems
Variable stator - performance of turbine at reduced thrust
Reliability of variable stator mechanism.

Maintenance

Completion dates

Ease of introducing improvement - eg variable diaphragm turbine.
Combination of dual rotor and variable stator.

Neumann 6/23/52

Assumptions regarding engine design which should be checked carefully before accepting recommendations -

Leakage - from seal at last compressor stage -
1% leakage reduces performance 3%
leakage may be used but when it is, pressure and load on thrust bearing usually increases

J47 - 1.2 lb/sec at PR 5:1
Leakage proportional to (D P½) -
Two seals on dual rotor machine

Bearing problem - thrust on bearing aft of compressor -

Turbine inlet temperature and blade cooling -
what are assumed turbine inlet temp?
is turbine blade cooling assumed

3310°R

Stresses - turbines at 1600°F - what life?
Turbine efficiency assumed -
Weight estimates -

Compressor performance - production clearances and tolerances.

Alford - Why not combine variable stator with dual rotor

La Pierre - 37" engine promised for F86 H and must be pushed - either be VS or DR -

14 Considerations in design -

Design method, assumptions, and physical ~~constants~~ data should be written in advance of calculations and drafting and should be reviewed and approved by next level of authority - Bound books -

Calculations, graphs, sketches, should show date, author, and reference to method

Scaling ratios - Report ?

Objectives - Specifications - performance, weight, Schedule

Conditions Speed, ambient total temp altitude, etc.

Strategic materials - cf British engines -
status of supply -
requirements of new designs

Materials - status of titanium fabrication.
high temperature materials for turbine

Combustion - volumetric rate of heat release
mass flow per unit area.
altitude starts.

scale effect - will development in 37" size settle J-79 length

luminosity of flame and cooling.
pressure drop in combustion chamber.
sensitivity to flow distribution
radial variation of turbine inlet temperature.

status of tests of new designs - same volumetric heat release - same mass flow per unit area
fuel air ratio - efficiency

Lubrication system - minimum temperature - max.
heat removal.
oil consumption -
effect of aircraft attitude.
type of lubricant - properties, decomposition.

Bearings - loads, speed size
status - available or special

Seals -

Compre

Cycle -

...e written in
... reviewed and
...ther -

...tion, and

...bient total temp
...ude, etc.

... effect - will
...point in 37"
...settle J-79 design

...temperature.

- same
...se flow per unit area

...mposition.

Seals - design proposed.
pressure drop, leakage,
effect of wear -
power loss -
review of types -

Compressor - Reynolds number effect at altitude -
de Haviland experience on Gyrone -
Max C_L assumed.
variation in axial velocity - radial
in stages -
prediction of off-design points - stall line
velocity diagrams at two operating points
analysis of VS effect. - optimizing VS stages - how many, and which stages
seals, seal leakage
balancing
bleed.
tip clearance - production clearance
steel, magnesium, titanium aluminum - casing
rotor } which stages?
mechanical design - stress limits stator
inlet choking - flow per unit frontal area.
tip speed
casing design -

turbine - compressor match -
stall margin, acceleration, control.
rpm -
corrected speed at altitude.

vibration, critical speeds.
stator mechanism

Cycle - thrust per lb of air per sec.
pressure ratio
bleed - effect -

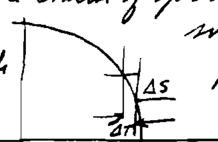
16 Starting - power required, torque
moment of inertia.

Refrigeration -

Reheat design - Mach 2.
diameter   length for altitude burning
transient on lighting flutters - re-temperature
control.

Control system - type   accurate control of speed - farming out to sub-contractors
Weight   small change in temperature
produces a large change
in strength near the
limiting condition.

definition of limiting
condition of operation
variations in production



Accessories - type
Weight

Turbine - wheel, composite or not
vibration
inlet temperature

Kuhs - Zeng - component program -
combustion
materials
fuel supply and distribution

application - 278 - grant 278
Mach 4 turbo-jet - better than ram jet -
lower weight 7% higher i/c than ram jet

X-333 turbo jet for hydrogen

Notes on Compressor development in AGT - (turbo-prop)

11/13/56

Schdy {TG100 started before GE took on the Whittle jet -
TG180.

Gene Hausser and Alan Howard have told me of the origins of these designs - NACA data plus Eckert and O'Brien on "Propeller Pumps and Fans" was all they had to start on - and they came out almost optimum the first time - Chap Walker also in group -

The engine responsibility and the physical engines were transferred from Schdy to Lynn - but none of the personnel involved in the design - (Cornell and some others transferred but were not active in design of these engines at Schdy -

TG100 went down-hill steadily after transfer with re-design producing progressively poorer results until it was abandoned -

TG180 - redesigned for more airflow and became TG190 or J47 -
E-23 version sold to Boeing and AF as an improvement never met the promises - compressor never achieved the desired performance or stall margin

MOB experience in 1950 trying to get a set of blade shapes and performance maps - records in chaos - no one knew which maps corresponded with which fix

14-stage research compressor - phenomenal peak efficiency - but a huge "hole" in the maps -

error in blade setting - design or luck.

J73 (Dash 21) sold on the basis of a growth version of

GENERAL ELECTRIC

SUBJECT

COPIES:

CINCINNATI, February 20, 1953

Dean M. P. O'Brien
Bldg. 100

Pursuant to our previous discussions, two projects seem most fruitful for your attention during the next few months:

1. Roy and I are very anxious for a review of the status of the Nuclear Project. As you may know, this Project is to be reviewed with Mr. Winne, Dr. Suits and Mr. Belanger on March 17, and we would like an independent status appraisal made well in advance of that meeting.

2. The J79 project appears of extreme importance to our business future. Consequently, as soon as the nuclear study is well in hand, we would appreciate your taking a look at the status of the J79.

C. W. La Pierre

CWLaPierre:feh

GENERAL ELECTRIC
COMPANY
CINCINNATI 15, OHIO

C. W. LA PIERRE
VICE PRESIDENT
AIRCRAFT GAS TURBINE DIVISION

December 6, 1954

Dean M. P. O'Brien
School of Engineering
University of California
Berkeley, California

Dear Dean O'Brien:

Project Hermes on which our A & O S Division has been engaged for some years is being terminated. I would like to know whether this Project has been carried on to the complete satisfaction of principal Army Ordnance personnel, or if not what are their criticisms.

Will you please investigate this question and give me your verbal appraisal with such notes as will not embarrass any sources of information.

I think you should start with General Simon with whom Mr. Fritz of our Washington Office will be glad to arrange an appointment.

Very truly yours,

C. W. La Pierre

C. W. LaPierre, Vice President
Atomic Energy & Defense
Products Group

ns

BERKELEY: OFFICE OF THE DEAN
COLLEGE OF ENGINEERING

March 27, 1944

PART IV. Documents Relating to Morrough O'Brien's Career at University of California at Berkeley

CONFIDENTIAL

President Robert G. Sproul,
Administration Building,
Campus.

Dear President Sproul:

Since our last discussion of the organization and administration of the College of Engineering and the engineering departments, I have reached the conclusion that it is absolutely necessary, under existing circumstances, ~~to consolidate the present~~ engineering departments into a single department in order to accomplish the objectives which you have in mind and which we have discussed on a number of occasions since my appointment. I recommend that the departments of Civil Engineering, Electrical Engineering, Irrigation, Mechanical Engineering, and Mining and Metallurgy be combined into a single Department of Engineering and that this consolidation become effective July 1, 1944. In making this recommendation I am mindful particularly that this step lies within your power and will not require formal approval and that it represents merely the culmination of a process initiated some years ago.

For some years, the budgets of the engineering departments have been printed in a form suitable for a single department. No change in the budget as submitted to you appears to be necessary to implement the consolidation.

Sincerely yours,

Morrough P. O'Brien

Morrough P. O'Brien,
Dean, College of Engineering.

MPO:G

President's Files
University of California Archives
Berkeley, California 94720

NOTES ON ENGINEERING EDUCATION

By MORROUGH P. O'BRIEN, *California Alpha* '25

IN ANY important human activity it is appropriate for each generation to pause for reflection upon their aims, objectives, and procedures, and to discover and eliminate those vestiges left by situations which have long since disappeared, and to refine and reformulate their philosophy. In so doing, it often appears that they are callously critical of their elders and are heedlessly abandoning a wealth of knowledge accumulated through long and painful experience. Education is a process which requires thinking. Professors must continue their education through life, and even deans occasionally think. In the process of thinking about engineering education, human nature and ability remain unchanged, but other circumstances do change; and the significance of these circumstances is variously interpreted, resulting in different conclusions as between one generation and another and between one individual and another of the same generation.


MORROUGH P. O'BRIEN

Educational problems are particularly difficult for engineers because their solution can seldom be based on quantitative criteria; or, if quantitative criteria are employed, there is usually involved a simplifying assumption eliminating imponderable but essential factors. Controlled experiments in education require long-continued and careful analysis and, unless used with discretion, may be harmful and unfair. On the other side are the facts that a good student is not harmed much by the worst program, and a poor one will not be greatly helped by the best. The gradual accumulation of experience with different educational techniques, especially in the form of case histories, will ultimately determine the pattern; but we must aid this process by attempting to formulate from time to time the practical procedures indicated.

The engineering societies and many individual engineers have attempted to define the boundaries of the engineering profession and to formulate the responsibilities and characteristics of the engineer with sufficient clarity and precision to mark him apart from the artisan, the business man, and others who may be engaged in engineering enterprises. K. T. Compton defines the engineer in the following terms:

"An engineer is one who, through application of his knowledge of mathematics, the physical and biological sciences, and economics, and with aid further from results obtained through observation, experience, scientific discovery, and invention, so utilizes the materials and directs the forces of nature that they are made to operate to the benefit of society. An engineer differs from the technologist in that he must concern himself with the organizational, economic, and managerial aspects as well as the technical aspects of his work."

Other professions are equally difficult to define accurately but, as in law and medicine, the practitioners are more generally segregated by arbitrary criteria, such as membership in organizations or licensing by public agencies. Separation of the professional engineer from the technician or artisan is difficult because engineering activity shades off gradually without sharp divisions from research and advanced design to routine drafting and other sub-professional work. Segregation of professional divisions, except possibly civil engineering, by valid definitions is almost hopeless. This haziness of the boundaries of engineering as a profession complicates increasingly our thinking on engineering education, licensing, unionization, and other important matters. We hesitate to adopt iron-clad criteria of education and experience because we all know of competent engineers who could not meet these criteria, and yet without clear-cut separation of the professional engineer from his sub-professional associates the standing of the profession is weakened. On the other hand, clear-cut differentiation of the truly professional group from other engineering personnel may well drive the latter group into unionization.

THERE is an important characteristic of the fields of employment open to engineers which bears on their professional standing and, to some extent, their education. Engineers are, in the main, salaried employees working in groups, frequently under non-engineering administrators. To an increasing extent engineers, particularly civil engineers, are employees of government. The nature of engineering imposes this group activity, but I have become concerned about the reasons for the failure of engineers to reach top administrative positions in organizations concerned primarily with engineering enterprises. There are, of course, many exceptions.

The nature of engineering may engender a viewpoint not suitable for administrative responsibility; engineers may deliberately avoid administrative responsibility, and this may be a wise decision; or the cause may be farther back in the educational preparation. Do we overwork the student in purely technical matters and fail to encourage collateral studies in social and economic sciences to the extent that he feels incompetent to cope with problems of general policy, taking refuge in purely technical work? Considering only personal satisfaction over a lifetime, the engineer who chooses to concentrate on purely engineering work is probably wise; but from the standpoint of public welfare and industrial progress it is desirable that more engineers move up into administrative positions. A usual answer to criticism of engineering education is to arrange a new course or a new curriculum. Thus, we now find engineering administration coming to the fore, but the problem is not solved so easily. The answer, if there is one, is likely to be found in reducing the number of curricula, softening subject requirements, and emphasizing the basic subjects and their analytical applications. An interest in, and appreciation of, the complexities and uncertainties of organized society combined with the existing factual and conservative viewpoint of the engineer will permit him to engage in a wider sphere of activity to the benefit of the nation, but I suggest that this movement will gain momentum only when its importance is emphasized at all stages of his development, particularly in the undergraduate years.

ENGINEERING is both an art and a science. The various professional fields such as civil engineering and metallurgical engineering combine art and science in different proportions, and the proportions vary with time. Civil, mechanical, and mining engineering developed essentially as arts and remained so until comparatively recent time, when the concurrent developments in other fields stimulated the introduction of a more scientific approach. This historical situation is important in thinking about established curricula which may need adjustment to incorporate later scientific development and changed responsibilities of practitioners. Other important branches of engineering, such as chemical, electrical, and metallurgical, stemmed from scientific discoveries and followed these discoveries by the interval necessary to temper the scientific facts with enough of the art to permit practical use.

Regarding the significance of such developments for engineering education, sound judgment based on experience will always remain important; but we can predict with certainty that science will infiltrate and ultimately permeate all branches of engineering. We can teach science efficiently and effectively; but experience can be transmitted in college only partially and at great expense in time, although it is rapidly absorbed in practice. The scientific base of engineering is expanding so rapidly and its intellectual disciplines are becoming so complex that we have no alternative to giving more attention to the basic principles of physics, chemistry, mathematics, and economics and to lessening the attention paid to details of current engineering practice.

A fallacy regarding specialization is the popular belief that the increasing tendency towards specialization in professional practice justifies specialization in the undergraduate years. I believe the opposite is true. We have all joked about the specialist who knows "more and more about less and less." These specialists are dangerous individuals if they do not build upon a broad foundation in science and if they are incapable of viewing their specialty in broad perspective and judging the effect on it of developments in other fields. The aeronautical engineer, judging from existing curricula in this subject, designs all components of an airplane including the engine and also makes wind tunnel tests of his design. In professional practice there is no such person. There are aeronautical engineers who specialize in structures, engines, power plant installations, wind tunnel tests, metallurgy, production, and other components but, in spite of this obvious fact, new omnibus curricula are even now being organized; I am sorry to relate that this development appears to be more a move to secure financial support than to meet demonstrated educational needs. A more logical development might be a sound background in mathematics, physics, fluid flow, mechanics, and other sciences which each specialist needs followed by a few elective courses in the specialties and a common design course emphasizing the interdependency of the specialists. The idea that a specialist is one who studies the same subject from cradle to grave is unsound because such an individual will soon be out of the main stream of engineering progress and permanently stopped in some quiet backwater.

I mention these views on specialization here because the trend of a few years ago was toward more and more specialized curricula. The experience of this war has emphasized the importance of the broad type of training and the fallacy of specialization, and, I hope, has started a move in the other direction. On the whole, engineering education has reflected, with some lag it is true, the changing status of the professional fields and has provided a scientific and analytical basis beyond the requirements of current practice and even beyond the requirements of the professional courses found in the same curriculm.

A PHASE of the development of engineering education which should not be overlooked is the fact that the early professors had been trained as mathematicians, physicists, and chemists and drifted to engineering because of their interest in practical applications. There were no trained engineers, and the initial pattern of engineering education was set by men of sound scientific background to meet the then-existing situation. The present tendency is to recruit the teaching staff from among graduates in the same engineering field and, furthermore, to induct them as teachers almost from the day of graduation or completion of graduate study. This situation produces a uniformity of viewpoint throughout departments which does not make for progress. Pressure of enrollments and limited funds has governed the choice of staff in many institutions more than other considerations, so the appointment of recent graduates who are familiar with courses and laboratories is understandable; but there is much to be said for the development of an engineering college as a team representing a variety of experiences and subjects, including, on the one hand, physicists, chemists, and mathematicians with an interest in and understanding of engineering problems, and, on the other, engineers of wide practical experience, possibly as lecturers teaching single subjects. Distinguished departments have a tendency to become so proud of their accomplishments as to think that only their own graduates can teach their courses; and when this condition develops, they have usually passed their peak of effectiveness.

THERE are common elements in all recognized engineering curricula in spite of institutional variations; and it is well to review these components. The basis on which the ECPD accredited the curricula is probably the best indication of their common contents.

1. Every four-year curriculum should embody four principal groups of courses:
   a. Those physical and mathematical sciences that are basic to the division of engineering under consideration. For all branches these would include mathematics, chemistry, and physics. In addition, other of the physical sciences basic to the field of the curriculum, such as geology in mining.
   b. The principal divisions of pure and applied mechanics relevant to the field of the curriculum, such as strength of materials, hydraulics, thermodynamics, electrodynamics, etc.
   c. A group or groups of courses of professional engineering content in the particular field of the curriculum. *Subjects of this group must bear a fundamental relationship to those divisions of mathematics and pure and applied science included in (a) and (b)* as preparation for the study of these professional courses.
   d. Subjects of a general humanistic and cultural nature, including English and general economics. There should also be included courses in drawing, descriptive geometry, and shop practice whenever they are necessary to later work or as elements in the particular professional field of the curriculum.
2. The treatment of the major subjects of every engineering curriculum, whether in basic sciences, mechanics, or professional subjects, should be quantitative and analytical in nature and not essentially descriptive or imitative or purely manual.

"Every engineering curriculum to be considered worthy of professional recognition must include sufficient work in design, or the creative phase of engineering, as related to machines, structures, systems, and methods. The *elements* of economy and costs must be included."

I HAVE quoted this statement of principles in full both because it represents the consensus of a large number of individuals and institutions and because it still represents more a goal than an actual achievement. Thinking not of my own or any other individual institution but rather the average situation in the well-known engineering schools, the principal deficiencies in achievement are, in my opinion:

1. On the average, high school preparation is generally inadequate to permit maintaining the work of the freshman year at the appropriate level. There appears to be a deliberate effort in the lower schools to eliminate mental strain with the result that students arrive deficient in factual equipment and in the methods of study. It is beyond the capacity of the universities to make up for twelve years of ineffectual training.
2. Basic sciences and mathematics are usually comprehensive and competently taught; but there is a psychological hurdle for the student deserving of attention. The engineering students apparently regard these science courses as foreign to engineering and only to be endured at best.

   A more fundamental problem is posed by the expanding scientific basis of engineering. If engineers are to apply science, they must understand the basic principles of science. They must do more than memorize facts about science. They must have sufficient experience to acquire a feeling for scientific phenomena and an intuitive solution for physical problems later to be analysed quantitatively. More time must be found for science courses, and one solution appears to be a combination of the sciences and mathematics with the applied mechanics courses in group (b).
3. The applied science, or applied mechanics, courses are being improved steadily but, in general, they quality. The students have been built up mathemat- and science courses. They make up in volume of problems what they lack in comprehensiveness and quality. The students have been built up mathematically and scientifically to a more analytical treatment, so many a discerning student regards these first engineering courses as "baby talk." The students have been taught differential equations only to meet many courses taught without calculus. Either the students should not be harassed with advanced mathematics in the sophomore year, or the courses of the junior and senior years should take full advantage of the mathematical approach. The student loses his grasp of mathematics when it is not used; and, worse still, the impression gets abroad that it is unnecessary. But the difficulty

lies not in the student's knowledge of mathematics but rather in that of the professor.

4. The professional courses characteristic of a profession necessarily reflect the state of that profession and cannot be carried much beyond current practice; but, from an examination of textbooks used and from conversations with teachers, I have the impression that many of these courses fall short on the analytical side of what is possible in the light of present knowledge.
5. Although English has been given increasing attention, other humanistic studies are usually impossible for the average engineering student.
6. The high mortality, amounting to approximately 600 out of every 1,000 entering students, indicates the need for more careful selection and guidance. Entrance requirements solely in terms of subjects appear not to be adequate.

The curriculum is merely a pattern of study. Success depends upon the efforts of individual instructors, and no amount of tinkering with curricula or expansion of physical plant will make up for deficiencies in competent teachers, but the effectiveness of good teachers is increased by a logical and efficient arrangement of subject matter and a reasonable division of student time between lecture, laboratory, and design courses.

WHEN one attempts to catalog what an engineer should know and brings into the discussion practicing engineers and professors representing a variety of viewpoints, the resulting list of subject matter and experiences, curricular and extracurricular, would require many years to complete. Every curriculum is a compromise between real and fancied needs, established practices, interests of the faculty, and efforts at forecasting future trends. Whatever is done will not please everyone, and engineering alumni will continue to report that they have needed only a small fraction of what they studied but that a new course in "such and such" should be introduced. However, we should not thereby be discouraged from efforts at improvement.

A development which has already been suggested by K. T. Compton, and which I think likely to come about in the near future, is a re-grouping of the engineering curricula along essentially functional rather than professional lines. Opposition from the professional societies is likely, but it is too logical an arrangement to be longer ignored. As engineering has progressed from art to a science, the elements common to the professional fields have come into prominence and differences between professional subjects have become less marked. The traditional divisions of the engineering field, such as civil, chemical, mechanical, and electrical, include engineers who are engaged in research, design, development, construction, operation, sales, maintenance, management, patent law, and many other functions. With only slight variations due to the choice of electives, the engineers performing these functions in each profession group are subjected to the same educational program. It is now becoming apparent that engineers engaged in design, research, and development in the various professional fields have more in common than they have with engineers performing the other functions in their own field. For example, the chemical engineer engaged in research employs analytical and experimental methods and must acquire very nearly the same viewpoint as the mechanical engineer or electrical engineer performing similar functions; but he has little in common as to necessary educational background and experience with the chemical engineer turned plant manager. Research and design require a more comprehensive scientific and mathematical preparation and more experience in analytical and experimental methods than the other functions but do not require extensive professional preparation. Furthermore, the research and design group should probably plan their undergraduate years on the assumption that one or more graduate years will follow. The other group, which should be designated in some fashion not indicating inferiority, should give more attention to economics, applied psychology, statistics, production techniques, and professional practice. Whether this functional differentiation can be arranged within present professional curricula will depend on the number of students, and, for strategical purposes, it may be desirable to make the differentiation within present curricula; but, once started, I would expect a re-grouping of the pieces along functional lines for the reason that the logical course of study for, say, the design and research group in mechanical engineering, will be found to differ so little from that in electrical and civil that all will be combined.

I am convinced that under present plans the men capable of design and research are not pushed as far as they can be along lines appropriate to their interests and that the other groups are carried further in mathematics and applied science than need be, to the exclusion of subjects of value to them.

To be effective, differentiation between the functional groups should develop gradually between the freshman and senior years, should be advisory rather than compulsory, and the designations of the functional groups should not indicate superiority of one over the other.

I WAS recently asked by a vice-president of a large corporation why universities desired fellowships in view of the fact that graduate instruction is much more costly per student than is undergraduate instruction. The question surprised me because I thought the answer obvious; but I realize now that it does require exploration and also I find that faculty members are not entirely in agreement with me.

The need for graduate study varies with the professional field. In mining, except for a few specialists, graduate study is unnecessary, while in electrical communication it is essential for those entering upon design and research work. My comments pertain to fields in which graduate study is desirable for the design group.

Graduate study and research are closely related because one directly contributes to the other. Without research, few graduate students will be attracted, while without graduate students as collaborators, few faculty members have time to carry on research. In a sense graduate work and research are the frosting on the cake but without that frosting it isn't much of a cake. My own line of reasoning is as follows: Good undergraduate instruction requires good teachers, and the only certain method of maintaining good teaching of engineering over a lifetime is participation in research in some form or other. It is the exceptional engineer who can confine his work solely to teaching and, at the same time, keep up with modern developments to the extent that his problems and classroom examples impress the students with the importance of his subject. A majority of the young engineers competent to teach will not remain at an institution where research is not fostered, and this attitude is becoming more pronounced.

The principal stimulus to faculty progress in research is competent assistance; and this need is met by graduate students who, in turn, derive immeasurable benefits from the close and continued association with a faculty member of experience, understanding, and judgment. Few engineering students can afford graduate work without financial aid and hence the need for fellowships.

To repeat the argument, fellowships aid not only the recipient but they also partially support research by the faculty through the assistance thus made available; and this research is essential to maintain the quality of undergraduate instruction, and hence the value of the four-year graduates to industry and the public.

The same line of reasoning applies to research grants. The scientific and technical results obtained are valuable, but the most desirable result is the stimulus to the staff and, secondarily, the advancement of the graduates who participate in the work.

Research can take many forms. It may be experimental or analytical. It may be directed toward educational problems or consulting services. In essence, it consists of thinking about and solving new problems and is a counterbalance to the necessarily repetitious teaching of courses. Some men can remain competent teachers throughout a lifetime but the chances of doing so are not good. When I find a young instructor who is not engaged in any sort of original investigation, I usually conclude that we are taking too much of a chance in keeping him.

I know that some of my colleagues do not agree with me regarding the fundamental importance of research to *undergraduate instruction* and that I have been criticized for a preoccupation with this phase of our work; but I am convinced that, after this war, research will assume even greater importance in engineering education and that only those schools will achieve preeminence which have a sound research program, suitable to their facilities and location, in which a large percentage of the staff participate.

SEVERAL war-time developments promise to influence the post-war trend of engineering education, namely, the Army and Navy training programs, the Engineering, Science, and Management War Training Programs, and the War Research Projects.

There have been many military training programs of limited extent in which the engineering colleges have participated; but those which have affected all colleges to a major extent are the Army Specialized Training Program and the Navy College Training Program, both intended to supply officers with technical training. I shall not go into detail regarding the plans followed or the operational difficulties encountered. Suffice it to say that the colleges have been under great strain both because of the actual work involved and the uncertainty over what might be in prospect. My own conclusions regarding these programs are:

1. They demonstrate the dangers inherent in a centralized educational system which forces uniformity and eliminates the individual policies and philosophies of engineering education, which the several institutions have developed to fit their own situations. The effect, if continued, will be a lessening of the feeling of responsibility of the colleges and their faculties. The Navy program is superior to the Army program, but both are undesirable.
2. Sponsored by the Army and Navy, there appears to be little danger that they will result in a centralized and regimented post-war system of engineering education.
3. Consisting of a military or naval program superimposed upon a heavy educational program, the students are overworked and the average student may not survive if standards are maintained.
4. These programs have not demonstrated that engineering education can be speeded up and condensed. The subtle processes of education require time. What is happening is that students are forced to depend upon memorizing and the fundamental quality of engineering, the capacity to analyze new problems, is being forced out of the program.

The Engineering, Science, and Management War Training Program under the U. S. Office of Education represents an entirely different policy of federal support. Organized when the shortage of engineers became evident, with the realization that no substantial increase in four-year graduates would be possible, the objective was to supply draftsmen, inspectors, laboratory technicians, and other sub-professional personnel who could take over some of the functions of the professional engineer. Unlike the Army and Navy programs, which dictated course content, the ESMWT program encouraged each institution to follow its established policies and procedures, to maintain the standards it thought appropriate, and to work out courses meeting the needs of local industries. The significance of the ESMWT program for post-war engineering education appears to me as:

1. Plans can be worked out under which government support can be made available, even to privately endowed schools, without domination and regimentation.
2. Many positions ordinarily held by engineering graduates do not require such complete preparation and can be filled by persons with less formal education. Many companies have learned the lesson that positions of a routine nature are best filled by individuals who do not aspire to greater responsibilities.
3. There is need for part-time instruction for graduate engineers to stimulate continuing study, to fill in gaps in undergraduate training, and to cooperate with industry in developing their research and graduate engineers.
4. The engineering colleges have been brought into close contact with industry and have learned the importance of close and continuing liaison to guide educational plans to meet industrial needs and to appraise the success achieved.
5. Many practicing engineers are competent teachers, are willing and interested in part-time teaching, and bring to their classes a knowledge of current practice and problems usually impossible for the full-time teacher to acquire.

The ESMWT program is not perfect, but, on the whole, it has done an important job, and the final reckoning will show, I believe, that it has benefited the colleges.

The volume of war research conducted by educational institutions reaches a staggering total. The last annual report of M.I.T. shows an expenditure of $15,000,000 for government projects and $800,000 for industry. Most of the work throughout the country has been conducted under the sponsorship of departments of chemistry, physics, and medicine; and only a small fraction of the total has gone to engineering schools, for reasons which are more than chance, although chance circumstances and personalities played a part. The scientific societies and leading scientists began more than three years ago to organize for war and presented concrete proposals for research on instrumentalities of war, resulting in the Office of Scientific Research and Development and, under it, the National Defense Research Committee. The engineering societies apparently made no move to guide wartime research and only recently has the engineering counterpart of OSRD, the Office of Production Research and Development, come into existence. However, there is, in my opinion, a more fundamental reason than simply the failure to take group action ahead of the scientific groups.

Engineers have done a splendid job of producing the equipment needed for war, and the current flurry over passing the production peak is better evidence of success than any statistics could be. However, in devising essentially new instruments and weapons, the engineer has not measured up to the physicist and chemist. I do not refer to the process of discovering new scientific principles but rather to the engineering problems of combining existing principles to devise a new weapon, a new machine, or an offensive or defensive procedure. For three years I have been connected with a succession of war research projects in which the physicists generally suggested the radically new developments which the engineers later designed in detail and built. It is a point which to me appears most important and I was disappointed to find a lack of imagination and too great a deference to existing practice.

The bazooka gun, radar, anti-submarine devices, and many other new applications of science to warfare are distinctly the product of scientists and not engineers, although the scientific principles were established and available to both groups. The question important to consider is whether we are stultifying the imaginations of engineering students by crowded curricula, too heavy assignments, in-breeding of faculty, and other undesirable practices.

A FEW conclusions which I have reached regarding trends in engineering education after the war are:

1. The more advanced schools will move toward emphasis on applied science and will reduce the strictly professional courses and curricula.
2. These schools will be found to derive much of their strength in *undergraduate* work from graduate study and research.
3. Technical institutes (or junior colleges) will take over sub-professional education of engineering personnel. The total enrollment and the number of purely professional engineering schools will decrease. Institutes will ultimately graduate more students than the professional schools.
4. Rehabilitation for returning veterans will produce a temporary and inconvenient bump in enrollments.
5. Selection and guidance of students entering professional engineering schools will assume importance equal to that of licensing engineers and accrediting engineering schools. There will be *fewer* graduates of the full professional curriculum; but they will be better selected and more adequately prepared than at present.
6. Part-time, night engineering courses will become popular in more metropolitan centers and will

play an important part in the graduate program.

7. Co-operative programs in which the student both earns his expenses and gains engineering experience during his undergraduate years will find increasing favor.

On reading what I have written, I find that it falls short in both clarity and scope of what I should like to say but, in these turbulent days, there is little time for reflection and it is difficult to concentrate on anything but the next deadline to be met. A comforting thought is that few engineers will have time to read these comments, leaving their crude and unfinished character undiscovered and unexplored except to the editors and possibly a few light sleepers who find articles on engineering education a cure for insomnia.

Dean O'Brien is head of the College of Engineering at the University of California. A civil engineering graduate of Holy Cross College in 1925, he taught at Purdue and worked for its experiment station for two years. While holding an American Society of Civil Engineers Freeman Traveling Scholarship in 1927-28, Dean O'Brien was an engineer with the Royal College of Engineering at Stockholm. On returning to the United States he became a professor of mechanical engineering at California. He was chairman of the mechanical engineering department when he succeeded Dean Charles Derleth, Jr., as head of the College in 1942. He has been a member of the U. S. Beach Erosion Board since 1938.

[June 22, 1944]

POST-WAR PLANS OF THE DEPARTMENT AND COLLEGE OF ENGINEERING

The Engineering faculty have given thought during the past year to the development of the educational opportunities, personnel, and physical facilities of the Department and College of Engineering and this report is a synthesis of recommendations and views expressed both individually and through departmental committees. The items discussed are not of equal importance as compared with others, and the report cannot be considered as complete and final but it is believed to represent current ideas of the Engineering faculty and, as such, it submitted at this time to stimulate further thought.

The basic objectives which, it is hoped, will be served by the developments to be presented in detail subsequently in this report are the following:

1. Undergraduate instruction leading to the bachelors degree and preparatory for professional engineering practice.

2. Graduate instruction and research.

3. Basic research in applied science. (One important function of the engineering faculty is to analyze, synthesize, and simplify the results of scientific research and present them to the professional practitioner in form suitable for use. This process requires a knowledge of both science and engineering practice and involves both analysis and experiment.)

4. Industrial research and development, particularly in support of new and small industries.

5. Public services through participation of members of the faculty in the work of governmental and private organizations concerned with the public welfare.

6. Enhancement of the standing of the engineering profession and, to a certain extent, representation of it in its relationship to society.

7. Cooperation with other components of the educational system both in the preparation of students entering the College of Engineering and in the life long education of practicing engineers.

The College of Agriculture serves agriculture, and the people of the State generally, in a manner which Engineering should emulate with, however, modifications in details of procedure to accommodate the differences between agriculture and industry.

COLLEGE OF ENGINEERING

Curricula.

All curricula represent a compromise between the limited time available for instruction and the extensive and ever-increasing body of knowledge

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supporting engineering practice. At graduation, the engineering student is, at best, prepared only to begin to learn about his chosen profession and he must serve what is essentially an apprenticeship before being accorded recognition as a professional engineer. The conditions surrounding initial employment of the engineering graduate varies with the professional field and the different curricula must reflect these conditions by varying the emphasis on the art and the science of engineering but it is believed that science and applied science plus methods of expression and analysis should be emphasized in formal instruction and that sound judgment representing the art of engineering should be developed through experience. The engineering curricula have remained static for some years and should be re-examined as to their desirable objectives and the extent to which these objectives are being met.

The abrupt decrease in the number of civilian students and courses and the increasing number of Navy V-12 courses being offered present a situation favorable to revision of courses and curricula. Unfortunately, members of the staff are at present too busy with war activities to give much attention to post-war developments but the problem will be studied during the coming year.

Curricula in Chemical Engineering and Engineering Physics should be established in the near future. A committee appointed by President Sproul is considering Chemical Engineering and preliminary discussions have been held with members of the Department of Physics regarding Engineering Physics.

Production methods, engineering economics, industrial psychology, and the range of subject matter frequently designated as industrial engineering should be given greater emphasis after the war to support the industrialization of the State. However, it is believed that this field will be best developed as a joint program under Engineering and Business Administration rather than as a separate curriculum.

Ceramic engineering is now being considered, possibly as an option in Mining or in Chemical Engineering.

In planning curricula and options and in recommending elective courses, the present and likely future activities of engineers should be recognized by greater cooperation with Agriculture, including Soil Science and Food Technology, Biology, Physiology, Psychology, and other basic and applied sciences.

The engineering curricula represent an integrated sequence of courses in which students must master the subject matter of each year before proceeding to the next. Certain techniques and procedures are prerequisite to subsequent courses. An unfortunate result of this situation and the pressure of a crowded curriculum is the tendency on the part of the faculty to accept the mechanical substitution in formulas as evidence of understanding and to emphasize established design procedures. In general, originality and ingenuity suffer. Although some improvement may be effected through modification of courses and curricula, the solution lies in smaller classes and in a faculty having broad scientific knowledge and an interest in the technique of instruction.

Classroom instruction, and to a lesser extent the grouping of courses in curricula, should be reoriented to give greater emphasis to functions such as research, design, operation, construction, maintenance, sales, and so forth. The present courses and curricula generally aim to prepare students for design

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functions which are only a fraction of the field of engineering practice. Emphasis has been placed upon the professional divisions, such as Civil Engineering, and little attention has been given to the functions within each division. Design itself should be given a different, and in some curricula a greater, emphasis.

Aptitude Tests, Counselling, and Placement.

It is a matter of concern to the engineering faculty that the mortality is so high among students entering the College of Engineering. The public relations of the University alone justify considerable attention to this problem. It is suggested that the College of Engineering undertake to:

1. Arrange lectures at high schools and Junior Colleges on the nature of engineering and its intellectual and social requirements. The history of engineering, the scope of the professional fields, salaries, conditions of employment, and other aspects of professional practice should be emphasized in popular lectures, amply and carefully illustrated.

2. Provide pre-college counselling through local alumni and engineering faculty.

3. Investigate and develop aptitude tests to support and supplement counselling.

4. Provide more extensive counselling in college.

5. Expand the placement service, especially to permit promotional work in industry and to assist the graduates after initial placement. Analysis of engineering success is necessary in the development of aptitude tests and as a basis for counselling.

These activities should be conducted in close cooperation with existing agencies of the University but members of the Engineering faculty must participate. The desired result will not be achieved unless the Engineering representatives in this work are relieved of a portion of their normal teaching assignments and their success is recognized by promotions and salary increases.

Articulation with Educational System.

The Colleges of Engineering at Berkeley and Los Angeles should collaborate not only regarding internal policies and procedures but should also develop a uniform policy regarding their relationship to the Junior and State Colleges and the vocational schools. In terms of numbers of students, vocational and sub-professional training should overshadow professional engineering education and yet this area is neglected while the Junior and State Colleges apparently aspire to offer larger segments of professional training. If this tendency is to be checked and the other components of the educational system deflected into suitable activities, the University should lead in the development of a comprehensive plan of engineering and technical education and should withdraw, in both regular instruction and Extension Division, from sub-professional and vocational instruction. A possible exception would be experimental groups in the latter categories mentioned in the Santa Barbara College and at the University of California at Davis.

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The War Training program has demonstrated a need for the training of draftsmen, surveyors, inspectors, laboratory technicians, and sub-professional personnel but there appears to be no plan to meet the need.

Scholarships and Fellowships.

A majority of the engineering students earn some of their college expenses and many are self-supporting. This experience is valuable but too many students are prevented from taking full advantage of the educational opportunities offered because of financial difficulties. Larger loan funds and more scholarships and fellowships are urgently needed.

If duplication of instruction in specialized subjects at Berkeley and Los Angeles is to be avoided, scholarships and fellowships should be provided to equalize costs to those students who transfer from one campus to the other.

Extension Instruction.

Especially at the upper division and graduate level, the College of Engineering should cooperate with the Extension Division in developing courses and sequences of courses for the following purposes:

1. Institutes of short duration to bring practicing engineers and engineering researchers up-to-date. Summer institutes for teachers of engineering should cover heat transfer, fluid mechanics, dynamics, elasticity, and other applied sciences as well as pumping machinery, refrigeration, instrumentation, and professional subjects. Examples of institutes for practicing engineers are: Highway engineers conference such as is held annually at Purdue University; production methods for designers and production supervisors; and similar subjects.

2. Upper division courses of the same character as our senior electives to permit graduates to broaden their training.

3. Graduate courses. (A majority of these courses should be offered at night on campus by the Department of Engineering and not through Extension Division but there is need for off-campus courses in industrial areas which should be offered by the Extension Division.)

4. Correspondence courses.

Prior to any expansion of the program of the Extension Division in Engineering, there should be a review of the Administrative, academic, and financial arrangement governing such work. There is an inconsistency in a system which sets a normal teaching load in regular courses in order to foster research and professional development and then permits additional teaching in Extension Division. The War Training program has demonstrated the need for continued supervision of technical courses taught by non-University instructors. These problems are mentioned merely as examples without attempting to formulate a solution.

Degree credit for courses in Extension Division for upper division and graduate courses might be obtained by examination while a registered student on approval of the Dean of Engineering, and upon admission to graduate status on approval of the Dean of the Graduate Division respectively.

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It may be desirable to add a Division of Extension Service in the Department of Engineering to handle the academic side of Extension courses. The staff of this division would be primarily Extension specialists but might also offer a few regular courses. This arrangement would provide both supervisors and instructors for the Extension Division, the salaries being covered by transfer from Extension Division on a proportional basis. The idea underlying this suggestion is that Extension specialists should teach some regular courses if standards are to be maintained, that all such courses should be under the supervision of the subject departments, and that Extension work in engineering will not prosper unless guided by a permanent engineering staff.

Relations with alumni

There is need for greater efforts to inform the alumni about the College of Engineering and to enlist their active support in its development. Present plant include:

1. Cooperation with the California Alumni Monthly and the University News Service in giving publicity to the College and its Alumni.

2. Annual open house, possibly at Commencement.

3. Informal on-campus meetings with small groups of alumni to discuss specific problems.

4. Visits by members of the staff to local engineering alumni groups.

Rehabilitation and Instruction of Veterans and War Workers.

Veterans without previous technical instruction who have been engaged in technical work during their military service, engineering students who enter the services before receiving their degrees, and sub-professional technical employees in war industries may decide to study engineering after the war. Their numbers added to the normal entrants will tax the facilities of the college and there will arise difficult problems because of variations in preparation, attitude, and interests. A fraction of these "war" students will be educationally and tempermentally qualified to enter the regular courses but the remainder, and they may be a majority, will require special instruction. The fact that they will not be released from military control exactly at the beginning of the regular terms will tend to force special arrangements.

It is suggested that these students be provided for as follows:

1. Organize full-time comprehensive courses with a single full-time instructor in charge but assisted by specialists in some of the instruction. These courses would meet, say, six hours a day, five days a week and would not be broken down into component subject courses.

2. Sections of students of approximately equal preparation would be formed on the basis of a comprehensive examination to fix the starting point for instruction.

3. Sections would start at irregular dates but would all be terminated by another comprehensive examination at the end of each regular term. These students whose examinations show sufficient preparation would then register as students in the College of Engineering and follow a program of regular courses.

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4. Instruction of this type should be limited to the subject matter of the first three years and the material to be covered would be approximately that of either the first, second, or third year, but with the understanding that each section will cover such subjects as will permit the majority of the students to enter the regular curriculum at the earliest possible date.

5. Sections will be limited to fifteen students to permit a maximum of individual instruction.

6. The cost of this special instruction should be segregated from that of the regular program.

Some returning veterans will desire to enter industrial employment immediately but will also seek to fill out their educational background through part-time courses at night. Their needs can be met through Extension courses.

An integral part of this program is a counselling service with adequate staff.

Administration.

Although the Colleges of Civil Engineering, Mechanics and Mining have been consolidated under a single dean, in practice there have been as many college offices handling student matters as there have been departments. The present plan is to separate college and departmental functions as completely as possible by assigning study lists, graduation and other student and curricula matters to the Study List Committee of the College under the chairmanship of the associate dean. The members of the Study List Committee will not be chairmen of divisions and all related work will be centralized in the College office.

In order to carry out these plans for the College of Engineering the assistance of clerical and academic personnel will be necessary.

DEPARTMENT OF ENGINEERING

This report has been arranged on the principle that departments are concerned with physical facilities, staff, courses, and graduate study but some components may be incorrectly placed because in the Engineering group the functions of the former colleges and departments have in practice been combined and generally confused.

Student Body.

The maximum student registration was reached in the Colleges of Engineering and Mining in August 1939 when approximately 2100 graduate and undergraduate students were accommodated by the engineering departments. The engineering faculty generally agree that the capacity in staff and facilities at that time did not exceed 1200 and that effective instruction was offered only because members of the staff exerted themselves beyond what could reasonably be expected of them. A few of the courses could have accepted more students at that time but the general situation was one of extensive overcrowding and it was fortunate that the war reversed the trend in numbers of students because the quality of instruction would ultimately have suffered.

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Looking ahead it is likely that the number of students desiring to register in the College of Engineering at Berkeley will reach 3000 after the war and a majority of these students are likely to choose mechanical, electrical, and chemical engineering. The last mentioned curriculum has not been inaugurated but it is a necessary development of the immediate future. This number, which is almost a certainty, cannot be accommodated in the present buildings even with an increase in staff. Even offices for the necessary increase in staff are not available.

After the temporary peak of 3000 at Berkeley, there will be a decrease partly accounted for by the College of Engineering at Los Angeles and partly by the reduced backlog of applicants. It is suspected that the number of students in Engineering will reach a firm level of at least 2000, of which at least 400 will be graduate students.

Physical Facilities.

In order to accommodate the expected number of students, it will be necessary to provide additional class rooms, offices, laboratories, drafting rooms, and study facilities. A committee of the Department of Engineering is at work on the design of buildings and facilities and their report should be available within the next few months.

The general need is for additional space but a few specific needs are as follows:

1. Chemical Engineering. A large fraction of the experimental facilities required are already available but there is need for laboratories devoted primarily to the process industries as such.

2. Production Engineering. The war production program has demonstrated not only the importance of production techniques but the need for extensive instruction and research in this field. The trend towards industrialization in California will be strengthened by an adequate program in this subject. Under the term production engineering is included the engineering aspects of industrial engineering.

3. Metallurgy. Through our war research program leadership in certain lines of metallurgical research has been achieved and this advantage should be maintained by further expanding the facilities which have been improved in the past two years. There is no recognized center of metallurgical research west of the Mississippi River and a remarkable opportunity exists for development of this subject.

4. Research Services. Pilot plants, calculating machines, spectrographs, differential analyzors, photogrammetric equipment, and other equipment and facilities required occasionally on individual projects but essential to a research program should be provided under administrative arrangements which will make them available to all campus projects as well as to Engineering. A Division of Research Services is suggested. A Standards Laboratory should be included in this division.

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5. Central Precision Shop. A small instrument shop is now maintained by Electrical and Mechanical Engineering. This function should be expanded.

6. Central Heavy Machine Shop. Each division requires heavy machines work only occasionally but engineering plus other campus departments require this service.

7. Ceramic Engineering. This subject may be conducted as a component of either Process Metallurgy or Chemical Engineering. The need for instruction and research in this subject is being studied and a recommendation may be made in the near future.

These items are mentioned as being essentially innovations. The basic need is for general expansion of facilities in all present fields with the possible exception of Engineering Materials.

Salaries.

Considered as a full-time position during twelve months, University salaries are inadequate to attract and permanently hold the calibre of instructor desired; for a half-time position during nine months, the salaries are high. The University must differentiate in its salary scale between men who give their full time and energy to instruction and research and those who engage in consulting practice, extension teaching, book writing, lecturing, and other activities which bring added remuneration.

It is suggested that University salaries be divided into two components, namely:

1. A basic salary for a normal teaching assignment during two terms.

2. A supplementary amount for:

   a. Extra teaching assignments in summer session or third term, extension, or in the regular terms.

   b. Administrative assignments.

   c. Research.

   d. Public service

Such a plan would leave those who choose it freedom to engage in outside work. Unless financial support is given to those who engage in basic research, young men in engineering will be impelled by circumstances into remunerative consulting work.

The basic on which are granted promotions and salary increases should be given further thought. Present salaries are inadequate in comparison with the standards for promotion.

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Staff.

There is need in the engineering faculty for a majority who have graduated in, and practiced, engineering but a certain percentage should be made up of graduates in chemistry, mathematics, physics, and possibly other sciences who have developed an aptitude for the application of their science to technical problems. Engineering is fundamentally the application of science and it follows that an understanding of scientific developments is essential to engineering progress. This understanding may be achieved in part by encouraging engineering students and faculty to study science but to a certain extent scientists must mingle with engineers and this is best accomplished by bringing them into the engineering faculty.

Chemical, production, and ceramic engineering, and metallurgy have been mentioned as possible fields for the expansion of physical facilities. Staff should be added in these fields and in hydrology, soil mechanics, aerial photogrammetry, and in some other newly developed subjects as well as in those now well established but inadequately supported. The historical and sociological aspects of engineering must be represented on our staff and engineers trained in these fields as well should be added to the staff. Plastics, petroleum refining, asphalt technology, welding, powder metallurgy, and many other specialized technical subjects might well be represented by research, and some instruction, in a major engineering school. The point involved is not to urge any particular subjeet but to emphasize the need for some spare capacity in facilities and staff which can be applied to new developments which appear to be important either educationally or in the economy of the State.

Research.

The degree to which members of the faculty participate in research will vary with the individual's interests and aptitude but the opportunity to engage in research should be available to all, especially to the younger men entering upon a teaching career. Equal in importance to the scientific and technical results of research is its effect on the quality of instruction. Space, equipment, funds, and a sound policy of industrial cooperation are needed to foster research on an appropriate scale.

Library.

Instruction and research in Engineering both require enlarged library facilities and services. A branch library serving Chemistry, Engineering, and Physics is desirable.

Industrial Research.

Research and development in cooperation with industry, especially with companies and associations of companies too small to support a research organization, is believed to be a function of an engineering cblege in a State University second in importance only to instruction. To an ever increasing extent, modern industry lives upon research but many small companies cannot afford research programs which may require more than their accumulated reserves before a profitable conclusion is reached.

From many conversations with persons interested in industrial research, it appears that a "label" is desirable for the administrative mechanism guiding this activity and to meet this need I recommend the establishment of a Division of Industrial Research in the Department of Engineering. This recommendation is fundamentally only a formal recognition of arrangements already in effect but it can be publicized as a new development in order to call attention to our efforts to cooperate with the industries of the State.

The Board of Patents appears to have made some progress but the University is far from having a patent policy. Remuneration of the inventor, conditions of licensing, disposition of royalties, and many other aspects of patents must be the subject of clear and public statements before cooperative research can proceed without delays for negotiations and embarrassing misunderstandings.

Cooperative Curricula.

It is desirable that engineering students acquire some industrial experience before they reach the senior year. An obstacle in normal times is that the students available in vacation periods exceeds the number of positions open. The cooperative schools fill a number of positions continuously by rotating students between work and study. Because of the dependence of the engineering curricula on non-engineering departments, this arrangement is difficult in a large University but a possible plan is to offer a sufficient number of basic courses in the summer term to permit a fraction of the students to attend the University two terms out of three with one-third of the group free each term. A summer session in engineering appears to be desirable in any event to aid regular students and enrollments would probably justify offering almost all of the courses of the first three years. If such a summer session program is developed, a cooperative plan would become possible without conflicting with the normal two-term program.

Service Courses for Non-Engineering Students.

Some years ago, the Department of Mechanical Engineering offered a course on the automobile for non-engineering students but it was discontinued because it had been criticized as being informational in character. Considering the importance of technology in everyday life and the fact that such practical courses, if properly taught, can stress fundamental principles of physics and chemistry as effectively as formal courses in these subjects, it is suggested that a few such courses in the automotive, aeronautic, and communication fields be again offered if their need is established by non-engineering faculty.

Coordination with the College of Engineering at UCLA.

It is hoped that close collaboration in major policy matters will develop between the Colleges of Engineering at Los Angeles and Berkeley. Exchanges of faculty and students, especially of graduate students, technical conferences alternately held on the two campuses, a joint series of publications, and other means may be found suitable to foster this cooperation. Uniformity is unnecessary but there should not develop obvious conflicts in basic policy affecting relations with the people of the State. Active and obvious competition in industrial research must be avoided.

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Dean, College of Engineering
Chairman, Department of Engineering
June 22, 1944

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1946

FIVE-YEAR PLAN OF THE DEPARTMENT OF ENGINEERING.

The College of Engineering emphasizes in its instruction the sciences fundamental to the practice of engineering, and gives only moderate attention to what is properly termed the art of engineering. In engineering practice, art, in the sense of sound judgment based on experience and of creative design, plays a more important part than science. An engineering school which does not include in its faculty a reasonable percentage of professional engineers, who have demonstrated their competence in the art of engineering by the practice of engineering, will not be successful in its primary function of instruction. Present policies regarding promotion fail to recognize this need and places unjustifiable emphasis on research as the only avenue of promotion. At Berkeley in engineering, we encourage and emphasize research, perhaps even over-emphasize it, but in doing so we are not unmindful of the fact that, no matter how renowned a staff may be for its research, engineering education will fail in its purpose if the viewpoint of the practicing engineer is not represented in the formulation of courses, curricula, and research programs. Under present promotion and appointment policies, men of the standing of Hyde, Derleth, Etcheverry, Harding and LeConte cannot be appointed or promoted. LeConte was an outstanding teacher and the others mentioned are known for their ability as practicing engineers.

Since my own personal preferences are on the side of graduate study and research, I am inclined not to urge this point again but you should understand that we are being forced into an unsound position. Men who have practiced engineering are as important to an engineering school as those who carry on research and, unfortunately, every possible inducement of academic rank and salary will be necessary to attract engineers with professional experience.

In Mechanical Engineering, a chain of circumstances has resulted in a staff which is too young, on the average, for a professional school. Rapidly increasing enrollments in the years just before the war required the appointment of an increasing number of young instructors and subsequently four senior members of this division have taken on other duties which reduced their participation in teaching and research. I refer to Boelter, Howe, O'Brien and Woods. There must be appointed within the next year at least three full professors in this division to correct this present unbalance in age and experience.

The Mackey Professorship of Electrical Engineering should be filled. It is our conclusion that the man appointed should be an electronics engineer with industrial experience, including some experience in manufacturing.

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Non-Academic Staff

Sufficient non-academic assistance should be available to relieve the academic faculty to the maximum extent possible of routine duties. In addition to an increase in the clerical and shop force, graduate engineers should be appointed in a non-academic status to design and manage laboratory facilities and equipment. The design and development of instructional equipment resembles research in its effects, but this function is not recognized in relation to promotion, and, it follows, that the academic staff should not be expected to do more than direct this work.

Research

The primary function of the University is instruction, both graduate and undergraduate. In order to maintain the competence of the instructional staff, research and professional practice by the staff are essential. Unfortunately, research in engineering has been hampered by inadequate funds and facilities. It is hoped that sufficient support can be obtained so that, in the future no member of the staff can justly claim that he had no opportunity to engage in research.

One means of obtaining support, and a means appropriate to an engineering school, is to engage in research sponsored by the government and by industry. We have already proposed establishment of a Division of Engineering Research, which is essentially only an administrative mechanism for carrying out University Regulation No. 4. If space can be made available and reasonable latitude given us in the matter of contractual terms and overhead, this proposal should bring sufficient support to make up for limited University research funds and for a salary scale below that of the Federal Government and industry.

It has become apparent that in the last few years a portion of the field of engineering research cannot be conducted in instructional buildings. Noise and vibration, fumes, smoke, physical hazards, and other circumstances either prevent such work on campus or make it prohibitively expensive. It has been proposed in the biennium budget and in a letter regarding transportation engineering that a field station be established for this research. The Fluid Mechanics Laboratory on College Avenue has been developed almost exclusively through sponsored research and it is believed that the same result will follow for all of engineering if space is provided. Fenced-in land to the extent of at least 80 acres, services, and a headquarters building are the minimum requirements to start a field station.

Buildings and Facilities

Funds are available and the design is in progress for Building No. 7 in the Engineering Plan. It is hoped that this building will be completed in two years to accomodate, as seniors, the present excessively large freshman class.

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Assuming that an off-campus research station is provided, the west campus building for engineering should be Building No. 5 of the Engineering Plan. This building will occupy the space north of the Mechanics Building and should be constructed in two steps. Because the Engineering Design Building cannot be razed until other space is provided, the first step should be to build in the presently unoccupied area. The second step is to move from Engineering Design, raze it, and complete Building No. 5.

Two more concrete slabs should be constructed in the area between Hesse Hall and the Engineering Building and a high fence should be built on the west side of the existing slab to join Hesse Hall and the Engineering Building. This area is needed for storage of heavy equipment and for instructional equipment such as the wind tunnel.

We have already submitted recommendations for the improvement and enlargement of the existing Engineering Buildings.

Instruction

The engineering curricula have been reviewed and revised during the past two years. We are now revising our courses to meet the requirements of the revised curricula.

Industrial Engineering will be recognized as a separate curriculum. It is proposed that instruction be inaugurated in ceramics and that our work in petroleum refining be strengthened. Aeronautical engineering, as such, should be emphasized at Los Angeles. No decision has been reached regarding petroleum engineering. In general, undergraduate instruction will continue in its present pattern with particular attention to the improvement of courses and curricula rather than to innovations.

Particular attention will be given to the development of graduate study. Plans for the encouragement of sponsored research will provide a powerful stimulant to graduate work both because more equipment and assistance will become available for graduate research and because this work will provide part-time employment for graduate students.

Major items in the plans developed by the divisions of the Department of Engineering follow.

Civil Engineering.

Develop a construction option within the Civil Engineering curriculum.

Develop graduate work in Sanitary Engineering in collaboration with the School of Public Health.

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Strengthen transportation engineering generally and emphasize highway and airport design. (See my letter of May 27, 1946.)

Initiate research on earthquake resistant structures.

Improve facilities for instruction and research in soil mechanics and in engineering materials, particularly highway materials.

Electrical Engineering

Develop courses and facilities for industrial electronics and electrochemistry.

Expand graduate study as laboratory facilities permit.

Plan and move into the new building.

(The Antenna Laboratory and related activities provide a sufficient plan for this division and the principal objective is to carry out successfully plans on which work has already started.)

Irrigation

Both Professor Etcheverry and Professor Harding are primarily professional engineers. Their reputations have attracted graduate students from many parts of the world, and they have been successful in the institution of graduate work. They are both approaching the age of retirement and their replacement will present a problem, because it is their professional ability and reputation, and not their research activities, which have made them successful as teachers.

Mechanical Engineering

Maintain and develop instruction and research in Marine Engineering and Naval Architecture.

Increase the enrollment in Agricultural Engineering and develop a similar type of cooperative arrangement regarding the engineering aspects of Forestry.

Continue the program of purchasing drafting machines.

Solve the problem of staff in lower division subjects such as drafting, by 1) eliminating the lower division, 2) by appointing nonacademic lecturers, or 3) by establishing appropriate criteria for promotion.

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Modernize the laboratories and particularly the Production Engineering Laboratory.

Provide space for graduate research.

Continue the development of the Time and Motion Study Laboratory.

Strengthen the staff in the field of thermodynamics.

Provide study space for graduate students.

Develop facilities for high-vacuum research.

Improve facilities for instruction and research in heat transfer, fluid mechanics, dynamics, elasticity, internal combustion engines, and industrial processes.

Increase the shop assistance available for undergraduate laboratory experiments, graduate research and lecture demonstration equipment.

Appoint non-academic managers of laboratories.

Mining and Metallurgy

Change the name of this division to the Division of Mineral Industries.

Decide on the location of instruction and research related to petroleum development and production. If this field is to remain the subject of instruction at Berkeley, the staff and laboratory facilities should be expanded.

Continue the development of facilities for physical metallurgy, particularly that branch which is of interest to Professors Dorn and Parker.

Anticipate needs for staff in Mining.

Develop ceramics as recommended in a joint letter of Deans Boelter and O'Brien.

Develop instruction related to a mineral exploration.

There is a considerable volume of reports and minutes of meetings behind the recommendations made here. A compact summary has been presented for your convenience. Additional detail can be supplied if you so desire.

MPO'B:ls

M. V. [illegible]

DEPARTMENT OF ENGINEERING

CONFIDENTIAL

November 5, 1952
938 - 52

President Robert G. Sproul
250 Administration Building
Campus

Re: "The University of California and Its Relation to Engineering Education in California", by A. A. Potter, Visiting Professor of Engineering 1952.

Dear President Sproul:

Dean Potter's reports of late last spring were distributed to our faculty for study and we now have the benefit of their comments in formulating the recommendations herein.

Dean Potter prepared two reports, one dealing with "The University of California and its Relation to Engineering Education in California" and the other with "Internal Operation of the Colleges of Engineering, University of California." In this letter, we deal solely with the former report, a copy of which is appended.

We are in general agreement with the facts and the conclusions of Dean Potter's report and believe that the State of California should adequately meet the need for public instruction in engineering but at the same time should do so at minimum cost.

We recommend that the University seek approval of a plan of publicly-supported technical education embracing the following components:

1. The Junior Colleges (two-year programs) should continue their lower-division engineering programs and should further develop technological programs of study (Technical-institute type) to meet local needs for engineering aides, and for other semi- or sub-professional positions in the general field of technology. These technological programs should be separated in facilities, teaching staff, and administration from trade school programs intended to develop the manipulative skills. The programs here suggested are definitely college-level in general scope, and will be unsuccessful unless they are treated accordingly.

2. Some State Colleges (four-year programs) should develop one of two feasible types of technical curricula described briefly as follows:

   a. Practical curricula of the technical-terminal type such as those planned and partially in effect at the California State Polytechnic College at San Luis Obispo. This category is to be accepted as the "practical" curriculum recommended by the Strayer Committee for offering in the State Colleges.

   b. Curricula in applied science, combining basic scientific courses of the engineering curricula with courses in general education.

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The University of California should adjust its curricula so that qualified students completing either of these programs may enter Engineering in the University and obtain a second bachelor's degree or a master's without undue duplication or loss of time.

The above programs would not only meet the established local industrial needs for personnel trained in applied science and technology but would also provide adequate training for students preparing to teach science and technology in the high schools and junior colleges.

3. The University of California should continue to be the sole State-supported educational institution authorized to offer undergraduate or graduate degrees in Engineering.

This statewide plan conforms to the policy recommended by the Strayer Committee. It recognizes the desirability of supporting regional colleges to meet local needs but also aims at achieving economy through concentrating the relatively costly phases of professional education in a few schools, thus avoiding the wasteful duplication of specialized faculties and extensive laboratory facilities. This plan should be subject to continuous scrutiny to assure the people of the State that engineering education needs are being met but at minimum cost.

Sincerely yours,

L. M. K. Boelter, Dean
College of Engineering, Los Angeles

M. P. O'Brien, Dean
College of Engineering, Berkeley

LMKB:MO'B/mjs:w
Encl.

CC: Chancellor Kerr
Dean Dodd

mailed to faculty 11-10-52
dm

Engineering Commencement
June 19, 1976, Hearst Greek Theatre
Address by Morrough P. O'Brien

Provost Maslach, Dean Kuh, Members of the Faculty, Graduates of the Class of 1976, and Friends of the University.

Looking over the audience at this Engineering Commencement in the Greek Theatre, my thoughts go back to the year 1928 when I joined this faculty. We were then a very small group. Electrical and Mechanical Engineering were combined in a single department, so small that a field trip to engineering works in the Sierras in the fall of that year--including nearly all of the seniors and the faculty--could be transported in a cavalcade of six or seven cars. Little did we anticipate growth to the size and stature indicated by this fine event.

Some years ago I addressed the Commencement exercises of the College of Engineering at Davis. On such occasions, people generally make polite remarks to the speaker, but one man approached me and said "Good talk but ten years out of date." On thinking over what I might say on this occasion, I decided to air this talk again--with some revision--and possibly find out whether I had been ten years ahead of--or behind--the times

You made a major decision regarding your lifetime career when you chose to study engineering, and you have now completed successfully an essential part of the preparation for professional practice in this field. However, you are not yet professional engineers--and you face some crucial decisions in the next few years which will determine the course of your careers. My brief remarks today may be of some help in making these decisions.

You may not remember the days following World War II - and the introduction of nuclear weapons when engineers and scientists were criticized for their participation in the weapons development programs - almost, it seemed, to the point of blaming ~~them~~ engineers for war itself. Then there was the introduction of ~~automation~~ automation and the scare that the engineers would take away the means of earning a living and ~~thus~~ cause widespread unemployment. Recently, engineers are receiving a major part of the blame for the state of our environment. Many of you who have prepared yourselves to enter this profession may be disturbed by this criticism and may wonder what you as ~~individuals~~ citizens, as well as engineers, should do about it.

An answer I like to give to the critics of engineering - but one perhaps too blunt - is that, if the clergymen and statesmen, the sociologists, philosophers, economists, political scientists and others, had done their job of education and

of practice as well as the scientists and engineers have done theirs - the world would be much better. This counter-attack is also unfair but no more so than the criticisms — and it is a good introduction to a lively conversation.

More seriously, these criticisms of the engineering profession imply that the engineer has decision-making authority over developments which have a major effect on the public welfare, and this implication is erroneous. The engineering profession has done - and done exceedingly well - what society asked of it. Broadly speaking, the charge was, in the early days of the profession, to replace manual labor by machines; more recently, it was to improve the productivity of labor and capital and thus to permit the application of an increased ~~share~~ portion of our resources to the enrichment of our lives. But society must decide what it wants - and must decide through the ~~proper~~ appropriate channels what use it will make of available resources.

When public policy required that the impact on the environment must be considered in the development of engineering projects, the profession responded; today, a large engineering force is engaged in environmental ~~studies~~ works.

Before pursuing this question further, it is appropriate to review with you some aspects of the nature and the practice of engineering as a profession.

The activity characteristic of engineering is design - not drafting or routine calculation - but design in the broad sense of (1) isolating a worthwhile technological objective, (2) conceiving a means of achieving this objective, (3) analyzing this concept for physical and economic feasibility, and (4) realizing the design in a physical system and proving its safety and effectiveness. Relatively few individuals have the combination of knowledge, experience, and creativeness for the first two steps in this process; a much larger number, who do most of the engineering, measured quantitatively, are qualified for analysis, test, and construction or production.

Frank Whittle - von Ohain - jet engine / One not better - Flash of genius - rare / different

Not much over a half-century ago, engineering was primarily an art in which judgment based on experience was the principal component. Science played almost no part. The absence of a scientific base was not entirely the fault of the engineers - for the science of that day had produced a few results which could be used to solve engineering problems; the discrepancies between theory and practice were glaring in some instances, as in the case of the theory of hydrodynamics and the practice of hydraulics. As science advanced and as some scientists became interested in engineering problems, the practical value of science and of mathematical analysis became increasingly clear, and the engineering schools have in recent years gone far towards providing ~~a~~ an adequate scientific basis for engineering practice.

Another major change which has come about in the last half-century in the practice of engineering is the replacement of the individual practitioner by the large engineering team. It was characteristic of engineering fifty years ago that an experienced engineer with a few junior engineers and subprofessional assistants ~~would~~ could design and execute the major works of that day. It was accurate and appropriate then to associate the name of a single engineer with an outstanding engineering development because one individual did formulate the problem, conceive a solution, carry out the design, and realize it in ~~stone and steel~~ production or construction. Today, teams comprised of hundreds, even thousands of engineers are engaged on major projects. The concept on which the design is based comes from an individual or a small group, but its execution requires many engineers, each skilled in a specialty and concentrating on a portion of the whole plan. This evolution of an engineering team to replace the individual practitioner has accompanied the growth of large organizations generally in industry and government. It has been made necessary by the number and magnitude of the engineering tasks to be accomplished.

Engineers who are effective in managing large high-technology projects are scarce.

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Another aspect of engineering activity as a whole is that some branches of the profession have reached a state of maturity in which the methods of design are almost standardized and the opportunity for innovation considerably limited. These activities are necessary in modern society, but they are ~~essentially~~ admittedly routine and can be carried on successfully by men of limited ~~ability and~~ technical education. It should be remarked ~~here~~ that many personal qualities are important in engineering practice which are neither enhanced nor tested in the educational process. Sound judgment about physical problems and about money relationships, personal integrity, the ability to explain and to persuade, and those intangible qualities which make for leadership all contribute in a major way to success in engineering, and men with a limited kit of scientific and technical tools ~~can~~ may practice successfully in these mature areas of technology.

In large engineering organizations, young engineers fresh from college are assigned subordinate positions in which they serve essentially as apprentices. As they gain experience and judgment, they are assigned tasks of increasing responsibility and ultimately some assume responsibility for the direction of other engineers and technicians. Increasing maturity and experience bring larger responsibilities and larger groups to direct, until the engineer becomes more a manager than a professional engineer. Budgets, promotions of subordinates, building plans, public relations and a host of other managerial types of activity generate "paper" which must be reviewed and acted upon, and the technical content of the engineer-manager's average day grows less and less.

Within these Large engineering organizations there are also some engineers who by choice or by chance circumstance become specialists in one field or another. They are for the most part, outside the direct chain of command in the sense that they give advice

on technical problems but they do not make the basic decisions - ~~But they are~~ However their work essential to the success of the organization.

In small organizations, or within small units in a large organization, the function of the manager of engineering activity and the engineering specialist ~~can~~ may be combined, but as engineering organizations grow the situation described generally prevails.

At this time of commencement, you should be proud of reaching the goal which you set for yourself when you chose engineering. Now you face another decision regarding the use you will make of the knowledge and the skills which you have acquired. Graduates with an engineering education have open to them a tremendous range of opportunities - and each one of you should appraise his own interests, his capabilities, his criteria of success and match these personal specifications against the opportunities offered. The lucky people in this world are those who find out early what they like to do, what they can do well and what they will be paid for doing. When these factors coincide, both a successful career and a happy life are almost assured. The unlucky ones are those who ~~get~~ become locked-in on a life work which they do not enjoy - for they will not only be unhappy but almost certainly unsuccessful.

Those of you receiving their bachelor's degree today face a decision as to whether or not to go on for a year or more of graduate study. Your final decision can be deferred; a year or so of engineering work often enhances the value of graduate study, but it is also true that returning to college becomes increasingly difficult with the years. Engineers in practice must continue to study to keep pace with advances in technology and the question before you is whether an organized program of study now will serve your personal goals better than the same years spent in practice - and you must decide this question on the basis of a realistic appraisal of yourself.

Judgment based on experience, ~~creativeness~~ creativity, integrity, energy and other personal qualities play so large a role in engineering practice that I do not regard graduate study as <u>essential</u> to success as a professional engineer, but on the other hand the body of knowledge basic to ~~any~~ most fields of engineering practice has become so extensive that for the majority of graduates four years of study is inadequate preparation for a career in development and design - that is, in professional engineering. My advice is to base this decision on what you like to do and what your studies to date indicate that you can do effectively. Don't let immediate money considerations dominate this decision.

Those of you who are <u>not</u> attracted to development and design as a life work, will find many other opportunities open to you. I anticipate that there will be a return to the pre-World War II pattern of employment opportunities for engineering graduates - when less than fifty percent of the BS graduates entered upon truly professional engineering careers.

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Previous to the expansion of the aerospace and other similar technical industries, which have in recent years employed a large share of the engineering graduates for research, development, and design, many graduates entered peripheral areas of technical work such as manufacturing, marketing of technical products, operation of plants and systems, patent law and others. An engineering education is good preparation for this type of work, and the rewards are comparable with those of strictly professional engineering. However, many companies stopped ~~trying~~ their efforts to recruit engineers for these functions because the glamour of the newer technologies attracted almost all of the graduates. These newer high-technology industries are now almost fully staffed, and this situation should permit industry and government again to employ engineering graduates for many semi-technical positions which have been filled in recent years by graduates ~~of~~ in other fields.

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Returning to the general question of the responsibilities of the engineering profession, the first step in any project is to choose and define the objectives of the work - and at this stage, the engineer does not act alone. In all major works, other specialists participate to define the requirements to be met, to estimate the benefits and the costs, and to assess the physical, economic or social effects which may follow. The decision to proceed or not, is made by those who control the funds - public or private. The engineer should feel responsible for fully informing these decision-makers about all aspects of the proposed program - and in these times, the effect on environment is a major consideration - but the decision is usually not his - acting as an engineer.

Much more might be said about the relationship of ~~the~~ engineer to society, to his employer and to the profession. I have perhaps over-emphasized the constraints and limitations under which the majority of engineers must work. Success as a professional engineer requires knowledge, good judgment based on broad experience, effective cooperation, and the ability to concentrate unwaveringly on the ultimate technical objectives. The rewards of success are substantial in tangible terms and in personal satisfaction. ~~However, not all graduates in engineering are well-suited to design and development - the central core of engineering practice - and for such individuals, a wide range of opportunities are open.~~ In the years immediately ahead you should appraise your aptitudes, your interests and your goals in life and seek work opportunities accordingly. You may not find exactly what you want - and you may find that what you wanted originally is not to your liking for the long haul. Above all, don't be discouraged by a mismatch on the first try - and ~~but~~ don't let up in the search because the work you do in the next few years will ~~probably~~ determine the course of your career over your ~~for your~~ lifetime.

In closing, I'd like to quote a statement about engineering by an engineer who occupied many high positions but who always regarded himself as primarily an engineer. Speaking to a student group, the late Herbert Hoover said: Quote

"As a profession, engineering has both joys and sorrows.

"The engineer has the fascination of watching a figment of his imagination emerge with the aid of science to a plan on paper. Then it moves to realization in cement, metal or energy. Then it brings new jobs and homes to men. Then it adds to the security and comfort of these homes. That is the engineer's high privilege among professions.

"The profession, however, does have woes. His work is out in the open where all men can see it. If he makes a mistake, he cannot, like the doctor, bury it in a grave. He cannot, like the lawyer~~s~~, blame it on the judge or jury. He cannot, like the politician, claim his constituents demanded it. Nor can he, like the public official, change the name of it and hope the voters will forget.

"Worse, still, if his works do not work, he is damned. ~~That is the phantasmagoria which haunts his nights and dogs his days~~. He goes to bed wondering where the bugs are which will i~~nevitab~~ly appear to jolt its performance. He awakens at night in a cold sweat and puts something on paper that looks silly in the morning.

"And the world mostly forgets the name of the engineer who did it. The credit goes to some fellow who used other people's money to pay for it. But the engineer, himself, looks back at the unending stream of goodness that flows from his successes with the satisfaction that few other professions can know." End of quotation

Today you leave a programmed and guided environment for a world of much turbulence and considerable uncertainty. Your talents and skill are needed. I wish you the best of luck in finding a life work of happy and constructive service in your chosen profession.

M.P. O'Brien.

12-12-78

/Prepared by M.P.O'Brien for Prof. Robert L. Wiegel and College of Engineering/

Dear Bob,

Until you asked for information, I had almost never taken time to reminisce. Since you asked I have spent some time in airports, on planes and in bed, thinking over the years since I came to Berkeley in 1928--and especially about the many individuals who had collaborated. The results probably are not relevant to your purpose because, for the most part, my contribution has been to get programs started and bow out--with the result that few resulting publications bear my name.

My educational background was entirely inappropriate and inadequate for an academic career--especially for a career in the fields in which I have worked. In the first place, I never took college seriously. After two years of a liberal arts program emphasizing Latin, Greek, English and scholastic philosophy--with chemistry, physics and chemistry as elective--I entered MIT (CE-hydroelectric option) with enough advanced credit and preparation to make the first years a breeze and I devoted only the day-time hours to classes and study--and the evenings to roystering with friends at Harvard with whom I lived to the end of my junior year--in Harvard dormitories. (I had a room near the Harvard Yard for mail and money.) I say this because the fact that I became a professor is still a source of wonderment; I had to learn while I earned after I came to Berkeley.

My first shock after graduation came when I read a paper on side channel spillways by Julian Hinds. I was working for the Hudson River Regulating District which had designed such a spillway for the Conklingville Dam. I was in the field in charge of a surveying party and not involved in design but the Design Engineer knew that I had specialized in hydraulics and he sent me the design and a copy of Hinds' paper and asked me to check his calculations. I realized then that I didn't ~~even~~ know much about hydraulics.

My two years at Purdue in Civil Engineering and the Experiment Station and a year in Europe as a Freeman Scholar were devoted primarily to open channel flow -- with a slight effort on hydraulic turbines. So I came to Berkeley with only the background of civil engineering--but as an Assistant Professor of Mechanical Engineering. What a shock that was! I reported one month late--Berkeley then started in August--and I immediately took over a section of ME 103 which Joe LeConte taught until I arrived. Furthermore, I was to take Course 127, Hydraulic Machinery the next year. My close associates--Boelter, Howe, Vogt, et al.--were not only mechanical engineers but with Boelter in the lead were in process of developing a new approach to the ME courses--fluid mechanics, thermodynamics, heat and mass transfer, dynamics, vibrations, etc.--and our seminar dealt with advanced treatments of these subjects. As to most civil engineers, dynamics was a mystery to me and thermodynamics just so much hocus-pocus (entropy was invented to terrify CE's) I have never worked so hard as I did in my first five years at Berkeley--trying to master what I should have known to get the job.

My preparation in math went only as far as ordinary differential equations and even that much had not been extensively exercised by the CE curriculum so I soon realized that drastic action was in order. I took (and worked the problems) in Tom Bucks Partial Differential Equations, Williams Complex Variable and Statistics and Probability;--also Lengens Graduate Course in

Physics, and miscellaneous courses and seminars around the campus in physical chemistry and thermodynamics, and geology. All this plus trying to catch-up on the graduate program in ME, which was not extensive but a whole new wonderland for me. Boelter was a tremendous stimulus and guide.

I still have many of the texts I pored over in those days -

Lamb, Barrett - Hydrodynamics
Jeans - Theory of Gases
Rayleigh - Sound
Page - Introduction to Theoretical Physics
Lewis and Randall - Thermodynamics
[illegible] - Thermodynamics
Glayert - Airfoil and Airscrew Theory
McAdmas - Heat Transmission
Walker Lewis and McAdams - Principles of Chemical Engineering
Stodola - Steam and Gas Turbines
Prandtl-Tietjens - Aero-hydro Mechanics
Prandtl - Physics of Solids and Fluids

These names and titles make me homesick for the good old days before I was dragooned into being an administrator. However, nostalgia is not the only reason for mentioning this background; intensive exposure to these new areas of applied science, under pressure to understand them for application in instruction and research, accounts for much that followed in the way of programs and policies.

At first I was in charge only of the Hydraulic Laboratory but ME was moving to integrate all laboratories and around 1933 I was assigned the Senior Lecture and Laboratory Course, ME 124-131. There were no suitable texts for either lab or lecture (ME was becoming more Chemical Engineering than traditional ME) and I had to spend much time revising as interesting lectures and lab notes on a wide range of subjects--heat transfer by conduction, convection, and radiation; aerodynamics; acoustics; compressible viscous flow; thermodynamics; etc. This was great fun while it lasted--but it came to an end when, without consulting me in advance, President Sproul appointed me chairman of ME--and I began work ~~on~~ in the "paper mill."

Prior to World War II, research and graduate study were very limited in Engineering at Berkeley. Raymond ~~Vais~~ Davis, a consultant on Hoover Dam, conducted a ~~source~~ sound and comprehensive program on the properties of concrete, Walter Weeks was working on the ventilation and cooling of mines, Boelter had some students working on heat transfer and the applied sciences related to ME, and I was engaged in work on coastal engineering and hydraulic machinery. Our graduate students were TA's and Corps of Engineers officers.

Although the graduate program in ME was small in numbers, I believe that the sound basis for the post-War expansion in Engineering generally was then developed, strongly influenced by Boelter and Dean Lipman of the Graduate Division. Emphasis on science and applied science, thorough search of the literature for the state of technology, elimination of empirical courses like power plant design, etc., were well underway when the "defense program" hit. A policy which began to be applied then--and which had profound effects on post-War research--was that the faculty were expected to do research, that approximately one-fourth time should be free to that end, and that full salaries were to be carried in the budget. The fact that our faculty were never on "soft money" was merely the continuation of a principle long held as essential to faculty freedom to choose research areas. Some programs that I remember with satisfaction:

- Electronic Research Laboratory. As a consultant in the Bureau of Ships, I had some contacts with the Electronics Branch. Towards the end of the War, two captains in this Branch, who had done graduate work at Berkeley, talked to me about the need for, and the opportunity for, a very large program of electronic research and graduate study at Berkeley - as compared with zero pre-War. I thought it a great idea but asked what would I do for money. They told me that if Berkeley put in a proposal for $500,000/year, they thought they could get it through the Bureau. I don't recall how we managed to prepare an acceptable proposal; I think that the two captains probably did most of the work. As part of the deal, the Radar Lab at Pt. Loma was moved to Berkeley - and with it the obligation of making studies of naval ship radar cross-sections. Engineers, technicians and some facilities and equipment were moved to Berkeley, and we were abruptly established in electronics; with that start, we could begin recruiting Whinnery, Silver - - - .

- Prosthetic Research. Towards the end of the War, we became acquainted with a Navy medical captain from Mare Island who was worried about the number of returning amputees and the sad status of prosthetic devices. Howard Eberhart had lost a leg and Gene Murphy was partially paralyzed from polio and I thought them to be qualified to sponsor a program of basic studies. Saunders, Dean of the Medical School, ~~was~~ and Prof Vern Inman were interested. Navy, and later Veterans Bureau and NRC support, was obtained and an outstanding program developed. When the basic work on human locomotion and the design of devices had progressed, [illegible] the work became largely clinical application, and I concluded that it was no longer suitable for sponsorship by Engineering, and I insisted that it be moved to the Medical Center in San Francisco.

- Metallurgy and Ceramics. Not long after I became chairman of ME, I became convinced that a major gap in the education of ME's was their rudimentary knowledge of the engineering properties of materials - particulary metals. When I asked for a position in the ME budget for a metallurgist, I was blocked by the College of Mining which claimed to teach metallurgy. It took me a year or so to learn that mining taught process metallurgy - a branch of chemistry - whereas ME's needed physical metallurgy - largely a branch of applied mechanics. I finally got a position and began the most thorough search for a man that I ever made - ending in the appointment of John Dorn, a Ph.D. in Physical Chemistry from University of Minnesota and a post-doctoral program in Physical Metallurgy at Battelle. He was appointed Assistant Professor of ME by an ex Civil Engineer - serving as chairman of ME. After John was aboard, progress was slow but in the right direction; when Mining was combined with Engineering, he moved to the Mineral Technology group.

Ceramics was established as the result of a contact I made during the War. The Ceramic Experiment Station at North Carolina State was credited with building up from zero a very substantial industry in North Carolina through work on local clays and through graduating ceramicists for the industry. The Director was on leave to the War Production Board, and I had many conversations with him about his work. War over, I borrowed a senior ceramicist from TVA to survey the industry in California and recommend a program. His report stressed the fact that the ceramic industry in California needed research support, that one job in the industry required only a tenth or less of the investment for a job in other California industries, and that ceramic programs at Alfred, Ohio State, North Carolina and a few others were primarily educational without much research. I got the space approved, induced Pask to join us and persuaded him to concentrate first on research. Pask and Searcy merged ceramics with metallurgy--and our outstanding program in materials developed, with Parker leading the way.

- Ship Welding Research. When the tanker Manhattan split open in a cold spell at Portland, the shipbuilding industry was in a panic. It had been fabricated by the Unionmelt welding process invented by Harry Kennedy--one of our graduates and a local man. Shortly after this failure, a representative of the National Research Council corralled me at the Cosmos Club and proposed that Berkeley undertake a program of research on the Unionmelt process as applied to ships. Under the circumstances--the War was on and hundreds of ships were being built by Unionmelt--I agreed to undertake the program without consulting anyone. When I returned to Berkeley, I found de Garmo ready and willing to guide the work. On that project we brought Earl Parker to Berkeley ~~and~~ as a full-time metallurgist--later on faculty.

- Low Pressure Project. As part of my effort to understand the applied science related to ME, I had read much about the kinetic theory of gases--and had acquired an interest for some reason in the molecular beam. Caltech, Cornell, MYU and others had wind tunnels--and post-War there was the Air Force program of more tunnels. Although we had sent many graduates into the aeronautical industry, I had always regarded it as an ephemeral specialty and we had never offered such a special curriculum--or engaged in research in the field. I was looking for something different and remembered the molecular beam, thinking of it as offering the opportunity to explore a new regime of flow. I did not foresee its importance to ballistic missiles and space flight. Folsom got interested in it. We consulted Leonard Loeb in Physics, somehow found funds to bring an expert (one of few; I think from Cornell) to help us prepare a proposal. Folsom got support, brought in Maslach and Schaaf and the project made rapid progress (Does it still exist?).

- Naval Architecture. Shortly after I came to Berkeley, I was asked to help in the design of a towing tank for ship models. The Bay Area shipbuilding industry was thriving and the SF Engineering Societies supported the idea of supporting the industry with model testing. I had visited the Versuchsanstalt fur Schiffbau at Hamburg and that made me as much of an expert as available in SF. In the course of this study, I learned a lot about ships and ship models--and also learned that my associate--the Associate Professor of Naval Architecture didn't know much about his field. The great depression killed this project--and the Associate Professor soon went into private practice. Naval Architecture languished in ME--with courses listed but no students. However, I had become convinced that there was justification for graduate work in Naval Architecture in a few schools--and after WWII I went to work on it--the first problem being to preserve a position in the faculty--and the second being to establish it as a separate discipline, independent of ME. Carl Vogt had worked with

Commodore Schade during the War. His recognized expertise and his Navy connections made him our prime candidate--so I went to work on persuading him to retire and join us. Wehausen and Paulling followed. Judging from the number of senior shipbuilding designers and officials who have spent a term or a year at Berkeley, I believe it became probably the most distinguished group in the world in its field.

- Operations Research in Industrial Engineering. During the War I had come to know a number of specialists in Operations Research and concluded that this technique would be appropriate for Industrial Engineering--if mixed with traditional time and motion study, plant layout and with Mfg. Technology (Erick Thomsen). Persuaded Ron Shepherd to leave Sandia Corp. for Berkeley to get this work started. The mixture I visualized didn't mix. Operations Research came to dominate Industrial Engineering, and I'm not entirely proud of this venture.

- Construction Engineering as the Industrial Engineering branch of Civil Engineering. The position Ben Gerwick fills went into the plans for Engineering soon after I became Dean--but a series of failures and half-successes occurred before Ben joined us. The concept I had was that the Construction Industry should be represented in Civil Engineering education in a manner similar to the relationship of Industrial Engineering to ME practice--planning of construction systems, design of false-work, economic considerations, etc. I don't know details of the present offering, but I sense that we still have some way to go.

Some additional notes on research projects -

- Submarine propeller noise. Mentioned in previous notes, but I may have failed to mention the essential result. Propeller cavitations had been studied for damaging effects on the propeller blades--and it was inferred that cavitation noise was only from this source. What we learned was that cavitation occurred first in the core ~~case~~ of the tip vortex and not on the blades and that this cavitation caused a large increase in the noise level.

- Compressible Flow in Porous Media. Jack Putnam and I became interested in flow in porous media (accounting for his paper on damping of waves by percolation) and went on from liquids to gases. About this time, the oil field at Kettleman Hills was coming into production and the several owners, led by SOCAL, had formed the Kettleman North Dome Association to develop the field as a unit. A major problem was to determine the optimum location of wells and the rate of production. A second problem was to allocate output among the many owners. "Reservoir dynamics" was a new field requiring field data, laboratory tests and much analysis. After some discussion, the Association decided to set up its own laboratory and analytical group with Jack as Director, on leave from the University.

This venture was one of a number of own small ~~mold~~ projects related to petroleum production in these years--flow of mud fluids for drilling, gas lift, porosity of petroleum formations, etc.--brought about in large measure by the fact that Jim Gosline, a Ph.D. graduate student, had grown up in the industry. (Jim became President of SOCAL)

- Suspension of Sediment in Steady Flow. We were following the early work on turbulence by von Karman and Prandtl. I happened to find in the library a paper by Wilhelm Schmidt "Die Massenaustrauch ~~un~~ den frien Luft und ...," which dealt with the transportation of spores, seeds, and dust by turbulence in the atmosphere. His formulation was complex; I summarized it in the Rivers and Harbors course, and after much discussion evolved the equation, $cm + \mathcal{E}\frac{dm}{dz} = o$ as the distribution of sediment. Christiansen, Ned Taylor and some other graduate students worked on applications. von Karman suggested to Art Ippen, then at Caltech, that he explore certain consequences of this equation; ~ forty years later I heard Ippen lecture at Gainesville on the results.

- Hydraulic Machinery. ME 127, Pumping Machinery, had been taught by Blake Van Leer as an empirical treatment of centrifugal pumps, drawdown around wells, etc. When I was told that I was expected to take it over, I had no background whatever in the subject--had never read a single paper on pumps. I stalled for a year, boned up on the subject, and developed notes on the theory of several pumping types. This was the start of a research program on the water jet pump, hydraulic ram, propeller pumps and fans, air lift and gas lift, centrifugal pumps, transportation of sand in pipelines--with Gosline, Folsom, and others.

Folsom and I had a contract to publish our notes on hydraulic machinery (ME 127) when the War diverted us to other work.

Association with GE. Because I have written few reports and published nothing about my work for GE, I only gave you the names of the principal people with whom I have worked. However, there have been a few assignments which I remember with particular satisfaction:

- Test Facilities at Evendale. A large and expensive test facility for testing jet engine components and complete engines had been designed by a well-known Boston firm. By chance, I travelled to Boston with the GE engineer in charge of the project and he showed me the design. The compressors, combustors, heat-exhangers, valves and piping, the test cells and the labs and offices were included in one large building. By the time we reached Boston, I had sketched an alternative design with the compressors in one building; the combustors, and all related piping out-of-doors between the buildings; and the test cells in a separate block and the labs and offices in an adjacent building, isolated from the noise and vibration of the cells. This design not only reduced the first cost by many millions but it was also much more convenient and flexible in operation than the original design. My design was followed--and the incident established my position with GE.

- Variable Stator Jet Engines. As the compression ratio of jet engines was increased to improve performance, the problem of choking and stall at part speed was first solved by dual-rotor systems of concentric turbine-compressor units which could operate at different relative speeds as the engine was brought up to full speed (Rolls Royce). ~~Some~~ 2 GE engineers invented the variable-stator principle which reduced the volume flow by rotating the guide vanes in several stages of the compressor. There was a violent difference of opinion among the compressor experts and engine designers even as to whether this principle was physically possible. The division manager asked me to make a thorough review of the subject and to recommend which principle of design--dual-rotor or variable stator--should be followed in the multi-million dollar program of development of the J79 engine. I made the review and strongly recommended the variable-stator--which is now universally used in turbo jets and the done ~~care~~ of turbo-fan engines. I still have the voluminous notes ~~which Dave Cochran~~ [illegible] took during this extended series of discussion.

- <u>Aircraft Nuclear Propulsion Project</u>. When GE took over this project from Fairchild in 1951, I was asked to join a small team at Oak Ridge to hold as many of the good Fairchild people as possible and to develop the GE program. I served as acting manager during most of the first summer at Oak Ridge--and spent most of my GE consulting time on the project over the next four years. I was skeptical of the feasibility of this project, but the Air Force wanted it done and GE had the job so I did my best to steer it in the right direction. The project was cancelled and the effort wasted--but I learned a lot about nuclear technology and the management of technology programs.

- <u>GE 4 Engine for the Supersonic Transport</u>. The manager of this project asked me to prepare a summary of the entire proposed project to include in the proposal--which was being written by at least a dozen groups in parallel and ultimately ran to several thousand pages. Clearly, no individual could read the whole proposal. My problem was to gather all the essential design features, test programs, etc., in a small volume which managers in GE and government could read--and which was absolutely correct. We won the job--and the engine was a success (now in Smithsonian). My summary was given some credit in getting the job.

- <u>Light-Water Nuclear Power Plants</u>. GE asked me to make an appraisal of the current status of the design, construction, operation and maintenance of light-water nuclear power plants of all manufacturers. I spent most of 1977 on this review visiting plants of GE, Westinghouse, Combustion Eng., Babcock & Wilson, Hitachi, Toshiba, and Kraftwork Union in the U.S., Germany Switzerland, Italy, Sweden, Taiwan and Japan with another GE consultant named Ed Schmidt. We visited 63 reactors in all stages of construction or operation at 43 sites. The report was voluminous.

BERKELEY: CIVIL ENGINEERING
HYDRAULIC AND COASTAL ENGINEERING

/Written shortly before M.P.O'Brien's death, this correspondence was O'Brien's last participation in University affairs. It was successful./

July 22, 1988

Chancellor Ira M. Heyman
200 California Hall
Campus

Dear Chancellor Heyman:

There is a proposal that the Water Resources Center Archives be moved from its present location in O'Brien Hall to a distant place in order to provide space for a possible computer project. The existing relationship of the Archives to the faculty and students in water-related subjects is such as to make this proposal incredible. The question which should be now considered is whether the Water Resources Center Archives should be abandoned.

The background for the creation of the Archives is pertinent here. Prior to WW II, the Department of Irrigation, consisting of Professors Etcheverry and Harding, enjoyed a worldwide reputation for their work on irrigation, drainage and other water problems, and on related economic questions; students from many arid countries studied under them. Both professors were also practicing consulting engineers and were associated in some capacity with every major project in the State. In that era, there was also a small Berkeley faculty group interested in the same field who sponsored annual meetings of the Section of Hydrology of the American Geophysical Union. These were held in California. This was the only forum for discussion of water problems in the West and they were well attended. Engineering at Berkeley was then the center for such studies.

This exposure to problems of water resources served to emphasize two important facts, namely, that many years of data on natural phenomena were essential to the analysis of almost any problem, and that proposed projects generally involved structures and operating procedures which should be evaluated for physical feasibility and cost. A good illustration is the proposed Salt Water Barrier, in which a dam would be built across the Straits of Carquinez to stop the intrusion of salt water and to create a fresh-water lake upstream. Even a majority of the opponents accepted as obvious that a barrier would accomplish this result. The controversy over this proposal became so heated that President Hoover and Governor Young appointed a Joint Commission to study the problem. The Commission sought the advice of the Corps of Engineers who identified the crucial technical problem to be the effect of the locks required to pass deep-water shipping in and out through the barrier. The University of California Hydraulic Laboratory conducted laboratory and field experiments which showed that locks permitted movement of water with the ships and that salt water would move through the barrier. Governmental interest died at this point although die-hard advocates continued to talk about it for years.

Another important event in the relationship of Engineering at Berkeley to water resources was the study of Professor McGaughey on the costs and the benefits derived from water projects in California. He identified clearly the basic problem then, and one which has become increasingly important since then, as the allocation of available supply between agriculture, industrial and domestic uses. Apparently this report was the first quantitative study of this subject; nearly every member of the State legislature wrote for a copy.

It was considerations such as those mentioned that the Archives was proposed as a permanent center for the collection and storage of data and reports. The bulk of this material consists of one-of-a-kind items not to be found in any other library. The technical files of Etcheverry and Harding themselves are now there. It appears that there is no similar facility in the United States.

There is an opportunity for Engineering at Berkeley to serve the State and its people in a manner similar to the contribution made by the Institute of Traffic and Transportation Engineering in the days of the expansion of the highway system. If it does so, this Archives will be essential.

It is not clear as to the reason for moving the Archives instead of putting the computer project directly in the new location. Moving the Archives to the present location was expensive and the collection was out of use for a considerable period. The budget for the move was inadequate and interested faculty donated funds to finish the installation in the new location. The volume of material to be moved is much greater now. The fourth floor of O'Brien Hall does not seem like a convenient location for a computer project; at least not so convenient as to justify disrupting an on-going activity.

Sincerely yours,

Morrough P. O'Brien
Professor of Engineering, Emeritus
Dean of Engineering, Emeritus
University of California, Berkeley

MPO:smo

cc: Provost Joseph Cerny
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Dean Karl Pister
President Emeritus Kerr
Professor R. L. Wiegel
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INDEX

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