ARNOLD O. BECKMAN
(1900-2004)

INTERVIEWED BY
MARY TERRALL

October 16–December 4, 1978

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Chemistry

Abstract

Interview in two sessions, 1978, with Arnold O. Beckman—alumnus, faculty member, and trustee of Caltech and founder of Beckman Instruments (now Beckman Coulter, Inc.)—begins with his recollections of his early interest in chemistry. He attends University High School at Illinois State Normal University. Brief stint in the Marine Corps near end of World War I. After the war, he continues his education at the University of Illinois in Urbana, where he studies with C. S. Marvel, Gerhard Dietrichson, and Richard Chace Tolman. B.S. (chemical engineering) 1922, M.S. (physical chemistry) 1923. Follows Tolman to Caltech; does his graduate work with Roscoe Dickinson. Recollections of Arthur Amos Noyes and the Chemistry Division. Leaves Caltech in 1924 before receiving his PhD, works for Walter Shewhart at Bell Laboratories on West Street in Manhattan. Noyes prompts him to return to his graduate studies; he does so in the fall of 1926; joins Caltech faculty after receiving PhD in 1928. His consultant work and development of the pH meter. Development and production of the Helipot (helical potentiometer) and the quartz (Model DU) spectrophotometer. Establishes National Technical Laboratories while still a member of the Caltech faculty; leaves Caltech in 1939 to become its president (name changed to

Beckman Instruments in 1950). Use of Helipots and spectrophotometers in World War II. In 1953, he returns to Caltech as a member of the Board of Trustees (chairman 1964-1974). Comments on Linus Pauling controversy; on changes in American work ethic prompting moving of plants overseas; on admission of women to Caltech. Founds Lincoln Club of Orange County, 1962. His interest in behavioral biology and creation of Caltech's Beckman Laboratories of Behavioral Biology. Recalls his involvement in air-pollution abatement in Los Angeles in the late 1940s and early 1950s and the work of Arie Haagen-Smit.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

Beckman, Arnold O. Interview by Mary Terrall. Pasadena, California, October 16–December 4, 1978. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Beckman_A

Contact information

Archives, California Institute of Technology
Mail Code 015A-74
Pasadena, CA 91125
Phone: (626) 395-2704 Fax: (626) 793-8756
Email: archives@caltech.edu

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH ARNOLD O. BECKMAN

BY MARY TERRALL

PASADENA, CALIFORNIA

Caltech Archives, 1981

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Interview with Arnold O. Beckman **by Mary Terrall**

Pasadena, California

Session 1 October 16, 1978

Session 2 December 4, 1978

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TERRALL: I know you were born in Cullom, Illinois. Is that a farming town?

BECKMAN: Yes. Right in the middle of a farming community, a little over a hundred miles south of Chicago.

TERRALL: And what was your father's occupation?

Fig. 1. Arnold Beckman (standing on step) with his siblings at the family home in Cullom, Illinois. Photo Caltech Archives.

BECKMAN: He was a blacksmith.

TERRALL: How long had he lived there? Was he from that area?

BECKMAN: He was born in Fairbury, Illinois, but he lived in Cullom virtually all of his life, until 1912, when we moved down to Normal, Illinois. That's when I entered high school.

TERRALL: I see. Did you have brothers and sisters?

BECKMAN: Yes, two brothers and one sister.

TERRALL: In Cullom, what were the schools like? Was it very isolated?

BECKMAN: Oh, we didn't think so. It had its own high school as well as grade schools. No, I thought it was a good school. My next older brother went to high school there, but then I went down to University High School at Normal, Illinois—this was part of the Illinois State Normal University [ISNU], as it was called at that time; it's Illinois State University now. University High was sort of a group on which the teachers practiced. It was a very good high school. The size was limited to, I think, 240 students maximum, and I got in by a scholarship.

TERRALL: How did you happen to move there? Was it specifically in order to send you to that school?

BECKMAN: Yes.

TERRALL: When did you first become interested in science? Was that before high school?

BECKMAN: Oh, yes. My interest was aroused by my finding in the attic an old textbook on chemistry—[Joel Dorman] Steele's *Fourteen Weeks in Chemistry*, printed in 1867. It was a textbook used by my aunt. I don't know where she went to school, but anyway this textbook was up in the attic. About half the book was instructions for carrying out simple experiments, and that intrigued me, and in this little town I tried to carry out these experiments. My father was interested, and he built me a little shed—we called it a shop—out there, about eight by ten feet, in which I fixed up a bench and tried to carry out the simple experiments, getting chemicals from the local drugstore.

TERRALL: What kind of experiments were they?

BECKMAN: Oh, electroplating with copper sulfate, and we'd use vinegar and baking soda—sodium bicarbonate—lye, ordinary things of that sort.

TERRALL: Was this really *your* interest? Were your friends interested in it, your brothers?

BECKMAN: No.

TERRALL: It was really just you.

BECKMAN: Just my own interest, yes.

TERRALL: So you spent a lot of time by yourself doing it?

BECKMAN: That's right.

TERRALL: You said that your father was interested; he built you the shed.

BECKMAN: That was for my tenth birthday. He built me the shed and bought me a set of tools. Of course, he thought maybe I'd be blacksmith or a carpenter.

TERRALL: So that really encouraged you?

BECKMAN: That encouraged me, yes, and the shed also. We used that as a local clubhouse. We had a Cub Scout group and we used the shed as a meeting room. You see, this is a small town, so there were only eight or ten of us involved—boys, that is.

TERRALL: Did the other boys follow what you were doing at all, or were they interested?

BECKMAN: They were not interested in chemistry, no.

TERRALL: So you moved, then, to go to high school. What kind of chemistry classes did you have in high school?

BECKMAN: Well, we had just the general high school chemistry class, but at that time I knew I was going to be a chemist. I was so sure of it that I persuaded the high school people to let me

change from the standard curriculum, which had four years of Latin. I said, "I want to substitute chemistry for Latin." So they allowed me to take the high school chemistry in the sophomore year instead of in the junior or senior year, where it normally came. And on the basis of that, then they allowed me to take university chemistry courses while I was still in high school. So I had two and a half years of university chemistry by the time I graduated from high school.

TERRALL: The university—Illinois State Normal University—was right there?

BECKMAN: We were part of the university. We were the students used by university students for teaching practice. The [high school] buildings were right on the university grounds. Fortunately, the university had a very sympathetic chemistry professor—Professor Howard W. Adams. He was the one who encouraged me. He allowed me to enroll as a high school student in the university classes. He encouraged me in many ways. I can give you one example. ISNU was not a research institution, it was a university for training teachers. Professor Adams persuaded me to enroll jointly with him in a correspondence course in metallography—this is where you polish metal specimens, etch them, and then photograph them. Well, they had no facilities there for photographing, so on weekends we would drive over to the University of Illinois, about fifty miles away, and use their photographic equipment. He did this, took his time, to drive over there on innumerable weekends, just to keep up my interest in chemistry. So I've always been indebted to him for the encouragement I got.

TERRALL: So you were satisfied. In high school you didn't feel that you were stuck, since you had a chance to do university work.

BECKMAN: Oh no, not at all. Of course I was busy, too. I was working my way through school.

TERRALL: Yes. I was going to ask you about what kind of jobs you had.

BECKMAN: I played the piano in the local moviehouse and in local dance orchestras—we called them orchestras in those days, not bands. Particularly in my junior and senior years, I was playing every night at the movies.

TERRALL: They had a piano, not an organ?

BECKMAN: I played both. This was the Irvin Theatre in Bloomington. It had a small orchestra and a pipe-organ, and I'd spell the orchestra by playing the organ, so I'd have to switch from piano to organ and back.

TERRALL: Did the orchestra play along with the movies as well?

BECKMAN: Oh, yes. This was in the days before talkies, you know. Particularly for the major pictures, we had music scores that were sent out with the film so that everything was coordinated and properly timed. When they had special scores, then the director would have to arrange a schedule of music. Of course, then I played a little in a smaller theatre, where I'd have to keep one eye on the screen and play appropriate music—mood music, hurry music, different music like that.

TERRALL: Have you kept up with that over the years?

BECKMAN: Not really, no. I play a little for my own enjoyment, but not much.

TERRALL: So that was the job you had through high school?

BECKMAN: All through high school, yes.

TERRALL: Well, let's see, then World War I came along, right?

BECKMAN: Yes. Then I joined the marines, in the summer of '18. I had one brother who was in the army and one who was in the navy, so to keep neutral I joined the marines.

TERRALL: Is that how you chose?

BECKMAN: That was one factor. But also, the marines were a glamorous institution. It was purely a voluntary unit at that time, you see. There were no draft people going into the Marine Corps.

TERRALL: Did you go directly into the marines from high school?

BECKMAN: No, there was a short interval. I left high school about two months before graduation day. I was valedictorian of the class, and so was allowed to leave early and go to work in a steel plant—this was a war-related job. So I went over to Pekin, Illinois, and worked at the Keystone Steel & Wire Company as a chemist. I had that job for about three or four months, then I left that to go into the marines.

TERRALL: I see. But they were doing war work at that plant?

BECKMAN: That was war work, yes.

TERRALL: What were they producing there?

BECKMAN: Wire. Barbed wire.

TERRALL: Were you involved in the research and development end of it?

BECKMAN: No, I was in routine chemical analysis. They had three large open-hearth furnaces—a hundred tons of steel in each furnace—and they needed to know when to pour the molten steel. When was it purified enough? So the chemists would grab a sample of the molten metal, analyze it just as quickly as possible, in a matter of minutes, and determine the percentage of carbon, sulfur, silicon, and manganese—sometimes phosphorous, too.

TERRALL: Did you enjoy that kind of work?

BECKMAN: I enjoyed it very much. I used to set records for myself.

TERRALL: Speed records, you mean?

BECKMAN: That's right, speed records—the shortest possible time. Now, that was in the days before eight-hour shifts. We worked alternate ten- and fourteen-hour shifts—two weeks on a ten-, then switch and do two weeks on a fourteen-hour shift.

TERRALL: Did they work nights also?

BECKMAN: Oh, yes. Yes, we had to work right around the clock, twenty-four hours.

TERRALL: So when you joined the marines, you were stationed in Brooklyn, is that right?

BECKMAN: No, I went down first of all to Parris Island, South Carolina, for the boot training, for about three months. Then one night we were called out. Late at night, some people were needed to go aboard a battlewagon; that's what we called a battleship. They wanted people over *5*'10", so I was picked out to go. We were put on the train to go to the Brooklyn Navy Yard. Well, the train was late getting there, and they'd filled the ship's needs with marines from the barracks in the Brooklyn Navy Yard. We were put into the barracks to take the bunks of those who had been transferred to the battleship. In fact, there was a big element of luck there. I've often thought about how little matters often determine the course of a person's life. In this case, I got the last bunk on the second deck. If I'd been one bunk later, and gone to the third deck, my life would have been completely different, because the people who were assigned to the third deck were shipped over to Vladivostok not long after that. As it was, I stayed there in the Brooklyn Navy Yard. I was there on Thanksgiving Day in 1918. Well, we had our Thanksgiving dinner in the barracks, and then we were ordered to eat another dinner. It turned out that a Red Cross chapter located in the Brooklyn YMCA was having a dinner for wounded marines coming over from Chateau-Thierry and Belleau Wood [in France], and there weren't enough marines to fill the places. So we were ordered to go there and eat another dinner right on top of our own dinner! Well, fortunately, I was only eighteen years old, and I had the appetite of a teenager, so I could do that. Well, the important thing about the dinner is that one of the girls helping out at the

tables is now Mrs. Beckman. If it hadn't been for this unusual double-dinner experience, I wouldn't have met her.

I got out of the Marine Corps early in 1919—the end of January, 1919. I got out too late to enter a university, so I spent a delightful summer out near Yellowstone Park, in Ashton, Idaho. I played the piano in the local movie theatre, all alone, without a single sheet of music; everything was memory or improvisation. I came back at their invitation a second summer, and I brought some music that time. But I mention that because those two summers were the most delightful of my life. Let me explain how I happened to get to Idaho. What happened was, I got out of the marines too late to enter a university, so a high school friend, Wakefield Boyer, and I decided to see the West. We just hitchhiked our way—rode freight trains and got jobs where we could and when we needed to. Somehow we got separated in Laramie, Wyoming. He went on to San Francisco, whereas I went to Ashton, Idaho, an entrance to Yellowstone Park. Ashton is the last division point on the railroad before you get to Yellowstone. I stopped off there to get a job and inquire about Yellowstone Park. I found a little movie theatre there and asked if they needed somebody to help. Well, they had a young piano player who wasn't very good; he could be described as having difficulty in playing "Come to Jesus" in the key of C. [Laughter] So I played the show. After it was over, the manager insisted I stay on. He took me home with him, put me up in his guest room, and persuaded me to spend the summer there.

TERRALL: Did you get a chance to see Yellowstone Park?

BECKMAN: Oh, yes. I went through Yellowstone Park and took a lot of pictures. It turned out that the projectionist at the theatre, Jerry Manning, also was the town photographer. I had nothing to do during the day—the movie ran only at night. So during the day, I'd help him out, developing the Kodak film that people would send in. And the result of that was I got a lot of experience; I knew what I wanted to see. When I went out with my own camera, I took a lot of good pictures and got to see Yellowstone. Jerry's brother, Togo Manning, was an old-time Pony Express rider and the two of them lived just for the outdoors—hunting and fishing. Every Saturday night, after the movie was over—there was no movie on Sunday—they would get in their old cut-down Model T Ford, the Horsefly we used to call it, that had a flat box on the back containing cooking utensils, fishing gear, and things like that, and they carried a tent on top.

They allowed me to sit up on top of this and go fishing with them. We'd drive out for two or three hours, get up into the mountains where we wanted to go, and pitch camp. Then up early the next morning, Sunday morning, and fish. I'd never caught a trout before in my life. Togo was a very kindly guy and took a liking to me for some reason. He used to regale me with stories about his early days in the West. He gave me a fishing rod—a very good one. I still have it, by the way—a split bamboo Colorado Special fly rod. It needed renovating, so he showed me how to strip off the old varnish, rewrap the guides, and put new varnish on. He even made me a pair of fishing shoes—we waded right into the stream, so he made me a pair of shoes with hobnails on them so that I wouldn't slip on the rocks. So I learned a little bit about trout fishing. Togo was a good cook, too—cooked trout and potatoes, even baked biscuits out there over an open fire. Well, that was a great way to spend a summer.

TERRALL: That must have been wonderful in those days.

Beckman: There was another man working for Togo—that was Hacksaw Tom. He was a prizefighter; he used to be a sparring partner for Battling Nelson—these names may not mean anything to you, but Hacksaw was a magnificent specimen of a man. He had biceps about this big! He also liked to hunt and fish, and he did a lot of his hunting just with a bow and arrow. He sometimes was poaching, and a gun would make a noise, you see. [Laughter] He didn't want to be found. Well, Hacksaw also worked in Togo's leather shop. Togo's main business was repairing saddles and harness and also making dog harness. One of the big events there was the annual dog race held in Ashton, Idaho.

TERRALL: Dogsledding?

BECKMAN: Dogsledding, yes. They had a big race, about a two-and-a-half or three-day affair, and Togo made most of the harnesses that were used. At the end of the first summer, Hacksaw wanted me not to go back to school; he wanted me to go with him to explore cliff dwellings down in Arizona. He offered to grubstake me. His plan was that instead of going up from below, we'd go down from the top, lowering ourselves with rope ladders. Well, I decided I should go back to school. I'd already lost a year, so I thought I'd better get back and start my

college education. Hacksaw went to Arizona and almost died. He ran out of water, but he was found, fortunately, by a geological survey party.

TERRALL: He went by himself?

BECKMAN: He went by himself, yes. Oh, he was that sort of a guy—fiercely independent, a magnificent specimen of a man.

TERRALL: So you had actually only been in the marines for just over a year.

BECKMAN: No, not even a year.

TERRALL: Because the war ended, is that right?

BECKMAN: That's right, the war ended in November of '18.

TERRALL: So you barely missed your only chance to get shipped overseas, then?

BECKMAN: That's right. That was the only chance.

TERRALL: How did you feel about that?

BECKMAN: Oh, naturally disappointed, because everybody was just gung-ho to get into action. That's the reason why we were glad to get called out. There were many rumors that we were going to be stuck in some isolated area, so when we got called to go into active duty we thought it was great. So, as it was, I was stuck in the Brooklyn Navy Yard for a while.

TERRALL: But at the time you wished that you had been able to go?

BECKMAN: I was sorry I wasn't able to get on the battleship, because I thought there was a chance of getting into action. But then I missed that.

TERRALL: You were planning to go back to college, though?

BECKMAN: Oh, yes. I still had that in mind.

TERRALL: Did you choose the University of Illinois because it was near your home?

BECKMAN: That was part of it. It was near home. And also because John Felmley—the son of David Felmley, the president of Illinois State Normal University—went to the University of Illinois. He talked to me and persuaded me to go to Illinois. There really wasn't too much argument. That was the university down there, and I just naturally went there. I didn't weigh very seriously at all the possibility of going to other schools.

TERRALL: I see. But you didn't live at home?

BECKMAN: Well, not at Urbana, no.

TERRALL: That's not close enough to where your family was?

BECKMAN: No. I lived in a fraternity house. I joined Delta Upsilon fraternity, and I lived in a fraternity house from then on.

TERRALL: What kind of jobs did you do during college to support yourself?

BECKMAN: Well, I played some in dance bands, but not much. I didn't play in the movies. It didn't cost as much to go to school in those days. I had a military scholarship, so I had no tuition to pay; of course, the tuition wasn't much anyway at the University of Illinois. And I'd saved enough money before, so I scraped along. As I recall, we paid about $50 a month for room and board.

TERRALL: What kind of chemistry courses did you take there?

BECKMAN: Well, as a result of having two and a half years of university chemistry in high school, I was permitted to skip freshman chemistry and start directly in analytical chemistry—the second year of chemistry—and also to take some organic chemistry. The professor of organic chemistry there, C. S. [Carl Shipp] Marvel—Speed Marvel—also came from Bloomington, Illinois.

TERRALL: You'd known him?

BECKMAN: Well, slightly. He went to Illinois Wesleyan University there. I started to do some research with him, for I wanted to become an organic chemist. So I started synthesizing some di-alkyl mercury compounds. Well, it turned out that these compounds are quite volatile. They're crystalline, but they will melt at room temperature, so I had to work with an ice bath. Well, even so, I didn't realize that the vapor pressure was so high and I absorbed a lot of mercury. Just before Christmas that year, I came down with mercury poisoning, so I was sent home. Fortunately, because of the Christmas vacation, I didn't lose too much time. I was out for about a month. Well, when I came back, the mercury poison symptoms reappeared, so I decided I had to change out of organic chemistry and switched to physical chemistry.

TERRALL: And when you stopped working with the mercury, the symptoms went away?

BECKMAN: Yes, they went away.

TERRALL: So up to that point, you had planned to be an organic chemist?

BECKMAN: Yes.

TERRALL: Now, were you thinking in terms of an academic career or an industrial career?

BECKMAN: Oh, I hadn't thought too much about it—probably industrial more than academic at that time. Because I was interested in synthetic organic chemistry, and I could visualize myself

making dyes and pharmaceuticals and things like that.

TERRALL: I see. But when you switched to physical chemistry, did you find someone else to work with?

BECKMAN: I was laboratory assistant for Professor Gerhard Dietrichson, who taught electrochemistry. Professor [Worth Huff] Rodebush taught thermodynamics and atomic structure. And, particularly, Richard [Chace] Tolman was there.

TERRALL: Oh, he was there?

BECKMAN: R. C. Tolman, yes. He came out to Caltech about the time I graduated. I had applied for scholarships, and I had offers of scholarships at MIT and Chicago and some other schools. I decided to come out to Caltech because of Tolman, primarily. Also, I think the lure of the West played a big part.

TERRALL: Had you been to California before?

BECKMAN: No. Idaho was the farthest west I'd been.

TERRALL: Well, before we get into Caltech—in college did you have other academic interests besides chemistry? In other words, were you spending most of your time on chemistry or were there other things?

BECKMAN: Most of my time was spent on chemistry. I didn't engage in any athletics. I was active in extra things in chemistry, though. I was editor of a magazine we put out—a darn good magazine, by the way—called the *Illinois Chemist.* Thirty-two pages, I recall, put out quarterly. That activity played a role in my subsequent career, because the business manager was a student named Glen Joseph, for whom I made my first pH meter. We put out the *Illinois Chemist* and also had fun in various other activities—for example, Chemistry Open House days, chemical exhibits. We would try to play some tricks on people. I remember one time that one of the

professors, Dr. Beale, was working on some drugs for anesthesia. We displayed a tub of water with a plastic fish floating in it. We had a fishing pole and hook with which we'd catch the fish. Then we'd put a drop of Dr. Beale's anesthetic in the water and we couldn't catch the fish. Obviously the fish was anesthetized! What we had was a little bit of iron in the nose of the fish, and a little electromagnet concealed in the hook. With a concealed switch, we turned the magnet off and on. We had a lot of fun in chemistry in those days, and we sort of lived in the laboratory.

TERRALL: So you were working on independent projects as well as your courses?

BECKMAN: Oh, yes. Another thing we were doing at the University of Illinois was preparing organic chemicals for distribution by the Eastman Kodak Company. Roger Adams and Carl Marvel supervised that program. We made extra money that way; Eastman would pay us for the chemicals we produced. So that was another source of income I had—not a big source, but still it helped.

TERRALL: So you had always been planning to do graduate work in chemistry, or had you thought of going directly into a job after college?

BECKMAN: No, I planned to do graduate work. Looking back, I don't ever recall talking too seriously with anybody for advice on this. My father had never gone to college, but he wanted college educations for all of us children, so we did have that drive to continue in graduate school. There was some advantage in having an advanced degree, so I decided to go on. I received my master's at Illinois, too. I got my bachelor's in three years and a summer school, and then I stayed on another year and got the master's in '23.

TERRALL: Had either of your brothers gone on for advanced work?

BECKMAN: No. My next older brother, Fred, was a lawyer, and a darn good lawyer, but unfortunately he died from a brain tumor when he was only thirty-one years old. My other brother was a salesman, primarily; he didn't go on for advanced work.

TERRALL: So when Tolman came to Illinois, did you have a course with him?

BECKMAN: Oh, yes. He taught statistical mechanics and some physical chemistry back there.

TERRALL: And then he came out here. Did he talk you into coming to Caltech?

BECKMAN: Well, I talked with him, but I don't recall any arm-twisting by anybody—Tolman wasn't that sort of a fellow anyway. He was persuasive enough, without putting on pressure, that I decided to come out.

TERRALL: What did you know about the Chemistry Division at Caltech?

BECKMAN: Almost nothing, except I'd heard of A. A. [Arthur Amos] Noyes and Robert A. Millikan. The head of the chemistry department at that time at Illinois was W. A. Noyes—no relation to A. A. Noyes. I had a classmate, a fraternity brother, named Bill Hincke. He and I were very close, and we decided we'd come out to California together. Maybe we had a synergistic effect on each other. We drove out in the fall of '23 in Bill Hincke's Model T Ford.

TERRALL: How was that?

BECKMAN: Oh, that was a great trip! Back in '23, there weren't any paved roads. We spent six weeks or more getting out here.

TERRALL: Did you go via the southern route?

BECKMAN: No, we went up north. Bill's folks lived in Pinckneyville, Illinois, in the southern part of Illinois, and we started from there and drove out up north through Wyoming. I remember going over Birdeye Pass in Wyoming. The bridge had been washed out at Thermopolis, so we had to go up over a terrific mountain pass in the Model T. The road was so steep that the only way we got up was one of us would…. Have you ever driven a Model T Ford? Well, it doesn't have a gearshift, just a foot pedal. With the pedal down, the car is in low gear, and when the

pedal is up, it's in high—only two speeds forward. One of us would drive and the other would follow with a stone to be placed under a rear wheel. We'd race the engine and jam the pedal down into low gear. The car would bounce ahead a few feet, then the other fellow would put the rock under the wheel. The engine would stall, of course, so we would crank it up—no self-starters then—and repeat the cycle. And this was the way we inched our way up over Birdeye Pass.

TERRALL: That's terrific! But the car made it?

BECKMAN: Oh, yes.

TERRALL: Did you have trouble with overheating?

BECKMAN: Oh, yes, yes. You wait until it cools down. We carried canvas water bags for extra water. Another problem, which we learned earlier, was that some of those hills were so steep that the gasoline would not run into the carburetor. The gasoline tank was under the front seat, so on a steep hill the carburetor was higher than the gas tank. That was something I learned years before, when I was out in Idaho. Togo and Jerry showed me how to make a pressure cap for the gas tank by inserting a bicycle valve in it. I had made one of those, so that's why we could go forward up these steep hills, instead of having to back up.

TERRALL: So then you came down through California?

BECKMAN: We went up into Washington and down the Columbia River, all the way around down through Oregon, and down through central California.

TERRALL: It must have been beautiful.

BECKMAN: It was. In those days, we'd often be isolated, see very few cars. It was great.

TERRALL: Did you camp?

BECKMAN: Oh, yes, we camped everywhere. Nowadays it's pretty hard to do, because it's all fenced in.

TERRALL: It's all private land, yes. Well, did you have a fellowship to come to Caltech?

BECKMAN: Yes.

TERRALL: And was your friend also coming to Caltech?

BECKMAN: Yes, he came here.

TERRALL: In chemistry?

BECKMAN: Yes.

TERRALL: What were your first impressions of the place?

BECKMAN: Oh, I was impressed by many things. Of course, I was only twenty-three at that time—well, I was getting along in years, that's right. But I was impressed by the climate. That happened to be a dry year, and every weekend we'd go out to the desert without worry about storms or rain. That was one thing that impressed me out here. Another was the freedom of discussion with professors—the relaxed atmosphere. There was a faculty club where the Athenaeum is now. A two-story farmhouse surrounded by orange trees was made into the faculty club. Graduate students and the faculty would go down and have lunch. The faculty and the graduates mixed freely, talked over everything from politics to auto racing to science—almost anything.

TERRALL: Was that very different from what it had been at Illinois?

BECKMAN: Oh, yes. Illinois was a large university, and they didn't have anything like that there.

TERRALL: Do you think it was mainly a question of Caltech's small size?

BECKMAN: Well, probably two things: One was the question of size; another was the question of my place in the educational cycle. At Illinois I was just a student, and I stood in awe of professors. When you get to be a graduate student, you get to know them better and better and lose some of the awe that you had when you were just a freshman or sophomore.

TERRALL: Well, you were working as a teaching fellow.

BECKMAN: As a teaching assistant, yes. Tuition was a $180 a year, as I recall.

TERRALL: So you had a fellowship for that money?

BECKMAN: Yes.

TERRALL: Did you have courses with Tolman, again, when you got out here?

BECKMAN: Yes, some courses in statistical mechanics. I did research with Roscoe Dickinson, who worked with Tolman. I worked in photochemistry, on the photochemical decomposition of hydrogen, azide, and hydrazine, some things like that. At that time, of course, everybody took a lot of physical chemistry. A lot of it had to do with statistical mechanics and thermodynamics. That was Tolman's primary interest when he came out. Later on, atomic structure and molecular structure began to come in.

TERRALL: So when you got here, you had a pretty good idea that you wanted to work in photochemistry, or did that develop after you got here?

BECKMAN: That developed after I got here, yes. I came out here without anything too specific in mind; I just wanted to go on for graduate work and get a graduate degree, so the photochemistry was more or less a happenstance. That was what Dickinson was interested in at that time.

Quantum theory was just beginning to be applied to chemical reactions, and it sounded like an interesting thing. Also, I've always liked to do experimental work—I like to blow glass and do things like that—and this involved a lot of such apparatus; a vacuum bench, we called it in those days. So it was a type of work that I enjoyed. I liked to make the different things. I made a vibrating-reed vacuum gauge, among other things, for example. In fact, I enjoyed glassblowing. The result was that I gave a course in glassblowing here. People used to come in and ask me to help do this or this; finally, in self-protection, I gave a course for several years. We fitted up the lecture table in Gates lecture hall. We had some transite boards we'd lay on top, and then we'd lay pipes for gas, oxygen, and air on top of that. Students would sit on high stools on either side. This transite was wide enough so that there was room for your knees to get under it. We'd have about twelve at a time, and I taught them glassblowing. To this day, some of the students still come up and say, "You know, I took your glassblowing course."

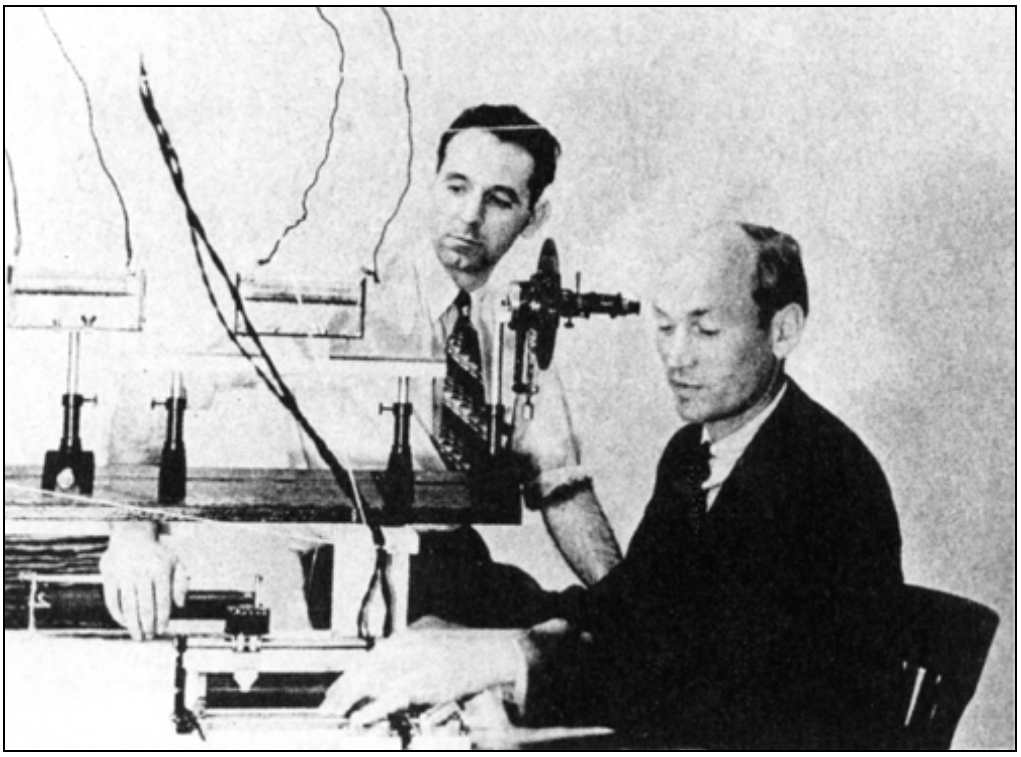

Fig. 2. Arnold Beckman (right) and James McCullough in a Caltech chemistry laboratory, 1934. Photo Caltech Archives.

TERRALL: How did you happen to be so good at that?

BECKMAN: I took a glassblowing course at the University of Illinois, so I did know a little about

it. I always loved to work with my hands, so glassblowing is just like machine tool work in that respect.

ARNOLD O. BECKMAN

SESSION 2

DECEMBER 4, 1978

Begin Tape 1, Side 2

TERRALL: I'd like to ask you about A. A. Noyes. How do you think his educational philosophy influenced the curriculum at Caltech?

BECKMAN: Dr. Noyes, I think, played a key role in establishing the educational policies at Caltech. I think he was much more interested in that than Millikan or [George Ellery] Hale were. Several things: First of all, he insisted on, let's say, a quantitative aspect. For example, he brought physical chemistry into general chemistry and even into organic chemistry, because he wanted a mathematically precise way of looking at things. That was one thing. Another thing was that he, along with Professor [William Bennett] Munro, was the one who insisted that twenty-five percent of the curriculum had to be devoted to humanities.

TERRALL: Now, how do you think that worked, from your contact with undergraduates? Do you think that they were thinking about the humanities, or was it just a chore for them to take that much?

BECKMAN: Depends on the individual. On the average, I think, some of it was considered to be a chore. Other individuals really relished it, and again, it depends on what professors they had. Now, we had Clinton Judy, for example. Well, anybody that took Clinton Judy's course was enamored with the subject matter, just because he was that sort of a professor—he made it interesting. For some of the other professors, teaching was more of a routine, mundane thing, and they didn't inspire the students as well.

TERRALL: But this was very much Noyes's idea of what the engineer's education should be.

BECKMAN: So far as I know, yes.

TERRALL: What about the chemistry curriculum in particular? Did it reflect his own interests?

BECKMAN: Oh, yes. For example, the freshman chemistry course in most schools was just a general chemistry course, more or less a repetition of what high school chemistry was. But here at Caltech, Dr. Noyes changed that, particularly in the laboratory work. We prepared a special laboratory instruction book which brought physical chemistry measurements into the freshman year—the sort of thing that was carried on in maybe the junior year in some other colleges, particularly with respect to hydrogen-ion concentration, law of mass action, and things of that sort. These were precise, quantitative experiments instead of the usual simple inorganic preparations that most general chemistry laboratory courses have.

TERRALL: I see. You were teaching in that class?

BECKMAN: Yes.

TERRALL: So you were involved in making up the lab books?

BECKMAN: That's right. In making up the notes and the book, and revising it.

TERRALL: He also had a textbook that was widely used, didn't he?

BECKMAN: Yes, he had two. One was Noyes and [Miles S.] Sherrill—that's probably the one you're thinking of—on physical chemistry. That was sort of a bible for the physical chemists in that era. Then he had another qualitative analysis book that was quite widely used.

TERRALL: What about the research directions that the division took? Was he responsible for that, or was that more the individual professors?

BECKMAN: I think you'd have to say that was more the individual professors. For example, in physical chemistry, you did have Stuart Bates there, but then Tolman came in and Tolman set off

on his own path in the field of statistical mechanics and related subjects, and Dickinson was working along with him. Then [Linus] Pauling and Dickinson went into the X-ray crystal structure field; [Howard] Lucas in organic chemistry. No, I think this was up to the individual professors. They were encouraged to follow their own individual interests.

TERRALL: What about the idea of having a division council, instead of calling one person the chairman? I understand that Noyes was very much in favor of that. Do you remember how that worked?

BECKMAN: No, I don't recall much about that.

TERRALL: In 1924, you took time off and went to work at Bell Labs, is that correct?

BECKMAN: Yes.

TERRALL: What brought that about? You didn't want to finish your degree immediately?

BECKMAN: No, that's a long story. You see, during the war—maybe I mentioned earlier how I met Mrs. Beckman. Well, she lived back in Brooklyn, and I thought it would be a good idea to take a trip back there and see her. So I sailed on a boat, through the Panama Canal, back to New York, and I thought I'd spend a short time with her and then come back. Well, by sheer chance, there was another Caltech student, Todd Nies, who went to work for the Bell Telephone Laboratories. It was called the Western Electric Company—down on West Street, in those days. He said, "Why don't you stay and get a job back here?" Well, I was low on funds, and in fact, I did have a job offered to me by Standard Oil of New Jersey. Well, Todd said, "You don't want to go way out there in Jersey, in the mudflats. Why don't you take a job down here at Western Electric?" So he introduced me—this is the middle of the afternoon—to a Dr. Walter A. Shewhart in the Bell Labs. Shewhart was just starting up a new field of research, the application of probability theory to inspection. You see, in the Bell Telephone system there was a tremendous inspection problem. Innumerable inspections of soldered joints, relay contacts, and fuses, et cetera; and on some things—take fuses, for example—you cannot have a 100 percent

inspection, because you destroy the fuse in testing it, and so you have to bring in probability theory: How accurately can one rely on test results of a 50-percent sample or a 5-percent sample? Well, we worked, theoretically and experimentally, on experiments to apply probability theory to testing. This was an interesting thing; although I was not a mathematician, I found that it appealed to me. I was Shewhart's first technical employee. I stayed there for two years. In the meantime, I got married after one year. Then, in '26, Dr. Noyes came to New York. I saw him and he said, "Don't you want to come back and finish your degree?" And I said yes. So that's when I decided to come back.

TERRALL: Now, had you been planning to come back?

BECKMAN: Oh, yes and no. I had my master's degree and one year beyond. Well, I think anybody would say, "Gee, I'd like to tie it up and complete the PhD work." I hadn't made a major issue of it; but when I talked with Dr. Noyes and he offered me the job to come back here again, it didn't take much persuasion to accept.

Fig. 3. Mabel Beckman. Photo by Francis E. Parker, Pasadena/Caltech Archives.

TERRALL: How did your wife feel about moving away from the East?

BECKMAN: Well, she wasn't very happy about it. Her mother was still living there, and they were very close, and she'd never been very far away from her mother. But she took it in good spirits. We drove across in a Model T Ford. That was a rather harrowing experience for her; she'd never been camping or driving out in the open country. Our Model T coupe was just loaded down. Our friends had a going-away party for us the night before, and among other things we were presented with a portable phonograph. Well, we had to tie that portable

phonograph onto our overloaded Model T! We had a tent and we had a steel cot, so Mrs. Beckman wouldn't have to sleep on the ground. Our first day out, we ran into rain, so we had to pitch the tent in the rain. But Mabel was a good sport; she never complained at all. She didn't drive; I had to do the driving. Finally when we were out in the middle of Wyoming, not a car in sight, she said, "Maybe I could drive here." So I let her drive, and I dozed off. After a while I woke up and said, "Hey, we're going in the wrong direction." She'd gone through a little town and got the direction mixed up and was heading right back home. [Laughter] So to this day, I kid her about that.

TERRALL: Was that where she first learned to drive?

BECKMAN: Yes.

TERRALL: So how long did the trip take you?

BECKMAN: Oh, it took about four weeks, as I recall. Because we weren't rushing. We stopped off in Illinois to see my folks and friends there; we stopped in Idaho to see Togo and Jerry, Cant-hook Pete, and Hacksaw Tom—my own characters up there.

TERRALL: Did your wife enjoy the trip, after she got into the spirit of it?

BECKMAN: Oh, yes. Yes, she enjoyed it.

TERRALL: How about when you got to California; was it hard for her to adjust to life out here?

BECKMAN: No, I don't think so. We found that people here were very friendly. I don't recall any problems she had. She got a little homesick. She went back to see her mother once. Then we drove back in '28 or '29. This time we had a Model A Ford.

TERRALL: Had you been working directly with Dr. Noyes before you took the job at Bell Labs?

BECKMAN: No, I'd been working with Roscoe Dickinson.

TERRALL: Oh, I see. Were you involved in the X-ray crystallography at all?

BECKMAN: No, no. Dickinson was working in photochemistry. We were studying the quantum yields in photochemical reactions.

TERRALL: And is that what you did when you got back out here as well?

BECKMAN: Yes.

TERRALL: So it wasn't related at all to what you had done at Bell?

BECKMAN: Oh, no. The Bell work was purely a mathematical study. I didn't even do any electronics back there. Just by being at Bell Labs, however, I picked up some knowledge of electronics by associating with electronic engineers, and I made a few home radios for my friends. So that was an important stage in my career. If I'd never gone to Bell Labs, I might not have developed any interest in electronics.

TERRALL: So that was your first exposure to that sort of thing. After you got your degree, you were offered a job here as an instructor, is that right?

BECKMAN: Yes.

TERRALL: Did you have other job possibilities? Did you like the idea of staying here?

BECKMAN: Yes, I liked the idea of staying here. I liked it out here, and Mrs. Beckman did, too. I've forgotten whether I had other offers. I don't think I ever applied for any other job, because I was asked to stay on, and I decided. to stay.

TERRALL: You mentioned that you taught the introductory chemistry course, and we talked

about undergraduates a little bit. You were also teaching engineering chemistry, weren't you? How did that fit into the undergraduate curriculum?

BECKMAN: This was Chemistry for Engineers, a special course, started initially by Dr. [William] Lacey, and I came in and took it over from him. It's a little different from what we ordinarily call chemical engineering—that's for chemists, chemical engineers, and you go a little more deeply into the subject. Chemistry for Engineers was sort of an overview of chemistry as it might apply to engineering problems, involving such things as corrosion and things like that.

TERRALL: Was this something that Noyes was interested in?

BECKMAN: Not particularly, no. We never had a chemical engineering department here—we had chemistry and applied chemistry, so this was in the applied chemistry part.

TERRALL: I see. But it was something that was really specifically for people who are going to go into engineering rather than chemistry.

BECKMAN: Yes.

TERRALL: I see. We haven't talked about Millikan at all in this time period. How much contact did you have with him?

BECKMAN: Oh, not a great deal. I took a course, maybe two courses of his, graduate courses. That was during my student days. Afterwards I saw him some, because one of the things that I was asked to do was to take care of outsiders that came in and wanted help on chemical matters of one kind or another. They'd sometimes come to Millikan, and Millikan would refer them to me. That's how I got referred to the National Postal Meter Company. Have I mentioned that?

TERRALL: No, I don't think we've talked about that.

BECKMAN: Oh, that was a precursor of my starting up in business. We also used to have

prospectors who would come in. In this period of the early thirties, there was a lot of bunkum on colloidal gold. Some prospectors would claim they'd found out in the desert a deposit of colloidal gold in a form that could not be measured by the ordinary fire assay techniques used for assaying gold ore, but they were sure it was there in colloidal form. Most were just rip-off artists that would come in. Several of them were turned over to me and I had to debunk their claims. They'd bring in ultraviolet light or X rays or whatnot, you know, and try to mystify the uninitiated. It was just the rip-off of that day.

But then other, legitimate people came in for help. One of particular interest was a company known as the National Postal Meter Company. They were one of the first ones in the metered-mail business, and they had problems with their ink clogging up the machines. The president—I. H. Lyons, "Buzz" Lyons—came to Millikan and wanted somebody to help. So Millikan asked me to help. I did, and I solved their ink problems. At the end of 1934, they incorporated a company—National Inking Appliance Company—to make these inks. I was given the job of making inks for them.

I was doing some other outside work, too. One client was another Caltech alumnus, Howard Lewis—he invented the hard-hat helmet and also a dust respirator, and he had to measure the efficiency of dust collection. So I set up some test equipment to measure how efficient different configurations of the mask were for dusts of different particle sizes. I worked on several other minor things like that.

Well, back at Illinois, a classmate was Glen Joseph. He and I had put out the great publication called the *Illinois Chemist*—this was a good publication, by the way—four times a year we put it out; it had articles by professors, and we sold enough advertising to make it a success. He was business manager and I was the editor. Well, our paths separated when we left Illinois; I came out here and he went up to Wisconsin, got his PhD, and came out and worked for the California Fruit Growers Exchange. Along in the end of '34, he came into my office one day and said he was having a problem. He had to measure the acidity of lemon juice that had been treated with sulfur dioxide. He was making by-products from lemon juice—pectin, citric acid, things like that. He couldn't use a hydrogen electrode or quinhydrone electrode, and he couldn't use colorimetric indicators, because the sulfur dioxide would react. So he had to use a glass electrode. And at that time the only glass electrode available was one made by Leeds & Northrup, and that used a high-sensitivity galvanometer. Well, because of the poor electrical

sensitivity of the galvanometer, the glass electrode had to be made so large in diameter that it was very fragile. The glass electrodes were always breaking, and if it wasn't that, the galvanometer itself would break. So, based on my experience at the Bell Labs, I said, "Use a rugged vacuum-tube voltmeter. The grid impedance is so high that you can measure voltages with much smaller currents than you have to have for the galvanometer. That would mean that you could have a much more rugged glass electrode. You could make it smaller with a thicker wall." So I built him an instrument in late '34, maybe early '35. He came back in two or three months and wanted to know if I could build him another one—others in the laboratory were using the first one and he wanted to have one for his own use. So I did build him another. Then I thought, "Gee, if he could use two of these in that little laboratory he has, maybe there's a market for them." In September of 1935, the American Chemical Society was meeting in San Francisco, and I went up there. In the meantime, we'd developed the acidometer into a nice-looking thing in a walnut box.

TERRALL: Now, who was working on this with you? Were you doing this by yourself?

BECKMAN: I was doing it pretty much by myself, except that I had a couple of students working part-time—Henry Frackers and Bob Barton. Bob got his Ph.D. degree about that time, but he didn't have a job, so they worked part-time. This was in the depression years. I was doing this work out at 3600 East Colorado Street, where Fred Henson had an instrument shop. Fred Henson used to be the instrument maker for the chemistry department. One of my jobs was to look after the instrument shop, so I got to know Fred quite well. Then he left and set up his own independent company. He was a very fine man, and a good friend of mine. He allowed me to set up space in the back of the sheet-metal shed he had, where he stored his lumber and his Studebaker truck. We partitioned off nine feet across the back of the shed, and that was where we started.

TERRALL: I see. So that's where you actually did the production.

BECKMAN: That's right, yes, I did it there. None of the work was done on the campus. The reason for that was that at that time there was a very strong feeling that commercialism should

never enter into the thinking of anybody on the staff. Consulting agreements were almost unheard of, and anything that would smack of commercialism was frowned on. So I leaned over backwards to make sure that none of this was ever done on the campus, just to avoid possible criticism from that standpoint.

TERRALL: Was this policy of no commercialism explicit, or was it just something that you knew that no one did?

BECKMAN: Well, there may have been an explicit statement, but certainly it was well known that the King, as we called him—Dr. Noyes—would frown on this sort of thing. That, of course, all changed with World War II. And there's been a 180-degree reversal now, as you know, as a result of the consulting work that professors did during the war. Now, particularly in the science and engineering fields, most professors do consulting today. But that was a complete change from the pre-war status.

TERRALL: Well, were there other people who were also consulting off campus?

BECKMAN: Yes. Dr. Lacey and I—and this was specifically approved by Dr. Noyes. Because we were teaching chemical engineering and applied chemistry, he thought we should keep in touch with what was going on in the real world of chemistry. That was applied chemistry. That's why many outside requests for help came to us. Dr. Lacey didn't want to handle all these Rube Goldberg things that were coming in, and that's why they were shoveled over to me. But we did this work with Dr. Noyes's specific approval, and in fact, with his urging, because he thought we should stay in touch with the real world.

TERRALL: As long as you did it off campus.

BECKMAN: That's right. As long as it didn't interfere with the academic work. But he encouraged us to do this, so that our teaching would be better. He recognized that while for theoretical science you can live in the unreal world of academia, when you come to teaching practical engineering, you do have to know what goes on out in the real world.

TERRALL: You mentioned that one of your jobs was overseeing the instrument shop. So you already had an interest and ability in instrumentation.

BECKMAN: Oh, yes. Yes, I've always had that; I've always liked to work with my hands. My father was a blacksmith; I guess that's where I inherited that. At home I was always making things, always had my own little shop. It even extended to glassblowing. I taught a course in glassblowing here at Caltech—the only such course, I think, that's ever been taught here.

TERRALL: No one else knew how to do it?

BECKMAN: Well, we had a glassblower here, of course—Mr. Clancy, an excellent glassblower. But he was busy making glassware, particularly for the physicists. The chemists were sort of on the back burner—oh, it wasn't too bad, but nevertheless, physics came first for glassblowing and for the liquid air machine. So I taught this course in glassblowing. It's been interesting, over the years as I've talked to alumni, how many of them remember that course in glassblowing. It apparently had a good deal of appeal to them. This was in the early thirties, when glassware was just going through the transition from the lime-soda glass—soft glass, we called it—to the Pyrex. Lime-soda glass has a very high temperature coefficient of expansion, and annealing is extremely critical; if you don't anneal well, the stuff will crack. You can be very careless with Pyrex. So this was right at the transition period, which was good for training, because I insisted on using lime-soda glass all the way through for most of the instruction.

TERRALL: So then, once you could do it with soft glass you could transfer easily to Pyrex.

BECKMAN: That's right. You could be careless about annealing with Pyrex. But if you learned with Pyrex, you'd have difficulty in working with lime-soda glass.

TERRALL: Did many people make their own apparatus in the instrument shop, or was it pretty much made by this one man?

BECKMAN: It was a one-man operation. And of course we had a little student shop. Most chemical apparatus in those days was pretty simple. It didn't involve a great deal—such things as stirrers, for example. There were a lot of water baths for running reactions at constant temperature. See, this was a physical chemistry type of thing. The student shop really wasn't much of a shop; it didn't have a lathe. Students who were qualified could be permitted to work in the regular shop, under the close supervision of Fred Henson, and then later on Mr. Searle—E. H. Searle—was the instrument maker for many years.

TERRALL: What was your job in the instrument shop?

BECKMAN: Oh, just to see that orders were handled properly and supplies were obtained, so the shop was run in some sort of orderly fashion. I had to see that people didn't jam in there and harass the instrument maker and insist that they be given priority over somebody else—the usual administrative problems. We did have, also at that time, an instrument storeroom. We did have a few instruments, like polarimeters and saccharimeters and things like that, that were used only part of the time. There was a course in instrumental analysis, too, which I taught for a while. That originated with Dr. Lacey, and that I also inherited from him. Well, we had to keep some of those instruments under lock and key, and that was part of my responsibility.

TERRALL: You were also involved with the construction of Crellin Lab. How did you get into that?

BECKMAN: Well, it was just sort of natural; somebody had to work with the architect and the contractors.

TERRALL: Had you ever done anything along those lines before?

BECKMAN: Well, we built our house up in Altadena, and I worked with the architect on that.

TERRALL: Was that around that same time?

BECKMAN: Yes, we built that in ’33, and Crellin was built in ’36 or ’37.

TERRALL: So did you actually work on the plans?

BECKMAN: Oh, yes. That was my job, to look over the plans, first with the architect to make sure that the laboratories were properly located and designed, that they had the proper services coming to them, and all this and that. And then during the construction, I represented the university on matters that would come up during the course of construction. If they had to make alterations or some other problem arose, it was my job to work with the architect and the contractor.

TERRALL: Were you satisfied with the way it came out?

BECKMAN: Yes, I thought it came out very well. And there were many things on which I put a lot of my own thought. We had a very useful man by the name of Hal Weis. Well, Hal Weis shouldn’t be overlooked. He was a person who lived down at Balboa or Newport Beach. He was sort of an all-around mechanic and handyman. You know about Mary McSweeney? Oh, well, just backtracking a little bit: Dr. Noyes’s housekeeper was a buxom Irish woman, Mary McSweeney. She was his nurse when he was a youngster—maybe when he was born, I don’t know. Anyway, she stayed with him all her life. Dr. Noyes had the house right next to what’s now the Kerckhoff Marine Laboratory in Corona del Mar. Well, at that time it was a house; I remember well. It had gray granite block walls and a green tile roof, and all finished in redwood inside. Dr. Noyes used to take some of the graduate students down there on weekends to work on revisions of his book, and also to enjoy the water. Well, Mary McSweeney liked to go into the water, but she didn’t like to climb down the rocks. So Hal Weis and I built some very sturdy stairs that went down from the little pier down to the edge of the water, so Mary could go out there without losing her footing. I got acquainted with Hal then, and I was impressed. He was sort of an all-around man. And so I finally persuaded him to come up here to Caltech as a sort of a handyman in the chemistry department. And he really was a handyman; whether it was metal work, woodwork or painting or whatnot, he could handle it.

Fig. 4. Arthur A. Noyes, Caltech's "King" of chemistry, on tour in his Cadillac, "Mossie" (Demosthenes), with chemistry colleagues and his long-time housekeeper, Mary McSweeney. *Left to righ*t: Stuart Bates and his mother, Howard Lucas, Noyes, James Ellis, Margaret McSweeney, James Bell, and Mary McSweeney. Photo Caltech Archives.

Well, when it came to designing the laboratory furniture in Crellin, we wanted to make it adaptable, so that regardless of what future needs were, alterations could be made readily. So we designed the laboratory furniture that is in Crellin, and I think it worked out pretty well. We designed interchangeable lockers, for example—things like that. On the whole, I was quite pleased with the way it came out.

TERRALL: Around this time, the last few years that you were teaching, Pauling became chairman of the division. How did that change the Chemistry Division?

BECKMAN: Oh, there was no dramatic change, because Pauling was there in the preceding years, and the work that he was doing was getting recognition, so he just sort of carried on. Dr. Noyes was ill in the last year or two, so there was a gradual phasing out of his interest and ability to handle affairs, and Dr. Pauling just went on. So there wasn't, as I recall, any major change in policies at that time.

TERRALL: He must have been quite a different sort of an administrator, though.

BECKMAN: Yes, I would say he probably did not have the deep personal interest in the overall education aspect that Dr. Noyes had. Pauling was more interested in the hard scientific training. I may be doing an injustice to him, but as I look back I don't recall any time when Pauling stood up for humanities, for example. He was more or less a-hundred-percent science, whereas the King was also interested in humanities.

TERRALL: You talked a little bit about getting into your business. How did working on the apparatus compare with the academic work?

BECKMAN: You mean the instrument business? Yes, well I might go back and pick up the pieces. I left you in San Francisco, didn't I, in September of '35. Well, I took this pH meter up there to show to a couple of my professors from the University of Illinois—G. Frederick Smith was one of them; Hobart Willard from Michigan was there—to see whether or not there was a market. This thing would have to sell for $195, and in those days that was a terrific amount of money. It was competing with a ten-cent vial of litmus paper, you see. So I said, "Do you think there's a market for it?" They didn't know. They were intrigued, and they suggested that I go around and talk to apparatus dealers. So Mrs. Beckman and I got on the train and we went across the country. We stopped at Denver, at the Denver Fire Clay Company; and Chicago, at E. H. Sargent, the Central Scientific Company, and the Chicago Apparatus Company; in New York, Eimer & Amend; Philadelphia, Arthur H. Thomas Company; and in Pittsburgh the Fisher Scientific Company; to see what they thought. Well, the most optimistic estimate I got was from the Arthur H. Thomas Company—from Ed [J. Edward] Patterson, who thought we might sell as many as 600 over a ten-year period, to saturate the market. Well, this was a pin-money side issue for me, a chance to make a little extra income. As I say, it was during the depression, so that even a few dollars looked pretty good. I persuaded the National Postal Meter Company to switch from making just meter ink and go into making acidometers. I didn't want to put the Beckman name on the company as long as I was at Caltech, because, again, of a fear that creeping commercialism might be suspected. So we took the name National Technical

Laboratories, and we kept that name until after I had left Caltech. By 1939, the company had grown to the size that somebody had to run it full-time. I was running it just part-time, evenings and weekends. So I had to make a decision—and this was a major decision—whether to leave Caltech, give up the academic field, and go into business, or to stay there and get somebody else to run the business. Well, by this time, I was having much fun with the business, and furthermore I found that I would be keeping in touch with science, because the instruments were, of course, being used in scientific laboratories. I was exposed to all sorts of new applications in science, so I felt I would not be divorcing myself from science entirely. So I made the decision to leave and go into business.

TERRALL: So, the pH meter was the first thing you really produced yourself?

BECKMAN: Yes.

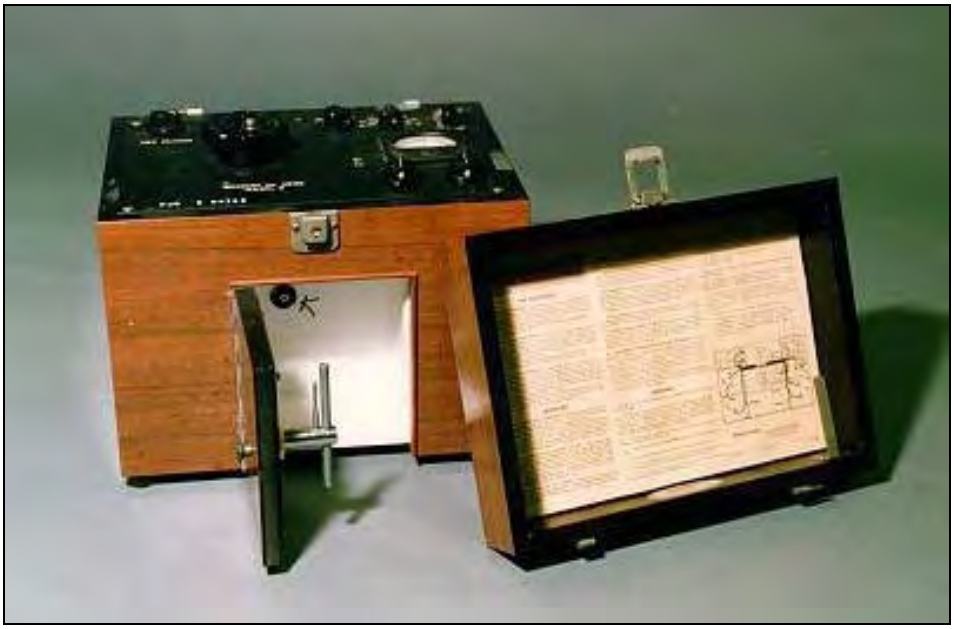

Fig. 5. Beckman Instruments' Model G pH meter, 1957. Photo Caltech Archives.

TERRALL: Prior to that, with the National Postal Meter Company, you hadn't actually been producing anything for them?

BECKMAN: We were making inks for them. But a few buckets full of ink will take care of an awful lot of meters, so that was not a major operation. Later the National Postal Meter Company

got into financial difficulties and I got them to split off the National Technical Laboratories from the Postal Meter Company. The Postal Meter Company finally was sold to some other company. But fortunately, as we were independent, we weren't carried along in that.

TERRALL: Oh, I see. So when you started going into production with the pH meters, where was your production space? Were you still in the same place?

BECKMAN: Well, we started the first ones in the back of the shed at Fred Henson's place at 3600 East Colorado. But then up the street, at 3330 East Colorado, there was a building owned by Ernest Swift. It had been a dry-cleaning shop. Well, in the depression, the dry cleaner went bankrupt, so the building was empty and I rented it from Ernest. I remember that the rent was $50 a month. Gee, our overhead shot way up, but that's where we started. We stayed there until 1940. By that time, I decided the growing business had to have its own quarters, and this also is when I left Caltech. We bought a site in South Pasadena—820 Mission Street in South Pasadena—and built our own building. That was the first time we had our own quarters; we were in rented quarters the three or four years prior to that.

TERRALL: But by the time you moved to South Pasadena, you were producing more than that one instrument, though, or were you?

BECKMAN: We were producing Helipots [helical potentiometers], and on the verge of producing the DU spectros [quartz spectrophotometers], but we were not actually producing them until we got into South Pasadena.

We've always carried on a heavy research program—heavy for our scale of operation. Even today, about eight percent of our sales dollar goes right back into research, which is a heavy load, compared to most companies. And as a result, we've gotten into many different types of activities. We also have a policy that we would make only the very best; we wouldn't settle for second best. That got us into some situations. For example, we had rheostats on the pH meters, much like radio volume controls. Well, the ones we could buy in the market just weren't very good; they would not hold up in the field. So we decided that to get a better rheostat we would have to make our own. In developing it, we hit on the idea of getting a very

wide range of control by using a helical multi-turn resistance element. Thus a ten-turn coil would provide 3,600 degrees of rotation, whereas single-turn potentiometers provide less than 360 degrees. So we could get wide range with fine sensitivity adjustment over the whole range. Well, we patented that, and made it originally just as a component item for our pH meter, under the name Helipot. It led us into making many kinds of electronic components.

Begin Tape 2, Side 1

BECKMAN: The pH meter, as I mentioned, consists of a vacuum-tube amplifier with a very high input impedance. Well, that meant that it would be suitable for working with a vacuum-type photoelectric cell. I mention that because the vacuum-type photo cell gives a linear response with light flux: The amount of current that comes out is directly proportional to the light flux going in. The gas-filled photo cells, which are more common because they are much more sensitive, do not give a linear response. So I recognized that this circuit we had would be suitable for working with the vacuum-type photo cell, which would give us linear response, and then we could make a spectrophotometer. I was familiar, of course, with visual spectrophotometers, and I realized that even in the visual spectrum there would be advantages if we could use a photo cell instead of the eye, because the eye gets tired. But also we could extend the spectral range into the ultraviolet, where the eye doesn't respond. And so we started working on a spectrophotometer. The result was the Model DU—the first commercially available quartz photoelectric spectrophotometer. It used crystal quartz prisms, because quartz is transparent in ultraviolet as well as in the visual range. Well, these were beginning to be sold when the war came along, which developed a need for infrared spectrophotometers.

When our country's sources of natural rubber were shut off, we had to go into the manufacture of synthetic rubber in a hurry. The basic raw material needed was butadiene, which is formed in the cracking of petroleum products. So there was a problem of analyzing refinery gasses containing C_3, C_4 or C_5 hydrocarbons. The butadiene molecule has four carbon atoms with two double bonds. Well, we had to analyze it in a mixture of other hydrocarbons quite similar in chemical composition. And that was a chore by conventional analytical procedures; it took an excellent chemist maybe two or three days to carry out one analysis. When you're controlling a refinery, it doesn't do you any good to learn two days from now how you should have had the controls set today. So there was a need for a rapid analysis. And infrared was

shown at that time to be a suitable fast way for analyzing hydrocarbon mixtures, but nobody was producing infrared spectrophotometers. Shell Oil had one, Standard Oil of New Jersey had one, Phillips had one, Dow had one—maybe half a dozen or eight hand-built spectrophotometers in the whole country. The Office of Rubber Reserve decided it wanted to standardize infrared analysis for the synthetic rubber program. There was a meeting in Detroit at which that decision was made, and we were given the job of making infrared spectrophotometers for the entire synthetic rubber program.

Well, I was feeling pretty good and waiting to catch a plane going back to Los Angeles, when I got an urgent telephone call from Boston. A fellow named Rosenberg wanted me to come back and see him—very urgent. I'd never heard of him, didn't know who he was; he couldn't tell me who he was. But he said it was important for the war effort. I figured it if was, he could arrange plane priorities. So I said, "If you can get me a priority within two hours to go to Boston and back from Boston to Los Angeles, I'll come on." He came through promptly. He turned out to be Professor Paul Rosenberg of MIT [Massachusetts Institute of Technology], and he was in charge of the potentiometer development for the radar program. Well, I'd never heard of radar; it was not even permitted to use the word in conversation in those days. It was a secret, very hush-hush thing. In radar, for range finding you have to have a precise linkage between mechanical movement and electrical output. Well, that's where accurate potentiometers are involved. It was found that our Helipots were far more precise than anything else on the market. We didn't know this, for we had never measured the linearity, because we didn't use the Helipot with a dial of any kind; all we wanted was the fine adjustment and wide range. Well, to make a long story short, Professor Rosenberg wanted to know whether we'd make Helipots to military specs, and I said we would. That's how we got into the Helipot business. I won't tell you the whole story on that, but we had some internal problems—our engineers wouldn't work on it. They didn't want to waste their time working on a trivial little radio part, is the way they put it. But nevertheless, this was a very important critical component for the whole radar program. And in fact Helipots began to be used in all sorts of electronic circuits where accurate potentiometers were needed.

TERRALL: So this was a refinement of one that you had already developed?

BECKMAN: Yes. Well, we had already developed the Helipot for our pH meter. So that's why when they said, "They'll have to meet military specs. Can you do that?" I said, "Yes, we'll develop one that will." Well, when I came back, I found that ours was absolutely worthless from a military standpoint, because if you hit the Helipot sharply, the sliding contact would spring off of the coiled resistance winding, causing a momentary open circuit, which they couldn't permit. So it meant that we had to start from scratch. We had the helical winding all right, but we didn't have shock-proof contacts. That's when our research people just refused to work on the problem. They were working on a complicated Rube Goldberg device that I'd like to forget. So this was one of the times in my life when I did lose a little sleep. I began getting telephone calls from the military, particularly from the navy: "Where are the Helipots? We have ships ready to go, but the radar gear is incomplete because we have this potentiometer."

One sleepless night I finally figured out how to do the job. Next morning I went down to our shop and talked to a mechanic, Ralph Stalder. I knew that if I put the design through our regular design department, it would be months before it came out, because they would keep it on the back burner. So I went to Ralph and with a penciled sketch said, "Ralph, make this part. When you get it done, bring it up to my office and we'll take the next step." In two days, we had a working model of this Model A Helipot potentiometer, of which we've made tens of millions. As a matter of fact, from a business standpoint, in our first twelve months of operation after we produced it, that little Helipot made 40 percent of our total profit, more than our spectrophotometers.

Well, that is just one example of how we got off into other things. The spectrophotometer was a deliberate attempt to get into the field and expand the usefulness of spectrophotometery. The Helipot was just a chance development that has led to our involvement in precision electronic components.

TERRALL: Did you spend a lot of your time actually working on development, or did you have to spend most of your time in the business end of things?

BECKMAN: Oh, it was spread between the two. We had others doing development.

TERRALL: But it was still a fairly small operation.

BECKMAN: Oh, yes. We all wore several hats. We might be sweeping out the office one day and doing research work the next, and so on.

TERRALL: Did the fact that you had not had business training ever get in the way?

BECKMAN: Oh, yes. When I look back, there were innumerable things I could have done or should have done much better than I did.

TERRALL: Did you ever have problems as a result of that, in terms of how well the company was doing?

BECKMAN: We probably could have increased our earnings. We had a few policies, though, that you don't have to have business training to know. The first was that we'd have no compromise on integrity, whether it was in the design of an instrument or whether it was in our relations with our customers or with our employees. We've never had anybody point the finger at us and say, "Hey, you were dishonest, you didn't tell us the whole story." That's ingrained in our company. I speak to the employees from time to time, and often I say, "Above all things, there's one thing that you should keep in mind: I never want you to put me in a spot where I'm ashamed to go into the office or laboratory of any user or anyone who deals with us." So we've got a reputation for quality. Having that policy, we were able to work out business problems with our users or with our dealers, because they recognized we were an honest, ethical company. I don't recall now any serious problems of any kind.

TERRALL: Did you rely on a lot of the contacts that you had from your years in the academic world for your customers?

BECKMAN: No, not really. At that time, pH was a new term. A few physical chemists knew about hydrogen-ion concentration and were familiar enough with it to work with the negative logarithms encountered in pH. But pH was just coming on the scene. The symbol had been proposed by the Danish biochemist [Søren Peter Lauritz] Sorensen years before, but it hadn't

taken hold. But now about this time, chemists, and particularly biochemists, were beginning to realize that hydrogen-ion concentration was an essential factor in many chemical reactions. So the interest in hydrogen-ion determination rose rapidly. That, coupled with the fact that our instrument made it so easy that anybody could measure pH accurately, without having a lot of training. So the ease of measurement, plus the recognition of the significance of hydrogen-ion concentration, opened up a worldwide new market. So our problem initially was not to find customers, it was to make the instruments fast enough.

TERRALL: But they weren't being used by and large in academic labs? They were being used in industry?

BECKMAN: Everywhere. I can tell you one example: One day a man from Mutual Citrus Products came in—they make pectin, which they sell to jelly makers—and he came in and wanted to know whether we'd give them the right to sell our pH meters among the backyard jelly makers, of whom we have hundreds out here. In making jelly, two or three things are important: First, you have to have a certain sugar concentration; the second thing is some pectin; and the third is that the pH has to be right. If the pH is wrong, the stuff won't jell. Well, Mutual Citrus was getting blamed for having an alleged inferior grade of pectin, when actually the pectin was good but the pH was wrong. So to protect their pectin sales, they wanted to have the right to sell the pH meters and insist that the jelly makers measure the pH. There was a market that never entered our mind; the customer came to us and told us about it.

TERRALL: So they then marketed the pH meter?

BECKMAN: Yes, they marketed it in that field, which was completely out of the field of the ordinary laboratory supply houses. There were many other cases—in the whole field of corrosion, for example, where again hydrogen-ion concentration is important.

TERRALL: So after a few years of being out of academic chemistry, how did you like the new life in your company?

BECKMAN: Oh, I enjoyed it. Because, as I say, I wasn't really out of science. I wasn't doing research and wasn't teaching, but still I was in touch. I was very active in the American Chemical Society in those days, so I used to see my associates frequently at meetings. Until more recent years, I never felt I was really isolated from the scientists. I'm not isolated even now; it's just that with the manifold problems of business, particularly with all the government regulations, I have to spend an increasing amount of my time on business matters. It's been years since I've been able to get out and do anything in the laboratory.

TERRALL: During the war, then, you were working on that potentiometer.

BECKMAN: Then we made the spectrophotometer—we were the sole source for the infrared spectrophotometers. And then the ultraviolet spectrophotometers, we were the largest producer in the world at that time. They were important to the war effort for many things. You could measure butadiene on our ultraviolet spectro, but also toluene for TNT, and many other things of importance in the war. Well, there were so many military applications that the DU had triple-A priority all during the war. That meant that we had the top right to get scarce materials—it's the highest degree of priority for things of critical nature. So we were making spectrophotometers, ultraviolet and infrared, just as rapidly as we could, as well as the Helipots.

TERRALL: So most of your production during the war would have been for military contracts?

BECKMAN: Yes. There are many ramifications. For example, this electronic circuit for the pH meter was ideally suited for a vacuum-type photo cell. It also worked well with ionization chambers. So we built many, many instruments for the atomic energy plants—for Oak Ridge and for Hanford, Washington. We equipped those plants with radiation-monitoring equipment. This is an outgrowth, again, of the pH meter amplifier. I recall we had a visit from a couple of professors from MIT one day. They were visiting Professor Royal Sorensen, and he suggested they come over and see me. Well, they had a project of trying to build a recording micro-microammeter, something that would record from 10^{-15} amps up to 10^{-12} or something like that. They had several PhD's working on it and they'd spent umpteen thousand dollars back there, and still didn't have it. Well, they came over and talked to me, and I said, "Well, we'll sell you one

off the shelf for $750." This was our recording pH meter. By merely substituting an input load resistor for the glass electrode, you can have a recording micro-microammeter. Well, that again opened up a whole new field. It never occurred to me to exploit the pH meter as a micro-microammeter. I'm not a businessman, as you can see. We developed the thing, technically it worked, but we didn't foresee the business potential of selling it in a new market. There were many things of that sort during the war years, where we made special apparatus.

TERRALL: What was your contact with Caltech over these intermediate years? When did you become [a Caltech] Associate?

BECKMAN: In 1939, when I left Caltech. In '53, I was asked to come on the Board of Trustees. I never really lost contact. We had so many friends here, and Mrs. Beckman and I would often come down to Caltech affairs.

TERRALL: You still lived in Altadena then?

BECKMAN: Yes, we still lived in Altadena, close to the Laceys and the Sages, and people like that. So we maintained our social contacts all during that period.

TERRALL: So how did it come about that you were asked to be on the Board of Trustees?

BECKMAN: I guess they must have been at the bottom of the barrel for a candidate. [Laughter]

TERRALL: Was that through Dr. [Lee A.] DuBridge, do you think?

BECKMAN: Yes, it must have been DuBridge. DuBridge came back in '46, that's right, yes.

TERRALL: Did you know him well?

BECKMAN: Yes, I knew him well, and Mrs. Beckman knew Doris, his wife. I mean, I didn't really know Lee that well—he was a physicist and I was a chemist. But still, Caltech was small

enough then that we knew each other. I don't know who was responsible for getting me to become a trustee. They had a nominating committee, you know. Let's see, Jim [James R.] Page was chairman of the Board of Trustees, I think, then.

TERRALL: And did you know him before you became a trustee?

BECKMAN: Not really, no.

TERRALL: So you don't have a clue as to why they chose you?

BECKMAN: No, I don't know how I got on, no.

TERRALL: And what was your reaction when you were asked?

BECKMAN: Oh, I accepted.

TERRALL: Immediately?

BECKMAN: Oh, yes. Yes, I felt honored. Norman Chandler and I went on at the same time.

TERRALL: As a trustee, how did you keep in touch with what was going on on campus?

BECKMAN: Oh, just visits, going to the lectures and various seminars.

TERRALL: Did it change? Were you around campus more as a trustee than you had been in the interim?

BECKMAN: Well, I was never really far away from campus; there was no gap. Even though I wasn't on the payroll, I maintained personal contacts with virtually everybody on the faculty that I'd known—and Ned [Edward C.] Barrett and others from the business office. So I never lost contact and there wasn't any radical change. I probably began to spend more time as matters

would come up before the Board of Trustees.

TERRALL: Well, Caltech was changing a lot in those days, after the war. Do you think that the growth and the changes in sources of funding and so on was a positive thing for the institute?

BECKMAN: Oh, yes, no question about that. The institute is a dynamic thing; it can't stand still. No, as I look back, it's really amazing that some of the policies that were laid down in the early days are still adhered to. For example, its smallness. Caltech could be two or three times its size if we hadn't put restraints on growth. In the early days, it was not too difficult to restrain growth, because every student had to take freshman chemistry and we had only 160 lockers in the freshman chemistry lab. So that was the maximum size of the freshman class for a long time. Then Hal Weis pulled the rug out from under that—he built some lockers on the wall somewhere, another twenty more, so we got up to 180 for a while. Then gradually to around 200, then 210. When they allowed girls to enroll as undergraduates, there was a little quantum jump upwards. But smallness has been a good policy, and it still is the policy; and the faculty, I think, by and large realize that the smallness is one of the assets that Caltech has. It permits the close interdisciplinary relationship that's likely to be lost in a large school. No, I don't think there's been any real change in attitude. There's still the striving to be Number One for excellence, second to none. I don't quite like that slogan; it implies that someone else might be equal! And I think Caltech has succeeded quite well, in all of the various disciplines it pursues. It has dropped some and gained some.

TERRALL: Well, was that something that came up before the board over the years, growth and expansion?

BECKMAN: Oh, yes.

TERRALL: Was it controversial, or did everybody agree that it was important to limit growth?

BECKMAN: You can never have a group of academic people, particularly faculty, without its being controversial. No, they'd argue on both sides, but by and large, as I say, the consensus has

always been that being small is an attribute we should retain. That's hard, particularly in a period such as we had in the last decade or so, when technological advances are coming on at such a rapid rate—it's hard to hold back.

TERRALL: There are so many new fields that you could go into.

BECKMAN: So many new fields, that' s right.

TERRALL: One of the problems that the trustees had, I guess, in the first years that you were on the board was the whole red-scare business—McCarthyism—and the problems that Linus Pauling had, for example. What was the sentiment on the board about his case, as an example?

BECKMAN: Well, that really started before I was on the board, and it was in my very earliest years, so I didn't get really personally involved in that, so it's largely secondhand and hearsay information. I think that what the institute had at that time were some people who were just so opposed to Communism that they were almost rabid on it. And Linus's behavior bothered them a great deal, particularly when Sidney Weinbaum was accused and then convicted of being Communist and he was one of Linus's close friends. So many people thought Linus was a Communist, and Linus, being the independent character he is, didn't do anything to try to mollify them, you see. So some trustees, I think, resigned at that time—one or two, I think. I was told that some potential donors withheld contributions because of this thing. As I say, I was not involved personally, so this is just....

TERRALL: Oh, this had already transpired by the time that you were appointed?

BECKMAN: Yes. That transpired between when I left here in '39 and '53 when I came back. The heated arguments took place before I was on the board.

TERRALL: Were there other issues like that, that divided the Board of Trustees in the years that you were on it?

BECKMAN: Well, I don't know of any issue that divided them quite as strongly as that, no.

TERRALL: Had you been involved in politics or political issues at all?

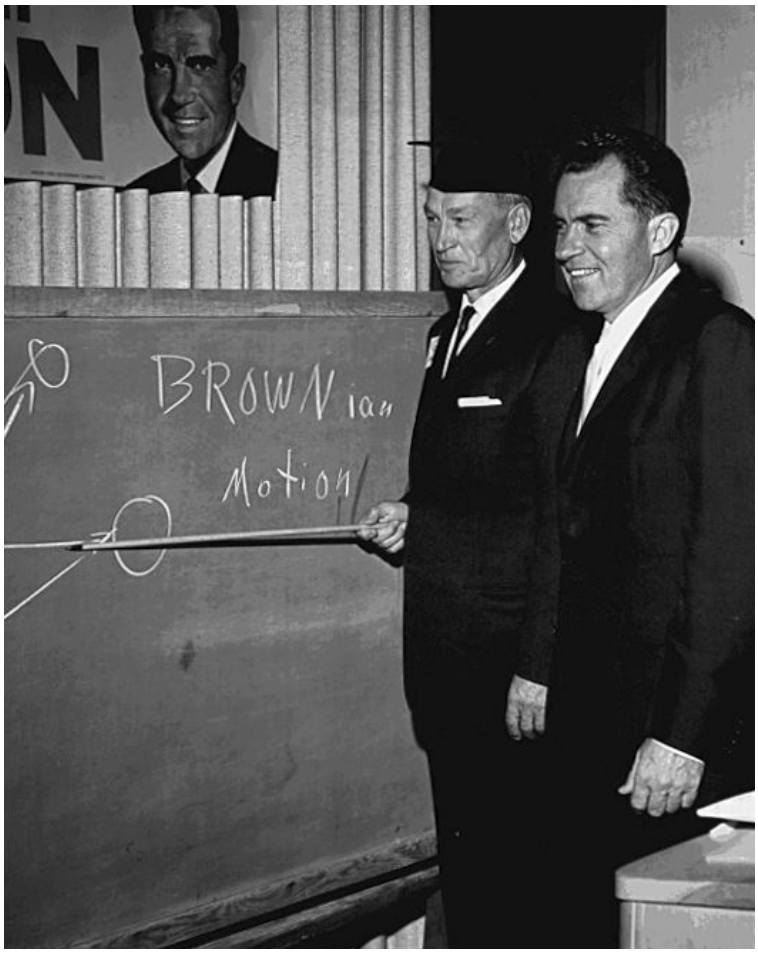

Fig. 6. Beckman gives Richard Nixon a lesson in Brownian motion during Nixon's campaign for governor in 1962. Photo by Frey Photos News Pictures, Long Beach, CA/ Caltech Archives.

BECKMAN: No. No, I didn't get involved with politics till about '62. I'm still not really involved with politics, but I began to take an interest in politics out there in Orange County, when I saw some of the unworthy candidates that we were pushing—people I wouldn't hire for my company—and were spending tens of thousands of dollars on campaigns to get them elected to political offices. I thought that business people ought to be able to handle affairs better than that, so I was instrumental in forming the Lincoln Club of Orange County—I don't know if you've heard of that. We try to dig up worthy candidates—people who are honest and competent and have the potential for growing—and we encourage them to run for office, which is hard to do, because most competent people won't touch politics. And then we try to help them get elected. We're interested in good government. We're not a Republican organization, although most of our candidates are Republicans. But we have no official connection with either political party—it's purely a voluntary group. We put our money and efforts behind people who we think are the best we can find in the field. Most of them are not too good, even now. [Laughter] Nevertheless, we think we've done some good over the years.

TERRALL: Is it mostly business people in the Lincoln Club?

BECKMAN: No, it's business and professional people, but we've got one psychologist from UCI [University of California at Irvine], for example, and others. We charge $500 a year, and that's it—no more, no less. The Board of Directors decides how that money's going to be spent and what we do. We do various things. For example, we began to look into how the Orange County supervisors are performing. We've had one of them in jail, two of them are now under indictment, so we're taking a look and saying, "Hey, we ought to be able to find some honest people to go onto the Board of Supervisors." So that's the sort of thing we do. Well, I was chairman of that from the time it was founded in 1962 until last June. Then I decided it was time to retire, so I retired. I was made chairman emeritus, the same as here at Caltech. I go to the meetings, but I don't have to worry quite as much about the day-to-day operation.

TERRALL: Well, I'd like to get back a little bit to the history of the company again, because we left off around the end of the war, I guess. Can you just go over how your company grew and changed?

BECKMAN: Well, we grew. Our growth rate was limited primarily by the capital we had. We were growing entirely from earnings, so that put a lid on growth. Now, I mentioned this National Postal Meter Company. Well, National Postal Meter was owned primarily by two shareholders—one was the Jergins Oil Company and the other was a man by the name of Murdock. He used to own the Keith-Orpheum Theatre chain and sold out to the Radio Corporation of America when RKO was formed. Well, Jergins Oil Company wanted to cash in its chips. A. T. Jergins and two or three others were getting pretty well along in years, and they wanted to make their estates more liquid. So they sold out to a syndicate of some eight or nine brokerage houses, one of which was Lehman Brothers. Their representatives came over to see what Beckman Instruments was. They'd never heard of it before. I got acquainted with Fred Ehrman, who was a partner at Lehman Brothers. He convinced me that we should go public. That would make available more working capital, so we could grow at a much faster rate than we could by growing only from our own earnings, particularly since we were spending so much of

our earnings on research.

TERRALL: So you mean you had never had a stock issue?

BECKMAN: We never had any outside shareholders up to that time. The only shareholders were just those who were in National Postal Meter—three or four people, something like that—and myself.

TERRALL: But you had broken away from National Postal Meter, though, you said.

BECKMAN: Yes. Well, we'd broken away, so National Postal Meter as a corporation did not own any part of National Technical Laboratory. But NPM shareholders had their individual pro-rata interests in National Technical Laboratory. The Jergins Oil Company in Long Beach was one of them. They wanted to sell their interest in everything. They owned oil fields and a big building in Long Beach. They were just liquidating everything they could.

TERRALL: I see. So you decided to sell shares.

BECKMAN: Yes, so we decided to go public, and in '52, I think it was, we went public. And then in '54 we went on the New York Stock Exchange. And we've gone on from there. We just had another stock split last month; it split two to one on the 27th of November. We've had, of course, ups and downs—everybody does in the stock market—but we've held pretty well. This current year we showed about a thirty-five-percent increase in market value; very few stocks have been able to show that sort of a thing.

TERRALL: So you started expanding the areas that you were making instruments for at that time.

BECKMAN: Yes. One thing led to another. I mentioned Helipots. Well, we made one model—Model A—an inch-and-a-half diameter, ten-turn pot. Well, pretty soon somebody wanted one half-an-inch in diameter; another one wanted three inches in diameter, and instead of linear they wanted nonlinear characteristics. So we began to build up special things. Well, potentiometers

began to build up into a business all of its own. It's about a $100-million business right now, just making resistive devices or things related to them.

We used in the early days some organic insulating materials, resins and things like that. Well, some customers wanted to use Helipots at high temperatures where the organic materials would decompose, so we had to get over into a new type of resistive materials which were ceramic—metal mixtures, cermets we call them. They're sort of ceramic glasses. They enabled us to build a whole new line of pots. To make them, we use a silkscreen process instead of winding wire around a core. That got us into a lot of applications of silkscreen techniques, and that in turn got us into many thin-film and thick-film devices.

Among other things, we make the electronic displays you see on cash registers and electronic games. We've got a plant out in Scottsdale, Arizona, that does nothing but make those displays. These are gas-discharge displays; they take a small amount of current. Remember back two or three years ago, we had the LEDs—light-emitting diodes—on wristwatches; you pressed a button and you'd get a display. Well, LEDs used so much electricity that battery life was short. Things called liquid-crystal readouts use almost zero current. We wanted to build a liquid-crystal readout for a portable pH meter. In so doing, we developed techniques for making liquid crystals that are superior to what anybody else has. Without ever planning it in advance, watchmakers come to us to make the liquid crystals for wristwatches. We're the world's largest manufacturer of liquid-crystal wristwatch dials. So this just came out of developing a new readout for a pH meter. You see, when you make a superior product, you never know what the end result is going to be.

TERRALL: And also, it seems that people come to you, so you didn't even have to think of its application for a watch dial.

BECKMAN: Of course, we do now. We do have applications and engineering people that spend all their time thinking of new uses for our products. But people do come to us with their problems and suggestions.

TERRALL: And then, if it works, you may go ahead with production on that.

BECKMAN: Yes. In another field, in the field of chemical analysis, which is really our original primary field, the techniques have changed. We've gone through gas and liquid chromatography, and things of that sort. We made the gas chromatograph for the Viking lander that just went to Mars. That was quite a feather in our cap, and it worked perfectly, too. Then, in spectrophotometry we've gone particularly into the field of biochemistry now. Of course, in the early days of vitamins, the spectrophotometer was a convenient way for assaying for vitamins. But now, when you get over into body fluids, hormones, and things of that sort, where the concentrations are very, very small, the spectrophotometer is not sensitive enough. So we've gone over into biological methods—using enzymes—and that's a whole rapidly expanding field. We've got a whole line of clinical analysis instruments for determining blood sugar, bilirubin, cholesterol, and things like that that are based on enzymes. When we started, we bought our enzymes from a German manufacturer. Well, that was not a very convenient or sure source of supply, so we decided that we had to have a domestic source, and preferably our own source. So we started making enzymes. We have a plant down in Carlsbad, California, that does nothing but make enzymes for our own use; we also make special enzymes for pharmaceutical houses. That's branched us off into another whole new field. That plant down there doesn't make any instruments at all; it does nothing but make chemicals by fermentation processes. Then up at Spinco—that's our plant up in Palo Alto—they make the ultracentrifuges and automatic amino acid analyzers. They also make an automatic sequencer. As you know, one of the big jobs in biochemistry and protein chemistry is to determine the sequence of amino acids. Well, that was quite a chore in the old days. The sequencer does it automatically—chops off the terminal amino acid, determines what it is, and goes on to the next one. Then, also, we've started making a synthesizer, which sticks the amino acids together in any desired order to make small protein molecules. We make peptides up there. Well, after making a synthesizer, we had to test it to make sure it was working all right. We actually had to synthesize some peptides as part of the test procedure. So we said, "Heck, we're synthesizing peptides and hormones, why don't we sell them?" So we now market maybe forty or fifty, something like that, of these special chemicals. Some of them are very costly and very rare. One costs as much as $10 million an ounce—we sell them in microgram quantities, of course. But some of the interesting ones are enkephalins and endorphins—are you familiar with them? They're chemical substances that have to do with the transmission of nerve impulses across synapses. In fact, one theory is that enkephalins are

the things that are produced in acupuncture: When a needle goes in near a synapse, enkephalins are produced, which block the transmission of the nerve impulse. Well, we synthesize natural enkephalins and various modifications. So that's another business operation, making unusual chemicals. Again, it was just a logical outgrowth. We're not in the general chemical business at all. These are just highly specialized things that owe their origin to something we were already doing.

TERRALL: How did it happen that you ended up with plants in so many different places, rather than in just one?

BECKMAN: Well, the reason we're in Palo Alto is that there was a company known as Spinco—Specialized Instruments Corporation—up there, making an ultracentrifuge. They were a young company, and their president, Morris Hannafin, came down to see me: "How do you market your instruments?" We got well acquainted, and it ended up that we merged the two companies. We just kept the operation up there—we're in the Stanford Industrial Park. We make certain instruments there that are family-related—the ultracentrifuge and the automatic amino acid analyzer and the synthesizer and sequencer are made there. Our entire business used to be in South Pasadena, but we outgrew that, and we moved to Fullerton. And when we outgrew the fifty-acre space there, we bought seventy acres down at Irvine and moved our scientific instruments division there. We still have space problems. We've got traffic problems, too, and we have employment problems. We have over 300 unfilled jobs just in our Southern California operation.

TERRALL: Because nobody wants to go there?

BECKMAN: No, no. Too many people don't want to work. It's pleasant being unemployed. The difference between the income, with all the welfare benefits—unemployment insurance, food stamps, and all these other things—the incremental difference in income between being unemployed and employed is so small that most people say, "Well, why work?"

TERRALL: How long has that been a problem?

BECKMAN: Oh, it's been growing over the last several years, ever since our welfare programs began to get out of hand, and when food. stamps came in. It's getting worse and worse; so much so that we're forced to build plants elsewhere. There are other reasons, too, sometimes. For example, we have two plants in Puerto Rico, because unemployment there is high and workers are readily available—good willing workers. Furthermore, there are some tax advantages. But then we put a plant down in El Salvador. There we don't get any tax advantage but there are plenty of good workers available. We ship unassembled components, small things, down there—we ship them by air—and the local people do a very simple operation, putting two or three or four parts together, and send the assembly back by air. There's a high labor content for that particular operation. With a thirty-percent unemployment problem down there, we don't have any difficulty in getting workers.

It's a shame we have to take the work out of the U.S.A., but that's a fact of practical life. We have plants over in Europe. They came about initially because of the difficulty of getting import and export licenses after the war, and also because of currency problems, for we'd have to get permission from the national banks to exchange local currency for dollars, a time-consuming task. We find it's better to have plants over there. We've got two plants in Ireland—again, to tap a good labor market and also to be in the European Common Market. We find that customers prefer to buy from a company that has a factory right in their backyard rather than going across the ocean. So these are some of the practical business aspects of plant location. I'm not sure we'll expand much more in California. It's become so unattractive—all the restrictions, the regulations that are coming in, the taxes, things like that. It's more advantageous to locate plants in other states, if not in other countries. That's one thing I'm afraid [California governor] Jerry Brown is just beginning to wake up to.

Begin Tape 2, Side 2

TERRALL: I'd like to get back to Caltech a little bit. How did it feel to be a trustee in a place that you had been a student in thirty years before?

BECKMAN: Well, it was very gratifying, personally. And I wore three hats. The students looked on me as being their representative on the Board of Trustees, as I'd been a student here. The

faculty did also; they thought I'd look after their interests because I'd been a faculty member. And, of course, my ordinary trustee hat. I knew and got along well with all the different groups, and I hope I was able to be helpful in seeing that programs were carried out that were good programs from everybody's standpoint.

TERRALL: Now, were you the first person who had been an alumnus?

BECKMAN: I was the first alumnus to be elected to the board.

TERRALL: Well, that could have been a factor, then, when they selected you, don't you think? Getting someone who had had that long association with the institute?

BECKMAN: Possibly, I don't know.

TERRALL: Why do you think it took so long for Caltech to admit women, both graduates and undergraduates?

BECKMAN: Well, it didn't take so long. Actually we had women graduate students for many years. When Thomas Hunt Morgan came out here in'28, as I recall, one or two graduate students came along with him. That was one of his conditions for coming out here. So we had women in biology. As far as I know, only in biology did we have women graduate students, for many years anyway. There was no policy against it; it's just that there'd never been any insistent demand. I don't think we had any applicants. It's only recently when the ERA [Equal Rights Amendment] and similar movements have made an issue out of this thing, that the question has really come up into focus.

TERRALL: Well, I think that a lot of places that had not been coeducational decided to admit women before Caltech did.

BECKMAN: Oh, yes, quite a few had, that's right. After all, we did have colleges for females and colleges for males—this was the way it was historically. And it's only because some groups feel

women's rights are being abused that they insist on integrating the two. There were some practical problems. You see, we didn't have housing facilities. And it raised the question: Do you have a dean of women for two female students or not? So there are a lot of practical things. When you start on this thing, you have to do it on a big enough scale to justify the economics.

TERRALL: Was the housing problem really the basic problem in postponing the issue, say, for the trustees? Where it must have come up, because I know it was being talked about long before they decided to do it.

BECKMAN: I can't speak for all the trustees, but as I think back, it was the economics more than anything else. Yes, people would say, "We're going to have to provide special restrooms in our laboratories and special dormitory facilities, and maybe such things as a dean, special athletic facilities, things of that sort." It was the practical difficulties, and the question was: Is it worth it for the handful of girls we would have? Wouldn't it be better for them to go somewhere else? But finally, the pressure built up until, "Well, heck, in the absence of any real opposition, let's try it." And I'm glad we did; it's worked out very well.

TERRALL: Well, the other thing is the change in the way things are funded now, in that so much of the money for research comes from the government. Do you think that's a problem in terms of maintaining independence?

BECKMAN: It isn't as much of a problem as some of us feared it would be years ago. It is a problem. I recall when Bob Minckler was chairman, at that time the percentage of our total research money coming from the federal government was slowly rising, and I raised that very question: Are we going to lose our independence? I was made chairman of the committee to investigate it. We never did come up with any instance where we could say that the government was responsible for knowingly changing the course of research at Caltech. I think without our realizing it, government funding does exert a pressure, because professors naturally, if they're thinking of two alternative research problems—one for which they can get federal funds and one for which they have to go out and scrounge somewhere else—they're likely to take the one for which funding is easy. So in that sense, there's been a subtle and maybe undetectable influence.

I don't think it has caused damage to Caltech, because I think the research that the government is funding is worthwhile research that is quite suitable for Caltech to be carrying on. And I don't know of any real effort of the government to try to come in and tell us how to run research. Now, this is a danger, particularly if fellows like [Senator William] Proxmire get in the act. He's going to come in and try and tell you how to run the research. If that ever happens, then we are in trouble.

That's one thing. A second thing is that we've become so dependent. About fifty-six or fifty-seven percent, as I recall, of our operating funds come from the federal government now. If in a time of cutback—supposing that following Proposition 13, Congress says, "Oh, well, heck, let's cut back on research because researchers don't represent very many votes; let's just cut the bottom out of research appropriations." If that happens, we could be in trouble. Fortunately, I think that the business people and the scientists have been vocal enough that they've convinced President Carter, at least, that one thing wrong is we have not been spending enough money on research. You may recall in his message about a month or so ago, he said we have to keep that up. And it's true. We are losing out as a country; we're losing out to Germany and Japan particularly, and to some extent to England also. So I think that realistically, for the next few years, we probably will have enough people in Washington to carry on the fight for adequate research funds.

TERRALL: Another thing that I know you're interested in is behavioral biology. I read in *Engineering & Science* that when they dedicated the building over here [the Mabel and Arnold Beckman Laboratories of Behavioral Biology], something that you had said about the importance of the field, as a new field. How did you get interested in that area?

BECKMAN: Oh, over the years my interest in biology and biochemistry just gradually increased. But at that time I was very much concerned about human behavior—What makes people tick? Why do they react the way they do?—and some of the work of [Roger] Sperry and others over here was beginning to shed a little light on that. Then I thought this was probably a very fertile field for worthwhile research. Human behavior is something that affects the lives of everybody in the whole world, you see. And if we can get a scientific understanding of human behavior…. And at that time, without knowing anything at all about it, I said, "Human behavior must

ultimately be chemically related." It seemed to be so obvious, for two reasons. After all, the body is nothing but a bunch of chemicals held together; it follows the laws of physics, like everything else, but essentially it is a chemical mass. We knew enough about such things as the effect of alcohol and drugs on behavior to know that chemicals do have tremendous effect on the thought process, on the mind. So it doesn't take much extrapolation to think, "Well, maybe chemicals control all of our behavior." So this sort of thinking was in the back of my mind. At the time, we had a strong behavioral biology group, so I said, "Well, we'll build a laboratory for them." Unfortunately, Dr. [James] Olds died. He used to be head of behavioral biology and then he drowned down in Laguna Beach. He was doing some interesting work on location of the pleasure centers in the brain—a very exciting field. I still think it's an exciting field, although now there's probably more emphasis on cell-membrane chemistry than there is on behavioral biology, because of everybody's preoccupation with cancer—which is good, but also I think behavioral biology is equally important from the overall standpoint.

TERRALL: Do you think that your support and the donation of the building was instrumental in expanding the division in that direction, or were they going to go ahead with that anyway?

BECKMAN: Oh, they were going to go ahead anyway. We had a program—the Court of Man concept. Back in '59, I think it was, I was made chairman of the first fund-raising drive we had. We had $19 million, as I recall, for our goal at that time—or was it $16 million? I forget. I guess I was chairman of the board at that time, because I know I was very much involved in the planning of this thing. Well, we had the [Beckman] auditorium up there, and we got the concept of a Court of Man, which was that on one side of a mall we'd. have the humanities and the other side we'd have the biological sciences; that was the broad concept. So gradually I got interested in the Court of Man concept. But the budget was set up and the behavioral biology building was put in the program long before I indicated I would provide the funding for it. Mrs. Beckman and I talked it over and said, "Well, why not?" So that was that.

TERRALL: Did you have any role in planning the building itself?

BECKMAN: Not very much. Of course, we had a Building and Grounds Committee, and as

chairman of the board I sat on that. And we had the same architect that planned the Baxter [Hall of the Humanities and Social Sciences] building—for symmetry, we wanted the same architect for the new building. But Bob Sinsheimer and his group were pretty much responsible for the facilities we have inside.

TERRALL: Well, it came out well.

BECKMAN: Yes, they seemed to be pleased with the facilities.

TERRALL: Something else that I noticed you were involved in is the air-pollution problem in Los Angeles. Is that through Beckman Instruments?

BECKMAN: No, that was just a personal thing. It goes way back. I was active in the Los Angeles Chamber of Commerce; I was president in ’56 and was active before that as chairman of what they call the scientific committee. We concerned ourselves with anything in the nature of science or technology that might affect the community. Well, the air pollution came on in ’47, ’48—right at the end of the war. In fact, what brought it on—I don’t want to get involved technically, but the Southern California Gas Company was going to make the same butadiene I was telling you about. They had a process for making that by heating oil and cracking it, and then suddenly quenching it to prevent more reaction taking place. Well, this stuff just spewed out into the air. All kinds of chemicals—aldehydes, phenols, and things of that sort—came into the air. They had to shut down the plant, even though it was a triple-A priority project for making butadiene. Well, then we had a group out here that got emotionally charged. They knew nothing at all about the technology, but they assumed that it was sulfur dioxide that was causing the problem. AB 1, 1947—Assembly Bill 1 by Mr. Stewart from Pasadena—was passed, which authorized the establishment of countywide air-pollution-control districts and set a limit for sulfur dioxide in stack gases. Legislation is written largely by lawyers, and they usually look for precedent. Looking around for precedent, they could find only one case, and that was a famous air-pollution case of Trail, British Columbia. There’s a smelter up on the border between Canada and the United States that smelted copper sulfide ores. Thousands of tons of sulfur dioxide were going into the air, killing the vegetation. Well, an international agreement was

reached that the stack gases should not have more than .2 percent by volume of sulfur dioxide. That's the law. So when they wrote AB 1, they put in that stack gases cannot exceed .2 percent sulfur. Well, right away, everybody jumped to the conclusion that sulfur dioxide was our pollutant down here.

This AB 1 made it possible to create air-pollution-control districts, and the Los Angeles Air Pollution Control District was formed in the fall of '47. Dr. Louis McCabe, whom I knew from the University of Illinois, was the first director. And he came out here, and right away he was subjected to pressure. After he'd been out here a few weeks, people said, "Air pollution is just as bad as it was before he came." People thought the problem should be solved overnight. So under pressure he went so far as to suggest bringing out a surplus ammonia plant—this was after the war, of course—and the ammonia would react with the sulfur dioxide and form ammonium sulfate, which could then be sold as fertilizer. Well, being a chemist, I've been around SO_2 plants, and I said, "Lou, we don't have any SO_2 problem here."

So finally we took our committee—which included Dr. [Robert E.] Vivian from USC, I remember, and I forget who else—and we went down and got the supervisors to agree to spend a little bit of money to analyze the air and see what the heck the pollutant was. Before bringing in millions of dollars worth of equipment to take something out of the air, we ought to know what the pollutant really is. Oh, you have no idea how emotionally charged people were. Everybody knew it was sulfur. We had preachers out here mentioning the sulfurous fumes in Hades and referring to the Bible. Nylon stockings were just coming out then. If they had runs in them, this was allegedly due to droplets of sulfuric acid in the air. It was unbelievable. I've got clippings on all this.

So I said, "Let's analyze the air." Well, that meant a good microchemist was needed, and I thought of Arie Haagen-Smit, whom I knew. I asked Haagie if he would analyze a sample of air. Yes, he would. Gordon Alles was interested in it at that time, and Reed Brantley, and two or three others who were chemists. So we passed, as I recall, 500 cubic meters of air through a liquid nitrogen trap, got a couple of little drops of brown, gooey, smelly stuff, and gave it to Haagie, and he analyzed it and found it was peroxy material. He couldn't identify it much beyond that: peroxy—organic material. So this pointed to hydrocarbons—to organic material, not to sulfur. Well, this created a great controversy, because at that time the Western Oil & Gas Association had retained Stanford Research Institute, and Abe Zarem, who was working for

Fig. 7. Arie Haagen-Smit in October, 1956. Photo Caltech Archives.

them, said that Haagie was all wet—that he, Abe, had worked with these peroxy compounds and they didn't smell, they didn't irritate the eyes, et cetera. Well, fortunately, I got Abe to give a talk over here in the chemistry lecture room, and I got Haagie to come and sit with me. This was when Abe said, "It's unfortunate that a good chemist like Haagen-Smit could be misled." Did you know Haagen-Smit?

TERRALL: No.

BECKMAN: Oh, he was a Dutchman. He just said, "Who says I'm wrong?" Well, this got his dander up. He didn't want to work in this before, because he was working on isolating the flavor constituents in pineapple, working on root-growth hormones, auxins, things like that. He didn't want to get involved in this stinky political mess. But Abe's remarks got under his skin enough that Haagie agreed to work on it. So Caltech let Haagie take a year's leave of absence and go work on the problem. That's when he worked out the mechanism of chemical oxidation of hydrocarbons, with nitrogen dioxide as the catalyst. So that's how I got involved in it.

TERRALL: So you really got him into it, too?

BECKMAN: Yes. But then Hal Kennedy, who was the chief counsel for Los Angeles County, called on me to help him draft the regulations. So I played a part in writing the ordinance controlling the alert system which we have now, and setting up the hearing commission to hear cases for variances and things like that. I was involved on the legal business side, because I wanted to make sure that we didn't go off half-cocked and get a lot of well-meaning but

uninformed people just creating chaos unnecessarily. I got Professor Daugherty to be the first head of the hearing commission, a job he held for eight or nine years. And he did a good job, because he was just the sort of person who should be in there—he had good technical background, he was a good level-headed guy, and his integrity was unquestionable. If it hadn't been for him, we might have had a political appointee in there. So I was involved—oh, it took a lot of my time. In fact, in '54, Governor [Goodwin J.] Knight, because there was so much hassle about this and we still had smog despite all the activity related to it, appointed a committee and made me chairman of it, to investigate and report on the current status of air pollution and to make recommendations. Well, that report was sort of the bible, for about ten years, that governed activities of the Los Angeles Air Pollution Control District.

We recommended such things as looking for alternative propulsion means to replace the internal combustion engine. We suggested the steam engine and turbines—also that auto engines should burn propane, which does not give off noxious gases—and some other practical things. I'd say for about ten years that was the reference book that the Air Pollution Control District used.

TERRALL: Do you think that any of the things they did made a difference?

BECKMAN: Oh, yes. Oh, you wouldn't be able to live here now if they hadn't done some of these things. They've gone overboard in some ways, particularly with respect to sulfur dioxide.

TERRALL: No one could forget about that?

BECKMAN: Well, but we're paying hundreds of millions of dollars for this. I was put on the Air Advisory Quality Board by President Nixon; this reported to the head of the EPA [Environmental Protection Agency]. This was when [William D.] Ruckelshaus was head of it. Well, I remember a meeting we had in St. Louis—we met for a week there—in which I argued about this: "We don't have a sulfur dioxide problem in Los Angeles. This business of making Los Angeles power plants buy costly low sulfur oil is just a needless waste." I checked with the Edison company at that time and found out that in the preceding year they had paid a premium of $100 million—just premium, mind you—to get low-sulfur Indonesian oil, over what they would

have had to pay for domestic oil. A hundred million! And before I went back to that meeting, I wrote to the various hospitals and county medical directors and asked, "Can you give me examples of cases you have of an illness that's either caused by or aggravated by sulfur dioxide, as distinct from other pollutants—just sulfur dioxide per se?" Not a single case. We've never had one. When sulfur dioxide gets high enough to be objectionable, you can smell it. It has a very pungent odor. We never get that here, with the exception of, if you walk by El Segundo, maybe, by one of the sulfur recovery plants. So we've never had a major sulfur dioxide problem. And yet because sulfur dioxide was written into the law, the district went one step further. Instead of analyzing stack gases, they thought they'd make it easy by limiting the sulfur content of fuel to less than .5 percent sulfur. We've wasted hundreds of millions of dollars needlessly. But that's what you get when emotionally charged people don't take the time to find out the facts. So that's how I got interested in air pollution.

TERRALL: That's an interesting story.

BECKMAN: Oh, another thing, there's a story about horse doping. You know that story? Well, one of the things Millikan asked me to do—there was a man named Church, Norman W. Church, who had race horses. One day at Santa Anita racetrack, his horse was accused of being doped. Church came to Caltech for help, so Millikan asked me to go out and do what I could. I found out that the track chemist had done a sloppy job. I said, "On the basis of your evidence, you couldn't draw any sound conclusion that the horse had been given strychnine." I threw an element of doubt into the situation. There was nothing else I could do, because the saliva samples were all gone; I could only look at his results and criticize his technique. Well, to make a long story short, this finally went through the courts and Church was exonerated. And as a result of that, we got the Church Laboratory [for Chemical Biology].

This is written up in a book, written by a reporter for the Los Angeles *Examiner*. I must try to find a copy of it. It's long since been out of print.

In fact, there was another alleged horse-doping case. The next time was Louis B. Mayer's horse—the first horse was Proclivity. Louis B. Mayer's horse was said to have been doped with caffeine. Haagen-Smit was on the committee at that time, and Gordon Alles. We went down to a horse ranch near San Diego and actually doped horses deliberately with caffeine

to see how much caffeine you'd have to give a horse in order for it to show up in the saliva. We found out, as I recall, that you would have to feed about four ounces of caffeine to detect it in the saliva. But, anyway, that was another suit. They had the horse boarded up in a stall with Brinks guards on duty twenty-four hours a day. They had the noted attorney, Jerry Giesler—a top criminal attorney—on the case. We had a lot of fun in those days.

TERRALL: These were two separate cases, then.

BECKMAN: Oh, yes, two separate cases. And the whole thing involved, apparently, a group that was trying to prevent the starting of the Hollywood track. They didn't want a competitive race track.

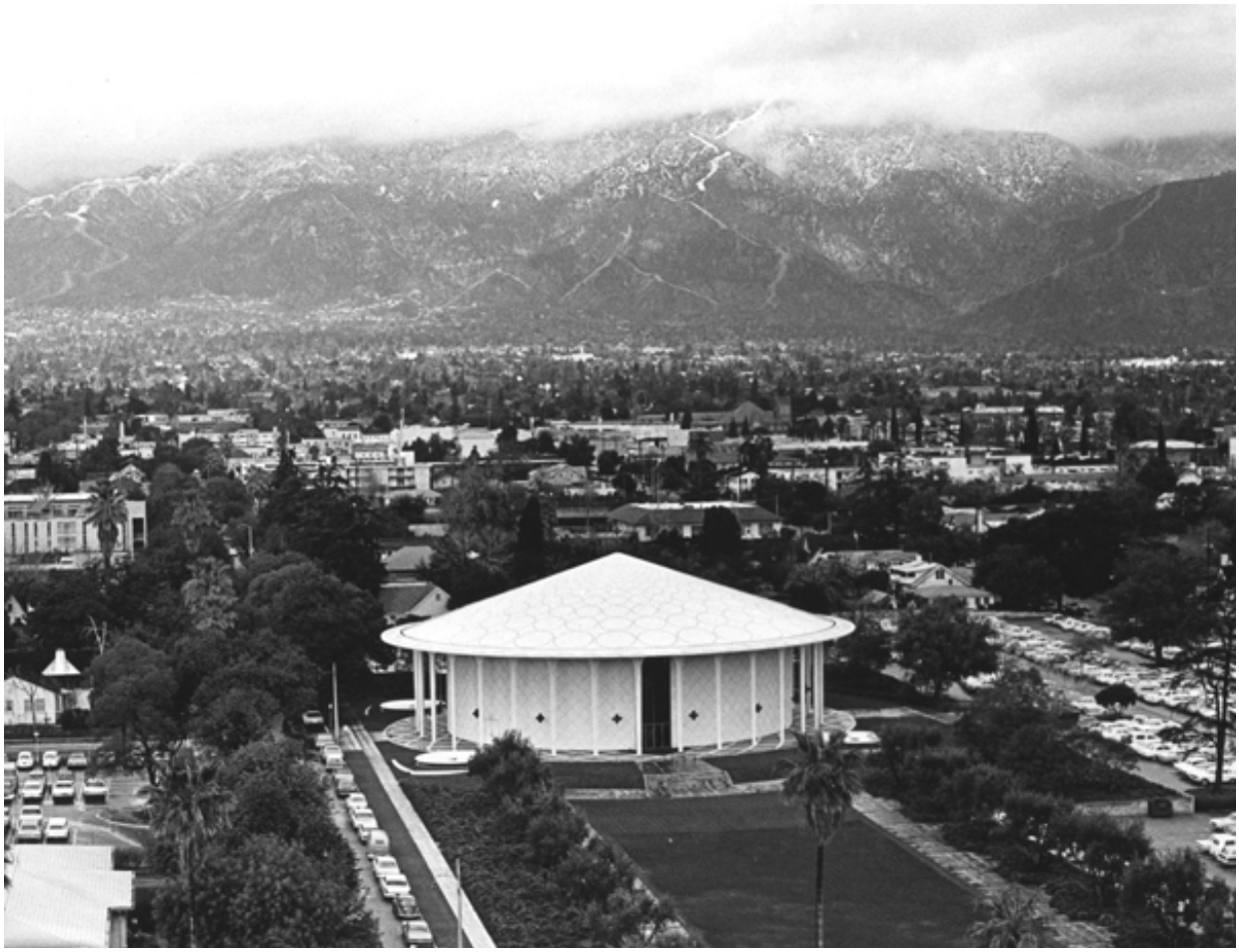

Fig. 8. Beckman Auditorium, one of several buildings on the Caltech campus bearing Beckman's name. Photo Caltech Archives.

TERRALL: Oh, this was all at Santa Anita?

BECKMAN: Yes. Well, these are some of the interesting things that livened up existence at Caltech. I never thought I'd get involved in horse doping, but I did. And also I was interested, because in the Louis B. Mayer case we used our ultraviolet spectrophotometer to show that what they thought was caffeine was just another purine body—a metabolic decomposition product; it was not caffeine. That was one of the early applications of the DU spectro.

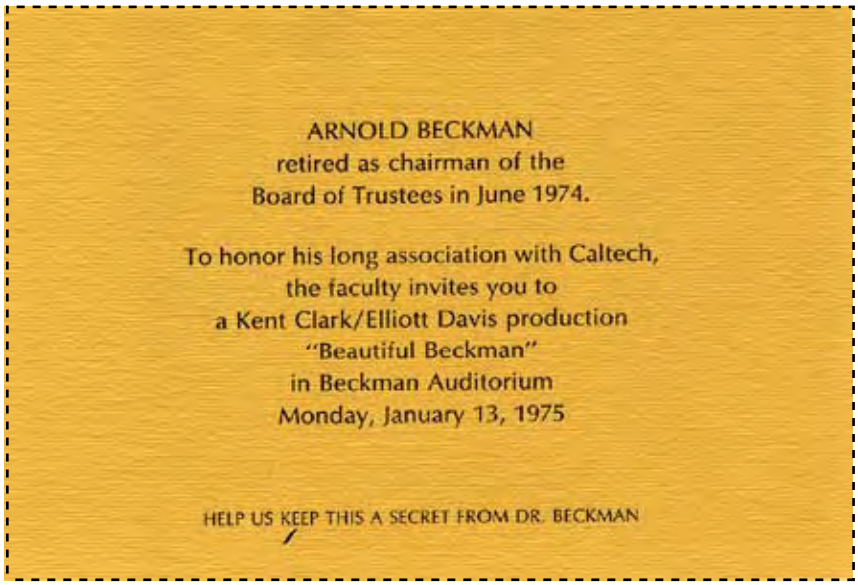

Fig. 9. The invitation to Kent Clark's and Elliot Davis's 1975 musical tribute to Arnold Beckman. Caltech Archives.

TERRALL: But he didn't give a building to Caltech.

BECKMAN: No. Mr. Mayer didn't give a dollar to Caltech, so far as I know. Mr. Church did.

Click on the image to listen to Ward Whaling and the CaltechStock Company sing "Beautiful Beckman." Used by permission.