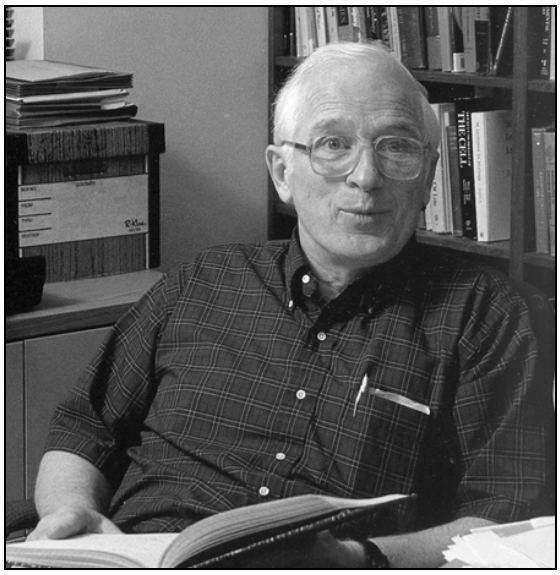

JAMES J. MORGAN
(1932–2020)

INTERVIEWED BY
SHIRLEY K. COHEN

May 25, June 1 and 16, 1999

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Engineering, environmental engineering

Abstract

Interview in 1999 with James J. Morgan, Marvin L. Goldberger Professor of Environmental Engineering Science, emeritus. Born in New York City to Irish immigrant parents, Morgan was raised in County Monaghan, Ireland, during the Depression. He studied civil engineering at Manhattan College, received a master's degree from the University of Michigan in environmental health engineering with C. J. Velz (1956), and after three years as an instructor at the University of Illinois took his PhD at Harvard in 1964 with the water chemist Werner Stumm. Morgan came to Caltech in 1965 to join the environmental engineering science program in the Division of Engineering and Applied Science, where he worked on manganese chemistry in water and the use of polyelectrolytes in water treatment. Recollections of colleagues Jack McKee, Sheldon Friedlander, Norman Brooks, and the early years of the environmental engineering science program. In 1966 he became first editor of the American Chemical Society's journal *Environmental Science and Technology*. Recalls stint on Caltech's Freshman Admissions Committee and as dean of students in the early 1970s. Coauthored *Aquatic Chemistry* with Werner Stumm. Comments on

his consulting for industry and government in the 1970s. Becomes vice president for student affairs (1980-1989). Recalls postdocs and students, including François Morel, James Pankow, Alan Stone, Howard Liljestrand, Yigal Erel, Windsor Sung. Awarded 1999 Stockholm Water Prize jointly with Werner Stumm (d. April 1999). In an epilogue to this interview, Morgan describes his trip to Stockholm to accept the award on behalf of Stumm and himself and his receipt that year of the Clarke Prize of the National Water Research Institute.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

Morgan, James J. Interview by Shirley K. Cohen. Pasadena, California, May 25, June 1 and June 16, 1999. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Morgan_J

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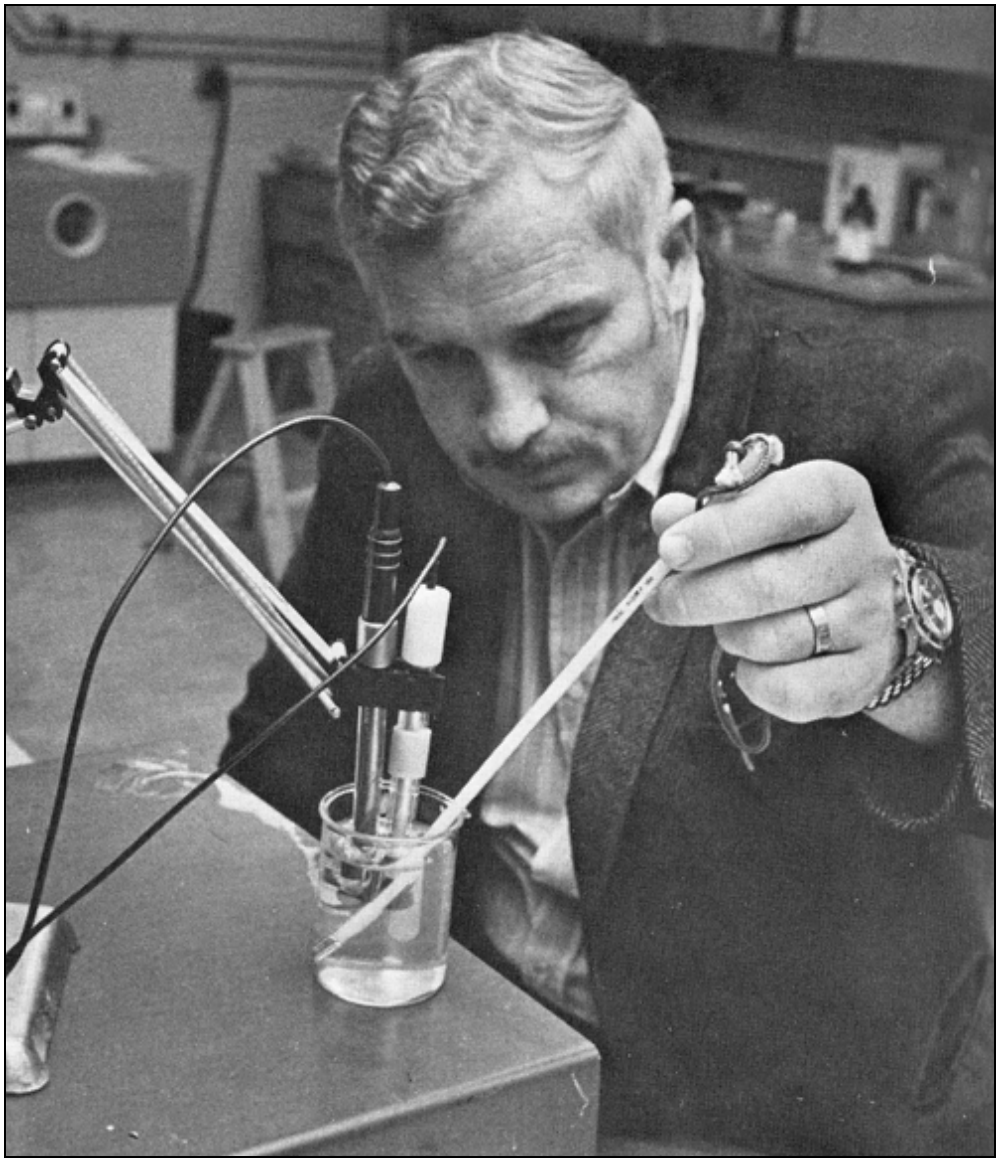

James Morgan tests the waters in 1970, the year Caltech started a degree program in environmental engineering science. His work on water quality and wastewater treatment would later win him the Stockholm Water Prize and the Clarke Prize (both 1999).

California Institute of Technology

Oral History Project

Interview with James J. Morgan

by Shirley K. Cohen

Pasadena, California

Caltech Archives, 2001

JAMES J. MORGAN
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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Interview with James J. Morgan By Shirley K. Cohen

Pasadena, California

Session 1:	May 25, 1999
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Cohen: Good morning, Dr. Morgan. Good of you to come and have this interview with us. Perhaps we could start with your telling us about your family background: your parents, what they did.

Morgan: I am the first child of Irish immigrant parents. My father and mother were married in 1927 and came to New York the next day. I was born in 1932 in New York. A year later my parents went back to Ireland, where they owned a farm, and I was raised there.

Cohen: Why did they do that?

Morgan: The Depression.

Cohen: Oh!

Morgan: I often say it was hard to figure out where it was easiest to starve most graciously. So they had the farm, and they went back to it and stayed there for about—oh, I guess it must have been four and a half years. And then we came back. I had the occasion to go back for the first time, just ten years ago, to see it, and it was a very poor-looking farm. I guess most of the farms

are, in that part of Ireland—which is the county of Monaghan, one of the border counties in the northern part of Ireland. It's very, very poor land, so it must have been a very scrappy existence. I only remember the pleasant part—being a kid on a farm and having a dog and things of that sort.

Cohen: So you had an extended family there? You had grandparents, aunts, and uncles?

Morgan: Grandparents, aunts, and uncles, that's right. And I still do have at least a dozen first cousins living in that small ten-square-mile area of Ireland. Anyway, the family came back about half a year before I started first grade. So I started first grade in a parochial Catholic school in the Bronx. And then after about three and a half years we moved to a different part of New York—Washington Heights, on the Upper West Side. In the fourth grade, I went to P. S. 189, whose song had the immortal lines "Enthroned between two rivers bright, our schoolhouse nobly lies."

Cohen: Oh, that's nice.

Morgan: Those turned out to be the Harlem River and the Hudson River. [Laughter] I finished public grammar school.

Cohen: What did your father do?

Morgan: My father had many different jobs over time. Actually, he came to the United States first in 1922, before he was married, and he was a motorman on the trolleys. And then when he came back, after he and my mother were married, I think he did that again for a while. Then when we all came back in 1937 or so, the Depression in fact was worse. I remember that one of his first jobs was as a construction worker on the new Independent subway—which I grew up calling the Sixth Avenue subway—and that was his job for quite some time. Then by the time I was in about the fourth grade and can remember such things, he divided his time between being a building superintendent and a plumber. And I remember still with great pleasure that when I

was about fourteen he actually taught me plumbing. I knew how to thread pipes and do elbows and joints and things of that sort.

Cohen: I see. So your interest in water started early. [Laughter]

Morgan: I didn't know it was an interest in water. And occasionally he would divert to being a high school custodian. I remember that he was custodian at Evander Childs High School, up in the Bronx, which I've never seen.

I have a younger sister who was born in Ireland. She's about four years my junior. She still lives in the New York area, out in Bay Shore. And as is often the case, we are now very, very close. When we were young, we couldn't stand the sight of one another. She's one of my best friends now. It's a very happy thing to say.

Cohen: Oh, that's nice!

Morgan: My mother passed away at the age of seventy-seven. My father passed away in Ireland. He went back to Ireland after he retired in New York. He passed away at the age of seventy-three.

Cohen: So you mean that after they were done working, they went back to Ireland?

Morgan: No. He went back to find them a place to live.

Cohen: Would you say that their attachment was never very strong to this country?

Morgan: Never, [except] in the following sense: I think their attachment was strong because they could see that it was where their children were going to have a better life. I often admire my father when I think about the perils [he faced]. He crossed the ocean five times in order to find a place to settle down and have a job and raise a family; my romanticization of that is that it

was a very pioneering sort of thing to do. So they were very attached to Ireland all the time that they lived in New York.

Cohen: Did they become citizens?

Morgan: My father did fairly early on. My mother never did. She didn't see the need for it as long as my father was a citizen. So when my father went back, in 1968, the idea was to find a place to live, because they had long since deeded over the farm to one of his relatives many, many years before. And while he was over there, he contracted bronchial pneumonia and passed away in a matter of just two and a half months. My mother was living with us in California temporarily at the time. In fact, we were in San Diego that summer—the summer of 1968.

So they had an attachment to Ireland which was very, very strong. Almost all the friends of the family in the West Side neighborhood in which I grew up were Irish people—not only Irish people but from the same part of Ireland back home.

Cohen: So it was like a little colony over here.

Morgan: But fortunately for me, I grew up as a typical New York kid—especially after I started to go to public school, which was just an accident. I would have gone to parochial school probably all the way through if it weren't for the fact that I had whooping cough when I was very young and lost a whole half year in parochial school. So when we moved to the West Side, Washington Heights, my mother tried to enroll me in the local parochial school, and they said, "Well, we'll have to leave him back six months, because we only work on a twelve-month calendar." So she refused and put me into the public school, which was the best thing that ever happened to me, personally, because now I met Armenians, Greeks, Jews, Italians, Germans, all sorts of different people. Personally, it changed my whole view of life. Although when I was through with public school I went to a Catholic high school—one of the large melting-pot Catholic high schools, which is still very, very successful at moving people up the social ladder. That's their mission. Cardinal Hayes High School, over on the Grand Concourse. I went there, and then I went to a Catholic college. But it had nothing to do with religion at that point; it had

to do with who would give me a scholarship. So I went to Manhattan College, which is a smallish school with an engineering program and a business program and a liberal arts and science program. They gave me a scholarship, and I had a New York State Regents scholarship. So I actually, believe it or not, made a little money going to college for four years.

And then, after that, I left New York, never really to return. I was not as attached to New York as—

Cohen: So you got a good education—between the high school and—

Morgan: I did. I was fortunate in that the high school gave me a very solid grounding in mathematics and science. One of life's little ironies, though, is that because of some sort of counseling mistake, even though I went to a Catholic high school—and everybody knows that if you go to a Catholic high school, you've studied Latin—I never studied Latin. I never studied French. I never studied German. And when I came to the guidance counselor to be certified for graduation, I remember him saying, "There's something terribly wrong here." [Laughter] And I said, "What's that?" He said, "You've never studied a language." And I could honestly say, "Nobody ever told me to." Now, there was nothing to be done about it, because I graduated in the top twenty of a class of 750.

Cohen: I was going to say that you probably were a good student.

Morgan: I was a successful student. My first love was history, actually.

Cohen: Is that right?

Morgan: And if I had won the scholarship that I was hoping to win to Fordham University—another Catholic university, as it turns out—I would have gone there, because they had a special program which would allow people to catch up if they had somehow not studied languages in high school. And that was my first love—American history. But no scholarship, no Fordham. So I went to Manhattan. I was also admitted to Columbia.

Cohen: But that would have been expensive?

Morgan: Well, it would have been free, if I had won the NROTC scholarship. I passed the academic part and then, ironically, failed the physical. All my life I've had what's called excessive curvature of the spine. I still have it. And it's the cause of pretty much lifelong back pain, which is a little bit hazardous for somebody who still plays basketball and tennis.

Anyway, that's the story of how I ended up at Manhattan, and that brings me to the end of my New York life.

Cohen: So then you finished college?

Morgan: I finished college and went to the University of Michigan.

Cohen: And what was your major [at Manhattan]?

Morgan: Civil engineering. In the fifties, when I entered college, it was the same year that the Korean War started. I received my induction notice in August of 1950, and I was advised by the college—since I was already admitted and would soon be enrolled—to simply tell my draft board that I was about to be enrolled in an engineering program. And everybody in that freshman class who was in engineering received an automatic deferment. And almost everybody that I can remember from my high school who was studying liberal arts was drafted. It gave me a very strange outlook on life, which had to do with probabilities and being lucky or being unlucky. And then two years later, many of those people who had been drafted came back and resumed. They were only in the service for two years. They came back in 1953 and picked up college where they had left off.

Cohen: And of course they had the GI Bill.

Morgan: That's right.

Cohen: Wasn't that extended to the Korean War veterans?

Morgan: Yes. So civil engineering was the thing that attracted me most, once I was enrolled in engineering. But the irony, again, has to do with that little mistake of not having studied language. Because after my first year in engineering, in which I did very well, I tried to transfer to the School of Arts and Sciences at Manhattan College.

Cohen: You still wanted to do history?

Morgan: And they wouldn't let me.

Cohen: Because of the language?

Morgan: Same deal. But by this time I was starting to find my feet in engineering. That is, I started to see things that were appealing. Civil engineering was appealing to me. And it was in my junior year that I really saw the social side of engineering. I became interested in water and water purification and some of those aspects.

Cohen: When did the word "environmental" come into being?

Morgan: That's interesting. If you trace the word "environmental" back to Caltech, which is a later part of the story, Jack McKee used it for the first time in 1960, when the faculty started a new degree program called environmental health engineering.

Cohen: Right. And we will talk about that.

Morgan: Early on, the part of civil engineering that I started to pursue was called, depending on the university you were in, either public-health engineering—which is what it was called at the University of Michigan, where I did my master's degree—or sanitary engineering.

Cohen: So it was environmentally focused.

Morgan: Oh, yes. But the problems were what I call the classical twentieth-century problems, which are the prevention of disease through proper water treatment and the prevention of water pollution through proper waste treatment. In other words, sanitation for human health protection and sanitation to protect natural waters. And that's what I saw as—

Cohen: It connected you to society in some way.

Morgan: Yes. Part of the disappointment for my father was that he could see that I was never going to be interested in making money. And, of course, one of the things that excited him about my going into engineering was that (a) it validated my education, and (b) he thought, "Well, this son of mine will make a good living." Well, in the long run I made a good living, but I never was attracted to what I would call the business side, or the professional engineering side. I took the first examination for my license and passed it, but I really got more and more attracted, as I went on into my twenties and early thirties, to the applied-science side of it.

Cohen: So you enjoyed your math and your science and your chemistry, and things like that.

Morgan: Well, there's a great irony about the chemistry, because chemistry was the course that I disliked most intensely in my freshman year at college.

Cohen: Is that right?

Morgan: Because it was taught in a way that I can only describe as militaristic. In fact, we used to joke—and it wasn't a very good joke—that the man who ran the chemistry laboratory for all freshman must have escaped from Nazi Germany, because he had such a militaristic style. So I left freshman chemistry thinking, "Whew! Boy, I'm glad I'll never have to do anything like that again!" I was wrong. [Laughter] But it took me several years to find out.

Anyway, the environment—other than in the sense of water supply and treatment—started to become a national issue in, I would say, the late fifties. And that's when chemical pollution started to be recognized. And it was in 1962 that Rachel Carson wrote her famous book, *Silent Spring*. Public consciousness of the environment in a broader sense, in my experience, dates to about 1957, 1958. And then Rachel Carson's book in 1962.

Cohen: That's quite interesting. Because I'm thinking, having just seen this Pauling exhibit and the to-do they made about the atomic bomb—that didn't enter into health consciousness?

Morgan: Oh, it did in the following way. In 1958, when I went to visit Harvard for the first time—because by that time I was starting to think of graduate school in a full-time way—one of the things I noticed was that there were these collectors scattered around the Harvard campus. So I asked one of the people [and got the response,] "Oh, those are the rainwater and fallout collectors of Professor [Harold A.] Thomas [Jr.]. He's studying the contamination of water from bomb fallout." So the AEC [Atomic Energy Commission], at a number of universities in the late fifties and well on into the sixties, but certainly for the first time in the fifties, was very, very interested in knowing where the bomb fallout went. And of course bomb testing was going on in the Pacific, as you remember. And the jet stream was carrying it across the Northern Hemisphere and dropping it across the United States, and then on over into Europe, and so forth. Apparently, one of the jokes at Harvard—Harvard students having some of the eccentricities that Caltech students are famous for—was that the only difficulty with these samplers was how to keep the Harvard students from urinating in them. They thought that would be a great thing to do. [Laughter]

Cohen: You know, the silence about this may have had something to do with national security. They didn't want any protests to stop them from testing.

Morgan: I guess so, yes. But that was an era in which, I think, Rachel Carson's book—and a few things maybe less dramatic than that—made the environment a much more general issue than it had been before. When I started to become aware of it, as an undergraduate, it was really

[only] a professional issue. You got a bachelor's degree, or maybe a master's degree, and you were qualified to work in a consulting engineering firm, a state health department, possibly with the United States Public Health Service, or possibly you went to other countries—before there was a Peace Corps—and did things like AID [Agency for International Development] work and so forth. And what happened with the advent of chemical pollution as a recognized fact is that more and more people became interested in it. People who didn't start out with any professional connection to the environment at all suddenly took a very strong interest in it and became very, very well recognized. One of the great public figures in the sixties was Barry Commoner.

Cohen: OK. I do remember his books.

Morgan: And one of the things that he did was to use student dental samples in order to trace the fallout and the accumulation of strontium in the human body [in] bones and teeth.

Cohen: Is that right? How interesting.

Morgan: And he did that in [Washington University in] Saint Louis. His original career was microbiology. Sheldon Friedlander, who was at Caltech for many years and was a good colleague and friend, used to joke that the modern sanitary engineers would be people like Barry Commoner. Partly Sheldon was joking because he didn't like that, and partly he was joking because he could see the truth in it—which was that science of a much wider scope was going to be important. It wasn't going to be a matter of a little, close community of people who did environmental work; it was going to become larger and larger. And Sheldon himself was a chemical engineer. He was one of the people who actually attracted me to Caltech.

Cohen: Is that right?

Morgan: Yes.

Cohen: OK. Well, let's do your graduate work first.

Morgan: Well, I finished my master's degree at the University of Michigan [1956], where I also met my wife. We both received our master's degrees there; then we were married in Ann Arbor and I moved to the University of Illinois.

Cohen: And what was your master's degree in?

Morgan: It was in environmental health engineering—what was then called public-health engineering.

Cohen: I see. OK. So already it was distinguishable as a field.

Morgan: Yes, that's right. There were two places you could study it at the University of Michigan, and I studied it in both—that is, I was a hybrid student. I worked in the School of Public Health as a research assistant, and I took courses in the medical school and also in civil engineering. Down on the main campus in civil engineering, it was called sanitary engineering; and up on the hill in Public Health it was called public-health engineering.

Interestingly enough, there's a long tradition in the United States of environmental protection going through schools of public health. Columbia was very, very famous in the twenties and thirties for its work on stream sanitation. And there was a great faculty member there named Earle B. Phelps who had a protégé named Clarence J. Velz, who was the man I eventually worked with for my master's degree at the University of Michigan. So they came to a concern about water from a public-health perspective. In other words, the survival of harmful organisms in the water was one of their interests. Disinfection was another one of their interests. So there were various threads that were separate through most of the twentieth century that now, in our time, are more or less all together.

Then I went to Illinois to teach. After having a master's degree, I liked to tell my students that I thought I knew everything there was to know.

Cohen: You still hadn't taken any languages?

Morgan: Actually, yes. I spent two years getting my master's degree at Michigan, because I worked half-time in the School of Public Health as a research assistant and the rest of the time I took courses at a half-time rate, so it took me two years to get a master's degree. And in that time I took my first language course. It was a German course, and it was called German Reading for Scientists and Engineers. That was my first formal contact with [a foreign] language, and I found out that I enjoyed it—although German was probably the most difficult thing for me to start with. I probably would have found it easier to start with French. But I was told by somebody that if you want to get a PhD, you're going to need to know how to read German and maybe another language. And the choices then tended to be usually French and German, then later Russian. So after these two years at Michigan, in which I studied a variety of mathematics and chemistry and microbiology, I took my first advanced mathematics courses. Because in civil engineering in the fifties, you essentially took two years of calculus, and that was it—unlike Caltech, which even in the forties and fifties was setting the standard for mathematics as a foundation for all of its scientists and engineers.

Cohen: But how about physics? Didn't you take physics?

Morgan: Oh, yes. I took two years of physics.

Cohen: The calculus was enough mathematics for the physics they did?

Morgan: Yes, it was, but it wouldn't have been enough for the graduate level. So when I went to the University of Michigan, one of the first courses I took was differential equations, which would have been the next [level of course work].

I went to Illinois because I saw a journal on the library shelf called the *Journal of Engineering Education,* put out by the American Society for Engineering Education. And I saw that it was published on the campus of the University of Illinois. So, completely on a lark, I wrote to the editor and said, "If your journal ever learns about teaching opportunities...." And I wasn't thinking of Illinois in particular, I was just thinking of engineering education

opportunities, because I had no real idea about how you did such things. So apparently this editor walked down the hall to the chairman of the civil engineering department and said, "Here's somebody who's interested in a teaching job." And sight unseen, they hired me. I never even went down for a visit. I took a job as an instructor; I was called an instructor of civil engineering, and one of the privileges of being an instructor in those days was that you could begin to work on your PhD part-time.

So I took the job. And then a year later, my wife and I were married and we then lived for three years in Champaign-Urbana. To make that story short, Champaign-Urbana was extremely valuable to me, because I learned how to do two things. I learned how to teach, because I had never taught before, and I taught for four years there. And I taught a variety of courses, not just in what we call sanitary or environmental [engineering]; I taught courses in surveying, in concrete, and things of that sort. And the second thing I learned was chemistry—for the first time. Because I then came upon the discovery—through some of the research that I had started to learn about at Michigan and through some of the research that I was asked to participate in in Illinois—that chemistry was much more important than I had ever realized before. So I started to specialize in my courses in two prongs: one was fluid mechanics and the other was all the chemistry I could take. You could take one free course at the University of Illinois every semester—that was the max. So I parlayed that into a collection of courses in fluid mechanics and chemistry. But then it became clear to me that there was no mentor for me at the University of Illinois. I had started to realize that getting a PhD requires that you really work with somebody who—

Cohen: Whose stuff you're interested in?

Morgan: Whose stuff you're interested in, and who can really be—the European expression is a "doctor father." The environmental faculty at Illinois was then in a state of intense transition, and there were really no senior leaders. The year I arrived, three people retired or resigned from the faculty. [Laughter] So after three years, my wife and I talked about it. We applied to various schools.

Cohen: Was your wife in public health also?

Morgan: No. She's an elementary schoolteacher.

Cohen: Oh, OK.

Morgan: So after a lot of thought, we applied to Johns Hopkins, Harvard, and Berkeley. We didn't apply to Caltech. Although my wife's a Michigan person, before I met her she had spent a year teaching in Long Beach, California, as an elementary schoolteacher. And she said, "We wouldn't want to live in California. It's so smoggy out there." Accurate in the 1950s.

Cohen: Yes.

Morgan: So we went to Harvard, to make a long story short. And that was a great success, because I knew who I wanted to work with. It was Professor Werner Stumm, who later invited me to coauthor *Aquatic Chemistry*. I only spent three years at Harvard, taking a few courses, doing my research, and then took a faculty position at the University of Florida.

Cohen: Now, just backing up a minute, I know from Shel Friedlander that his mentor at Harvard was actually Molly Scudder's father, the man who invented the iron lung.

Morgan: That's right. [Philip] Drinker.

Cohen: Did you have anything to do with that?

Morgan: No. Harvard had two completely different environmental worlds, geographically separated. The School of Public Health was in downtown Boston, and the program that I was in was in the Division of Engineering and Applied Physics, which is a very rough analog of our Division of Engineering and Applied Science here. So I took all my courses in Cambridge or—actually, I took one course down at Woods Hole, in oceanography. We went down once a month

and had a series of lectures and we did some field work. That was for my minor, which was marine biology.

So we were only three years at Harvard. I wrote a draft of my thesis. I took the teaching job at the University of Florida. I wrote a second draft after we had been in Florida for a few months. And then I came back and defended—I think it was April. So although I was in residence at Harvard only three years, it was one of the most intense learning experiences of my life. I developed a collegial and friendly relationship with Werner Stumm, so that when he passed away this past spring, I had known him for thirty-nine years.

Cohen: That's a long time.

Morgan: A teacher, colleague, and friend. And he was the person I'd been looking for—the person who would be my mentor. And I had the great good fortune to be his first PhD student.

Cohen: Now, he was Swiss?

Morgan: That's right, he was educated in Switzerland. He was, by early education, a PhD in inorganic chemistry. He worked with one of the very well-known Swiss inorganic chemists of the time, Gerold Schwarzenbach. And by sheer chance, he discovered the environment not as a graduate student but when he was looking for his first job after finishing his PhD. Jobs were tight at the time. Apparently what you did if you got a PhD in Switzerland was you went to work for the Swiss chemical industry. Werner had some sense that he didn't want to work in industry, but in any event the market wasn't all that great. There was a job at one of the federal laboratories, which happened to be located in Zurich no more than a block away from where he had gotten his PhD at the University of Zurich. It was part of the ETH [Eidgenössische Technische Hochschule], the Swiss federal technical school, and it was a job as a water chemist—that is, doing water analysis and designing measurement programs. He took it and he discovered water chemistry. He had never even thought about water chemistry.

Cohen: These things are often serendipitous.

Morgan: Yes. So after two years of doing that, he thought, Well, there must be more known about this than I can see. So he made some inquiries, he read the literature, and he found out that Harvard—this was in 1953—actually had an educational and research program in what was then called sanitary engineering and did a lot of work on water treatment, disinfection, and things of that sort. They had the leading program on the East Coast. And I would say that Berkeley, at that time, had the leading program on the West Coast. So he applied for a postdoctoral fellowship and so did his wife, who was a PhD geneticist. They each got $1,000 as a stipend—period. And they packed up and went to Harvard. Now, one of life's little ironies is that if I had gone to Harvard right out of Manhattan College, I would have been a first-year graduate student when Werner Stumm was a postdoc. But I went to Michigan, and then from Michigan to Illinois, and then I finally came to Harvard. And when I came to Harvard, he was just up for tenure that year, and he received it. And his philosophy had been "I take no graduate students until I have tenure," because Harvard was notorious, in his view, for very seldom granting tenure. He thought it was just too risky for a student, and for him. So I arrived at just the time that he was in the tenure process. The following spring, he received tenure, and he said, "Do you want to be my graduate student?"

Cohen: Ah, so you were first.

Morgan: I said, "And how!" [Laughter] So that's how that happened. He was a pure chemist by background. He discovered the environment through his first real full-time job. And what he learned was that many of the ways that things were being done were not all that imaginative and not all that chemically sound. So he, sort of on his own, discovered some things. Then he went to Harvard. And the man that he was going to work with, Edward Moore, without telling him, had already planned to be on sabbatical that year. So Werner Stumm went to Harvard and worked alone for twelve months. They gave him a little corner of the laboratory. He didn't speak very much English. He learned how to write on his own. And at the end of the year, he wrote a paper and showed it to Moore, who had come back from sabbatical at about that time. And Moore said, "This is tremendous. How did you do this?" It was on calcium carbonate

inhibition of corrosion of metals, which was water pipes. It was a chemical attack of a sort that never had been done before. And Moore said, "You should publish this. You should send this straight away to the leading journal in the field," which at that time was the *Journal of the American Waterworks Association*. And Werner, thinking it was the polite thing to do, offered to have Moore as a coauthor. And Moore honestly said, "I have nothing to do with this. This is your work. Submit it." It's one of the really classic papers.

Cohen: Is that right?

Morgan: And he did it all on his own. Anyway, Harvard had the good sense a year later to invite him back.

Cohen: To keep him there?

Morgan: No, he went back to Switzerland. But a year later they had a search for a young faculty member in what was then called sanitary chemistry, and they hired him in 1956. When I arrived at Harvard, four years later, he was just coming up for tenure, as I said—which was very early at Harvard, because Harvard usually takes about eight to ten years to make a tenure decision. So that means—now that I look back—that he must have been sensational.

Cohen: Well, he was a real pioneer, too.

Morgan: I just knew that he was a brilliant teacher. I took his course when I arrived, and he had a way of teaching that was entirely original, very imaginative; he had flair. I learned how he wrote. One day I came into his office when he was working on a paper. He had a desk near the window, a big desk. There were about a dozen books spread out, opened to different pages. They were all examples of good scientific writing. And he was—

Cohen: Plagiarizing? [Laughter]

Morgan: No, he wasn't plagiarizing; he was learning words. He didn't have a vocabulary. So he would have a word in German, and then he would try to find the corresponding word. So as a result, some of his early papers tend to have some unusual sort of self-invented clichés in them.

Cohen: [Laughter] What fun!

Morgan: Which were his adaptations of classical chemical textbooks and journal articles and so forth. I thought, "How would I do it, if I had to learn?"

Cohen: That's certainly a good way to do it.

Morgan: He just taught himself.

Cohen: It sounds like he had a lot of gray matter.

Morgan: Yes. Very original. Very original.

Cohen: So you were there with him for three years?

Morgan: Three years.

Cohen: And evidently you established a really fast, firm friendship, aside from everything else.

Morgan: That's right. And I took the job at Florida because (a) it was the only job that was available to do water chemistry, as I now saw myself—

Cohen: So you called yourself a water chemist.

Morgan: I now saw myself as a civil engineer turned water chemist. One of my close friends at the University of North Carolina, a man very senior to me, often refers to me as an engineer gone

wrong. When Werner and I received the Stockholm Water Prize announcement, Dan Okun sent me a very nice letter saying, "You haven't done so badly for an engineer gone wrong." [Laughter]

Cohen: Oh, how nice! So you lived where in Florida?

Morgan: Gainesville. The University of Florida at Gainesville. We were there for two years. It was pretty clear we didn't want to stay.

Cohen: Oh, it's very different from Cambridge.

Morgan: It was very different. It was a very backwater area, although the university was a good university. And I was in the chemistry department there. That is, my labs and my office were in the chemistry department. And I had a joint appointment between chemistry and environmental engineering. It was now called environmental engineering at the University of Florida already.

Cohen: Oh, OK. So we're in the early sixties?

Morgan: 1963.

Cohen: So the word "environmental" is creeping into the language.

Morgan: Yes. And Jack [McKee], as I said before, had already used it for the name of the new program here in 1960. Keck [W. M. Keck Engineering Laboratories] was to be part environmental health engineering, [part] water resources engineering, and [part] material science, which it still is to this day.

My wife and I had a sense that we didn't want to stay [in Florida] because she couldn't stand the bugs, I couldn't stand the humidity, and together we couldn't stand the culture of Gainesville. But I wasn't looking for a job. It's been my habit that good things happen to me—that if you do good work, your work gets heard about. So it was the following very, very

unusual set of circumstances: In 1965 I came to Caltech. And in the year leading up to that, Caltech had decided they wanted to hire a young water chemist. Apparently, recommendations about me came from three quite unusual sources, in the words of Jack McKee, because he couldn't imagine that these three different people could all agree on one person, or on anything! And those three people were Gordon M. Fair, who was the great man of sanitary environmental engineering at Harvard—

Cohen: Fair?

Morgan: South African, I think, originally. I think he was probably a student or a colleague of Drinker's, because he had an appointment both in engineering and in public health. And the second was Werner Stumm. Jack had already met Werner Stumm and knew that he was eccentric and, from Jack's point of view, probably slightly insane, being a Swiss. And then the third was Sheldon Friedlander. And how I came to meet Sheldon was that at the very tender age of thirty-two I got appointed to a National Institute of Environmental Health Sciences review panel for reviewing research grants. I still don't know to this day how that happened, but somebody must have put my name into the hopper. And they needed somebody who was water-quality oriented and so forth. That was a multidisciplinary panel; it had about a dozen people. It covered air pollution, biology, water pollution, and industrial health. And who else was on the panel but Sheldon Friedlander—he had just come to Caltech, in 1964.

Cohen: He was at Johns Hopkins before that?

Morgan: Yes, he had just come to Caltech. And he went back and told Jack McKee, "That's the guy you've got to hire." [Laughter] So that's why Jack told me later, with some amusement, that it was the strangest set of circumstances, because he couldn't imagine—

Cohen: Three people like that agreeing on anything.

Morgan: Right.

Cohen: Well, maybe if they'd known the others were of the same mind, they might not have.

Morgan: To make a long story short, they invited me out, I gave a seminar, they offered me a job, and then I almost blew it. I was so busy at Florida trying to help teach in the program, and I had already taken on helping finish a graduate student, and Fred Lindvall [professor of electrical and mechanical engineering], who was the chairman of the engineering division, wrote me a letter offering me the job—an untenured faculty position in the division at such-and-such a salary. And I said, "Gee, that's nice." And apparently, in my typical preoccupied [mode], I put it aside. And three weeks later, Jack McKee called and said, "Did you receive our letter?" And I said, "Yes, I did." And then he said, "And ?" And I said, not thinking, "Well, I'm thinking about it." And Jack said, "Thinking about it?" [Laughter] And then he said, "If you're thinking about it, I'm not so sure that you're our man." So I said—I didn't call him Jack McKee—I said, "Professor McKee, I accept. I'll send a letter tomorrow." And I did.

Cohen: And your wife didn't mind going back to California? Maybe after Florida, anything is all right.

Morgan: By this time, California looked good. In fact, Jean wouldn't think of leaving California. We've lived here for thirty-five years now.

Cohen: Yes.

Morgan: Furthermore, the air, for whatever reason, has gotten better.

Cohen: It has gotten better, yes.

Morgan: So that's how I arrived at Caltech, through that circuitous pathway.

Cohen: Well, you had visited, because you had given a seminar here.

Morgan: I gave a seminar in January of 1965.

Cohen: You were coming from Florida, so it didn't matter, but January is the month when you see the flowers blooming.

Morgan: It was entirely accidental, because I was going to one of these NIEH study-section meetings, which was held on the [University of California at] Riverside campus. So they said, "As long as you're in Riverside, why don't you come over?" So I came over to Caltech and gave a seminar. I remember that I gave the seminar in the same room we use to this day: 142 Keck. And I remember what I talked about. And for some reason the room was absolutely packed and people were literally sitting in the aisles on those little steps that they had in those days in 142 Keck. And all I remember is that I was on a fifty-minute high. I just talked and talked and talked. It turns out I was talking about new approaches to treating water using synthetic polymers, polyelectrolytes, which were becoming available at the time. It was the main research that I was doing with my doctoral student at Florida. And afterward I remember people coming up and asking me questions and saying, "Boy, this is really exciting work!" I was really on a high. And then I went back to Florida. [Laughter] That was January. And then the job offer came about six weeks later.

Cohen: And so you went the following fall? You moved to California?

Morgan: We arrived September 1st. I remember, because I drove out and took my graduate student with me, who was going to become my first postdoc at Caltech. And Jean and the children flew to Detroit to visit her parents, and then they flew from Detroit to Los Angeles.

Cohen: When you came then, you didn't have to worry about a grant? The money was here for you?

Morgan: No, I wrote grants.

Cohen: I see. You were doing that already. OK.

Morgan: I wrote grants. I had already had two grants at the University of Florida, and I transferred both of them, which you could do as long as the University of Florida agreed to let you do it. And some of the equipment actually came with me. And my dowry at Caltech was a $16,000 piece of equipment that Fred Lindvall agreed [on]. That was it. Start-up packages today for young faculty members tend to go, in some areas, up toward a million, depending on the field—a few hundred thousand anyway. He asked me if there was an important piece of equipment that I needed. Well, there *was* an important piece of equipment I needed, and it seemed to me that $16,000 was an awful lot of money, because in those days a grant itself would be something like $50,000 for two years. So they kindly bought a Coulter counter, which is a particle-size—

Cohen: Coulter?

Morgan: It was used for red-blood-cell analysis. It was for counting red-blood-cell particles and sizing them. And I was interested in using it for particles in water. So they said, "We'll buy it for you." When I arrived, they gave me the money and I bought it.

Cohen: And you were in Keck right from the beginning?

Morgan: I was in Keck right from the beginning. My office, at first, was in the basement. And my lab was pretty much right across the hall in the basement for many, many years. And the only reason I ever moved to the first floor was that Sheldon Friedlander left in 1978 to go to UCLA and I had always coveted an office on the first floor, because there was no view from the basement. You looked out at a wall. So I inherited his office.

Cohen: He took his rugs with him, though? [Laughter]

Morgan: He did. That's right. Sheldon had a lot of style. I remember one of the things about his office was that he moved wooden furniture into it. He didn't like the steel furniture. But he was a very, very good colleague, and he helped me learn the ropes. [Tape ends]

Begin Tape 1, Side 2

Morgan: I had been warned beforehand that a young professor might come too much under the influence of a senior professor and not be seen as his own person.

Cohen: That's true.

Morgan: And that danger, according to Werner Stumm, was none other than Jack McKee, because Jack was one of the most strong-willed and single-minded people that I had ever met. And he had to have those qualities, because he built the program at Caltech essentially from scratch. He came out in 1949 to become a professor. Prior to that, I think Franklin Thomas had taught a water course, but that was it. So over the next ten years, Jack was it. And he got the license to build a laboratory. He actually got money from the United States Public Health Service to build the Keck Laboratory, and the other half was raised by Caltech from the [W. M.] Keck [Foundation] and the Superior Oil Company. When the building came to be built, Jack learned—or Caltech learned—that you couldn't put the name of the Keck family on the building if half the money came from the United States government. So Keck said, "We'll pay for all of it." Jack said it was one of the hardest things he ever had to do—to send back $2.5 million to the federal government. [Laughter]

So I was very fond of Jack. I liked Jack. And I never had a bit of trouble with him. But Werner warned me, because he thought—just as Jack thought that Werner Stumm was very eccentric, you know, being a European—Werner thought that Jack McKee was very eccentric: both eccentric about science and eccentric in his strong-willed determination and so forth. So apparently, without telling me, Werner told Jack, "I want to give you a warning about Morgan." I learned this ten years later.

Cohen: Yes?

Morgan: He said, "Morgan gets very, very angry if you try to push him around."

Cohen: [Laughter] Did he just make that up?

Morgan: As far as I can tell, he made it up! Nobody ever tried to push me around at Harvard. I was always pretty strong-willed, but not to the point of [being a problem]. So I used to wonder, "Why is Jack treating me so gingerly?" He never even tried to tell me what courses to teach. Every year, he would say, "Now, you're teaching your course on water chemistry. And such-and-such a subject is important. I want to be sure that I'm not interfering with the way that you [function]." I said, "Don't worry, Jack. That's fine." So Sheldon Friedlander, I think, was much more a guide to me in those early days, because Jack was avoiding me and I was avoiding Jack. [Laughter]

Cohen: Oh, funny. OK.

Morgan: So I systematically never participated—

Cohen: I would guess he didn't push Shel Friedlander around.

Morgan: No! Jack hired three very, very single-minded people—and this is to his credit—in about a two-year period. He hired Wheeler North [professor of environmental science, emeritus], he hired Shel Friedlander, and he hired me. Prior to that, although I wasn't here, I think that his hires had not been successful. He hired in those same areas—chemistry, biology, air pollution—and he hired I think three people, whose names aren't even known to me, but they didn't make the Caltech standard and they didn't stay even long enough to find out that they didn't. They were here and gone. So I've often thought that Jack really must have been a superb student, because the next time around, as far as I'm concerned, he hired—

Cohen: Independent people.

Morgan: Independent people. And he let them be independent.

Cohen: Well, that's the Caltech style.

Morgan: That's right. I think he may have made a miscalculation in the first incarnation, but he didn't make any mistake the second time.

Cohen: So was this a novel kind of department?

Morgan: Yes, it was. It was a novel kind of department nationally. The closest similarity was actually the department I had earned my PhD in at Harvard. But what was really novel about this department that Jack constructed is that it had, in the same group of faculty, expertise in water and air and in chemistry and biology, and that was very unusual. At Harvard, if you wanted to do air work, you went over to Drinker's operation, which was then Les Silverman's operation over in Boston. If you wanted to do water work, you were in Pierce Hall, in Cambridge. And never those twain met. Here, in the same building, we had people doing.... Sheldon Friedlander was absolutely one of the world's best atmospheric scientists when I arrived, and he only got better. So that was very, very unusual.

Cohen: That's a strength of Caltech's, of course—putting people together closely.

Morgan: Of course. Yes. I'm aware that one of the models for that is the biology-chemistry synergy over the years at Caltech. And that was also a strength at Harvard, but on a more modest scale, in that it was pretty much all water-centered in Pierce Hall. They didn't extend it, and still don't, in a sense. At Harvard today, which now has one of the very best atmospheric chemistry programs, from a global perspective, in the United States—the air people and the water people are sort of still separate, although geographically much closer now. There's still a very, very strong interest in health over in the School of Public Health in Boston. And indeed,

my colleague Glen Cass [professor of environmental engineering and mechanical engineering] has a very active collaboration with faculty members in the air program at the School of Public Health in Boston.

Cohen: Of course, this place must have been different in the sense that there wasn't much [work in] public health—I mean, not being connected with medical schools or anything. So Caltech must have been different.

Morgan: Absolutely. There's always been a strangeness. And that's why I think the name "environmental health engineering" only survived five years after I came. Because in 1970 a new degree program was created by the agreement of McKee, [Norman] Brooks [Irvine Professor of Environmental and Civil Engineering, emeritus], who was a senior person and one of my closest colleagues—

Cohen: Now, was he part of this group when you came?

Morgan: When I came, the Keck building was divided north and south into two areas. At the northern end was hydraulics and water resources—[Vito] Vanoni, Brooks, [Fredric] Raichlen—and at the southern end, with my arrival, were McKee, Friedlander, North, and Morgan. So there were seven people, but they were in two parts. And I don't know how it originated, because I wasn't tenured at the time the decision was made. My tenure decision was made four years after I came, in 1969, but the wheels were already in motion to create a new degree program called environmental engineering science—to take the "health" part out of it, in recognition that it really wasn't that. Although what Sheldon Friedlander did was very much related to health, because his genius was finding out what the exposure of a human population would be to a pollutant, in his reconstructions of things like the famous London fog, and smog, and so forth. So exposure was what he was very, very good at modeling and predicting and analyzing. Though I think it was a coming together of Friedlander and Brooks. They agreed that it would be healthy for us to have a program with a different name, which would not be a subset of civil engineering. Both of the others were subsets of civil engineering. And the strategy was really

very, very simple. It was to attract students from a much wider range of undergraduate disciplines by calling the program environmental engineering science and by trying to attract scientists from chemistry, physics, biology, and mathematics and engineers from mechanical, chemical, and civil [engineering]. And it worked. The visibility of the program as an entity which was different from any other program in the United States was immediately established. And we started to attract, I think, some of the very best students in the United States over the next twenty-five years. We're approaching thirty years of existence as a degree program. As is very often the case in change, there was an unintended casualty. And I think the unintended casualty, from my point of view, may have been civil engineering, because I think a hole was left—

Cohen: When you left civil engineering.

Morgan: Yes. When all of us left civil engineering, in a sense. We all left civil engineering. And then civil engineering was still very strong, because it was earthquake engineering and geotechnical engineering. It used to be called soil mechanics—what Ron [Ronald F.] Scott [Hayman Professor of Engineering] did is now called geotechnical engineering, but it has to do with the soil as a structural-bearing entity. So you had this great civil engineering department still, with Scott and [George W.] Housner [Braun Professor of Engineering, emeritus] and [Donald E.] Hudson [professor of mechanical engineering and applied mechanics], and [Paul] Jennings [professor of civil engineering and applied mechanics]—the young Jennings—and [Wilfred] Iwan [professor of applied mechanics]. And some of those people are still here. But hydraulics and water resources, although the original intent was to have a dual existence—that is, the people at the northern end of the building would be members simultaneously of the civil engineering faculty and the environmental engineering science faculty—was partly successful and partly unsuccessful. In the first ten years, there was no problem with it. Here I'm only telling you what I believe personally, because I don't know that anybody else would agree with this. But it is one of the hardest things to keep doing the same thing over and over again successfully. There's a certain kind of genius needed within academic institutions to reinvent—

Cohen: To know when to change direction.

Morgan: To know when to change.

Cohen: How about the Seismo [Seismological Laboratory] people? Did they take away from these people also when that became very strong?

Morgan: No. That was always a very fruitful collaboration, and still is. Clarence Allen [professor of geology and geophysics] was a member of both the National Academy of Engineering and the National Academy of Sciences. [Thomas] Heaton [professor of engineering seismology] today is a member of the civil engineering faculty as well as the geo faculty. That—

Cohen: Didn't bother anything?

Morgan: I don't think so. So that brings you to 1965, when I started as a faculty member. And then I started to attract my own PhD students. And now, thirty-five years later, I'm ready to retire. In the intermediate time, I will have been the "doctor father" to some thirty PhD students at Caltech.

Cohen: Yes. Is this a good place to stop?

Morgan: Sure. [Tape is turned off]

JAMES J. MORGAN

Session 2

June 1, 1999

Cohen: It's good of you to be here again. Just going back a bit to our other interview, perhaps there's something you'd like to add before we proceed.

Morgan: Yes. I think I'd like to add a little bit about the college professor who really was my motivator to become interested in environmental matters. That's the late professor Donald O'Connor of Manhattan College, where I did my undergraduate [work]. I met him, I think, when I was a junior.

Cohen: Donald O'Connor?

Morgan: Donald J. O'Connor.

Cohen: That's the name of a—

Morgan: Famous dancer.

Cohen: Right.

Morgan: We used to kid him about that, but he—

Cohen: He couldn't dance? [Laughter]

Morgan: My guess is he could do anything he wanted to, including that. But he was one of the most enthusiastic and well-organized teachers that I had encountered at that time in my life, including high school and college. He had great zest for the water environment. At the same time that he was teaching at Manhattan College, he was also doing part-time work as a

consulting engineer, and he was also pursuing a PhD at New York University. This was a very, very energetic man. And he had a very great influence on me, although I don't think he realized it at the time. He reinforced my idea that I wanted to be a teacher, but now it was a teacher of something else—it was a teacher, in fact, of civil and environmental engineering. And he also simply made it clear to me that I had to go on for graduate studies, at least. And that's how I came to choose going to the University of Michigan—largely because it was a well-funded research assistantship. Ironically, more than ten years later, he told me that he was disappointed that I had chosen the University of Michigan and that if I had consulted with him at the time, he would have recommended that I go to Harvard straightaway, because he thought I had wasted some of my talent at the University of Michigan. That was complicated probably because of some personal resentment that he had toward the man I worked with at Michigan, but also, I think, because, as he later told me, "I underestimated your commitment to being a professional. I thought that you were perhaps too broad in your outlook." I had been student body president at the college and I had been editor of a student engineering journal and so forth. He said, "I saw in you the same qualities that I saw in myself ten years before, because I had been active in a similar way, and it took me several years to catch up. So I wasn't quite sure that you really wanted to be a successful engineer."

Cohen: What was it that made you go to Michigan, again? They gave you the best deal, as I recall.

Morgan: My role model had become O'Connor, and his specialty was the analysis of the response of rivers to the inputs of pollutants and the oxygen balance. The man from Michigan who had been the chairman of the department in Manhattan ten years before, and who had been a student of the great Earle B. Phelps of Columbia School of Public Health, came and interviewed students. And out of the people he interviewed, he picked me and offered me a graduate assistantship. Very late in the game from others: I received an offer of a GRA [graduate research assistantship] from MIT, which I knew nothing about at the time. The perspective at the time was that I was a very, very uninformed New York Irish kid who had a very limited perspective—whose perspective was just increasing progressively. So I never thought of

applying to Harvard. MIT encouraged me to apply to them, but it was getting to be about April or May and I had already made a commitment to Michigan. I also had an offer from the University of Connecticut.

But it worked out very well for me. Because as it worked out, six years later I ended up at Harvard, but for a purpose different from the one that Professor O'Connor thought might have been suitable for me.

Cohen: Maybe it was better.

Morgan: Yes.

Cohen: If you had gone straight to Harvard from Manhattan College, that may have been difficult.

Morgan: When I became vice president for student affairs here, I was interviewed for one of the staff-alumni newspapers—I've forgotten what it was called. There's a different format now. And in that interview, I mentioned anonymously the two great teachers who influenced me. For some reason, in the interview I didn't mention their names, and the interviewer didn't ask me for the names. One of them was Donald O'Connor and the other was Werner Stumm. At different stages in my life, those were the two key teachers. Now, the first man was a great teacher and a role model but not a mentor, because he didn't see my potential at the time. Stumm was my role model, my teacher, and my mentor—all of those, because six years later it was clear both to me and to others that I was very determined to be a success in the environmental field, to earn a PhD, et cetera.

So that's a little bit about Donald O'Connor. He died just two years ago, and then just this past spring, almost two years to the day later, Professor Stumm passed away. At the time of their deaths they were both about the same age, seventy-four or seventy-five.

Cohen: It must have felt like losing parents, in some sense.

Morgan: In the second case it did. In the first case, I had come to view Donald O'Connor more as an older brother. But in a sense they both were parents, intellectually speaking. They both had a very formative influence [on me]. One, I think, didn't realize it at the time—Professor O'Connor. And Werner Stumm certainly did realize it; I was his first PhD student, as you know.

Cohen: Yes. Well, very good. So then you came here. And it's very interesting—I'm looking at your vita here. You arrived in 1965 as an associate professor. Already in 1972 you were picking up administrative duties.

Morgan: Yes. There's a way that that happens at Caltech. And I think that the way it happened in my case was the committee system, the faculty committees that one either gets drafted for or volunteers for. I had always had an interest in student advising and in student welfare, to use a term that we don't use very much anymore. So I think in 1969 I volunteered for the Freshman Admissions Committee. And I came to serve four years on that committee. And I also—

Cohen: Were those the days when you traveled to interview students?

Morgan: Yes. For four years—'69, '70, '71, and '72—I traveled to the states of Washington and Oregon, which was the interview territory, the way things were divided up. And I did it during spring break, which in later years would have been a great hardship. But I think the fact of traveling during the spring break, for many faculty members, later on became much less attractive than it might have been. Probably it never was really attractive; it was a sacrifice, because the spring break is a breathing space when you catch up and get ready to teach in the third term. In those days I taught—like everybody else in engineering—all three terms, every year. So I think the reason I was asked to be the dean of students in 1972 is that I became known to other faculty members and to some of the people in the administration through my service on that committee and on the Student Housing committee. Usually the Student Housing Committee duties were less demanding, but it was another way that I simply got to be better known.

I hadn't served on any committees until 1969, when I became tenured. It's the advice that I have later given to junior colleagues, which is, "Committee service is a very important

thing, but my advice to you personally would be not to get very much involved in it until you've been established as a tenured faculty member." Some of my colleagues follow or followed [the advice] and some don't. It worked out well for me. So I was asked to become the dean of students by Harold Brown [Caltech president 1969-1977].

Cohen: How did that come about? I mean, did he come directly to you and say, "Would you be dean of students?" That doesn't sound like Harold Brown.

Morgan: No. There's a committee. I think I was unaware of the existence of the committee. But there must have been a faculty committee. And in those days it was not unusual for students also to be members of that faculty committee. It was usually an ad-hoc committee to make a recommendation to the administration. I had no idea about it in particular. So one day I got a telephone call to come over to Harold Brown's office, which happened to be in Throop. And he offered me the job. My guess is that it would have been—let me see. The earthquake was 19—

Cohen: '71, I think.

Morgan: '71. But they didn't take Throop down immediately. That's funny, because the timing doesn't sound right. So maybe by that time he was already in temporary quarters over in Millikan [Robert A. Millikan Memorial Library], which is where they moved to. In any event, I accepted. I didn't accept immediately; I thought about it, because I could see that it would be a lot of time. My predecessor, by the way, was [professor of history] Robert Huttenback. And when he left the job of dean of students, he became division chairman of Humanities and Social Sciences. He had been quite a figure as dean of students, as he had been even more of a figure, I think, before that, as master of student houses. [He had] a larger-than-life personality and so forth. My style as dean was much more low key. I spent about half of my time at it. I tended to be a dean of students in the morning and a professor in the afternoon.

Cohen: So you really had to protect yourself in that sense, if you were going to do anything.

Morgan: Yes. Because I didn't give up any of my other duties; I simply had to be sure that I did them in an organized way.

Cohen: So you continued to teach and—

Morgan: Yes. I continued to teach my course, which in those days was a very, very high-enrollment course, because the early seventies were also a time of environmental awakening, so to speak. For a course that before the 1970s usually had about eight students in it, I remember in 1971 that I had thirty-four students in my course on water chemistry. That was partly because of the environmental curiosity. So students from all sorts of different disciplines—chemistry, geology, chemical engineering, as well as civil and environmental engineering, a mixture of graduates and undergraduates—would come and take it. Anyway, I served for three years. And in the meantime, in 1974 I was also tapped to be the executive officer for environmental engineering science, which was the new program we had started in the early sixties.

Cohen: That had to do with the EQL [Environmental Quality Laboratory]?

Morgan: No. Interestingly enough, it's commonly thought that EQL and EES had administrative overlap. It's quite the contrary. The EES was the academic program for the master's and PhD [degrees] in environmental engineering science. And the executive officer reported to the chairman of the [Engineering and Applied Science] division, who at that particular time was Francis Clauser—no, it had just become Robert Cannon, I think.

What happened was the crunch. The crunch was this: In 1966 I became the first editor of the American Chemical Society journal, *Environmental Science and Technology,* which occupied an enormous amount of time and intellectual energy because it was a new journal in a new field. And my job as the editor was to establish and maintain intellectual standards. I may have mentioned to you last time that the director of publications for the ACS came out to the campus to visit me—to look me over, so to speak—and he offered me the job. And I remember walking to the Athenaeum to take him to lunch.

Cohen: What was his name? I don't know if you spoke of him.

Morgan: His name was Richard Kenyon. He was the director of all American Chemical Society publications. And I remember saying to him, "You know, I have a feeling that I'm really too young for this job," because in 1966 I was just thirty-four going on thirty-five. And he was a very friendly but very businesslike man. He said, "We've thought about it. We don't think you're too young. We hope you'll take the job." He reminds me a little bit of Harold Brown, actually. [Laughter] Because that was Harold's manner, too, not to invest more words—

Cohen: "Don't think for yourself; I'll tell you what to do"?

Morgan: Well, it was that, plus "We know what we're doing. Do you want the job?" That would be Harold Brown's [style].

Cohen: So let me get this straight. You had three big administrative jobs, then, at one time?

Morgan: Yes. So in 1975 I gave up [being] the dean of students.

Cohen: What sort of thing does the dean of students do? What did you have to do?

Morgan: The dean of students is an ex-officio member of the Undergraduate Academic Standards and Honors Committee, which has to do with maintaining academic standards and the readmission of students who have come into academic jeopardy. Also the person who decides on implementing, or usually implementing, recommendations concerning the undergraduate student honor system. A lot of what the dean of students did was meet with students to counsel them about academic trouble, or the happier side would be to counsel them about academic success. I used to see two ends of the distribution of the undergraduate student body. I would see one end of the distribution—students who were in academic jeopardy. And I would see the other end—people who were having great success and needed letters of recommendation from the dean to compete for a Marshall [scholarship] or a Rhodes [scholarship] or—

Cohen: Gee, that makes it hard on the ones in jeopardy, because you're not comparing them to the average [student].

Morgan: I actually put more energy into the ones in jeopardy.

Cohen: Of course.

Morgan: And then there was the great middle. And the great middle, unless you took extraordinary steps, you'd never meet. So my typical morning would be interviews with students who needed to see me. They would be scheduled through my secretary. The dean of students was essentially the spokesman for the administration with respect to undergraduate student life, along with the master of student houses.

Cohen: I see. So it was something you worked [on together].

Morgan: Yes. Probably one of the most important connections was the connection between my office and the office of the master of student houses, who was David R. Smith, of Humanities and Social Sciences.

Cohen: OK. And he was living over here at Steele House at that time?

Morgan: That's right. He and his wife were the first couple to occupy Steele House. So in 1974 I gave up the ACS journal, because I had wanted to serve just for a limited time. I served eight years as the editor of the journal. And in 1975 I relinquished the position of dean of students.

Cohen: You cleared the decks.

Morgan: And for the next five years, my life was rather simple. It was simply that I was the executive officer for environmental engineering science, which is essentially being the

spokesman for your colleagues within your division with respect to the division chairman, especially on matters of what I would call hard-money administration—that is, [issues regarding] secretarial staff, budget, who occupies what space, where you put the students, and so forth. And Caltech, as you very well know, doesn't have a very layered administration. So the faculty don't really expect the executive officer to speak for them on intellectual matters. The executive officer calls the faculty meetings, but when it comes to promotion, tenure, recruitment, and so forth, the division chair deals with a committee—

Cohen: Of the faculty?

Morgan: Of the faculty. So it was not a demanding job from the point of view of issues like academic promotions and tenure. It was simply like being the first among equals. At that time, whoever was the executive officer, your time had come to be the spokesman and the go-between for all the rest of your colleagues. And I liked that job, and I held it for six years.

Cohen: Do you think it's true anymore?

Morgan: I think that in our division—Engineering and Applied Science, which is a very, very complex division—there are eight, or possibly nine, executive officers. There were fewer at that time. They all report to the division chair. The division chair's style changes with division chairs. I think at the present time our division chair [John H. Seinfeld, Nohl Professor and professor of chemical engineering] likes to make much more use of the executive officers as a channel of communication than was the case in my time, twenty-five years ago. I think it's very different in different divisions. Engineering is a very large and polyglot division. We have seventy-some faculty members. There are at least eight, nine, or ten distinct areas that you would recognize as departments.

Cohen: So it's the biggest division.

Morgan: Yes, it is. And it's diverse. It's very diverse.

Cohen: Yes.

Morgan: I don't know how to compare it to others. I think the most important characteristic of our division over time is what the style of the division chair is with respect to communication—more meetings, fewer meetings. I remember back in the seventies I used to go to meetings with other executive officers. It seemed almost monthly, but in fact it was probably about every two or three months. That was quite a lot of meetings to go to. The division chair wanted all the executive officers to sit in the same room, and he wanted to discuss—

Cohen: Get things done.

Morgan: Yes.

Cohen: Did it make a difference when you actually had an engineer for a president?

Morgan: Well, I got along very well with all the presidents, including the most recent ones. And there was Lee DuBridge [Caltech president 1946-1968], with whom I had no administrative duty but with whom I had a lot of fun, speaking at meetings and so forth. I remember he introduced me at one alumni seminar day, or it might have been a meeting that we used to hold at Caltech in which high school students from all over Southern California came and spent a Saturday on the campus. I'm not sure. But I remember him introducing me somewhat provocatively by saying, "Professor Morgan is going to tell us about water. Now, I don't see what the problem is. I've made a rough calculation about how much water there is on the earth, but he'll probably tell us something interesting." [Laughter] I found him very friendly—usually very much short and to the point, but friendly. Harold Brown was a very different kind of president. He was much more businesslike. "Businesslike" isn't quite the right word, but he always seemed to know exactly what he wanted to talk to you about before you stepped into the office. In other words, I think he was a very, very thorough study of whatever the issue was.

The reason I didn't stay as dean, in part, is that in 1975 a select committee chaired by Professor [Fred C.] Anson [Gilloon Professor of Chemistry] had recommended that the job of dean of students be elevated to that of vice president, and that one person be both vice president for student affairs, which would be a newly created position that didn't exist before, and also dean of students. Now, I had been the dean of students for three years. I found it a lot of work. I found it very, very emotionally demanding, because I tended to internalize to some degree some of the more intense cases I dealt with. So I thought the recommendation was a strange recommendation. And I know the reason for it: I think it's the deep-seated suspicion of the Caltech faculty, by and large, toward freestanding administrators, at least at that time. So I think the image of that committee was, "You don't want somebody to be just a vice president, for heaven's sake. There's no need. We want somebody who's going to stay close to the students. And therefore, we think the two jobs should be one." I told President Brown that I didn't think that was a good idea. And to the degree that I was being considered, I told them that I wouldn't consider it.

Cohen: And it's been that way ever since, hasn't it?

Morgan: No. I'll bring you up-to-date on that. They changed it again, and five years later I put my money where my mouth was, so to speak.

Ray Owen [professor of biology, emeritus] became the dean of students and vice president for student affairs (1975-1980) and really loved doing the two jobs together, I think partly because Ray had a tremendous devotion to students—and still does—but also because it was a time in his career when he was beginning to close down his laboratories, so he had the time. Not full-time, but he had the time and energy to be both the vice president and the dean. I think the part he loved the most was being dean, because it brought him—as it had me—into day-to-day contact with a wide range of students.

Cohen: I think he'd been very involved with the decision to admit women to Caltech.

Morgan: Absolutely. That's right. Ray had chaired a number of key committees and was also very much involved with the decision-making process about undergraduate women. In the 1960s, Ray had been chairman of a committee on the reformulation of the curriculum for the freshman year. I remember that that committee was still working when I arrived, but I had no particular knowledge of it, because I think it worked for three years before it issued its report. And it was very consequential, I later learned, in changing the curriculum for freshmen. The key thing was to create more electives, to offer more choices, to make it possible for freshmen to sample different areas of the undergraduate curriculum so that they might more intelligently choose a major, or what we call an option.

But in 1980 I was asked once again, by a different president—Murph [Marvin L.] Goldberger [Caltech president 1978-1987]—[whether] I would become the vice president for student affairs. And I said, "No. I'm happy doing what I'm doing now, which is being a professor and being an executive officer looking after the welfare of my colleagues." Remember, at this time I had been at Caltech only fifteen years. And I had already, it turns out, done more administration than is typical for a Caltech professor—although neither in retrospect nor at the time did I feel that it was something to complain about. Some of my colleagues at other universities used to tell me, "Morgan, you do it because you like it. It's clear." Some people would wring their hands and say, "We wish that Jim weren't so much involved with that, because if he weren't involved with that, then he could spend more time on this." And "this" was usually chairing a committee in the American Chemical Society or being involved in some professional activity. But the people who knew me best—that is, on a personal level—I think recognized fairly early on that I was doing these jobs because I enjoyed doing them, not because I was carried away by some sort of exaggerated sense of duty. The key thing, as you've already remarked, was not to be doing them all at the same time. I got into that sort of without looking ahead, and I got out of that as quickly as I could.

So in 1980, Murph Goldberger approached me again. And for six months I couldn't make up my mind. I was disinclined to do it. And I finally accepted under the same condition that I had discussed with Harold five years earlier: I don't think that's one job; I think those are two different jobs for two different people. So I proposed to Murph that if he was serious, I would be the vice president for student affairs, but he would have to get someone else to be the

dean of students—and that I was willing to play as large a role as he wished in helping to bring that process about, to work with the search committee and so forth. And he did. And I think he made the right decision. Now, the faculty always look with suspicious eyes upon what they see—now that I have been for ten years simply another faculty member, I can understand the point of view. We view with suspicion an ever-expanding administration.

Cohen: [Laughter] Yes.

Morgan: Which in retrospect, in this area of student affairs, is much more appearance than reality. But in any event when I decided to become the vice president for student affairs, and David Wales [professor of mathematics] was recruited to become the dean of students, I thought that we had a good working relationship. David was devoted to caring for the intellectual and personal welfare of the undergraduates—very, very serious, but very, very good-natured.

Cohen: You did give up your other job, though?

Morgan: Then I said, "I'm not executive officer anymore." I had now gained enough presence of mind to learn to give up jobs as fast as I accepted jobs—furthermore, to accept no more jobs than were really necessary. So, yes, I was succeeded in 1980 by my colleague John List [professor of environmental engineering science, emeritus]. So I continued to teach, and I continued to have an active research program but at a somewhat reduced level with respect to the number of PhD students who were under my care. That was a necessary outcome.

Cohen: When you say "reduced," what did you reduce to?

Morgan: Oh, instead of having five or six students at a time, during the eighties I would typically be working with about three.

Cohen: That's still a lot.

Morgan: The style of research that I favor—and there's no particular reason for it other than my evolution—is each student working on a problem of his or her own choice for which I am the right advisor, rather than—

Cohen: Some problem that you want to solve?

Morgan: Well, rather than a programmatic schedule.

Cohen: Oh, OK.

Morgan: And there are good reasons for either one of those [styles]. In many parts of science and engineering, a long-term program of research to achieve certain goals is absolutely essential. My colleague Glen Cass is probably the closest example of that to me, because Glen has been working for twenty years on the visibility problem and making steady progress on it, and all of his students do work on some aspect of that. They may do other things as well, but they all make a contribution—and a different contribution—to understanding the causes of impaired visibility in the Los Angeles atmosphere. And now their geographical reach is wider; it extends over a larger area of the Southwest. But my style has always been to ask students who are interested in working with me what they want to work on and then to try to find if it's a good match, essentially. So it was easy in that sense, since I wasn't working through a ten-year program but was working on a variety of problems I thought were important and letting students either work with me or not, depending on their interest. During the seventies, my group of advisees working for their PhDs was usually around five, six, or seven. So I scaled back just by taking on fewer students.

Cohen: Did you have postdocs also, or was that not the style in that area?

Morgan: Sometimes I did and sometimes I didn't. When I first came to Caltech, I had three postdocs over a period of four years, on sort of a rotating schedule. I found that, for me, the only feasible way to get off to a good start in building a research program and developing a laboratory

was to start right out with a postdoc. And I think I may have told you last time that my first postdoc was a student that I had co-advised at the University of Florida. He came with me to Caltech as a postdoc and worked with me for two years. Frank Birkner was his name. He came with me in 1965, and he was a postdoc for two years. He was an outstanding man, who has had an academic career. He's been a professor at the University of Maryland ever since he left my laboratory.

Cohen: Now, you had to raise money for the postdocs?

Morgan: Oh, yes, the usual. I think I may have told you last time that in my brief time at Florida, 1963-1965—

Cohen: That's right. You said that you had gotten money and you brought it with you.

Morgan: I had already had two successful applications for research grants: one was on manganese chemistry in water; the other was on the use of polyelectrolytes for improved methods of water treatment. I succeeded in bringing both of those with me to Caltech. And those two grants provided support, starting out, for three postdocs between 1965 and 1970, and also provided the basis for support for my first two graduate students, who started in 1967 or 1968. In the eighties, while I was vice president, for whatever reason—it's no longer clear to me—I lost some of my interest in working with postdocs and pretty much gave myself over to working with graduate students, but not exclusively. From time to time I would have a postdoc come and spend a year or two with me.

Cohen: And they would have to come to you? You didn't go seeking anybody?

Morgan: I never advertised for postdocs. But that's just my own style. I think that postdocs are very important in science, but I think that historically they're viewed as much less essential in engineering. The typical profile of a PhD student in the parts of engineering that I'm most familiar with—civil, mechanical, aeronautic, and so forth; areas that in a sense are historically

the least molecular—seem to have the least need for postdoctoral experience. So all my early students, if they chose academic careers, went right into an assistant professor position.

Cohen: I see. Well, I know in biology you need much more time to develop intuitive knowledge.

Morgan: It's the same in chemistry. The same intellectual style marks chemistry, biology, and the parts of engineering and applied science that are now primarily molecular. I'd include applied physics and material science, certainly, as examples. In chemical engineering, which is in a different division, the postdoc route is now the normal route for a person finishing a PhD and going on toward an academic or an industrial career.

Cohen: Of course, that may have something to do with the job market also.

Morgan: Yes. Those are all mysterious to me. And I've decided not to invest a lot of energy in understanding it.

Cohen: [Laughter] OK.

Morgan: So anyway, that's a little bit of insight into how I became the administrator that I was. And then in 1989, after nine years as a vice president, I decided that I wanted to return to being a full-time professor. [Ed. note: In 1987, Morgan had become the Goldberger Professor of Environmental Engineering Science.]

Cohen: Let me ask you a little bit more about being a vice president, because this was giving you a window into a whole other world. Maybe you'd like to talk about that.

Morgan: What one of my colleagues called the world of levers of power. He said, "It's obvious that you're familiar with the levers of power." And I always thought that was very amusing, because the last thing I thought I was involved in when I was in administration as vice president

for student affairs was the levers of power. But I think what was most interesting to me about my nine-year stint under two different presidents—first Murph Goldberger through 1987, when he retired and moved to the Institute for Advanced Study to become the director, and then from 1987 to 1989 under Tom Everhart [Caltech president 1987-1997]—was how much administration I had to learn, that is, how much the job of vice president for student affairs had become progressively more administrative. And I used to remark to my colleagues back in the division, where I was always a presence and never really left, that one of the things that I noticed as vice president was that I hardly ever saw a student, except at the social events where you were always invited. And this was characteristic, I think, of the times we were in, not just at Caltech but everywhere. What was happening at Caltech in the eighties was that the jobs that previously could be fairly readily handled half-time by a professor, or by a former professor, say, who had retired or had decided to switch over to research and administration rather than teaching.... It used to be that the director of the career center—which we called the placement office—the registrar, the director of admissions, the assistant director of admissions, and several others I'm sure, were all either former or part-time faculty members. Caltech had a very small undergraduate enrollment. It still does, but it was even smaller then. When I came to Caltech, there were about 600 undergraduates. Now it's 850 or thereabouts. It was [during] my watch in the eighties when decisions had to be made about the need for increased professionalization of certain offices in the student services area. What's called student affairs could equally well be called student services, because it involves both academic affairs—for example, the dean is really looking after the academic standing of the students and is sort of the front representative person for the faculty. The old meaning of "dean", from the Latin, is the first among ten. I used to have some difficulty explaining to some of my colleagues, when they would say, "You didn't speak up for the students on that issue." I would say, "Let me explain to you. You misunderstand the position of dean, if you think that the dean is to speak up for the students. The dean is to make a judgment on behalf of the faculty regarding the students, just the way any other faculty member would." The idea that the students need an advocate and that the student advocate should be the dean of students from my point of view was absurd. And I, not being bashful, never hesitated to tell people that privately, if they asked me. In other words, in order to make proper judgments about the welfare of students, I think you have to think of yourself as a

faculty member representing other faculty members, with delegated powers from other faculty members. And that's the way I acted when I was dean of students for those three years. But now as vice president for student affairs I found that that was not the same kind of job. I wasn't a representative of the faculty, although the vice president for student affairs has always been a faculty member. And although the institute has weighed the alternatives from time to time—that is, recruiting a professionally-trained student affairs administrator to be vice president—they've never done it.

Cohen: They've stayed with the faculty.

Morgan: Yes.

Cohen: I know at a place like MIT it's a professional who comes in to do that.

Morgan: Yes. MIT was professionalized in all of these jobs well before we were. And I learned that because when I was early in the job and I would go to visit MIT, say, for environmental reasons, I would sometimes spend half a day, or a few hours at the most, just visiting my counterparts. And I was surprised to find that none of them were professors. They were all people who had worked their way up the career path in administration. The thing that I found was the hardest to learn was to be an administrator whose major job was administration, rather than to be an administrator whose avocation was administration.

Cohen: All this time, you were still teaching your course?

Morgan: Yes. And the reason for that is that I really like teaching. [Tape ends]

Begin Tape 2, Side 2

Morgan: The attraction to academic life, for me, was first to teach. And then after looking into it I found, as most people do, that you can't be a good teacher in a first-ranked university or

college without research experience and without ongoing research—in other words, freshly fueling the teaching enterprise. So I didn't give up teaching. Although in retrospect I might still be the vice president today—that is, I might have found it possible to continue to be the vice president—if I had given up teaching. But I didn't.

Cohen: You mean it was fast becoming a full-time job?

Morgan: Yes. But it's still technically not a full-time job. And the accommodation that subsequent administrations have made—intelligently, I think—is that the person who becomes a vice president is given some options, like during the time you are in the administration you will not have your teaching duties. If a Caltech professor in our generation has to decide to give up research in the prime of life in order to take on an administrative job, I think the answer would almost inevitably be no. But the option of taking a leave from teaching during the time that you're an administrator is much more understandable. And to me it's certainly understandable. I didn't [take a leave from teaching]. Probably I might have thought about it. I might have actually done it, four or five years into the job. But I thought I would be a vice president for five years, period.

Cohen: Things commonly have a five-year appointment around here.

Morgan: My suggestion that five years be the case was received with good humor but not with seriousness by the president, who said that he couldn't imagine that. "No, no," he said, "that's just out of the question. We're not even going to talk about that." And that was when I had the opportunity to step aside but didn't reflect upon it. And sometimes I wish I had said, "Well, we are going to talk about it, because five years is fine for me."

Cohen: It's enough. So now, of course, you were meeting trustees. Was that a new experience?

Morgan: No.

Cohen: Not really. But you were meeting with them on a regular basis?

Morgan: It was new experience only in part. And the reason that it was a new experience only in part was that from 1977 to 1979 I was chairman of the faculty.

Cohen: OK. We haven't talked about that.

Morgan: That's not an administrative job; it's a leadership position among the faculty, which objectively should be viewed as, "Well, it's so-and-so's turn now to be the chairman of the faculty, to preside at the meetings of the faculty, to preside at the meetings of the faculty board, to work on shaping the agenda." It's a very interesting job. How interesting it is depends upon whether you have really major crises or only minor crises when it's your turn to be [laughter]—

Cohen: On watch.

Morgan: So, I was faculty chair from 1977 to 1979. I succeeded Robbie [Rochus] Vogt [Avery Distinguished Service Professor and professor of physics] and I preceded David Goodstein [Gilloon Distinguished Teaching and Service Professor and professor of physics and applied physics].

Cohen: And did you have any crises on your watch?

Morgan: No. I had little difficulty on my watch. There was the usual run of things that required faculty discussion meetings. Basically, there are two different kinds of meetings that the faculty of the institute regularly have. There are the meetings that must be held once a term in order to conduct whatever faculty business is to be conducted, presided over by the chairman of the faculty. Usually it's an opportunity for the president and the provost to discuss what's on their mind. And it's an opportunity for committees that may want to meet to report to the highest levels, et cetera. But then there are also things called faculty discussion meetings, which are meant to air a subject, to get faculty views, to take no votes, et cetera. I think the issue that

required faculty discussion at that time was widespread faculty interest in some of the faculty benefits, like insurance and things of this sort, which were very dry subjects but nonetheless needed to be discussed. I think I had one or two of those. But the major thing that was happening during my time as chairman of the faculty, from 1977 to 1979, was that a new president was being searched for.

Cohen: This was the end of Harold Brown's tenure.

Morgan: Yes. Harold Brown left. The faculty chairman, in my experience, is usually consulted by the chairman of the board of trustees and, in different ways at different times, a faculty search advisory committee is constructed. Robbie Vogt constructed a search committee. And Stanton Avery was the chairman of the board of trustees. And I ended up on that search committee before I became chairman of the faculty. That is, it was constructed in January of 1977, when Harold Brown left to join the Carter administration.

Cohen: Right.

Morgan: [It was] something that the faculty wags say that anybody could have foreseen and why didn't we see it? But the reality is that you can't appoint a search committee until your president actually says, "I'm leaving." [Laughter]

Cohen: Right. Well, I remember that when he came he said, "This is a good job for seven years." And everybody was saying, "That's the Nixon administration." [Laughter]

Morgan: Well, yes. So Robbie constructed a search committee and I agreed to be on it. The idea was to have at least one representative from every division. And I ended up being the representative for the Engineering and Applied Science Division. But six months after the search started, I was elected chairman of the faculty.

Cohen: Was there any contradiction there? Or just the timing?

Morgan: I don't recall the details. It was just that I had obviously been a visible person: I had been dean of students; I was an executive officer. I had a certain amount of visibility. I was the sort of person who, from time to time, would actually—not to an extraordinary degree—make remarks in faculty meetings if I happened to be there. In any event, I was asked to do it, and I did. And it turned out to be awkward, because now I was the chairman of the faculty but I was also a member of the presidential search committee. Personally, I wish that set of circumstances had been anticipated better by me. If I had really thought about it and realized how much demand there would be on my personal time during that period, I probably would have declined to stand for election as chairman of the faculty, because it was another year, essentially, before Murph Goldberger accepted. And that was a very, very tough year, because search committees meet essentially all the time. So managing that while I was also managing the scheduling of the faculty meetings and presiding and so forth turned out to be very, very energy-demanding. It's one of those things that in retrospect I wish I had done differently, because there were many other people in my own division who could have been certainly as good, probably better, representatives of the engineering division. It was partly my inexperience. If I had been paying full attention, I wish that when I was approached to be a representative for the engineering division I had had the wit to say, "And who else from my division will be on this committee?" If I had been thinking and had a little bit more sense of the moment, I would have said, "No fewer than two representatives from our division. It's simply unacceptable. No fewer than two."

Cohen: Oh, because it's such a large division.

Morgan: Because it's a large division.

Cohen: But they didn't do that.

Morgan: No. I came under some criticism from some of my colleagues in the engineering division, not because it was said that I wasn't qualified to be a member of the search committee

but because I think they understood probably better than I had at the time that such a large division might seriously be asked, "Why not have another voice?"

Cohen: That seems right.

Morgan: Yes.

Cohen: Did you have any sense of the engineering division not getting its due when you were meeting with the rest of the search committee?

Morgan: No. I worked very, very hard—and I tried to make it very, very clear—to be sure that every viewpoint in engineering was represented, by meeting widely and frequently with members of the engineering division. But, as you can imagine, it was a very big chore, so that took quite a lot of energy. I didn't get the sense—although such matters are appropriately confidential at the time and in my own judgment need to remain confidential for a long time thereafter—that there was a lack of enthusiasm on the part of my colleagues on the search committee for bringing in prospective engineers and applied scientists. That was never an issue. I think the issue was simply to find a strong leader for Caltech as president. You probably know as well as I that there's some tendency toward choosing a different president every time you choose a president—I mean in style. So Harold Brown was a president with a certain style. And there was some felt need on the part of the community, expressed by some people, for a president with a different style. And then after Murph, it was probably "Well, now we need somebody with a different style." No one was saying anything about the intellectual qualifications. I think there is some sense that personal styles, in the way that the institute would be led and represented, tend to be on the minds not just of the search committee but of the people who are feeding ideas to the search committee. And one of the most important things the search committee does is listen to what people are telling them and follow the leads, usually funneling them all through the chairman so that you don't end up with a cacophony of communication. I think our committee did that well, and I think we maintained the confidentiality of the process very well.

Interestingly enough, many faculty members don't understand why a presidential search needs to be confidential. I probably didn't when I went on the search committee. I think what one sees at other universities very often, in retrospect, is that people who might be candidates get spooked. They don't want to be seen as the candidate who didn't make it. So one of the ways to make it easy for people to walk away is to not let it be known that they are candidates before they are candidates. There is a whole set of reasons like that. In the UC [University of California] system the process is much more public.

Cohen: It's a public institution.

Morgan: But it's not meant to be literally public. But it becomes almost unavoidably somewhat public, because you can almost always, it seems to me—I'm going from memory here—read short lists in the *LA Times*.

Cohen: Of course. Well, it's a public institution.

Morgan: Yes. We never wanted that to happen. And to my knowledge that didn't happen in any of the searches that I'm familiar with. If you're the representative from a division, or one of more than one representative from a division, when you meet with your colleagues in the division, both as a group and as individuals, what many of them want to know is, "Tell me who the committee is thinking about." And I say, "I'm sorry. I'm not free to talk about that. I'm here to learn who you think are the people we should be pursuing." And then the dialogue might go something like, "Well, I really can't give you suggestions unless I know the kind of people you're thinking about." And then I would simply say, "Well, that's not possible. So please give me any suggestions that you think are reasonable, and we will pursue them as carefully and as thoroughly as we can." And I think the committee did that. It's the sort of thing about which I once said, "I'm glad that you can only do something like that once in your life." I had never wanted to be involved in it. It was a very strenuous activity.

Cohen: Well, you had to be running around the country interviewing people all the time.

Morgan: We did that, yes. We did that.

Cohen: And I talked to Peter Goldreich [DuBridge Professor of Astrophysics and Planetary Physics]—he bought a suit, even. [Laughter]

Morgan: That's a very, very severe move for Peter Goldreich [laughter], who is one of the dearest people I know at Caltech, and I know him only through athletics. Because we were gym rats.

Cohen: Well, I remember the suit.

Morgan: I know the intellectual esteem in which he is held, and I know why, because you don't have to have a very long conversation with Peter to understand that. But we've always related at the level of guys who meet to play basketball.

Cohen: Well, that's probably what's more important to Peter anyway, so never mind. [Laughter]

Morgan: Yes. Anyway, that's about administration.

Cohen: OK. So you then gave up this job as vice president.

Morgan: 1989.

Cohen: I did ask you, and I'm not sure if you answered me, about your relationship with trustees.

Morgan: Oh, yes. You did ask, and I didn't answer. Personally, I found that to be one of the most satisfying aspects, first, of having been chairman of the faculty, because it was in that role

that I first met, I would say, any trustees. And then there was a hiatus of a year when I was no longer chairman of the faculty. And then, when I became vice president for student affairs, I met a much larger set of trustees, for a couple of reasons. But the most important reason is that the vice presidents, along with the division chairs, were always invited to the annual meeting of the board of trustees, which is a weekend meeting usually held out in Palm Springs but sometimes at other places. And in that regard I had the opportunity to meet trustees socially, but also about business. That is, occasionally I would be given the opportunity, as is everybody in the administration, to speak to the trustees, to give a presentation. Sometimes [it was] about my research, which at the time, in the early eighties, was the study of the chemistry of the precipitation from the atmosphere in California—acid rain, in other words. And on another occasion, I was asked just as vice president for student affairs to make a presentation about what I thought were some of the most important needs from my perspective as a vice president for student affairs. And that was a surprising request, because the trustee meeting started on Friday, and I received the request from the president's office on Tuesday. And I don't know the reason—it may have had to do with somebody else canceling, I don't know. But Friday is the purely administrative part of the trustee meeting. The enrichment talks—that's where a scientist comes in and talks about his or her science—are usually on Saturday morning. This was on a Friday afternoon, during the business part. So Murph's office simply asked me, "Is there something you'd like to talk about?" Well, I knew there probably must be something I wanted to talk about, even though I had only been vice president for one month at the time. [Laughter] So I gave it a lot of thought that night, and the next day I went and got the Caltech photographer and went and photographed different physical facilities that I thought were consequential for Caltech undergraduate and graduate life. Because by this time it was clear that the portfolio of the vice president for student affairs involved not simply undergraduates but increasingly graduate students, who were very, very outspoken about their welfare and the sense that they had been somewhat neglected. So I took the opportunity. And to make a long story short, I gave about a forty-minute presentation in which I used the photographs, which I had gotten quickly turned around—I got slides. And I showed the athletic facilities and the music facilities, such as they were at the time, in 1980. And I concocted a talk. This was entirely my own; I had no chance to consult with anybody. It was my own, out of whole cloth. I thought the two greatest needs for

physical facilities for undergraduates were for things related to music and performance, and improved facilities for athletics and physical education. And I still remember that as soon as I was finished, John Braun came up to me and said, "How fast could you have some preliminary drawings that I could look at?" And David Morrisroe [then Caltech's treasurer and vice president for business and finance] happened to be standing next to me, and he said, "Oh, we could [have some ready] in a week." Well, as you may know, the eventuality of that is that John Braun, in two stages, first gave the funds for a second swimming pool and a preliminary version of the weight-training facility, and then eventually gave the funds to build the Braun Gymnasium.

Cohen: Good for you.

Morgan: And I was very happy that it worked out. But as things go at Caltech, by the time the thing you proposed comes into being, you're no longer on the job. But I was very happy, because I use the gym myself. And furthermore, what it really did was to take the crunch that I was so aware of out of having just one gymnasium for all the different things that faculty, staff, undergraduates, and graduate students wanted to do.

Cohen: And the dependent hangers-on. [Laughter]

Morgan: Absolutely. So I always thought that was one of the interactions with the trustees that I enjoyed the most.

Cohen: Well, that's terrific.

Morgan: And to tell the truth, I miss, a little bit, schmoozing with Lew Wasserman and Mary Scranton, who used to be on the board of trustees—some people with whom I had what I call a good rap. What I found was that the trustees didn't hear enough, from their point of view, about undergraduates.

Cohen: Well, there was no one to present anything particularly.

Morgan: So I took the step of inviting—I don't think it was my idea originally, but certainly when I was given the chance to run with it, I ran with it—undergraduates out for maybe three or four hours to give a performance, like the glee club performance, and then eventually to have undergraduate leaders become dinner guests at the Saturday night dinner. And that still goes on.

Cohen: So you really gave the students a presence there.

Morgan: It should have happened long before. I think I had what I would call a small part. But my trustee interactions over the athletic facilities were really my happiest moment. And strangely enough, it was my first moment, because it was after one month on the job.

Cohen: Well, that was good of Murph to ask. I mean, he must have had some feeling about that.

Morgan: Yes. Well, Murph and I have maintained a correspondence. We don't see one another very much, except maybe when he comes up for the inauguration of a new president. But we correspond. I sent him a copy of the new edition of our book [Stumm & Morgan, *Aquatic Chemistry* (1996)], which, by the way, I've ordered and will give to the Archives.

Cohen: Good, good.

Morgan: Yes. And I share with him my good news, if I receive an award. And sometimes he responds by sending me a quart of Irish whiskey, which he thinks is a terrible waste, because I don't drink alcohol. I still have an unopened bottle of pure malt.

Cohen: Well, so that ended your administrative life here, being vice president.

Morgan: That's right.

Cohen: And you went back to being a professor. This may be a good place to stop.

Morgan: OK. [Tape is turned off]

JAMES J. MORGAN

Session 3

June 16, 1999

Begin Tape 3, Side 1

Morgan: This is Bloomsday.

Cohen: Bloomsday?

Morgan: The day in 1904 on which the events of James Joyce's *Ulysses* are supposed to have taken place.

Cohen: Is that so?

Morgan: It's celebrated in many parts of the world, usually by drinking or by touring, especially in Dublin. It's a big day in Dublin.

Cohen: Well, OK.

Morgan: I've never made it there, but I would like to some day.

Cohen: OK. Let's get this little story in here. What is this that I see in my notes that you were a quiz kid?

Morgan: I went to a Catholic high school in New York. And there was in those days—'49, I guess—a New York version of what nationally on the radio was called *Quiz Kids*. This was a television version, which I think must have lasted for about a year or two. It was on one of the local television stations, and I no longer can remember which. But they recruited high schools to

send them students to participate in their television program, which wasn't called *Quiz Kids;* it was called some New York [name, a] more dignified version of it. So I was a quiz kid.

Cohen: You were sent from your school as a representative?

Morgan: I was sent from my school. And I really only did it once, because I think everybody only did it once. It was a half-hour format, and you were on camera, and there were three or four other kids from three or four other high schools. I remember that my specialty, as I thought of it at the time, was American history. And the question that came my way, or the question to which I responded was, "Name the present states of the United States which are the result of the Louisiana Purchase." And I didn't get all of them; I got most of them. But I think it was the next day in high school [that] one of the priests met me in the hallway and said, "Hi, Louisiana. Too bad you couldn't get all of them." [Laughter] And I think there were probably some other questions about math and physics that apparently were easier, because I don't remember having any trouble with them. But it was a nice little irony for me, and a little humbling, that being a specialist in American history—I received the history medal from that high school, Cardinal Hayes High School, in the Bronx, when I graduated. So when I remember that, I remember it as both.... Also, they paid $50.

Cohen: I was going to ask, "Did you win any money?"

Morgan: They simply paid you $50. It wasn't a competition. You were paid $50 for appearing. That was my moment of glory. And then my other moment of glory was the William Randolph Hearst American History Essay Competition, which was held every year and was open to all high school students in New York City. I was chosen by my school to be the representative, and I think I finished fourth or fifth in the City of New York. Basically, they got you all together in a big room and then they announced what the essay question was. And then they'd say, "Go!" and you'd write for an hour or two, or whatever it was. I can no longer remember. But the essay, I remember, was "Discuss the history of the American political party system over the period from such and-such to such-and-such." And I think I finished fourth.

Cohen: Not bad, not bad.

Morgan: That was my last great brush with history.

Cohen: Oh. [Laughter] Then you got serious?

Morgan: Well, I was serious about that. I wanted to study history when I went to college. But through a set of circumstances beyond my control, which were largely financial, I needed a scholarship to the right university which would waive my lack of foreign language experience in high school, which was another one of those oversights—

Cohen: You did speak of that, yes.

Morgan: Yes. So I was unhappy at the time, because I thought that my true love would be history. Although I had no idea of what an historian did. On the basis of high school history, as far as I could tell, you absorbed a lot of facts and you wrote about them. So at the time, I was disheartened. But in the end I went to Manhattan College. I also had the opportunity to go to CCNY—City College of New York—and the opportunity to go to Columbia. But everything depended on where I could get financial aid, so that's how I came to go to Manhattan, and that's how I came eventually to be an engineer. I like to characterize it as a meandering river. I picture my life that way. At the right point, the river meanders in the right direction, and I get on it and I go. [Laughter] So my life as a quiz kid was brief, brilliant, and over very quickly.

Cohen: [Laughter] Well, let's talk about some of these committees you've been on. Do you still continue to do this consulting? Have you been on the one-day-a-week bit that so many people seem to do in engineering?

Morgan: My most important activities outside Caltech have not been in consulting as much as they've been in what I would call professional service and governmental service. When I

became the editor of the new American Chemical Society journal, *Environmental Science and Technology,* in 1966, I pretty much decided that that was going to be a major—more than equivalent, I would say, as it turned out—commitment of one day a week. Although in my first year at Caltech, encouraged by Professor McKee, who was a very, very vigorous consultant and highly regarded for his consulting on water and wastewater treatment, I did some consulting—pretty much one day a week or one day every two weeks. And I did that for 1965 and a good part of 1966. And I must say that it was very stimulating, because it helped me to have a different window on the world. I was actually involved in the process design of a few different water-treatment plants in the state of California. I was an advisor on corrosion problems and things of that sort. But when I was tapped to be the editor and finally decided to do it, I came to the realization that for a new journal—and as the only editor, and with no staff other than myself and a part-time secretary at Caltech—it was going to take all my time. And indeed, most of the next eight years—

Cohen: You were involved with this journal?

Morgan: I was involved.

Cohen: Where was home base for this? Was that at Ohio State, too, with the rest of the American Chemical Society's—?

Morgan: No, no. The American Chemical Society's editorial headquarters are in Washington, DC. And Columbus, Ohio, is the site of ACS Chemical Abstracts and some of the support structure, but all the journals have their administrative headquarters in Washington, DC, in a rather large American Chemical Society headquarters.

So for that period I did very little consulting. I also played a major part in an American Chemical Society committee which ran from 1967 through 1969. And that took a lot of time. A major meeting [was] usually at some dismal airport or in Washington, DC. And the purpose of that was to produce a book which would be given out widely, entitled *Cleaning Our Environment: The Chemical Basis for Action*. We were very well aware of chemical pollution.

We were very well aware of what I will call the more familiar kind of urban waste pollution. We were certainly very, very well aware of air pollution and, even then, differences in the kinds of air pollution that predominated in different American cities—so-called LA smog versus East Coast sulfur dioxide and things of that sort. So I helped draft that report; it came out in 1969. And I remember that one of the highlights of my young career was that I was part of the group from the committee that made the presentation to the relevant committee of Congress, which was chaired by Congressman [Emilio Q.] Daddario [Subcommittee on Science, Research & Development]. And that was the first and last time I was ever in the House of Representatives. It was very exhilarating, I must say. And I found Congressman Daddario very perceptive, intelligent, and—

Cohen: I think he had a good reputation.

Morgan: Well, on the basis of my contact with him, he certainly deserved it. So after 1974, when I decided that eight years as a boy editor was enough, I gradually became involved in external activities, most of which were still not really what I would call consulting; they were government advisory panels. For example, I was involved in several National Research Council panels, which is not unusual for a Caltech faculty member, I must say, or for a faculty member in many universities and agencies, because on these panels you work for free in order to have an impact on some subject that needs researching. And as a side joke, in parentheses, once my wife discovered that I was working for free and making those three-day trips to Washington, she said, "I'd like to discourage you. [Laughter] I thought we were losing your time because you were making some money." But that was partly a joke, because she noticed that I would often come back completely exhausted from traveling back and forth. Most of them took place in Washington, DC.

But I have been involved—as you can tell by looking at one of my many CVs—as a consultant with a number of public and private agencies. The basis for it has always been water chemistry of natural waters, polluted or unpolluted, and in some cases the treatment technology of water and wastewater. So I've been involved with the City of Los Angeles Department of Water and Power on several different occasions about their waste treatment or water treatment

plants. I've been involved with a number of industries and companies, including Procter & Gamble. Well, Procter & Gamble is one of the most interesting cases, because—

Cohen: Now, that you would have been paid for?

Morgan: That I was paid for. I was also involved in consulting for an organization which started in about 1970, called the Southern California Coastal Water Research Project, whose acronym was SCCWRP [pronounced "skwerp"], which was a very, very helpful organization. It was an organization which was essentially a consortium of the counties from the Mexican border up to Santa Barbara, all of whom had ocean discharges. So that left out Riverside.

Cohen: Yes. Now, the one you just mentioned that you said you wanted to say something about?

Morgan: Procter & Gamble?

Cohen: Procter & Gamble, yes.

Morgan: The consultation with Procter & Gamble was to help them develop a computer code, or actually to help them learn how to use our computer code, which one of my advisees and postdoctoral fellows, François Morel, developed. They were trying to learn how to use the code, but they were having some problems with it, I think partly because of their chemical understanding. So I remember spending several days, both in Ohio at their research center and out here, in order to go into some of this. The reason they were very interested in the code—and they were very perceptive about this—is that they understood that it would allow them to calculate the forms of potentially hazardous compounds in water, what environmental chemists have come to call speciation—that is, the quantification of the concentrations of actual chemical forms of the elements. You can see the history of environmental chemistry in water over the century as a progression from measuring elements to measuring some typical collections of elements in radicals such as sulfate—and then eventually to: What are the metal complexes that

actually would exist in seawater or coastal water? That is, What would be the complexes of, say, copper with citric acid? Or what would be the complexes of calcium with a phosphate ion, polyphosphate, and so forth? So that was a fairly fruitful collaboration. The joke among my students about me is that I'm a very successful consultant, which means that I don't consult very long on any given project because I solve the problem. [Laughter]

Cohen: [Laughter] Or you may tell them what they don't want to hear.

Morgan: That can happen, too.

Cohen: Yes.

Morgan: So I would say that my consulting activity went up between 1974 and 1980. And then it went down again, because in 1980 I became vice president for student affairs.

Cohen: Now, [during] the consulting part, you were still doing your teaching here and all these responsibilities?

Morgan: Yes. It's a cliché, but it's certainly been borne out in my experience that the consulting I did for industry and government and my service on public panels was very enriching for my teaching. The major beneficiary of my consulting was my teaching and my interaction with my students, in some cases putting them on good problems that had nothing to do with a client anymore but where the stimulation came from a consulting assignment.

Cohen: Well, I think that's why engineers have always been urged to do consulting. Is that correct? I mean, that's the impression I have.

Morgan: I like that idealistic explanation. I can tell you another explanation that I've never heard at Caltech but that I've heard at other universities, which is that it's one of the ways that they expect you to make working for that university worthwhile; that is, you can augment your

income. I was never so committed in available time to consulting to view it as a moneymaker. I always thought that if it was interesting and sounded interesting, I would talk about it with the client and then it would either become something I would do or something that I would not do. I've also consulted with Bechtel on water reclamation projects. But in every one of these cases, if it didn't look interesting, my first impulse would be to suggest somebody that I thought might find it interesting. And if I did find it interesting, my habit was to spend the minimum amount of time required to help the client solve the problem and then go on to something else.

Cohen: So that doesn't loom very much in your history as something that you—

Morgan: No. I think that, like every other Caltech member who has something interesting to say, sometimes there's a consulting opportunity in which you say it. But to me the consulting for public agencies and advising the NRC and working with the American Chemical Society has been a much larger part of my outside Caltech life than has consulting.

Cohen: OK.

Morgan: I just thought I learned more. I maybe had more influence. But to this day I still do continue to consult. If somebody brings me a question that I think is interesting and I can see that I have the tools or I can help them invent the tools, it's still worthwhile doing. But I think it would be honest to say that it's a small part now of what I enjoy doing.

I don't have a single patent. [Laughter] Partly because I was advised by my advisor when I was getting my master's degree at the University of Michigan that his experience had been that every patent he had applied for or had received had caused him nothing but grief. [Laughter] Now, I was impressionable and young, so it stuck somewhere in the back of my memory.

Cohen: So you'll have to be a millionaire in some other life.

Morgan: But in this era at Caltech, as I'm sure you may know from other circumstances, the transfer of technology into useful results is a very, very important agenda item. A number of my colleagues in environmental engineering science have patents going up into the fives and tens as a result of their work.

Cohen: Well, it's certainly being encouraged.

Morgan: Yes, and they're doing it. So the environmental area is one important area. And one footnote about knowledge of the real world: My father-in-law had been an executive with Chrysler Corporation before he retired in the sixties. He enjoyed talking about future technology, because after he retired he devoted his whole energy to investing in the stock market. And he was quite good at it. I remember more than one conversation in which he would quiz me very, very closely about this or that technology or process: "Do you think that that's really likely to be important?" And I always felt satisfied when I could give my father-in-law what I thought was useful advice. [Laughter] And maybe that was the most important outcome of consulting for me: family credibility.

Cohen: Yes. [Laughter] Well, very good. So what about the spin-off businesses that some people do—the companies? That seems to be the next step.

Morgan: Well, I have to be truthful and say that I don't know very much about that, because I've never been interested in it myself. I'll confess to a certain amount of skepticism about the tension between duties as a Caltech professor—especially if you have my profile, which was as a Caltech administrator very deeply involved in the day-to-day life of the institute, through my own choice. I never became interested in spin-offs, so I would say that I don't even have a very good window. I know that some of my colleagues have a much more day-to-day commitment—or week-to-week commitment, to be more accurate—to their consulting activities than I do. So when you talk to some of my colleagues, you'll get a better perspective. I tend to agree with a humorous remark once made by Robbie Vogt when he was provost and some contentious issue was involved. He said—to a reporter for *Science,* I think—"You really ought to think of us as

more like monks, detached from this world." [Laughter] I might be a little more like that than Robbie may have imagined at the time.

Cohen: Well, I guess it depends. The climate is certainly going in that direction, though.

Morgan: He wasn't talking about me, by the way.

Cohen: Yes, OK.

Morgan: When my students, some of whom—maybe about half of them—are now professors at other universities, ask me for advice, I always say, "Take care of business first. And your business is to be a good research advisor, teacher, if possible an author of textbooks, and certainly an author of really important scientific papers." And that's what I believe.

Cohen: Well, I'm glad you brought the textbook up yourself, because it's really a very handsome book [*Aquatic Chemistry,* Werner Stumm & James J. Morgan]. I want to talk about writing that book a little bit.

Morgan: Writing that book became a commitment the day I defended my PhD thesis at Harvard in 1964, when Werner Stumm, who was my professor, said, "I would like you to agree to join with me in writing a textbook," because there was no textbook that he considered adequate or even close to adequate. So that was in 1964. The first edition came out in 1970. It was a very arduous journey, partly because my time clock and the time clock of my advisor were not synchronous. As you may remember, I had the good fortune to be his first PhD student. We had a very congenial working relationship. I think we enjoyed working together. Well, I certainly know we enjoyed working together from my point of view. But writing the book was blood, sweat, and tears. And Werner Stumm used to say, or his joke was—of course, these were back in the days when he was in fact a smoker—"One cup of coffee, one cigarette, one line of text." [Laughter] Now, I didn't smoke, but I certainly drank a lot of coffee, and I could identify with that. What do you learn out of this? You learn that it's not so easy to write a book as you might

have imagined before you tried it. And we were both neophytes at that. You learn that the publisher thinks that once you've signed the contract, the book should follow essentially immediately. So after a few years have passed and you haven't produced a manuscript, then they start to, I would say, badger a little bit. And the third thing you learn, which almost every professor knows who doesn't write a major, widely used textbook, is that you don't write books to make money. Now, the exception to that, as I'm sure you know, would be very, very successful textbooks that everybody in the world has to use, like [Paul A.] Samuelson's *Economics* or [certain] very successful general chemistry books. Earlier in our time at Caltech, probably Linus Pauling's book.

Cohen: Pauling's *The Nature of the Chemical Bond*.

Morgan: If you start out to write a book to make money, this is not the book to write. But the book was very well received, and I think it had the influence on the field that we both had hoped it would. It became widely adopted as a text—in graduate programs, mainly, and graduate programs across engineering, limnology, oceanography, soil science. It became very, very widely known. And it sold very well. And then after about—

Cohen: How did you write it? I mean, this was the precomputer days, so you couldn't e-mail back and forth. Were you ever physically together?

Morgan: We wrote it by visiting one another. I would visit Werner Stumm at Harvard, because the first edition was finished while he was still a professor at Harvard. So it was easier to visit. I would visit for a few days or a week at Harvard, and he would visit Caltech for usually just a few days, give a seminar and so forth. So it was done by mail and by visits and by dividing up: "You take the lead on this chapter, I'll take the lead on that chapter." And Werner Stumm—I never hesitated to say this—was the dominant force in writing the book. Norman Brooks once asked me, "Why is it called Stumm and Morgan rather than Morgan and Stumm?" I said, "Because it was the brainchild of Werner Stumm and his ability to move fast and be decisive. My role was to write certain chapters, to perfect other chapters, and to be what I would call a

detail man." [Laughter] That changed progressively over the three editions. And the other thing that happened, which is not unrelated to what we talked about before, was that after making this commitment and starting to work on the book, we agreed that the natural way to evolve the book was to have it grow out of our lectures, mine at Caltech and his at Harvard, and also to take every opportunity when invited to give a talk that wasn't a detailed science talk, like an overview talk at a symposium, to use that opportunity as a way of shaping material that eventually would find its natural place in one part of the book or another. And that's really how it evolved. It evolved out of teaching and out of symposium lectures on certain subjects. The first six or seven chapters were the basic chemistry that you needed to apply to understand natural waters. And then the last three chapters were more applications to treatment systems, ecological systems, and so forth.

Werner Stumm was always very, very interested in ecology as a subject. He called himself, facetiously, a theoretical ecologist. I never had a very strong interest in ecology. My interest was always in applying chemistry to engineering problems. So it was an interesting match. The middle was chemistry. Then on one side of the middle was my very strong interest in using chemistry for treatment processes. And this is, of course, an exaggeration in part. But on the other side, Werner really wanted to say something about what the ecological consequences could be for a proper understanding of chemistry. And I think that message earned the book a lot of respect over a wide range of disciplines. Because in the sixties and early seventies, it was now very, very clear that environmental problems were not going to go away; they were going to become better understood and they were going to become even more necessary to work on. So I think the book found a resonance with a fairly wide audience. And a number of the students who took my course at Caltech in the late sixties and early seventies were graduate students from other departments who were becoming interested in water chemistry as something they might find useful. That included students from geology, chemistry, and chemical engineering. Those were the three areas where a number of the enrollees in my classes came from. So we revised the book, and a second edition came out in 1981. It was much larger.

Cohen: Is that when you spent a whole year—

Morgan: I've never spent the whole year. I've spent the equivalent of a full year in summer visits and other visits to Zurich. I spent the summer of '73, the summer of '78, and the summer of '90 in Zurich. And Werner Stumm spent most of the academic year in 1977 and 1978 as a Fairchild Scholar at Caltech.

Cohen: So that gave you an opportunity to really work together.

Morgan: I would say that a large chunk of progress was made during the time that Werner was here at Caltech, from '77 to '78.

Cohen: OK. And then this 1996 edition— [Tape ends]

Begin Tape 3, Side 2

Morgan: The third edition is about fifty percent larger than the first, and it contains much more information that I would call new. Chapters on new subjects, and so forth, which I think is simply a reflection of how much the field of environmental chemistry and the field of aquatic chemistry [have grown]. The name "aquatic chemistry," by the way, is a creation of Werner Stumm's.

Cohen: Is that right?

Morgan: Yes. And I think you will recall Sheldon Friedlander, who was a member of this faculty from 1963 to 1978. When he asked me what the title of the book would be, I said, "Aquatic Chemistry." And he said, "It sounds a little fishy to me, Jim. I don't know. What's it going to be about?" And I would say, "It's going to be about the chemistry of the aquatic environment." And he had the association, which many people have, that "aquatic" meant fish and wildlife. But that wasn't what the book was about. And in spite of his humorous forebodings, it sold very well. People understood what it was about once they opened it, because it had a subtitle. The original subtitle was "An Introduction Emphasizing Chemical Equilibria in

Natural Waters." The subtitle has changed over the years, because our emphasis has changed much more toward kinetics. So it's fair to say that the first edition was almost entirely about the application of chemical equilibrium principles to understanding natural waters. And the third and final edition is a blend of equilibrium thinking and kinetic thinking. And it has grown to include photochemistry and transfer atmospheric chemistry—the transfer between the water and the atmosphere. So those are new features.

Cohen: Now, he has just recently died. Is that correct?

Morgan: Werner Stumm died on April 14th, 1999—two months ago.

Cohen: Oh, that's not very long.

Morgan: And I have already written extensively about it in pieces to journals that wanted memorial statements. And it's been a very, very emotionally charged time for me, because I've had to realize that he isn't with us anymore. But because of the enormous amount of work that he produced in his life, he still is very much with us. So it's sort of like he is and he isn't. Anyway, I wrote a major letter, one page long, to the editor of *Environmental Science and Technology,* which came out, oh, just ten days ago, in which I tried to lay out both his intellectual and personal qualities, which I thought had been successful.

But he chose the term "aquatic chemistry." Most of the twentieth century, the application of chemistry to environmental engineering problems was about the chemistry of treatment plants and the chemistry of corrosion. And it was called—understandably—in the United States, water chemistry. And in Germany it was called *Wasser Chemie*. And the French had no word for it, which is fairly typical. [Laughter] And there was a practice of industrial chemistry in Great Britain. So what he wanted to do, and what we discussed, was to try to blend—give a sense of the unity of applications of chemistry to environmental problems, whether it was a lake, groundwater, an estuary, a river, or a treatment plant. And the argument we made was that aquatic systems, whether they're engineered or natural, are going to be guided by very much the same core body of principles: acid/base chemistry, solubility, precipitation, electron transfer,

complex formation. And the choice of "aquatic chemistry" [as a title] was not a frivolous choice; it was an attempt to say, "This is what we're going to embrace. We're going to embrace the chemistry of the aquatic environment whether it's contained in a treatment plant or whether it's out there." And I think that it worked, even though there was some skepticism on the part of some of my colleagues.

Cohen: So that's really been a very large part of your professional life—getting out the message of this book.

Morgan: Yes.

Cohen: And it's certainly more important all the time. Here I see you were a visiting scholar at Woods Hole. What were you doing in Woods Hole?

Morgan: That must have been 1974.

Cohen: That was '74. Did you go for a summer?

Morgan: No, I went for a week. The Woods Hole visiting scholar program is a very intensive week of giving lectures and seminars and interacting with students and staff. And they invite a number of scholars each summer from different fields. I remember that I gave an evening lecture to the geochemistry and chemistry group. And I also gave an afternoon lecture at the Marine Biological Laboratory. And if you're familiar at all with Woods Hole, there are really two institutions that are at Woods Hole. One is the Woods Hole Oceanographic Institution and the other is the Marine Biological Laboratory. They are whole and separate, and I'm told they have very, very different cultures. And at the MBL, as it's called, they have some outstanding summer courses in microbiology and molecular biology—now, not then—and ecology. And I gave a guest lecture over in the MBL on speciation of copper in natural waters and how that might affect the nutrition or the toxicity of plants that lived in natural waters. [Laughter] It was the most amusing thing: After I gave the lecture, one of the students came up to me and instead

of asking me a question, he said, "Do you know who you remind me of?" And I said, "No, I'm sorry." He said, "Norman Mailer." [Laughter]

Cohen: [Laughter] Did you ask him why?

Morgan: He said, "You look like him, and you talk like him. You're very aggressive in the way you lecture," and so forth.

Cohen: I'm sure he thought he was complimenting you.

Morgan: I've never read very much Norman Mailer, so I decided to take it as a compliment. But that was a very stimulating cross-connection, because it was a move a little bit toward having chemistry connect with marine biology, which was something that my friend and colleague François Morel had already begun to do just the year before, at MIT. He had been a postdoc with me, and he had been my co-advisee, along with Harold Wayland [professor of engineering science, emeritus]. François Morel is one of the very, very successful aquatic chemists who has written his own book, *Principles of Aquatic Chemistry*. And another one of my PhD students, James Pankow, a PhD from 1978, has written his book on aquatic chemistry [*Aquatic Chemistry Concepts*].

Cohen: Well, they say there's no higher form of flattery than imitation.

Morgan: Morel and Pankow really represent the great benefits of coming to a place like Caltech to be a faculty member, along with several others whom I feel very, very happy about, because they were constantly challenging me. Did I make a remark to you once that Werner Stumm used to say that after I'd been at Harvard a few years it wasn't clear who was the professor and who was the student? I don't know if I said that to you. But I did say it to somebody recently. And I felt exactly the same way about François Morel and Jim Pankow. They were such bright and comprehensive thinkers already as young men, and they have both become great successes in their own careers.

Cohen: So François Morel is at MIT?

Morgan: No. He went to MIT and stayed there for twenty-two years. And then about four years ago he moved to Princeton, into a completely different area—geology. At MIT he was in environmental engineering, and he started to open up this connection between aquatic chemistry and marine biology, which has really become his signature area. And that's why, I think, he was recruited to move to Princeton, but in earth sciences, because, forward-looking, they saw that understanding the chemistry of the ocean, for example, as a part of the global system is going to require much deeper research into the chemistry/biological interface for marine organisms.

Cohen: And maybe he was ready to do something else anyway.

Morgan: Yes. But it's a great and true cliché about Caltech: a Caltech education, either at the undergraduate or graduate level, will lead you to places you might never have imagined you would go.

Cohen: You mean it leaves you with an open mind and willing to do anything?

Morgan: And cultivation of fundamental skills, which doesn't leave you dependent upon one way of looking at things.

Cohen: Well, that's certainly very significant.

Morgan: I think that's a great strength at Caltech.

Cohen: And how about Pankow? Where has he gone?

Morgan: Pankow has been at the Oregon Graduate Institute of Science and Technology in Portland since 1980. He is a professor and chair of the department of environmental science and

engineering. He has already published three books, including one on water chemistry and two on other subjects. He has recently received the American Chemical Society award for creative advances in environmental science and technology. He's been everything I thought he would be.

Cohen: So were these your two most significant students?

Morgan: I would say that I have co-advised or advised some thirty-two students at Caltech. I think twenty-five have been uniquely my own PhD students. François Morel was a co-advisee with Harold Wayland. I've had several other students who are very dear to me and whose work is very dear to me. Alan Stone, PhD in 1983, is now a professor at Johns Hopkins University. He considers himself an environmental chemist. Interestingly enough, Pankow was an undergraduate chemist before he came to Caltech, and Alan Stone was an undergraduate chemist. So it's been one of my great opportunities at Caltech, within the framework of our very, very flexible program called environmental engineering science, to be able to work with people who have undergraduate backgrounds from different fields, like chemistry or civil engineering or chemical engineering. I always include Alan Stone among the five or six students that I think have had the greatest impact in their generation on the field. And I also include Howard Liljestrand, from the University of Texas at Austin, who was the young man who worked with me on acid rain.

Cohen: Liljestrand?

Morgan: I think it means "along the riverbank." It's a Scandinavian name. He got his PhD in 1980. He was the first one to build a network for sampling the chemistry of the rainfall in Southern California and to make connections between the chemistry of the rainfall and the chemistry of the atmosphere from whence it came and then to relate that back to sources, such as the automobile versus the power plant, and so forth. I consider him one of my very, very best students. And there have been many, many more. One of the students I co-advised with the late Clair Patterson [professor of geochemistry] was Yigal Erel, now at the Hebrew University of Jerusalem. He is an outstanding earth scientist. And I was so tickled when he got his degree

about eight years ago. After I got to know him, he said, "You know, I came to Caltech to work together with two people." And I said, "Really? Who are they?" And he said, "Patterson and you." [Laughter]

Cohen: Well, he did.

Morgan: And he did. He did his field work with Patterson, and he did his lab work with me.

Cohen: Was he into lead, or something?

Morgan: Lead. His thesis was about the environmental chemistry of lead. But it's interesting that somebody from so far away—in my world [that's far away]; I've never been to Israel.

Cohen: Well, I think Israel is tied into the intellectual community of the world, frankly.

Morgan: Oh, yes. I was going to give you my joke today, [because] I'm wearing my white shirt. I was going to say, "I'm here posing as a member of the Knesset, because every time I see video footage of the Knesset in action they've all got white shirts open at the neck and long sleeves. [Laughter] But I haven't been to Israel yet. I hope I will go to Israel. I was invited by Yigal two years ago to come over and spend two months and give lectures. My wife said, "I wouldn't come with you, because I'm too frightened by things over there," to which Yigal said, "Come on. Life is more dangerous in Los Angeles." But I said, "Well, you and my wife will have to work that out." [Laughter]

Cohen: Well, that's interesting! Everybody will agree that the basic problem in Israel is the water: Where is the water going to go?

Morgan: Yes, that's right. And that's the first question I was asked by a reporter when I was in this telephone press conference after learning about the Stockholm Water Prize. One of the first questions was, "What do you think about the future of water wars." And I said, "I think they

would be pretty bad. What else do you have in mind?" [Laughter] He said, "Well, what does your work mean?" I said, "My work is about technical possibilities. So the serious answer to your question is that I think we're going to have to use a combination of technical possibilities and superb politics and diplomacy in order to solve that. It's not just a technical issue. But there certainly are technical issues." Israel derives a very large part of its water from the Sea of Galilee, and they've constructed an enormous aqueduct system to carry the water from the northern part to the southern part. I know several engineers and environmental scientists from Israel very well, so it would be natural for me to visit. Last year's recipient of the Stockholm Water Prize was Gedeon Dagan, who is a groundwater hydrologist from Israel.

Cohen: You know they've had to reflood some of those marshes they've drained over the years, because this turned out to be a disaster?

Morgan: Yes. Well, maybe it's another demonstration of what I quoted [Harvard sociobiologist] E. O. Wilson as saying in *Consilience*. [He said that] we're just not able enough to manage complicated natural systems, and the further we get from harmony—to use an overworked word, but I think it means something, a harmonious relationship with the natural world from which we evolved, and he's looking at it essentially as a sociobiologist—the further we get from understanding and staying close to that, the more in jeopardy we are as a species.

Cohen: Right. Well, shall we go ahead? Because we've got a lot to talk about yet. Do you want to do another session, because you've already talked over an hour—

Morgan: I'd rather just go on right now, because this is the summertime and the living is easy. [Laughter]

Cohen: Good. That's fine with me.

Morgan: It's not going to be as easy come August, when I have to go to Stockholm and give all these speeches.

Cohen: Yes. Will they make some videos of those?

Morgan: Yes.

Cohen: OK, because that would be good for the Archives to have.

Morgan: I have last year's video. It's not, I think, as good as it's going to be this year, because last year it was done by one of the sponsor companies. The Stockholm Water Prize has a variety of financial sponsors.

Cohen: Well, let's talk about these in order. OK?

Morgan: Go ahead.

Cohen: Now, I'm not sure how much you want to talk about these early ones. Here I've got Chi Epsilon. That would be just an honor society.

Morgan: That's the civil engineering honor society, at Manhattan College, which made me feel happy. And that was it.

Cohen: Did you buy a key?

Morgan: I did. It's like a Tau Beta Pi key. This one had a little red stone in it, and I kept it for almost thirty years, and now I've lost it.

Cohen: [Laughter] Well, you've gone on to bigger things.

Morgan: No, it's not that. I think I just lost it. It's probably in the house someplace. You know the old theory, "Nothing is really ever lost."

Cohen: Are these also going with your school—that is, the Ridgeway award.

Morgan: The Ridgeway award was something that was given to an outstanding civil engineer and graduate in order to encourage their future involvement in the American Society of Civil Engineers. And with it came a year's free membership. So I've been active for a large part of my life in the American Society of Civil Engineers, but not as active as a typical civil engineer would be, because I'm not a typical civil engineer, as you probably have already figured out.

Cohen: Now, the Cardinal Hayes? Was that a church award?

Morgan: No, no. At Manhattan College I was the student body president and I was the editor of the engineering journal and I was sort of the *capo di tutti capi,* the chairman of the council of presidents of all student organizations. In other words, I was what I would now call a big wheel on campus, and I think I have mixed feelings about it now. But the Cardinal Hayes award was a social-action award. It went each year to the student who in the judgment of the selection committee—whoever that was; faculty members, I'm sure—had contributed the most to the social welfare of his classmates. Then it was "his"; Manhattan College has since become coed. So that's what that's about. Those are all college-level things that you've mentioned.

Cohen: Right. Now, where would you have won the Danforth Foundation teacher award?

Morgan: I was at the University of Illinois from 1956 to 1960. I had gone there, as I think I explained to you before, in order to build on my earlier experience in stream research with Professor Velz at Michigan. And I had the opportunity while there to work part-time on a PhD. And I used that opportunity by taking courses in chemistry. I had already, on my own, figured out that chemistry was going to be very, very important. This was 1956. So I started to invest my free courses in chemistry and fluid mechanics. But I never got a degree from the University of Illinois, because I never found a research project or a mentor that I thought would be doable, practicable. So in 1959 I applied for a Danforth teacher award from the Danforth Foundation.

Cohen: OK. Danforth was from Illinois, wasn't he?

Morgan: No, Saint Louis. They made all that dog food and all that cereal. They're a very wealthy family, and they had a foundation. And the foundation at different times would encourage different activities. And at that particular time, somebody had identified that we needed more PhDs who were qualified to teach and that there was a niche. And the niche was that there were a number of people in a number of different disciplines who hadn't finished a PhD but who probably could if they were given a year or so of support. So I applied for that. There were, I think, ten awards each year, and I got one of them. And instead of staying at Illinois, as Illinois hoped I would, I moved to Harvard.

Cohen: And took it with you?

Morgan: I took it with me; it was portable. So that supported me for my first year—tuition and cost of living for myself and my family. That's how I was actually able to afford to go to Harvard, because Harvard never would come up with that much financial aid.

Cohen: Well, very good. And then here we start with the ACS. You got the certificate of merit.

Morgan: That's for having given a successful first—and I must tell you, terrified—lecture.

Cohen: [Laughter] Where was that?

Morgan: That was in Chicago, at the National American Chemical Society meeting in September 1961. It was after my first year at Harvard. I spent a very intense summer in Werner Stumm's laboratory making experiments on coagulation. That is the process of removing particles from water using novel chemical approaches. And I've always appreciated this so much about Werner Stumm: he had so much confidence [in me that] he said, "You give the talk." And it was a Monday morning at nine o'clock, the first talk. And I looked out and saw

what must have been 200 or 300 people. And I suddenly realized, "My God. I'm terrified." [Laughter] But it was too late to do anything about it except give the talk, so I gave the talk. The American Chemical Society is organized into divisions, and each division identifies at each meeting one or two talks that they consider exceptional first outings. So that's what that was about. And that talk then was transformed into one of our most successful papers.

Cohen: Is that right? Oh, very good.

Morgan: And that paper then received an award from the American Waterworks Association—in 1963, I think.

Cohen: Right.

Morgan: The paper was called "Chemical Aspects of Coagulation." And then it received another award in 1980-something.

Cohen: Is that the American Chemical Society for Creative Advances in the Environment?

Morgan: No. That's the Association of Environmental Engineering Professors publication award. That society decided about ten or more years ago to give an award to publications that had, as they said, stood the test of time.

Cohen: That's a good idea.

Morgan: Which meant that they had been published some time ago and that [they were]—although they didn't use this word, that much overused word that we like to use in academics—"seminal." So that paper was good for three awards: making a first presentation, a publication award from the American Waterworks Association, and then many, many years later as a publication that has stood the test of time.

Cohen: It's a seminal paper. Oh, that's very good.

Morgan: So that paper was very, very influential, even though it's not the subject of my dissertation. Because after that first summer with Werner Stumm, he said, "Now, you've done fine. Is this what you want to work on, or is there something else you'd rather work on?" I learned from him to tell your students that the choice of a PhD subject—to first order—is theirs, as long as it matches something that I can help them with and as long as together or separately we can find the funding for it. And I said, "Well, I'd really rather work on manganese." And he said, "Well, why is that?" And I said, "Because I'd like to do for manganese chemistry in the environment what you've already done for iron chemistry in the environment." And he said, "Done!" So that's how I received an award for a paper that wasn't actually part of my thesis.

Cohen: Well, that shows your breadth. And then we have here [that] already you were being recognized here as a teacher: award for excellence in teaching, 1980.

Morgan: That was one of the early Caltech ASCIT [Associated Students of Caltech, Incorporated] awards. It's for the course I've taught continually since 1967, called Chemistry of Natural Water Systems—Env 142 ab. And I was very touched by that, especially because I had just become vice president for student affairs and I felt some trepidation about becoming an administrator. You probably know, or you may have heard, what most Caltech faculty members tell you when you become an administrator. They like to tell you, "Well, I guess you're becoming senile," or "You've gone over to the other side," or "You're the enemy now," or things of that sort.

Cohen: My experience in my own life in the school system was that administrators develop instant amnesia. They no longer remember how hard it was to do anything. [Laughter]

Morgan: Yes. Well, I did. I remembered how hard it was to do it, and that's why I was very touched by that award.

Cohen: Very good. And then we went to the Nalco award.

Morgan: That was for my graduate student—

Cohen: Windsor Sung?

Morgan: Windsor Sung. Windsor Sung was also one of my very, very best students, although he hasn't continued in an academic career and therefore I think of him less. Although I should think of him more, because his work was very, very original. And that was for his thesis.

Cohen: What is he doing now? What has he gone off to do?

Morgan: He has spent most of the last twenty years—in 1980 he received his degree—working for the Massachusetts Water Resources Agency. He took a brief stint at teaching at the University of New Hampshire, and for some reason he didn't find it congenial, or they didn't find it congenial for him. And he had a visa problem, and he got angry with them.

Cohen: Sung. Korean?

Morgan: No, from Hong Kong. So he decided to throw in his lot with the real world. And he was very successful but not in an academic sense. He's written some good papers sort of on the side, without being an academic. And now, recently, after about five years as an independent consultant in Massachusetts, he's gone back to the Massachusetts Water Resources Agency.

Cohen: So he's still doing water stuff.

Morgan: Oh, yes. He's been working completely in water. Anyway, that award was for his thesis. We submitted his thesis and—

Cohen: Nalco? What does Nalco stand for?

Morgan: That's one of the major water-treatment chemical companies. All these awards and almost all of the professional societies, whether you are aware of it or not, are funded by somebody. [Laughter] Nalco sells water-conditioning chemicals. And I don't know if they still exist; they have probably been bought by somebody larger. But at that time they were a real presence. And I knew a number of the technical people from Nalco who gave talks at the American Chemical Society national meeting. So that was an award I appreciated, partly because there was a real recognition of Windsor Sung's work, which was very original and which really turned a lot of people on to the subject of his PhD work, which was, in brief, "What's the connection between iron chemistry and manganese chemistry in natural waters?" They are side by side in the periodic table. That doesn't mean that their chemistries are necessarily similar. Usually you look down the periodic table to find [similar chemistry], and they were side by side. And the connection, in brief, was that oxides of iron—insoluble oxides—turned out to be very good catalysts for making it easy to take manganese 2 and oxidize it to manganese 3 and manganese 4, using oxygen as the electron acceptor. And his was the first work to show that.

Cohen: Oh, very good. Now, you've got here an Association of Environmental and Engineering Professors research publication award. And you won that with Werner Stumm in '83.

Morgan: Yes. That's the one I mentioned before.

Cohen: Right. And with Morel in '94. That's eleven years later.

Morgan: The paper with Morel, when he was a graduate student and then a postdoc here at Caltech, was the development of this computer program which was very efficient for calculating chemical species in natural waters—models or actual natural waters. So this was the second paper that was honored by the AEEP with an award [because it was] a paper that has stood the test of time.

Cohen: I like that. And then the American Society of Civil Engineers—a lecture, and the Simon Freese Award?

Morgan: Oh, yes. Simon Freese was an outstanding environmental engineer who lived most of his professional life in the state of Texas, and an award was named in his honor, I think at least twenty years ago. [Tape ends]

Begin Tape 4, Side 1

Morgan: Simon Freese was a very successful and practical environmental engineer famous for designing literally hundreds of treatment plants and reservoirs and pipelines in the state of Texas. And I had the pleasure of learning, after the announcement of the award appeared in one of the Caltech publications, that he was the grandfather of David Rutledge, a professor of electrical engineering at Caltech who called me up and told me about it. And it was great fun. He told me that his grandfather, as he remembered, was a very, very singular man. [Laughter] I had read a little bit of his biography on my own before that, and I decided he must have been a very singular man.

Cohen: OK. Let's see, that was fairly recent. Where did you have to give the lecture, then?

Morgan: I gave the lecture at an international American Society of Civil Engineers conference in Edmonton, in Alberta, Canada.

Cohen: OK. And then we get to the Stockholm Water Prize, which is a big one.

Morgan: Yes.

Cohen: What is that prize exactly?

Morgan: The Stockholm Water Prize was initiated nine years ago to call attention to achievements in water resources development and water resources protection, essentially. It carries with it an award of $150,000. All previous awards have gone to one individual; this year it was decided to make the award to two individuals, Werner Stumm and myself, in recognition of our dual role in establishing, developing, and applying aquatic chemistry to the water environment.

Cohen: Did this money have any of the same sources as the Nobel money?

Morgan: Well, it turns out—I was reading the [Nobel] poster outside [the Archives] [laughter]—it turns out that Nobel prizes have different sources of money. For example, the Nobel prize in economics is apparently from one of the Swedish banks. Even though it's called the Nobel prize in economics, I don't think it depends upon the Nobel money. The source of this [Stockholm Water Prize] money is a consortium of Swedish and international industries all of whom have an interest in water one way or the other, either because they're equipment manufacturers or because they're large international consulting firms, or what have you. I think even SAS Airlines is one of the cosponsors. And when I tell you that, I tell you all I know, because I'm not yet familiar enough. I'll probably know more after August, when I return. But right now I know that it's supported by a multiplicity of industrial donors who were persuaded, I imagine, that water is big and water will be bigger and that this is a natural thing for them to do.

Cohen: So you're going to go over there and get this.

Morgan: Yes, in August, the second week in August.

Cohen: You'll have to get it for both of you.

Morgan: Yes. That will be very, very—I was going to say "difficult". I don't think it will be difficult; it will just be very, very emotional for me, I think, because I have almost a lifelong—

certainly a professional lifelong—attachment and friendship with Werner Stumm. So I will be thinking a lot about what to say.

Cohen: Yes. Will your family go with you?

Morgan: My wife will go with me. Our children are all fairly well grown. Most of them have children of their own. It's not the easiest thing to put a package together. A number of professional colleagues from Caltech and former students of mine and people from other parts of the world whom I know professionally will be there. But most of my family will be able to attend the other award, which you haven't gotten to yet.

Cohen: Now, this is the Clarke Prize?

Morgan: The Clarke Prize.

Cohen: Yes. What is that?

Morgan: I'm the sixth recipient of the Clarke Prize. It was started by the National Water Research Institute, which was formed about six or seven years ago by a gift from the Irvine family. And the purpose of the NWRI is to encourage research in the development and protection of water resources. It's called the Clarke Prize in Water Science and Technology. The NWRI is located in Orange County, California. Although it's not a California award, it happens that the award is presented in California. This year it will be presented, in fact, in Pasadena, because they decided that as an experiment they will move the award to the hometown of the recipient of the award.

Cohen: Oh, I see. Very nice.

Morgan: That will make it possible for—

Cohen: Colleagues and friends to come.

Morgan: It's especially attractive because now most of our family will be able to be there, except for one branch, the Wisconsin branch, which is going to be leaving for a six-week tour of Europe the same day. You can't win them all.

Cohen: Right. Now, does this one come with money also?

Morgan: $50,000. And I think this work recognizes.... No, I was visited by a reporter from Scandinavia last week. And he had his camera and his video and so forth. And one of his early questions was, "Why are you receiving this award?" And being literal-minded and a little bit argumentative by nature, I said, "I don't know why I'm receiving the award. The people who chose me know why I'm receiving the award. I can tell you what I've done, and you can make your own inferences." So in that spirit, I think that the reason I might be receiving the Clarke award is (a) I've lived a long time, (b) I have a certain profile in water chemistry and aquatic chemistry, and (c) because I'm fairly well recognized for the applications of my science to engineering problems such as improved water treatment, wastewater treatment, and so forth. But beyond that, I can only guess. I think you probably know very well why people receive awards: (a) because their work has been judged in a large community to be significant, and (b) and probably very important, colleagues nominate them and do the hard work of making the case.

Cohen: That's true. But you need the first one to get to the second one.

Morgan: Yes. Probably it has something [to do with] necessity and sufficiency. [Laughter]

Cohen: So you must feel very good being recognized this way at this point.

Morgan: Yes. There's another award. I don't know if I put it there. But I was the Association of Environmental Engineering Professors lecturer in 1994.

Cohen: Yes. Yes, it is here. I didn't see it.

Morgan: That was a pleasure, although physically it almost finished me, because there I visited twelve campuses all over the United States and Canada and gave fifteen lectures in a period of two months.

Cohen: Oh, wow!

Morgan: To me, one of the most important organizations I belong to is the Association of Environmental Engineering Professors, even though I didn't get around to joining it until 1982, because it's a self-help professors' organization—that is, the dues are minimal, it's sort of a do-it-yourself organization, there is essentially no staff, and there are about 400 or 500 members. It's recently changed its name to the Association of Environmental and Engineering Science Professors. In alternate years, they ask a lecturer from the United States or Canada and a lecturer from other parts of the world. Werner Stumm was chosen as the distinguished international lecturer in 1981.

Cohen: And he did the circuit for them?

Morgan: Yes. He did about twelve lectures, I think.

Cohen: It sounds exhausting.

Morgan: It was exhausting. I didn't do it all continuously, though. I couldn't, because I had my teaching duties. So I divided it up into four major trips. It took me to places I probably never would have been before, like an isolated place in the province of Quebec, where I gave a lecture to a multiplicity of universities and students and faculty. And it took me to South Dakota School of Mines. It took me to Colorado School of Mines. I tried to follow a formula, because I was given some fairly major role in deciding where I would go. I decided to go where people hadn't gone before, for the most part.

Cohen: It takes a lot of traveling to get to these corners.

Morgan: Anyway, I enjoyed it. I worked up two lectures, and each university could choose which of the lectures [they wanted]. And these lectures represented broad themes from the research of my students and myself over the preceding fifteen years. It was very satisfying—tiring but satisfying.

Cohen: Well, you've done a lot. So at this point I should ask, How have you found your time here at Caltech?—but I think you've indicated that it's been good.

Morgan: I wouldn't have had it any other way, when I look back on it. When I first thought about coming to Caltech, I was both happy and intimidated, because I knew something of the awe in which a few American institutions are held—MIT, Caltech, and Harvard. Fortunately, I had built some self-confidence in my ability to function in an elite institution by being at Harvard and by having spent time at the University of Illinois, which is, in my view, also an elite institution. But nonetheless it was with trepidation. When you're younger, you're not quite sure what you're really up to, and you depend very, very much on the encouragement of others. Fortunately, I received the encouragement.

Cohen: Yes. I was just going to say that this is a hard place and everybody doesn't get it.

Morgan: I received the encouragement. And I've mentioned some of those people to you before: Sheldon Friedlander, Jack McKee especially, and, in the middle stages of my career, Norman Brooks. And in my time in the administration I had a lot of strong personal support from Murph Goldberger, who was the president whom I served, and Tom Everhart, whom I served more briefly. And one of the things I was going to say before is that it's sort of an academic cliché that administration is just not a worthwhile activity for a sensible person. But I don't regret any of the time I spent in administration, because the administration was essentially all directed toward student welfare, and that's just one of the things that I've always thought

important. So when I finished my term as vice president, I don't think anybody ever heard me say, or ever will hear me say, "Boy, I'm sorry I did that," because I was glad I did that. It was strenuous, but I learned how to do it. And I may be flattering myself, but I may have even gotten adequate at the job. But something told me that it was time to move back to professoring. And that was also a choice that I now addressed with some trepidation: "Can I do this now? Can I go back and be what I was at the beginning, a full-time professor?" And I'm happy that it worked out well. I've had the opportunity to work with some very good students in the ten years since I stepped aside. I don't think of it as either stepping up or stepping down; I think of it as stepping aside. [Laughter]

Cohen: But I see you even took up the EO, the executive officer, position in '93 to '96.

Morgan: That was to bridge.... I did that with some reluctance, but I knew it was probably necessary at the time. It was a matter of finding the right order for moving from Norman Brooks to my successor. So I've forgotten, but I think it was three years that I served.

Cohen: Three years, '93 to '96.

Morgan: Yes, and it wasn't onerous. That's a job that I've actually done three times, as you've probably [noticed]. [Laughter]

Cohen: Yes. I see you did it [from] '74 to '80. OK. That's a double term.

Morgan: And I had a very brief one-year fill-in, I think, for someone.

Cohen: Right.

Morgan: So I knew how to do it. I wanted to make the transition smooth. Now our executive officer is Professor [Michael R.] Hoffmann [Irvine Professor of Environmental Science]. If I had my druthers, I wouldn't have done it, because I would have liked to just keep going doing full

time the thing I enjoyed. Of course, all faculty members serve on committees, so I've continued to do that. In the last three years, I've been on the Freshman Admissions Committee. I've served on the Academic Freedom and Tenure Committee. Professorly life has a certain amount of service. And one of the things that I've always appreciated about Caltech is that you can be both an administrator for some of your time and a faculty member for most of your time, and it will work out all right if you give it all you've got.

Cohen: Well, certainly people do move from one to the other here. I mean, they don't bring in all that many outside people.

Morgan: I think Caltech has its own way of doing things and probably for good reasons. And I think your observation is accurate, but you'll notice that the presidents of Caltech are always from the outside. My guess is that we just know ourselves too well. [Laughter] And we always believe that certainly there's someone out there who will serve us better than any one of us could serve us. But I don't know the deep reason. I just know that even when [Robert A.] Millikan came, it's my understanding that he came in order to become the chairman of the council.

Cohen: Right. He was never called president.

Morgan: That was his job description.

Cohen: Yes.

Morgan: So in a sense, it's always been an outside person. What I remember about Millikan, just from reading, is that he probably eased in over a period of years by spending part of several previous years here.

Cohen: Right. And the people he brought, similarly.

Morgan: Yes.

Cohen: So how do you feel it's going to go now, with the new president [David Baltimore]?

Morgan: I don't know. Who said, "I only know what I read in the papers"?

Cohen: It sounds like Will Rogers, but I don't know.

Morgan: It might be. I'm not personally acquainted with David Baltimore. There's no question that he's a stellar scientist and has all the impulses and insight of a stellar scientist. He's finished almost two years at Caltech. What I read in the papers, to be serious—both our own papers and our own minutes of our meetings and so forth—is that he has hopes for making Caltech a more vital force in the Southern California community with respect to applications of technology. And that's an area that is beyond my immediate ken. I've had a few small meetings where I was one among five or six people, and he makes an excellent impression of a man who can articulate ideas and respond forcefully and with understanding to things that have been said. But beyond that, let's say that our paths have not crossed very often.

Cohen: OK. Well, you can wish him well. OK. Anything else? It's been a wonderful interview.

Morgan: I'm looking forward to a scholarly leave.

Cohen: When are you going to start that?

Morgan: I hope July 1st. I hope to receive a letter that will confirm that arrangement. And in the scholarly leave over the next two years, I hope to travel a little bit more widely than I've been able to. I'd like to see some waters, literally, in other parts of the world.

Cohen: Maybe you'll get to Israel.

Morgan: Yes. Well, Israel is on my agenda. The Southern Hemisphere is on my agenda. And I would like to make some progress on a smallish book—a lot smaller than the third edition of *Aquatic Chemistry*—about water chemistry in a wide variety of everyday applications. I would say that it's an idea that I've done some work on. People have many different questions that I encounter from time to time about what actually goes on with water. They range from a young person asking me the other week, "Where does water come from?" And I had to probe a little bit to find out what she meant by the question. She actually meant from the very, very beginning. And I didn't know the answer. And to tell you the truth, I still don't know the answer. It's sometime after the Big Bang. [Laughter] And in various epics and myths there are various accountings for how water came to be. Water is feminine.

Cohen: Water is life.

Morgan: Water is life, and so forth. But the earth is also a mother. The sun is probably a father or a brother. In Saint Francis of Assisi's *Canticle of the Sun* it says, "Thanks be to God for our sister water who is humble and precious and pure." So he gives the identification of water as feminine. In that *Canticle of the Sun,* he goes through a number of other parts of the earth. But those aren't the sort of questions I really have in mind for the book. The sort of questions that I have in mind for the book are the sort of questions that somebody might want to know, [like] how does a water-treatment plant really work, and what are the differences among different water-treatment plants. One of our former Caltech students asked me, "What would be a good way to think about the chemistry of cooking with respect to water?" So I've started a sort of catechetical exercise. I've sent out letters to a few friends saying, "Please send me from time to time what you think are questions about water that many people would be interested in knowing." I will have to prepare now very seriously, even beyond Stockholm, for a symposium in Washington, DC, in August of 2000, in my honor, where I imagine that all of my former students will be invited to come and present papers at the national meeting of the American Chemical Society. And of course I will have to prepare a paper. And I want it to be a good exit song. [Laughter] [Tape is turned off]

Epilogue: James Morgan's Account of Receiving the Stockholm Water Prize and the Clarke Prize

The Stockholm Water Prize, 1999

At 9 AM on March 16, 1999, I received a phone call from The Stockholm International Water Institute, or SIWI, telling me that the 1999 Stockholm Water Prize (the ninth) would be awarded jointly to my Swiss colleague, Werner Stumm, and to me for our work in aquatic chemistry, the chemistry of natural waters. There seemed to be many Swedes on the line, for I spoke to several persons, and I recall being really taken by surprise and pretty much at a loss for words (and I was still sleepy, not being an early person). I managed to ask if Werner already knew. Yes, they had just spoken to him by phone, and he was very pleased. Would I be able to stay up until midnight the following Monday, for the official announcement and press conference by phone on March 22, World Water Day? Yes indeed, I said. I was asked to maintain secrecy until then, except for sharing the news with my wife. I called Werner Stumm that morning. He was indeed pleased (and surprised) by the news. However, our happiness was tempered by the illness he had been suffering from for the previous five years. He told me that he would be unable to travel to Stockholm and said that I should prepare to speak for both of us.

At midnight, as March 22 arrived in California, I joined a conference call originating in Stockholm, at the City Hall (Stadthus), and was connected up with Werner Stumm's home, and (eventually) with a group of journalists to ask questions of us. It is still all a blur, but I heard the announcements in Swedish and English, I was asked to make a statement, which I did, having discussed the content with Werner during the days preceding. I then answered a number of questions about water in the world, water pollution, the chances of warfare over scarce water, and so on. I stayed up until 3 AM in Pasadena to be interviewed by Associated Press and the BBC. I remember little, except that it was hard to get to sleep after all that.

Werner and I spoke by phone and exchanged faxes over the next couple of weeks, settling on what I would present in August at the award ceremonies. Then, a shock in the news from Werner's wife on April 17 that Werner had passed away that evening. He was approaching

age 75. His scientific colleagues found it hard to believe that he was gone. He had retired (compulsorily) in 1992; but he had continued to publish papers—eighteen in all—over the next half-dozen years. We had published three books together and ten journal articles. It was now time for memorials.

In the second week of August my wife Jeanne and I traveled to Stockholm. We were joined by several close colleagues from Caltech and from other institutions. We were feted by the US ambassador to Sweden and saw a good deal of Stockholm's waters by boat.

I accepted the prize for Werner and on my own behalf and shook hands with the King of Sweden. We attended a grand banquet with some seven hundred guests after the awards, and I gave a number of talks, both technical and personal.

Jeanne and I then traveled to Zurich to convey Werner's prize to his widow Elisabeth. We stayed for a week, sharing memories of earlier times (I met Werner and Elisabeth in 1960 at Harvard, as a new PhD student there). I believe the time in Zurich helped us all.

The details of the events in Stockholm are pretty well summarized in the documents accompanying this brief account [*See*: James Morgan Historical File, Caltech Archives]. There are two "speeches," two lectures, and a summary in a SIWI report. It was a wonderful week. Being able to share it with Werner and his family would have made it complete.

The Athalie Richardson Irvine Clarke Prize for Water Science and Technology, 1999

On April 27 I received a call from Ronald Linsky, Executive Director of the National Water Research Institute (NWRI). "Congratulations," he began. I assumed that he was referring to the recently announced Stockholm Prize and so thanked him, then expressing my sense of loss that Werner Stumm had passed away little more than a week before. He said that he had just learned that, and then went on to say that his congratulations were not for the Stockholm Prize, but for the Clarke Prize, which I had just been chosen to receive for my work in water chemistry. I was to be the sixth recipient. The award would be made in Pasadena in late June. A second surprise in the span of six weeks!

I later recalled the good-natured remark of my Caltech colleague Sheldon Friedlander (now at UCLA) who had told me over thirty years before: "Remember, the idea is to live long

enough to receive the prizes." Good counsel, I thought. I was about to be 67, and my cup (of water) was running over. It was a sweet and bittersweet spring. I felt glad to learn of a second prize for a lifetime of research, but everything was tempered by the loss of my teacher, colleague and friend, Werner Stumm. It was a time to write *In Memoriam* pieces as well as prepare for award celebrations.

The Clarke Prize ceremony was held in Pasadena at the Ritz Carlton. The NWRI had adopted the practice of presenting the award in the recipient's own town. It was an evening affair in black tie, starting around six o'clock. We were asked to provide our list of invitees. It was a happy event, a good party. Several of our children and their families were able to be there. Many of my Caltech doctoral students from the past thirty years came. Colleagues from Caltech, Stanford, Berkeley, the University of Southern California, Texas A&M, and Michigan, among others attended. Three previous Clarke laureates joined in.

The presentation of the prize by Mrs. Joan Irvine Smith followed the garden reception. A gold medal was conferred (which I was later urged to have insured), along with a check for $50,000. I gave a talk, personal in character and recalling my life from the perspective of water and the roles my mentors played in what I was able to do. Many photos were taken to be shared happily with family and friends. An informal dinner followed. A great time was had by all. We were home and in bed by midnight.

The program for the ceremony accompanies this account [*See*: James Morgan Historical File, Caltech Archives]. I had been asked to provide my text several weeks in advance. What I had written included a number of quotations from spiritual and poetic sources, which were placed at the beginning as a kind of preface; I had called that part an "Invocation for Celebration." In the printing design someone decided to intersperse the quotes throughout the text. It worked out fine, even though it was not what I had first intended.

The Clarke Prize evening was personal, informal, and over rather quickly. Stockholm was six weeks ahead. That would be a very different experience. After all, there would be a king to meet.

James Morgan