CENTER FOR HISTORY OF CHEMISTRY

ARNOLD O. BECKMAN

Transcript of an Interview
Conducted by

Jeffrey L. Sturchio and Arnold Thackray

at the

University of Pennsylvania

on

23 April 1985

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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(Revised 24 February 1988)

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ARNOLD O. BECKMAN

1900	Born in Cullom, Illinois, 10 April

Education

1922	B.S., chemical engineering, University of Illinois
1923	M.S., physical chemistry, University of Illinois
1928	Ph.D., photochemistry, California Institute of Technology

Professional Experience

1924-1926	Research Engineer, Bell Telephone Laboratories
1926-1929	Instructor, California Institute of Technology
1929-1940	Assistant Professor, California Institute of Technology
1934	Vice President, National Inking Appliance Company
1937-1939	Vice President, National Technical Laboratories
1939-1940	President, National Technical Laboratories
1944-1958	President, Helipot Corporation
1946-1958	President, Arnold O. Beckman, Inc.
1940-1965	President, Beckman Instruments, Inc.
1965-	Chairman of the Board, Beckman Instruments, Inc.

Honors

1960	Illinois Achievement Award, University of Illinois
1964-1974	Chairman, Board of Trustees, California Institute of Technology
1965	Honorary Sc.D. degree, Chapman College
1969	Honorary LL.D. degree, University of California at Riverside
1969	Honorary LL.D. degree, Loyola University in California
1974	Scientific Apparatus Makers Association Award
1977	Honorary LL.D. degree, Pepperdine University
1977	Honorary Sc.D. degree, Whittier College
1977	Arnold O. Beckman Conference in Clinical Chemistry, established by American Association for Clinical Chemistry
1980	Arnold O. Beckman Professorship of Chemistry, established by California Institute of Technology
1981	Hoover Medal, American Association of Engineering Societies
1981	Life Achievement Award, Instrument Society of America
1982	Diploma of Honor, Association of Clinical Scientists

ABSTRACT: In this interview Arnold Beckman begins with his teen-age experience as an industrial chemist at a local gas works in Bloomington, Illinois and the Keystone Iron and Steel Works. This is followed by reflections on his student days at the University of Illinois, with special emphasis on some of the faculty and students. The central portion of the interview considers Beckman as a student and faculty member at Caltech and includes his early experiences with instrumentation, patents, and serving as an expert witness. The interview continues with Dr. Beckman discussing the origin of the pH meter and DU spectrophotometer, and concludes with the beginning stages of manufacturing and sales, emphasizing the principles used to build National Technical Laboratories, the company that would become Beckman Instruments.

INTERVIEWERS: Jeffrey L. Sturchio holds an A.B. in history from Princeton and a Ph.D. in the history and sociology of science from the University of Pennsylvania. He is Associate Director of the Center for History of Chemistry and Adjunct Assistant Professor of History and Sociology of Science at the University of Pennsylvania.

Arnold Thackray majored in the physical sciences before turning to the history of science, receiving a Ph.D. from Cambridge University in 1966. He has held appointments at Oxford, Cambridge, Harvard, the Institute for Advanced Study, the Center for Advanced Study in the Behavioral Sciences, and the Hebrew University of Jerusalem. He is Director of the Center for History of Chemistry, and Professor of History and Sociology of Science at the University of Pennsylvania, and the 1983 recipient of the Dexter Award for outstanding contributions to the history of chemistry.

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INTERVIEWED BY: Jeffrey L. Sturchio and Arnold Thackray

PLACE: E. F. Smith Hall, University of Pennsylvania

DATE: 23 April 1985

Sturchio: This is Jeffrey Sturchio. We're interviewing Dr. Arnold Beckman at the Center for History of Chemistry today, April 23, 1985, in Smith Hall at the University of Pennsylvania. With me are Arnold Thackray, Director of the Center, and Eric Elliott.

Dr. Beckman, from the Caltech oral history memoir, the ACS Eminent Chemists videotape and the discussion in Golden Past, Golden Future, the fiftieth anniversary history of Beckman Instruments, we are relatively well informed about your childhood interest in chemistry excited by Steele's Fourteen Weeks, your early education, your tour of duty in the Marines, and your decision then to go to the University of Illinois.* In today's session we'd like to focus on your career beginning at the University of Illinois and leading up through the end of World War II, with an emphasis on the technical side. But before we start at Urbana in the fall of 1919, I wonder if you could tell us more about your experience working as an industrial chemist while still a teenager, first for the gas works in, I believe it was Bloomington, Illinois, and then later for the Keystone Iron and Steel Works in Peakin, Illinois?

BECKMAN: Yes, as you know I went to the University High School which is part of the then Illinois State Normal University. I was permitted to take some university chemistry courses in lieu of Latin, and the professor in charge of chemistry was Howard W. Adams. He was indeed a great friend of mine, and he encouraged me to set up a little laboratory in my home to analyze the iron oxide-coated chips that were used to absorb hydrogen sulfide by the local gas company. I had a little laboratory in the corner of my home that I worked in to perform this analysis.

THACKRAY: That's where I began my industrial career, too, in fact, doing the same problem--town gas and iron oxide analysis.

BECKMAN: Is that so?

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* "Interview with Arnold O. Beckman" (Mary Terrall, interviewer), 16 October & 4 December 1978, California Institute of Technology Oral History Project, Caltech Archives, 1981; "Arnold O. Beckman," videotaped interview with A. James Diefenderfer and Arnold Thackray, Eminent Chemists Videotape Series, American Chemical Society, 1985; and Harrison Stephens, Golden Past, Golden Future: The First Fifty Years of Beckman Instruments, Inc. (Claremont, CA: Claremont University Center, 1985.)

THACKRAY: Yes.

STURCHIO: Did the gas works just farm all their analyses out to you or were there other people who did this? How characteristic was it?

BECKMAN: I suspect that Professor Adams had something to do with seeing that I got some orders on that, because he was very helpful to me in many ways.

STURCHIO: Was he also behind the job you got at Keystone Iron and Steel Works just before you graduated from high school?

BECKMAN: He may well have been; I wouldn't have been surprised, because this involved being excused from school early to engage in war work. You see, I started at the Keystone Steel and Iron Works, oh, March or April, something like that, of 1918, two or three months before the close of the school year. And that would have required approval of the school authorities. So, undoubtedly he was involved in that.

STURCHIO: You mentioned when you were interviewed for Caltech that you were doing chemical analyses of molten steel to find out the percentage of carbon, silicon, sulfur, and manganese.* I was wondering what sort of apparatus they had available. Was there a plant laboratory?

BECKMAN: Oh yes, they had a regular analytical laboratory, with the Alberene stone table tops and the analytical balances. The procedure was that the chemist would go over to the open hearth furnace with a little mold, reach in and get a sample of the molten iron, put it in the mold, and then with tongs carry it back to the lab while it was still hot. We would quench it enough so we could handle it, then drill it out for chips, weigh these on the analytical balance--four different samples for the four tests made--by hand, and then carry them through in the proper sequence. As I recall, I got to the speed where I could run all four analyses in 30 minutes.

STURCHIO: What did they do then with the information that came from the analyses?

BECKMAN: That determined whether or not the melt was ready to pour. When you have a hundred tons of steel in there, it's very important to know the right time to pour. And the minutes are important, too. So they'd wait until the lab results were in and a man would run back and tell them either to carry on or else to pour.

STURCHIO: How many chemists did Keystone have? Were you in competition with other chemists to run your analyses quickly?

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* Beckman interview, Caltech Archives (1981), p. 6.

BECKMAN: Not at the same shift. There was only one per shift.

STURCHIO: And there were two shifts, three shifts?

BECKMAN: We worked two shifts. Alternate ten and fourteen hour shifts. Two weeks on one shift and two weeks on the other.

STURCHIO: Would you say that Keystone was advanced for using chemists for this kind of process analysis, or was that something that was coming into more widespread use?

BECKMAN: Oh, I think it was standard practice at that time for open hearth furnaces.

THACKRAY: Though not very standard for an early graduate from high school.

BECKMAN: Oh no, I imagine that not very many high school students had that opportunity.

THACKRAY: And presumably this experience did reinforce your enthusiasm for chemistry.

BECKMAN: I enjoyed it very much. Of course, on the long shifts we had periods where there wasn't much for us to do, waiting for the...I remember stretching out on the cold Alberene stone top and taking a nap until it was time to go over and collect a sample.

STURCHIO: You stayed there through the summer, I believe.

BECKMAN: I enlisted in the Marines in early August.

STURCHIO: And you've told us the story of how you met your wife while you were in the Marines, and then had a chance to apply for a discharge and were discharged at the end of January 1919.

BECKMAN: That's right.

STURCHIO: Then you went out West, spent some time in Ashton, Idaho, and elsewhere. That's all discussed in the Caltech interview.* But by the fall of 1919, you were back at the University of Illinois and had started your undergraduate training there. I'd like to focus on a few aspects of that, to fill out the outline of the years that you spent at Illinois. For one thing, you mentioned in the Eminent Chemists videotape with the ACS that at the time you thought you wanted to be an organic chemist, to synthesize dyes and other organic compounds. I was wondering what the attitude was in the chemical community about the American chemical industry. We know about the attempt to build up the American chemical industry at the time, and I just wondered what

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* Beckman interview, Caltech Archives (1981), pp. 7-11.

discussions there were, what people were reading, whether they were talking about those issues at Illinois in 1919?

BECKMAN: Keep in mind, I was a young lad in 1919 coming from a small rural community that had no contact with the industrial world at all. So I really am not in a position to tell you what the average thinking was. I wasn't worried about the future of the chemical industry at that time. I was interested in making some interesting chemicals--dyes or things that smelled well. I'm sure there must have been great concern from the demand for chemical engineers. At that time, they commanded the highest starting salaries of graduates. That again is one indication of interest in the future of chemistry.

THACKRAY: What about the people who went to Illinois with you? That was a very unusual moment in all sorts of ways, not least in people returning from the war. Were there other freshman who had the sort of chemical experience that you had had?

BECKMAN: I don't recall anybody who had other than just the normal high school training.

STURCHIO: In the Illinois Chemist there were a number of editorials on the tension between the chemists and the chemical engineers at Illinois and whether there should be a college of chemistry.* I wonder if you would tell us a bit about that, the background of those discussions?

BECKMAN: Well, obviously we were very much concerned about whether we could be honored by being elected into the Phi Beta Kappa or Tau Beta Pi, and we were excluded from both by being in between. And chemical engineering was actually part of the College of Liberal Arts. But the Phi Betas wouldn't accept chemical engineers as being suitable material, and the engineering school wouldn't accept us as being, oh, not really engineers--they thought of us as chemists. A chemical engineer was just a chemist with a pipe wrench. That was the attitude there. Of course, at that time, whether or not you had a pin to wear was a very important part of a person's life.

STURCHIO: Did you write those editorials in the Illinois Chemist?

BECKMAN: Apparently I did. Yes. (Laughter)

STURCHIO: What's intriguing about them is that, for instance, in one of the editorials you refer to the example of MIT and the School of Chemical Engineering Practice.** So there was informa-

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* See, for example, "Recognition" and "What is a Chemical Engineer?" in Illinois Chemist, 6, no. 1 (November 1921): 16-18; and "A College of Chemistry," ibid., 6, no. 2 (February 1922): 48-49.

** "What is a Chemical Engineer?" Illinois Chemist, 6, no. 1 (November 1921): 17-18, at 18.

tion about what was going on at MIT and elsewhere in the Illinois department.

BECKMAN: Yes.

STURCHIO: Tell us a bit about the faculty you worked with. You mentioned that you studied with Dietrichson and Rodebush. I wonder if we could start with Dietrichson and your work with him as his lab assistant?

BECKMAN: Well, I started, of course, with C. S. Marvel in my freshman year. Because I'd had this extra chemistry during high school I was allowed to omit the regular chemistry.

THACKRAY: How old was he at that moment?

BECKMAN: Speed Marvel, he's ninety-one I think. He's about five or six years older than me.

STURCHIO: He must have been about twenty-five.

THACKRAY: So he was just feeling his wings as an instructor.

STURCHIO: What was Speed Marvel like in those days?

BECKMAN: Oh, he was a very likeable person, friendly. He commanded the respect of everybody, first of all from his knowledge and competence, and also from his general behavior. He was a great fellow.

STURCHIO: Was he playing poker in those days?

BECKMAN: I presume so. I was not in the poker playing crowd at that point. He and Roger Adams, I know, had a reputation for that.

STURCHIO: Shortly after you contracted mercury poisoning working on dialkyl mercury compounds with Speed Marvel you switched over into physical chemistry, starting to work then with Worth Rodebush. We have here in the Illinois Chemist a description of "Thermodynamics" Rodebush.* I wonder if you'd tell us about your relationship with him and what he was like.

BECKMAN: Well, he came to Illinois from Berkeley, the University of California at Berkeley. He had a reputation for being a tough professor. You really had to come up with good solid, well-reasoned answers if you were going to get favorable reports from him. He was, oh, a little aloof, not like Speed Marvel. Speed Marvel was one of the gang. I never felt Rodebush was quite like that. More reserved.

STURCHIO: You took his courses in physical chemistry?

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*Illinois Chemist, 6, no. 1 (November 1921): 14.

BECKMAN: Oh, yes.

STURCHIO: He had worked with G. N. Lewis at Berkeley. This was really the cutting edge of research in thermodynamics and physical chemistry at the time. What did you feel about the material you were learning in Rodebush's classes and your other courses?

BECKMAN: Well, I thought it was interesting and worthwhile. When you mention thermodynamics, see I've had three types of thermodynamics. Under Rodebush I had chemical thermodynamics. Then I was in engineering, I had Goodenough--thermodynamics of steam engines and things like that, you know. Then when I went out to Caltech and got thrown in with some physicists like Fritz Zwicky, I got the physicists' concept of thermodynamics. So, I really had a lot of exposure to thermodynamics.

Sturchio: And how did you start to work with Dietrichson? That was just an assignment as a teaching assistant?

BECKMAN: Yes. He had...also there's White, T.A. White was doing some research on the thermodynamic properties of aqueous ammonia solutions. He needed a research assistant, so I started in with that. Then Dietrichson had given me a job as a helper, an assistant, in the course in electrochemistry. This I look back on also as a factor in guiding my career, because I did gain some first-hand experience with making standard cells and calomel half cells, hydrogen electrodes, and things of that sort. So maybe that was a factor also in Glen Joseph coming to me. He knew that I had worked with hydrogen ion determination at Illinois. I hadn't thought about that, but it was a factor that had him come to me.

STURCHIO: Had Glen Joseph been doing similar work at Illinois, or was he in a different field entirely?

BECKMAN: No. Different field. He was, well, we would call him now more of a biochemist.

STURCHIO: Also at Illinois at the time, let me ask about some of the other faculty, because one would think that the Arnold Beckman who would later found Beckman Instruments might have been in contact with them. Let's just see if you have had any contact with people like Samuel Parr, who was near retirement, but was...

BECKMAN: Yes, he was quite famous at the time I was there. The Parr calorimeter, for example, was a standard instrument over there. He was an older person and highly respected. He wasn't active very long, after I left, as I recall. But I recall going through the standard course on using the Parr calorimeter.

And of course, then there was W. A. Noyes, Sr. I took organic chemistry lectures from him and, as you pointed out earlier, this was after World War I. The World War had a great effect of him, and I appreciated his lectures, because he would bring in something more than organic chemistry. He would bring

in philosophy. He was very much affected by the war with the Germans because he had many friends in Germany, and he would wander off into philosophical discussions. He'd lecture us about unfortunate aspects of human character that permit them to generate hatred and fight each other, things of that sort. I got a lot out of his lectures other than just some knowledge of organic chemistry.

THACKRAY: To go back for a moment to Samuel Parr. He had been the essential founder of the Illinois department and his calorimeter had had a very wide use in its day. The utility of that calorimeter, its general acceptance in the chemical community, is something that you would automatically be aware of.

BECKMAN: Well, it was the method, as I recall. There wasn't really any competing method.

THACKRAY: And he was a sort of elder statesman in the department?

BECKMAN: Yes, right. He was elder statesman at that time.

STURCHIO: Had Donald Keyes arrived when you were still there?

BECKMAN: No. He may have come in my last year, but at least I didn't know him there.

STURCHIO: And also in industrial chemistry was G. Fredrick Smith, I believe.

BECKMAN: Oh yes. Well, I wouldn't call him an industrial chemist. He was an analytical chemist who got involved in a successful chemical business. We used to call him G. Frederick or we used to call him "Perchlorate" Smith, because he made perchloric acid and perchlorates. And he was a very jovial person, full of jokes. He also was one of the inventors of the aerosol type of dispensers of cream. And at that time he was involved in quite a bit of litigation. As I recall, he had a patent to use nitrous oxide as the aerosol. And if you used carbon dioxide, something like that that had enough pH, it would pretty much curdle the milk. So nitrous oxide had some advantages. As I recall he was involved in litigation for several years before his patents were finally sustained, I think.

THACKRAY: That statement and the linking with Parr does point to a sort of style of doing chemistry that was very different from say, Johns Hopkins, which was much more academic with a capital A. Were you and other Illinois people very aware that Illinois was different?

BECKMAN: I would say Illinois was more of a down-to-earth, practical school. For example, it did a lot of agricultural chemistry. That was a big part of what we used to do. Soil analyses and

water analyses for citizens of the state. We had a special division in there just doing that. So it, yes, it was much more oriented to practical uses than Johns Hopkins.

THACKRAY: Did students get drawn into that at all? The water analysis, the soil analysis?

BECKMAN: Oh yes, they had courses, specific courses in these various things out there. I forget what the course was called now that involved running calorimetric methods, but that was part of the standard training for at least chemical engineering students, I think for chemistry students also.

STURCHIO: The industrial connections certainly were evident to students at the time. Here's another issue of the Illinois Chemist with an editorial on the chemist and the business world in which the editor undoubtedly argued that it was important for students to know something about business, since students who were successful in chemistry in the industrial world were going to need to know more than chemistry.*

BECKMAN: Well, I have to accept credit or blame for these, though I'm a little surprised as I read them now. (laughter)

THACKRAY: A prescient article.

BECKMAN: I'd hold to that.

STURCHIO: How did you get into the Illinois Chemist?

BECKMAN: I really don't know. I was active in Alpha Chi Sigma at that time and maybe it was through that connection that I... I really don't know how it was started.

STURCHIO: Well, before long you had ads from Central Scientific and a number of other companies. What sort of response did it get from the alumni and from faculty and students?

BECKMAN: Well, the thing was financially in the black. I don't think it made any money, but at least it could carry itself primarily from advertising rather than from sales to the students.

STURCHIO: Do you recall recruiting Rodebush to write on "A Physical Chemist in Industry"?

BECKMAN: Probably I did that. Yes, that was my job. Rodebush and B. S. Hopkins and the others who wrote the different

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* [Arnold O. Beckman], "The Chemist and the Business World," Illinois Chemist, 6, no. 3 (April 1922): 81.

articles in there.*

STURCHIO: As long as we're on the subject of the Illinois Chemist and the student body, I wonder if we might ask you about some of the other chemistry students who were at Illinois at the time. Glen Joseph will come up again, as you had a very important encounter with him later on. But your class included Wallace Reed Brode, who went on to become a Guggenheim Fellow at Leipzig and eventually president of the ACS, and Clifford Rassweiler, who worked for Du Pont for many years and also became president of the ACS. Do you recall having dealings with them or any of your other classmates?

BECKMAN: Yes, Wallace Brode--he was one of triplets as you may recall. They were in spectrophotometry, so I had a lot to do later on with Wallace Brode in that field. Cliff Rassweiler--I had no direct technical connection, but I saw him quite often at chemical society meetings and things like that and knew him very well.

STURCHIO: Were they fraternity brothers of yours at Illinois?

BECKMAN: Not social fraternities. I imagine they were both members of Alpha Chi Sigma, because most of the active chemists were at that time.

THACKRAY: Would you talk about the fraternites, the chemical and social fraternities. Were you in both these worlds, or...

BECKMAN: Oh yes. I lived in the Delta Upsilon house, the DU house. That was the social fraternity. And Alpha Chi Sigma had its own house. Of course, then we had the honorary fraternities, Phi Delta Epsilon, and also Sigma Xi. I'm trying to think of another honorary fraternity for graduates only.

STURCHIO: Was that Tau Beta Pi?

BECKMAN: No, Gamma Phi... This may have been a local for Illinois, as far as I know.

THACKRAY: Just to stay with the sorts of worlds theme, was the Illinois student body in chemistry small enough so that you would know all the names of the other freshman, sophmores, juniors or...

BECKMAN: I would know many of them, but I'm not sure I would know all of them, because it was a sizeable department then.

STURCHIO: Did you have contacts with some of the other people who must have been on campus at the time, for instance, John Johnson and Wallace Carothers?

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* W.H. Rodebush, "A Physical Chemist in Industry," Illinois Chemist 6, no. 1 (November 1921): 7-8; B.S. Hopkins, "Spectroscopic Study of the Rare Earth Group," ibid, 6, no. 3 (April 1922): 74-75, 86-87.

BECKMAN: Yes. Jack Johnson--he went from Illinois to Cornell. He was outstanding, he was one of my teachers, too, an outstanding organic chemist. And I know Wallace Carothers went to Du Pont and invented nylon. Dr. Du Vigneaud, do you have his name down there?

STURCHIO: Yes, V.R. Du Vigneaud.

BECKMAN: He went to Cornell Medical School and won a Nobel Prize later. I recall him well.

STURCHIO: Did you have much contact with people like Du Vigneaud and Carothers at Illinois, or was it...

BECKMAN: Well, I didn't work with them in the laboratory. But of course, at frequent seminars and things like that we'd hobnob and get acquainted with each other.

STURCHIO: How about Samuel McElvain, who later went to Wisconsin. He was class of '21.

BECKMAN: I don't recall him so well. The name is vaguely familiar, but I don't recall anything special about him.

STURCHIO: And then, the year you got your masters--you mentioned Bill Hincke, who went to Caltech with you. And Ralph Shriner got his masters the same time you did.

BECKMAN: Yes. I saw him later on, of course, when he came back to Illinois.

STURCHIO: And C. D. Graves got his Ph.D. that year and went to Du Pont and worked with Carothers.

BECKMAN: I don't remember Graves.

STURCHIO: And Albert Elder, who was...

BECKMAN: Yes, that name is familiar, but I don't recall what he looked like. But that name is familiar.

STURCHIO: To get back to the fraternity question, having mentioned some of the other people who were there (and you also said that you would see them at colloquia and lectures), reading again through the Illinois Chemist it's clear that the chemical fraternities would have visiting professors for an evening of talks, or Roger Adams might give a lecture on Mellon Institute fellowships and things. How big a part of social life within the department was that? How important do you think it was to Illinois chemists and their later careers to have that kind of exposure?

BECKMAN: Well, you'd have to divide the students into two groups. Those chemists who lived in the Alpha Chi Sigma house. I recall they had thirty or forty--something like that--living in

the house. This would be a big part of their routine. For those who lived in social fraternity houses, it was nowhere as big as that.

THACKRAY: And which group were you in?

BECKMAN: I lived in the Delta Upsilon house.

STURCHIO: One other question that I had about Illinois--you've given us some interesting background on some of the other aspects of it. But you said, I believe in the Caltech interview, that you also had offers to go to MIT and Chicago when it was time to go to graduate school and decided, as we know, to go to Caltech.* Do you recall what it was that attracted you to either MIT or Chicago?

BECKMAN: Oh, they were the recognized top schools in the field of chemistry and chemical engineering.

STURCHIO: Did one of your professors steer you toward any one of the three?

BECKMAN: No, I think when the time came to apply I just sent out several requests for admission. That was it. I don't recall any professor pushing any particular university. Except to say that Tolman did mention the advantages of Caltech being a small school. Also, as I mentioned earlier, I think the attraction of the state of California as such played a role. I was young and curious to see what this fabled land was like.

STURCHIO: You didn't think about staying at Illinois?

BECKMAN: That was an option. But there was a feeling often expressed that a student should change schools, not have all of his training in one school. And this is a philosophy that in fact we had at Caltech. We urged even our good students (even though we had to lose them) to go somewhere else to get a different view.

STURCHIO: So in the the fall of 1923 after your trip out west with William Hincke, you ended up at Caltech. As you said before, it was partly Tolman's going to Caltech at the time that leaned you in that direction. And Tolman's style of physical chemistry (or chemical physics) was something that was more aligned with Lewis's school at Berkeley. You'd had contact with Rodebush who'd come from that school. What was Caltech like and what sort of things did you do that first year?

BECKMAN: Well, I taught freshman chemistry, for which I was paid, and took the usual courses. Chemists at that time had to take quite a bit of physics. This was when quantum theory was in its very controversial stage. Our text was Arnold Sommerfeld's

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* Beckman interview, Caltech Archives (1981), p. 13.

Atombau,* which we had to read in German which was bad enough in itself, especially since Sommerfeld's style was not particularly clear. To try to get this controversial quantum theory--that was a tough job. People like Pauling and that could go through it, but I know I had a struggle with it. But it was exciting.

We had visitors, of course. Einstein was there, Sommerfeld himself was there, Heisenberg came in, Bohr--the real chief actors in the drama visited Caltech. This was undoubtedly due to the preeminence of Millikan and Hale, plus Noyes's emphasis on, if we're going to be in a discipline, we want to be second to none. So they went out of their way to bring these leaders to Pasadena so the students could be exposed to them.

STURCHIO: You referred in an earlier interview to the atmosphere at Caltech being very intimate between graduate students, because of the size.

BECKMAN: That's still the situation.

STURCHIO: Could you talk about the contrast between Illinois and Caltech in that first year, in addition to the luminaries coming to visit? What other differences did you find?

BECKMAN: Well, you have to keep in mind when I was 19, 20, 21 years old, I looked up to any professor with a good deal of awe. When I got a little older, some of that awe diminished. Nonetheless, when I went to Caltech I remember that when Millikan called me in to see him, I went in with a great deal of trepidation. Later on, when I got to know Millikan, those feeling disappeared. I found that the faculty and the student body worked closely together. A student had no difficulty seeing a professor. He was always welcome. That was one of the positive features of being in a small school like Caltech.

STURCHIO: Apparently Noyes was known as "the King."

BECKMAN: That's right. He was called "the King" and Millikan was called "the Chief". How they got those names, I don't know. They just developed.

STURCHIO: What did that reflect about Noyes's style in running the chemistry department?

BECKMAN: Well, I think it was the fact that both were at the top. The top of the heap. You know the King was not, what shall I say? He didn't throw his weight around, tell people to do this this way. But his influence was quietly felt. For example, he set the policies of the Insitiute. And, he was a very shy person, just the opposite of Millikan. You'd almost never see Noyes at a public gathering, while Millikan would always be

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* Arnold Sommerfeld, Atombau und Spektrallinien (Braunschweig: F. Vieweg, 1919). Revised editions appeared in 1921, 1922, and 1924.

there, usually fundraising as well as talking about science.

THACKRAY: The explicit style of Caltech seems in so many ways different from Illinois--a large institution, a small institution, and so on. What about the industrial connections and linkages? Did you sense a difference in style there?

BECKMAN: Caltech did not have a chemical engineering department at that time. They did have what's called applied chemistry; Dr. William Lacey was the head of that. In fact, he was it. Later on Bruce Sage came in and the two of them... Well, this was a recognition that there was in fact an applied science side of chemistry. Dr. Noyes knew that. But the curriculum was rather limited; it was devoted more to theoretical aspects of chemical engineering than it was to the experimental side of it. And it became rather narrowly focused, particularly when Bruce Sage came to study the thermodynamic properties of hydrocarbons under various conditions. That took up a big part of their time.

THACKRAY: Did students like yourself move naturally between that and the regular chemistry or were these really separately organized?

BECKMAN: No, there was no sharp borderline at all.

STURCHIO: Did you get to know Roscoe Dickinson your first year there?

BECKMAN: Yes, I did.

STURCHIO: Had you made a decision who you wanted to work with that year, or was that after you came back to L.A.?

BECKMAN: No, that was after I got to Caltech. Dr. Tolman decided that Dickinson might be a good person to work with. He was a good experimentalist, and I enjoyed working with him. But he was working with boron X-ray diffraction, and I was not so much interested in that. I was interested in applying quantum theory to the study of chemical reactions.

STURCHIO: So that dates from your very first year at Caltech?

BECKMAN: Yes.

STURCHIO: As you told us earlier, at the end of that year you had a chance to go back east. After traveling through the Panama Canal on a steamer back to New York to see the future Mrs. Beckman, you ended up with a job at what became Bell Telephone Laboratories. I wonder if we can move to that for a while. First, why was it that you went first to Standard Oil? Was it just that you knew of, that Standard Oil was ...

BECKMAN: I knew of them.

STURCHIO: And they needed analytical chemists?

BECKMAN: Right.

STURCHIO: But later that afternoon, you found your friend from Caltech, Todd Niece, I believe.

BECKMAN: Todd Niece, yes.

STURCHIO: And ended up working with Western Electric Engineering in Walter Shewhart's area of statistical analysis for quality control. Shewhart's work in inspection engineering was an aspect of Bell Labs that wasn't really connected with chemistry at all. Did you have any contact with the chemists at Bell Labs while you were there?

BECKMAN: Not really. No, the only contact would possibly be an occasional talk at a seminar or something like that, but I was never in the laboratory. In fact that was one reason why I wanted to go back and get my degree. I missed the association of being with chemists and the smells of the laboratory and being able to do things with my hands.

THACKRAY: Did you at that moment have a conscious determination of where you were heading, what career you wanted or felt you were going to ...

BECKMAN: No. In all honesty, I wasn't thinking that seriously about the future. I needed some money to go back to school and had a girlfriend there. Those were the thoughts that were uppermost in my mind.

THACKRAY: That should be extracted and put in a text, because I think very often students worry that they haven't got a clear enough...(laughter)

BECKMAN: No, I never worried about that. I think one reason may be the way I spent the summer of 1919. Remember I told you how I bummed out west with a friend? I learned I could take care of myself no matter what. So what my future life was going to be really never was a great concern to me.

THACKRAY: That's very interesting. It's a good argument for having breaks between high school and college, and college and graduate school.

BECKMAN: That's right.

STURCHIO: Well, while working with Shewhart on statistical analysis and quality control, in the West Street Laboratory of Bell Labs you mentioned that there was a vacuum tube shop run by Mervin Kelly at the time.

BECKMAN: Yes, down on Hudson Street.*

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* See M.J. Kelly, "The Manufacture of Vacuum Tubes," Bell Laboratories Record 2 (June 1926): 137-144.

STURCHIO: This must have been your first encounter with the vacuum tube?

BECKMAN: Yes. I recall that when we applied the inspection techniques we found out that Western Electric was throwing away its money doing inspection on tubes, because all of them fell within the limits. There were no defects. (laughter.) It was one of the first applications of this technique.

STURCHIO: Is that what piqued your interest in these tubes?

BECKMAN: No, I would say more it was the seminars that they would hold from time to time on applications of electronics--curcuits, telephonic communications, things of that sort.

STURCHIO: There were also in the Bell Labs Records and in the Bell System Technical Journal at the time a number of articles on curcuit theory and applications in the telephone industry. There was Karl Darrow's series on the quantum theory. Did you follow those while working there?

BECKMAN: Well, yes at the time. I was often envious of the editor. Here's a fellow who didn't have to do anything except read the literature and keep informed on what was going on. I say what a heck of an interesting job that was. And he did a good job, too.

STURCHIO: It was Darrow who wrote them?

BECKMAN: Darrow. That's right.

STURCHIO: Those were read by most people at the Lab?

BECKMAN: Oh yes, I think so.

THACKRAY: Once again, going back to style, Bell Labs obviously was not an academic institution. Did you feel a very great difference in the workaday world or was it really not all that different from Caltech's ambience in certain ways?

BECKMAN: I don't think it was all that different, because my job was applying probability theory. I was doing really theoretical applications of the various probability theories to different types of inspections. I had a little field work, of course. I'd go out and inspect soldered joints or telephone relays and then apply them back. But still it had pretty much of an academic approach. We were trying to find out to what extent do the theories stand up in practice.

STURCHIO: In reading Shewhart's writings during the years on quality control charts, on applying statistics to manufacturing quality control, it comes to me very strongly that here is a man who has a very fine sense of the importance of only pushing your

data as far as they will go.* He comes back again and again to the importance of statistics in analyzing experimental data. Did you find that that was any use to you in your career when you got back to Caltech and then later at Beckman Instruments?

BECKMAN: Well, I hadn't thought of that, but now that you bring it up I think so, because I've often said that one important thing, whether in science or business, is to have intellectual integrity. Don't allow yourself to have wishful thinking. And that was what Shewhart did, he would not go on until he found some results that agreed with his preconceived ideas. He'd go along, just as completely objective, I think, as a person can be. And I think that probably I got a little bit of the spirit of that type of investigation from him.

STURCHIO: That also must have been difficult work. Here's an example from his article on quality control** of the sorts of forms that...

BECKMAN: Skew factor, kurtosis, all these things, yes. We tried many different types of probability theory--a priori, a posteriori a little bit of Bayes's theorem--like that you know, and small numbers of samples.

STURCHIO: But these all had to be calculated by hand, didn't they?

BECKMAN: Yes, that's right. That was in the days before computers.

STURCHIO: Well, that must have also increased your appreciation for doing things right...

BECKMAN: And our inspection of course also had to be by hand. I mean, we'd flip pennies, and do other sorts of things to check them. Then I would go out in the field. I recall coming here to Philadelphia, for example. This is one of the highlights of my career. I came down here to the Pennypacker Exchange. (I thought that was the strangest name!) But they're just putting in the Pennypacker Exchange, and I go in to inspect soldered joints

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* See, for example, Walter A. Shewhart, "Some Applications of Statistical Methods to the Analysis of Physical and Engineering Data," Bell System Technical Journal (BSTJ), 3 (1924): 43-87; Shewhart, "The Application of Statistics as an Aid in Maintaining Quality of a Manufactured Product," Journal of the American Statistical Association, 20 (December 1925): 546-548; Shewhart, "Correction of Data for Errors of Measurement," BSTJ, 5 (1926): 11-26; Shewhart, "Correction of Data for Errors of Averages Obtained from Small Samples," BSTJ, 5 (1926): 308-319; Shewhart, "Making the Best Use of Experience," Bell Laboratories Record, 2 (July 1926): 189-194; and Shewhart, "Quality Control Charts," BSTJ, 5 (1926): 593-603.

** Shewhart, "Quality Control Charts," BSTJ, 5 (1926): 600.

and relay contacts by different methods, to see what the reliability factor was for different percentages of sampling, things of that sort.

Then I had another interesting experience, it had nothing to do with inspections but shows how side issues come into your life. I was down one summer evening with nothing to do and walked down a street. I'm not sure of the name of the street now, but it was one where they had the medicine shows out there. Perhaps you know the name of the street, it's like Main Street in Los Angeles, I don't know what it's called here. Anyway, here was a fellow going to sell snake oil to the public. He had a banjo player out there bringing a crowd. And I like banjo, of course, so I started listening to the fellow--he was good. Pretty soon the barker came out with his oil. He's going to sell this medicine, a bottle of it will cure everything from falling hair, to stomach pains or whatnot, you know. We got a very impassioned pitch--for only $1.00 you can get this thing--but not a single buyer. I thought, gee, that fellow must be kind of discouraged after all the good banjo show there, and then not a first buyer. Then he broke out in a great big smile and said "Folks, you don't know how happy you've made me. You don't know me. I could tell you anything. But I'm glad to see that you're smart enough not to be taken in by that. But folks, You don't have to take my word for it, you can read it on the label yourself." And he passed the stuff out and the money began to come in like that. Well, from that I learned the value of the printed word over the spoken word.

STURCHIO: A valuable lesson to have. I take it you didn't buy a bottle!

BECKMAN: I didn't buy, no.

STURCHIO: To return for a minute to quantum mechanics while you were at Western Electric Engineering, and then in January 1925 it became Bell Labs. Would you say that the knowledge and interest in applying the new quantum theory was greater at Caltech than at Bell Labs, or how well informed were people in the 20s? Because these were clearly two of the centers of academic and industrial work in the twenties in the U.S. where knowledge of the new quantum theory was most...

BECKMAN: It was a number one priority at Caltech, both in chemistry and in physics. Bell Labs, I just don't know to what extend that dominated their thinking. They had so many practical problems connected with communications and inspections like that so I couldn't...

STURCHIO: Do you recall any of the colloquia that might have dealt with...

BECKMAN: No, I don't recall any colloquia or seminars devoted to quantum theory.

STURCHIO: So it was mainly just Darrow reading the literature and writing?

BECKMAN: Yes. Karl Darrow undoubtedly had articles on it, because he covered the waterfront.

STURCHIO: What sort of personal relations were there among the people at Bell Labs? Was it such a large organization that you felt lost in it? Did you deal only with Shewhart?

BECKMAN: Well, it was a large organization but they had small groups. Our group dealing with inspection had maybe 15 or 20 people, something like that. And we knew each other quite well. Two of the secretaries lived out on Long Island. I used to commute with them on the Long Island Railroad, things like that. So we had very friendly, personal relations on a small group basis.

STURCHIO: So Dodge was there also? He later made quite a name for himself in quality control.

BECKMAN: That's right, he came in after me. I was the first technical employee and then Dodge came in and, let's see, Edwards was on the side; There's a Miller, I forget his first name, who was very active, too.

STURCHIO: Was R.L. Jones somebody who was...he was just the person to whom Shewhart reported?

BECKMAN: Yes. I don't recall his role.

STURCHIO: Here we have an article from the Bell Labs Record on the Inspection Engineering Department, with information on some of the people who were there and photographs of them.*

BECKMAN: Oh, Don Quarles, I remember him.

STURCHIO: What connection did he have with your group?

BECKMAN: Well, he started carrying on parallel work as I recall in, was he in the tube shop or something like that? Anyway, in another part of the Bell system. Maybe it says it on here. Yes, Quarles had five engineers reporting to him in the apparatus inspection department.

STURCHIO: Then I believe further on are Shewhart and Edwards.

BECKMAN: Edwards, yes I remember, G.D. Edwards. And F.C. Miller. Well they worked very closely together, sort of a family relationship, our desks were not far apart. We'd see each other often. Of course I was working with Dodge. I was pretty young at the time and later on, if I had gone back there now, I probably would have looked for relationships, something like that, to which I was completely unsensitive.

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* Francis J. Hallenbeck, "The Inspection Engineering Department," Bell Laboratories Record 2 (July 1926): 243-247.

THACKRAY: So, you said that Noyes came to visit you in the spring of 1926.

BECKMAN: Yes. He didn't come just for me, he let me know that he was going to be there and so I went...he was staying at the Plaza Hotel, as I recall. I had a talk with him. He said didn't I think I ought to go back and finish my degree. And by that time I was getting a little nostalgic for the smell of the chemistry lab and I did want to get my degree anyway. I had saved enough money by that time. I thought I could finance my way through it.

STURCHIO: Did you have any thought at that time about possibly coming back to Bell Labs or did you not think that far ahead?

BECKMAN: I hadn't thought that far.

STURCHIO: So, in the fall of 1926 you were back at Caltech beginning to work on your thesis with Roscoe Dickinson. Could you tell us a little bit about how things were when you got back to Caltech and how you chose to work on photochemical decomposition, the quantum yield of photochemical reactions?

BECKMAN: The quantum theory was a hot topic at that time and I got a grant with Roscoe Dickinson through Tolman. He reported to Tolman on the scale of authority out there. Tolman was not an experimenter. He was a theoretical physicist, so he suggested I work with Roscoe Dickinson if I wanted to do experimental work. It just evolved that way.

STURCHIO: In 1928 as part of that work you published this paper on a quartz fiber manometer that was part of your research.* You said before that you were always interested in working with your hands and building instruments, and that you had been doing some work on instrument making for the Caltech department. Was that around this time or after?

BECKMAN: That was at the same time and as part of my work assignment. I'm not sure if I was still a student, but certainly when I went on the faculty, I was given custody of the instrument shop. So I had a close association with instrument making and instrument maintenance. In fact I had a course in instrumentation. I taught that by using saccharimeters and things of that sort. So I taught that as part of my job.

STURCHIO: It was a very different course in instrumentation than one that would have been given 10 or 15 years later...I guess it was a very traditional course.

BECKMAN: Yes. It was just very routine instruction on how to use standard instruments, saccharimeters, refractometers, and

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* Arnold O. Beckman, "An Improved Quartz Fiber Manometer," Journal of the Optical Society of America, 16 (1928): 276-278.

things like that. Nothing in the way of theoretical development or anything like that. Just a practical course primarily for industrial chemists. Those who go around and work in the sugar mills, for example, have to know how to use a refractometer.

STURCHIO: Would you have taught them how to use hydrogen electrodes and that sort of thing in that course?

BECKMAN: No. We didn't teach that. These were primary... I don't think we had any hydrogen electrodes taught in that course at all.

STURCHIO: That would have been a much more advanced course?

BECKMAN: Yes.

STURCHIO: If we could just go back for a moment to Dickinson. You told us earlier about Dickinson and Tolman and Noyes. Some of the other people around the Caltech department by the late 1920s included Linus Pauling and Ralph Wyckoff. Could you tell us something about them, about the people you were interacting with?

BECKMAN: Of course they were both in X-ray diffraction work, which is not my field. I can't tell you much about what they did. Corey was another who did very good work in the X-ray area. This was the time when Pauling was working on his hydrogen bond theory. He was a very active, influential member of the group of chemists out there. Wyckoff I don't know too much about.

We had of course Lucas in charge of the organic chemistry department. He was one who brought a lot of physical chemistry into organic chemistry. I don't know how well that is recognized, but he was constantly making organic chemists know physical chemistry and how to apply it in their research. Stuart Bates taught physical chemistry. And Ernest Swift taught analytical chemistry. He was a very tough professor, I'm told by the students who had to take courses from him.

STURCHIO: Is that the same person you later rented a building from in Pasadena?

BECKMAN: Yes.

STURCHIO: I see, so you knew him...

BECKMAN: He owned a building out on East Colorado Street. He rented it to a dry cleaner who couldn't make a go of it, so we rented it from him. An exorbitant rental of $50 per month for Main Street frontage! That was the Depression--he was glad to get $50 a month.

STURCHIO: We should come back to that because that's the first place into which Beckman Instruments (at that time National Technical Laboratories) expanded.

BECKMAN: That's right.

STURCHIO: You mentioned some of the faculty who were at Caltech at the time and a little bit about the interest in quantum chemisty, the applications of the quantum theory to chemistry. Did you recall if there was much contact between Tolman and Dickinson and the other students who were doing research with them and Lewis's school at Berkeley, for instance? It was another center for that kind of interest.

BECKMAN: As far as the link of Berkeley and Caltech, I'm sure there were cordial relations there. But, I was not in that loop directly, so it was only by indirection that I would know that. We frequently had people down from Berkeley in our seminars. That's one thing that Dr. Noyes had, regular weekly seminars in which one of the main items was the preparation of cocoa. Have you heard that story?

STURCHIO: No, please tell us.

BECKMAN: Well, we served cocoa. Dr. Noyes in his typical method, his book on quantitative analysis procedures, everything is spelled out in detail. Well, that's the way he spelled out making the cocoa. The cocoa had to be made directly from cocoa beans, no ground cocoa. So he had printed up on the back of the door how long you had to boil the beans, when you had to strain them. This was the test of the skill of the incoming graduate students. Every graduate had to take his turn and make cocoa for the seminar.

STURCHIO: That's the sort of detail that we don't get from the history books. That Noyes's style percolated down even to that level. Who were some of your fellow students at that time? Did you share a lab with anyone?

BECKMAN: Bill Hincke got his degree the same year or the next year after I did. No, I didn't share. I had my own laboratory.

STURCHIO: Did Dickinson have other students at that time?

BECKMAN: Yes, a few others. I remember there was Jimmy Ellis. He wasn't a student. He was a faculty member who came from MIT with Noyes. He played quite a role in shaping the way chemistry was taught. Oh, and Jimmy Bell was the professor of general chemistry. All Caltech freshmen had to take a year of chemistry at that time. Jimmy Bell was in charge of that. I also took my turn giving lectures on that. That was part of what I liked. I devised new types of electrochemistry apparatus to illustrate phenomena, particularly chemical phenomena.

STURCHIO: Robert Barton in the Beckman history said that you had quite a reputation for those demonstrations.*

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* Stephens, Golden Past, Golden Future (1985), pp. 24-25.

BECKMAN: Oh, and Clarence Dunn--he went up to Shell Development until he retired. There's Sam Eastman. Dames and Moore set up a geological consulting firm together while they were there. And McMillan--later on, he won a Nobel Prize. He was in my class. We divided the freshmen students into sections of about 20 each. There were 160 freshmen. The top section was the honor section and I had the pleasure of manning it. He was one of the members of that.

STURCHIO: Do you have an impression of where most of the other people who were getting their Ph.D.s at that time ended up? You stayed on at Caltech. Was it common for your class members to go into academe?

BECKMAN: Yes. Most of them went into academe. They still do. I mentioned the emphasis in the chemistry department was on basic research and not upon either applications or applied research. Charles Holden Prescott was another one of that period, too.

STURCHIO: If we could, let's go back to something I meant to ask you about. You coauthored a paper with A.A. Noyes on a new interpretation of the periodic table based on some of the new ideas on atomic structure.* How did that come about?

BECKMAN: Oh, "the King" was interested in this. This is really not a very profound thing. We made a big chart of this thing. We presented it in Reno at the meeting of the American Chemical Society out there one time. It was just one of those things that came along. There was, of course, a lot of interest at that time in atomic structure and the periodic table.

STURCHIO: Do you recall any of the reactions to that paper? Did people write to you about it? Was there anything interesting following it up?

BECKMAN: No. The thing I remember about that mainly was we drove up to Reno in Dr. Noyes's "Brown Betty", the big Cadillac he had. And coming back we had to cross some irrigation ditches and water got splashed up into the engine. We had to wait for a service man to come out. So we camped overnight out there and Dr. Noyes made cornbread his way. Cooked the cornmeal and made a mush out of it and then he put that over the open fire, so we had the pleasure of eating cornbread that Dr. Noyes himself made. Oh, and Don Yost was with us, too.

STURCHIO: It was quite a group at Caltech in those years.

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* Arthur A. Noyes and Arnold O. Beckman, "A Periodic Table of the Structure of Atoms and Its Relation to Ion Formation and Valence," Proceedings of the National Academy of Sciences, 13 (1927): 737-743. See also Noyes and Beckman, "The Structure of Atoms as a Periodic Property and Its Relation to Valence and Ion Formation," Chemical Reviews, 5 (1928): 85-107.

BECKMAN: Yes, they really were.

STURCHIO: We should move on to some of your thesis research itself. I believe this is your first paper in the Journal of the American Chemical Society on the research. Certainly the method you were working out was very successful because you then went on to publish a series of papers using the apparatus that you constructed.* Would you tell us a little bit about that set of reactions?

BECKMAN: Well, we wanted to study a molecule that was not too complicated. Ammonia had been studied, so hydrogen azide was the next one to come to mind. The thing I remember about that was I could have lost my eyesight because hydrogen azide is a very explosive substance. I had to make it there, and I had just a drop or two of it in the vacuum seal. We kept it frozen with liquid air but then somehow we took it off and something got disturbed enough so that it blew up and blew glass in my eyes. It shows you how lucky I was; I had glass in both eyes but it didn't get into the pupil. Just on the outside of the eyes. It was painful as the dickens. I had to hold my eyelids out and somebody took me down to the doctor's. He probed around--which is not a pleasant experience because glass has about the same index of refraction as tears so you couldn't see the glass in there. The only way to find it is to probe until you hear a little grating noise. Well the half of it, we were having a party, the students at our house that night. So I went home with a patch on one eye. The next day that eye was alright but the other eye hurt. I had to go back to the doctor again and this time I went home with a patch on my other eye. People would say he can't make up his mind. But I realize how lucky I was. I could have destroyed my vision. From then on I had a protective glass shield.

STURCHIO: That would have given me second thoughts about studying that particular reaction. Were you pleased with the results you got from that line of research?

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* Arnold O. Beckman and Roscoe G. Dickinson, "The Products of the Photochemical Decomposition of Hydrogen Azide," Journal of the American Chemical Society (JACS), 50 (1928): 1870-1875; Beckman and Dickinson, "The Quantum Yield in the Photochemical Decomposition of Hydrogen Azide," JACS, 52 (1930): 124-132; L. Reed Brantley and Beckman, "The High Temperature Equilibrium of Titanium Dioxide and Carbon with Titanium Carbide and Carbon Monoxide," JACS, 52 (1930): 3956-3962; Ralph R. Wenner and Beckman, "The Quantum Yield in the Photochemical Decomposition of Gaseous Hydrazine," JACS, 54 (1932): 2787-2797; Albert R. Myers and Beckman, "The Mercury-Sensitized Decomposition of Hydrogen Azide," JACS, 57 (1935): 89-96; Norwood L. Simmons and Beckman, "The Mercury-Photosensitized Decomposition of Arsine," JACS, 58 (1936): 454-459; and Henry J. Welge and Beckman, "The Photodecomposition of Ammonia," JACS, 58 (1936): 2462-2467.

BECKMAN: Yes. That was published in...

STURCHIO: 1930. What I was trying to get at is--you got your Ph.D. in 1928 and (in reading these papers one can see) you were beginning to develop your own style that incorporated instrument making, the quartz fiber manometer, this somewhat complicated vacuum system, with a sophisticated knowledge of both analytical and physical chemistry. The next element that comes in is the electronics that you picked up at Bell Labs. I just wondered what your thoughts were as a young faculty member around 1930 about the research program that you were planning to follow for the next few years?

BECKMAN: Well, I wasn't looking very far into the future. What I was doing was interesting and what it might lead to, I really didn't give much attention to. I was doing a lot of miscellanous things for people I mentioned earlier, people who were coming to ask me to do a little glassblowing for them because the glass blower at Caltech was in the physics department. Chemistry was down low on the priority list for getting his time, so I spent a lot of time helping them out, making glass things for them which led finally to where I gave a course of instruction in glass blowing at Caltech. Well that sort of thing took up my time. Also I helped William Shockley by making some thermocouples for him. So I was kept quite busy helping other people in their research. I was in charge of the instrument shop, so that took up time. It wasn't just a case where I was sitting there doing research. I was doing lots of miscellaneous things.

STURCHIO: And you began to attract graduate students over the next few years. Here's a paper in 1933 with Reed Brantley.* Again, the same general research area, still studying quantum yields and photochemical decomposition of various reactions. Where did Brantley go?

BECKMAN: He wound up the head of the chemistry department at Occidental College. He stayed there the rest of his active life.

STURCHIO: And how about Ralph Wenner? Do you recall where he went?

BECKMAN: Oh yes. Ralph went back and unfortunately he died. We went on several trips to the desert with Ralph. I recall one trip we were going up into Yosemite Park. It was cold and we built a campfire. He got cold, so he spread his blanket over the place where we had the fire, a little hotter than he thought. He ended up with his blanket catching on fire. He was killed unfortunately in an auto accident just 2 or 3 years after that.

STURCHIO: Oh, that's terrible. Albert Myers was another student of yours.

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* Brantley and Beckman, "High Temperature Equilibrium of Titanium Dioxide...," JACS (1930), cited in note on page 23.

BECKMAN: Albert Myers, he ended up working for some industrial company. I think it was one of the oil companies but I don't recall which one.

STURCHIO: Myers was working with you right about the time you began to get involved with the National Inking Appliance Company. The end of 1934, shortly before or just about the time that Glen Joseph came with his problem about measuring the pH of lemon juice that had been dosed with sulfur dioxide. The story of Glen Joseph and the origins of pH meters is something I would like to get in more detail. This is where the electronics comes together, and as you said in some of the earlier interviews, had it not been for your experience at Bell Labs maybe you never would have thought of using a vacuum tube voltmeter when Glen Joseph came.

BECKMAN: Well, I think I'd correct that now. Maybe it was my experience building hydrogen electrodes at Illinois that brought him in to see me. That probably was more like it. Now my willingness to help him with his problem--there the Bell Laboratory experience came in. Probably he was brought in because of my laboratory assistant work at Illinois.

STURCHIO: Well, it's interesting how these seemingly unrelated episodes come together. Before we get to that, though, let me just ask you finally about the late 20s at Caltech, the early 30s. How would you say that the laboratories in Illinois, Bell Labs and Caltech compare? Probably Illinois and Caltech more particularly, since you were't involved in chemistry at Bell Labs.

BECKMAN: I think from a standpoint of pure chemistry, Illinois stood out. Bell Labs did chemistry, but it did a lot of other things, mainly physics, more physics than chemistry. That's when they did the Davisson-Germer experiments, you know, and they were going ahead in basic physics research. Caltech was still a pretty small and unknown entity at that time. There was no one among the inner circles. But the general public was very much aware of Caltech.

STURCHIO: Did the kind of work consulting for the National Inking Appliance Company, or rather the consulting work that led to that (the inking device and National Postal Meter were connected with that, of course)...Was that kind of work encouraged by Noyes and Millikan in the hope that it would raise funds for Caltech in the long run?

BECKMAN: It was not encouraged by Noyes, because he was never really involved in fund raising at Caltech. He would never go out and address a Kiwanis Club meeting, for example. That was Millikan's job. And I suspect Millikan always had in the back of his mind that thoughtfully raising funds, whatever he did, he was a master at that. And also there was at the time a perceived conflict between science and religion. He had a religious background. His wife was a daughter of a Baptist minister.

Millikan's father was a minister. Millikan, because of that did a masterful job in breaking down the suspicion that developed between scientists and religion-oriented people. Plus the fact that he was a very friendly, outgoing person. He had little difficulty in raising enough money to keep Caltech going on the way he wanted to run it.

STURCHIO: But people continued to approach Caltech for technical advice?

BECKMAN: The people came in and particularly we had a lot of prospectors come in with claims at that time. In the late 20s colloidal gold was one of the popular fads. They would come in and say they had discovered a deposit of gold that could not be assayed by the conventional assay method, the fire assay method. But that's why they would come to us to see whether or not we could give them a really true assay. How they knew they had colloidal gold, I don't know, but in fact they didn't have it. But they would come in, sometimes with buttons of gold and even gold and silver alloyed together. They'd say they got that from a secret deposit in the desert. So these people had to be dealt with in some way, even though they were a nuisance. This was when ultraviolet light was being taught. Some of them had developed ultraviolet light techniques of extracting gold, a lot of phony things.

California at that time was the focus of oddballs. It still is. But we had more goofy people coming in and we just had to be happy now and then that a legitimate person came along like I.H. Lyons and National Postal Meter with a legitimate technical problem. Or Union Lithograph. I solved a problem for them. I think that Millikan felt that this is part of the obligation that Caltech owed to the community. It helped them solve their problems.

STURCHIO: You mentioned Lyons and the National Postal Meter Company. Here's one of your patents on an inking device.* Was that your first patent, or the first one that you applied for?

BECKMAN: No, my first patent was on a device put on a speedometer to tell you when you were up to a certain limit.**

STURCHIO: When was that?

BECKMAN: That was when I was about 26 or 27, before I got my doctor's degree.

STURCHIO: And how did that arise?

BECKMAN: I think I had an offer for $600 from the Chrysler

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* Arnold O. Beckman, "Inking Device," U.S. Patent 2,041,740, issued 26 May 1936 (application filed 8 June 1934).

** Arnold O. Beckman, "Signaling Device," U.S. Patent 1,684,659, issued 18 September 1928 (application filed 7 May 1927).

Corporation and I don't think I took it. You might draw the conclusion from that that my interest in that was derived from the number of tickets I was getting for fast driving. (laughter)

This though, the National Postal Meter Company, had an inventor by the name of Jewell who had a patent of sorts on the concept of reinking typewriter ribbons at the same rate that the ink was used up in typing. But he didn't know how to do it. So that problem was tossed in my lap and I finally came up with this solid ink device to fit different typewriters. Well, as soon as they put it out in the field, it became apparent that that was not going to be a success. The last thing secretaries wanted to do was to get their fingers dirty with ink to save the boss a seventy-five cent ribbon. So I encouraged him to get out of that and he did. But that was where the name "inking appliance" originated. We hoped that this typewriter inking device would do it.

STURCHIO: I'm interested in this case and in the earlier one, how did you go about getting a patent? Who did you turn to for advice?

BECKMAN: Lyon and Lyon. They were the big patent attorneys in Los Angeles.

STURCHIO: I see. Is that the same Lyon as...?

BECKMAN: No that's a different Lyon. "Buzz" Lyons, I.H. Lyons, was the president of National Postal Meter. This is Leonard Lyon and I knew them from other patent things. Then also he was on the Board of Trustees at Caltech when I was so I got to know him.

In fact you might be interested in a little story about the Cox Oil Process. This goes back to 1932. Leonard Lyon called me up and said he wanted me to come in and talk to a client who had a problem. Do you know about the Dubbs petroleum process? Carbon Petroleum Dubbs, you know, was the one who got the patent on that. A group out there called Cox Oil Refining Company picked up, they said, from where Dubbs left off. But Dubbs really didn't know the whole story. He got the measurable part of it, but the man who really knew about it was a fellow named Cox who worked with Dubbs in the Santa Maria oil fields. At that time oil refining was a batch process. They would get a still and boil it down to coke. Then they had to go in and open it up and chisel off this hot coke so they could get ready to distill the next batch. Well, that's what Dubbs and Cox were doing. Dubbs got the patent on, instead of taking the liquor down to coke in the retort, getting it really hot and then put it over into another insulated thing, away from the hot fire and let the reaction take place there. So they avoided this chiseling off. But Cox was the one who had to go into this hot still (they put wet burlap on their feet) and chip off this carbon.

Well, Cox and a promoter called Weatherill seemed to come out with the story that Cox really had the secret on this and Dubbs didn't know the whole story. Such was the interest at that time that they put on a tremendous stock selling campaign. Weatherill was a Hollywood character. He wore jodphurs and that's when they had the 16 cylinder Cadillac and he rode around

in that. It turned out that they were selling stock to widows and orphans and anybody else. Well there's a man in Santa Monica, Bernheimer, a wealthy man. He found out that they were selling stock on the strength of the fact that he put $10,000 into this. Ten thousand dollars to him was nothing, so he was disturbed. He found out there were no patents on this thing, and said, "Suppose it's legitimate and Cox dies. What will we do?" So he insisted that Cox go and file a patent application or else he was going to report him to the district attorney's office. So Cox came into Lyon and Lyon and started talking a language they couldn't understand. Cox had invented his own terms for everything. For example, for a solvent, he called that a wrapper. And other things. He had his own terminology so they couldn't make any sense of it. Leonard Lyon wanted me to sit down with this guy and try to reduce his claims to conventional language.

That led to my going up to Oakland, and this is when Earl Warren (remember the former Chief Justice?) was district attorney of Alameda County. Cox, who was an excellent welder, an artist with the welding torch, had built a small-scale refinery up there. And they claimed that they were taking crude oil, increasing the gasoline yield, increasing the octane number, decreasing sulfur, doing everything that people wanted to do to benefit oil in this process. He claimed that he had a special deposit of clay that had a catalyst which was responsible. Of course, I smelled a mouse as soon as I got into this thing and I had to determine what to do. I commuted back and forth between Oakland and Hayward where this plant was. I would go up there, ready to have a demonstration and something would always happen. Either the truck broke down and they couldn't get the oil supply up there, etc. I spent all summer shuttling back and forth with this thing. Finally I said I can't waste any more time. Let's either have a showdown or I'm going. So they put on a test. I had to make up my mind on how I was going to catch him. I knew it was fraudulent. Should I bring a bunch of my Caltech students up there and have them really gauging the whole place? I decided not to. But they were so stupid I could catch them at their own thing.

So, I worked out a big data report for them which had the data on everything you could think of, from time of day they went and dispatched. Well, one night when they're running this thing they took me into town to a nightclub and when I came back they showed me the results of the test to show me how much increased gas they had able to go back. This showed that the gasoline came into the system from one loading dock. They would back up there and dump in this several hundred gallons of gasoline. It proved that this whole thing was a phony. Well, you think these people would be dismayed. Weatherill said, "Beckman, I'm glad you found it, because Cox didn't think you knew what this was all about. He did this deliberately to see whether you were smart enough to catch him on here. Now since you won his respect, we will show you this thing." Well, this kept going on some more and each time there was something I'd catch. Finally in September I wrote them a registered letter and said this thing has no merit, is fraudulent as far as I can see, and I want nothing more to do with it.

A couple of months later the sheriff's deputy came up to me with a subpoena. He decided he knew this thing was phony. The district attorney couldn't bring a charge unless somebody made a charge. Weatherill was smart. If somebody came in and protested and said I don't think this is going, he would say you want to sell your stock back? We're glad to buy it. Here's your money because we have got people who are waiting to buy. So the poor fellow would either accept the money in which case he had no interest or else he thought, gee, maybe I'd better hang onto this. So this went on and finally he got one guy who wouldn't be taken in. He had only $250. Furthermore, they had to arrest them in Alameda County, not San Francisco. They got the guys on the ferry and arrested them. They told them oh sure, we've had this fellow Beckman working with us all summer to improve our process. I found out that they had been selling additional stock on the grounds that they had had a professor from Caltech polish up the details of the process, so they're out there selling more and more. They were clever.

We had a trial and I was active for the state. I went back to Oakland and appeared before Earl Warren and his staff. They were all standing around ready to arrest me, you know. I showed them my notebook. Fortunately, I had kept appropriate notes at the time and I told the story and I showed them the whole thing. Their jaws dropped because Weatherill had taken them in. He was such a persuasive guy. He was convinced that I was in collusion with them. When they found the facts, they swung around the other way and I became a state witness. They finally sent them up for five years each for selling stock without a license.

There's more to it than that, though. They had a professor from Berkeley over there to testify how big a sample it took. I really don't want to go into too much detail on this. But it is part of chemistry because it finally came down that Cox was going to put on a demonstration in the courtroom. They asked the professors from Caltech to justify whether it was safe from a fire standpoint or not. And these people got themselves confused. Also at the beginning of the trial, I saw how lucky I was. I was there as an expert and they were going to show that I was not an expert. They came up and said Beckman, now you're an expert. Well that's irrelevant because I know nothing at all about chemistry to handle the facts. Well in that case suppose you tell the judge about chaulmoogra oil. Do you know chaulmoogra oil?

STURCHIO: Roger Adams worked on it.

BECKMAN: That's right. So I said, yes, that's suspected of being something that can help in leprosy. Dr. Adams is at the University of Illinois. They closed their book and put it down. They had a battery of five or six lawyers and another of them said, "Suppose you tell us about the law of mass action and can you explain this?" and they gave a very simple exhibit. I said, well, I'm teaching this to my students. That's a very good simplified version. What you really ought to do is to put activity coefficients in here. They closed the book, sat down and they could have shown me up on anything but the two things I was

familiar with! That's part of the same background.

Cox is going to show that this catalysis clay he had was fixed with something made with dehydrated oil. And he said that this wrapper he was using was just an inert castor oil. I had done some work with Tretolite at this time on sulfonated oil used for dehydrating wet crude. And this looked like Tretolite to me. And so I asked for a sample. Well, how big a sample? This is where they got the professor from Berkeley involved. He couldn't tell them how many cubic centimeters were in a pint. I said well a few cc's will be enough for what I think it is. But they just got him so riled that finally I got about a 100 cc sample. In fact I told them, what I think it is, one drop would be enough. I took that and the next day, I took this sample and shook it up. I thought it was soluble in water. Then I took castor oil and found it completely insoluble. I said this is not castor oil, this is a sulfonated castor oil. They were pinned, I found out, for perjury. They claimed they got it from a Union Oil station that had been closed down from the year before. So to make a long story short, that was the Cox Oil Refining bit. It sounded like a Hollywood scenario...I went to this plant up there with a moat around it, a drawbridge, and an armed guard with six shooters. And Weatherill going around in his 16 cylinder Cadillac. That's some applied chemistry of a little different nature.

STURCHIO: You worked with Earl Warren on your testimony in that trial?

BECKMAN: Yes. He thought he was going to put me in jail.

STURCHIO: That's very interesting. Was that your first experience with expert witnessing?

BECKMAN: Yes, probably with expert witnessing. I had a rather unusual, rare case, the same sort of thing. The Tretolite Company, which had patents on sulfonated castor oil, was being sued, and the litigants, they had a judge there who couldn't understand what was being said, so he got both sides to agree to have an expert come up and help him. They called me out of class, right in the middle of class at Caltech. Both defendant and plaintiff agreed that they'd have me with them, and I sat on the bench beside the judge and if somebody didn't understand, he would ask me and I'd ask him a question or I would suggest a question for him to ask. This went on and on and as far as I know this was the only time where an expert actually sat up alongside of a federal judge and did this. When it was over, both sides said, I forgot which side won, but both sides said they felt they never had a fairer trial; and for the first time they had a judge who really knew what the score was in a technical subject. So that was another unusual experience.

STURCHIO: That was before the Cox Oil controversy?

BECKMAN: It was at about the same time. Whether it was just before or just after, I don't know.

STURCHIO: Well it sounds like you were spending more and more of your time in these sorts of activities. Do you recall how much time in the early 30s, before National Postal Meter and then Glen Joseph, how much time you were spending on the outside?

BECKMAN: I don't know, but it took a considerable amount of time. Especially when I got into this Cox Oil Process thing, because that took a lot of commuting. During the summer, when I would go and fly up. This was when the Varney Airlines had the Lockheed Vega. They had these little four passenger planes. They would make it in an hour and fifteen minutes. Quite often I would be the only passenger up there. The cockpit lifted up and the passengers had to climb in and the pilot sat right down in the lap of the front two passengers. But it was a fast little plane. That must have been about 1932, because I was building my house at the time in 1933. It must have been just before that because one of the charges--the lawyers were pretty rough. They accused me of forging my testimony because I needed money to finish my house. It was under construction. They accused me of having affairs with the secretary. They accused me of everything, but fortunately the lawyers told me in advance to be prepared for such tactics.

STURCHIO: You said that you had gotten into the Cox Oil thing through Lyon and Lyon. So you had had contact with them before you finally met for the inking device and the pH meter. The reason I raised that to begin with--I'm glad it led into that fascinating story--it's just that you don't learn in graduate school in chemistry how to file for a patent. Would you say a little bit more about your first few experiences with doing that?

BECKMAN: You talk to a patent attorney and he usually drafts the thing for you. You give him a technical description of what you think your invention is and then he puts it in proper terminology and form for filing a patent application.

STURCHIO: When did it occur to you that it would make sense to file for a patent for the pH meter? Here's the patent you filed in the fall of 1934, which must have been shortly after Glen Joseph approached you.*

BECKMAN: I guess about the time I decided to produce these things. You see this is not a pH meter. This is just the amplifier. It was later on that we put the thing all together in the little walnut case. I think this is going to be in business, so just the concept of having a patent is a necessary thing to protect your rights.

STURCHIO: That would come out of the earlier experiences.

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* Arnold O. Beckman and Henry E. Fracker, "Apparatus for Testing Acidity," U.S. Patent 2,058,761, issued 27 October 1936 (application filed 12 October 1934).

BECKMAN: If you made an invention and anybody said gee, I'll get a patent on it.

THACKRAY: Were there other people on the Illinois faculty who had done this sort of thing? Parr and his calorimeter would be an example.

BECKMAN: It probably had but I had no relations with them. I don't recall ever discussing patents with anybody at Illinois until I showed the pH meter to G. Frederick Smith and others at San Francisco.

THACKRAY: Were any people on the Caltech faculty doing similar things at this time?

BECKMAN: No. I had a brother who was a lawyer and he was the one who filed a patent application on my first patent for the speedometer warning device. My brother took care of that. So I may have soaked up from him what the necessities were. He didn't have anything to do with this one, though.

THACKRAY: When was that?

BECKMAN: That was in 1928 or 1929.

THACKRAY: Speedometer in a car?

STURCHIO: You missed that. Dr. Beckman said that it may have had something to do with the fact that he was getting too many tickets.

BECKMAN: I have a reputation that's highly unjustified of being a fast driver. (laughter)

THACKRAY: I'm glad to know there's someone else.

BECKMAN: Well, you see, I had to do that in self protection. I wanted to cut down my chance of being involved with an accident. If you go faster than the rest, you'll never get hit from the rear. So you cut it down, probably by a factor of two.

STURCHIO: Unfortunately, the highway patrol must not have agreed. Well, I've alluded to your encounter with Glen Joseph in the fall of 1934 a couple of times. Earlier I was showing you this book by LaMotte about pH and its applications.* In there they talk about the state of the art in the early 1930s for measuring pH. There were two methods mainly. The colorimetric method (using indicator solutions) and the electrometric method (using hydrogen electrodes). I wonder if you could say a few things about what you began to discover as you worked on the problem.

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* Frank A. LaMotte, William R. Kenny, and Allen B. Reed, pH and Its Practical Application (Baltimore: Williams & Wilkins Company, 1932).

BECKMAN: I'm surprised that book did not mention the glass electrode. The glass electrode was known and was in fact being made commercially by Leeds & Northrup at that time. In fact clear back to Fritz Haber you know, 1909, I think. He was the one who demonstrated this phenomenon. Later on there was a British woman biochemist in the 1925 or '26 period who did some excellent work with the glass electrode using the properties as an electrometer. So the technique was known.

THACKRAY: That was published in the literature.

BECKMAN: So I told Glen Joseph that a galvanometer is the wrong instrument. You need an electrometer. The galvanometer is a current measuring instrument. Even the Leeds & Northrup galvanometer, which was the best at that time, needed as I recall 10 amps. That's a lot of current. And you work it back. If you want to have a sensitivity of let's say 59 for a pH at a tenth of a millivolt, you have to have a maximum electrode resistance of 3 million ohms. That meant it had to be a pretty thin wall, large kind of thing. So I said your galvanometer just requires too much current. Use a vacuum tube voltmeter. Here again in my naivete I assumed that the grid current of a vacuum tube was zero. Therefore, you really had two electrometer type of things on it. It was almost zero but not quite. Later on when we got into this thing, we found that it was 10^{-13} or 10^{-14} amps, something like that. That led again to our use of our recording pH meter as a micromicroammeter. I think I told you about that for monitoring the safety ionization chambers in the atomic energy plants. I thought at that time that the grid current was zero. I realized this would make it possible to use a much higher electrode resistance and a much more rugged electrode.

STURCHIO: Where did you get these parts? Did you pull them off the shelf or did you have to make them yourself? Did you make the glass electrode yourself?

BECKMAN: Oh sure. There's nothing to them. In fact, another proof that this...Corning made a special glass, Corning 015 glass, which had the particular combination of the lowest melting point and highest electrical conductivity of the soda-lime glasses that were being used for making the glass electrodes. There's no chore to blow a bulb on them. I put an arm on the side to fit on in this thing. There are a lot of little things in here that are not patentable, for example, one of the problems is if you scrape the electrode against the side of the beaker, the electrode will break. So we put on a square electrode rod, a ring stand rod, instead of that, and put it so that it could rotate with stops on there. And that was rather difficult--to put stops so that we wouldn't push electrodes to the bottom of the beaker. But also that meant that we were immersing the electrode to a constant depth.

Now at about the time we came out with our pH meter, there was a very disturbing publication, I think by a professor at Stanford University, saying that a glass electrode really measures the depth of immersion rather than pH. They made an

electrode out of a length of 015 tubing and they found that it got different pH readings depending on how deeply it was immersed in the liquid. That was devastating to us; we finally looked into it and found out that sure enough, it was true. What we didn't realize was that there was a leakage path over the outside of the electrode and down inside. The higher your electrode rod, the shorter this leakage path. That's when we started to overcome that. The surface resistivity of Corning 015 glass is pretty low. We started coating it with shellac and with resins of various kinds and with picein wax and all we'd melt on that. Finally, we decided these are just temporary, unsuitable things. The only way was to get rid of that interface. That led to our patent on the sealed electrode.* That was the thing that really gave us a stranglehold on the glass electrode business.

STURCHIO: How soon after the introduction of this instrument was that?

BECKMAN: Not too long after. But, I'd have to look it up and see. We got basic patent protection on that. I can't recall the date, but I remember our consternation when we read about this work at Stanford.

STURCHIO: I can see why that would have given you pause.

BECKMAN: That's right. We then got into other results too. We found that when you start measuring a high pH the electrode begins to function as a sodium electrode as well as a hydrogen electrode. So, we started our glass electrode research. We studied thousands and thousands of compositions to come up with glasses that are specially suited for this or that. These are high pH. These are for the sodium measurements or potassium measurements.

STURCHIO: One thing I wondered in reading over the story of your encounter with Glen Joseph in your early acidimeter work: did you ever go to visit his operations?

BECKMAN: Oh, yes.

STURCHIO: We have an article that appeared in the Journal of Chemical Education on the California Fruit Growers' Exchange.** Was it common for you to go and visit the laboratory or the places where you were doing things?

BECKMAN: Yes. Joseph's boss out there was a Caltech student named Bill Baer. He was always research-oriented. I think it was a result of his exposure to Caltech. The California Fruit

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* Henry H. Cary and Warren P. Baxter, "Electrode for the Electrical Determination of pH." U.S. Patent 2,256,733, issued 23 September 1941 (application filed 20 September 1937).

** Eloise Jameson, "Chemistry of the Citrus Industry in California," Journal of Chemical Education, 3 (1926): 1117-1124

Growers' Exchange, that's Sunkist, were always investigating things, new fertilizers and pesticides, ways of eliminating scale on oranges, and the development of by-products. There was a research atmosphere, you see citrus by-products were associated with it. It was not a very preposessing quarters out there. This is in Ontario where they were.

STURCHIO: Did you check the methods he was using at the time? As you said, a high-sensitivity galvanometer?

BECKMAN: That had to have an optical lever on it. It had a mirror on it and you had to shine a light on that. Then you had a meter out and a big scale. That's what he was using and he had his galvanometer suspended in a hopefully vibration-isolating suspension with springs on it. But that wasn't enough. There was enough plant vibration. The thing would break quite often.

STURCHIO: If we can move on then, just briefly from the introduction of the acidimeter. There are two other things that I would like to cover. I hope we can get to the DU because that's one thing that in all the interviews that we've been reading, hardly anyone has talked about the early years of the development of the DU.

BECKMAN: You didn't see the film that SmithKline made for our 50th anniversary? That was shown last night and also shown out at our place. That's an interesting film. You ought to try to see that. Bruce Merrifield makes a statement. He said that the DU is probably the most important instrument developed in the advancement of bioscience. I'm surprised that he would be so generous, but we do have him on record for that.

STURCHIO: We shall certainly have to get hold of that and take a look at it. I would like to get on to that, but at this point one thing I wanted to ask you about was...You told the story of the shift from the National Inking Appliance Company to the name National Technical Laboratories as you began to explore marketing the acidimeter, and how you went to see the various scientific instrument companies in the fall of 1935 after the ACS meeting in San Francisco. Arthur H. Thomas was the first company to take an interest in it. But shortly after that we find this Eimer and Amend catalogue with an illustration of your apparatus.*

BECKMAN: That's right. In a short time we had all of the apparatus companies--28 of them--selling our instruments. Eimer & Amend, Central Scientific, E.H. Sargent and the rest. And we had a one-man sales force, Tom Herrington. All he did was go around and pick up orders from the different dealers.

Then in 1960, we shifted it from our dealers. This was a major business decision to make because here we had all of the operators and dealers in the country selling our product. If we

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* Eimer & Amend, Laboratory Apparatus and Supplies: 85th Anniversary Catalog (New York, 1936), pp. 316, 435-436.

cut them off, we knew we had all of them as our competition. Yet, we felt we had to do it because we were not getting information back from our users. That was the first thing. Some of our dealers, particularly Fischer and Eimer, were coming up with their own competitive thing and the know-how that developed. But more important, they were not passing on to us word from our users. The operators and dealers, many of them, not Arthur H. Thomas, they were always a fine group. I'm sorry we had to break with them, but we could not make an exception. We didn't know what the experience of users was, what they wanted in the way of new instrumentation. So we decided from a long range, that we had to have direct connection with our users. So we broke with all of our dealers. This created quite a little surprise on Wall Street. They thought we were out of our minds, and we thought perhaps we might have been, too. But, it has worked out fine since. Now we work closer to our users, for their benefit and for ours, too. That was a major, very tough business decision to make.

STURCHIO: At this time, right after you talked to the Arthur H. Thomas Company, which was the first to pick up the pH meter, the dealers might have seen a larger market for it then they saw initially and they began to carry it right away.

BECKMAN: I think it's like a bunch of sheep. One dealer saw something and pretty soon the rest of them follow in line.

STURCHIO: And this is just one of the early brochures.

BECKMAN: This is called an acidimeter. We had to change that name because there were two types of acidimeters. One would determine total acid, which is just a titration, and the other is the hydrogen ion concentration and we changed them over.

STURCHIO: This must have been before 1940 because it still has the 3300 East Colorado address there.

BECKMAN: Oh yes. This goes back to probably 1935 or 1936. This is our first promotional brochure. I think our first publication was a little thing called, "What Every Executive Should Know About pH". That explains the pH scale in non-technical terms.

STURCHIO: Well the growth of the pH meter and the growth of NTL's business is well covered in Golden Past, Golden Future.* What you might say a few more words about is how you began to expand into other areas based on the kinds of electronics and optical systems you were using in the early equipment.

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* Stephens, Golden Past, Golden Future (1985), pp. 9-15, 33-37, 53-55.

BECKMAN: I've mentioned to you how we got into the Helipot business.* That was by chance. The Radiation Lab found we had this thing that was a result of our determination to have a better component. We didn't even have sense enough to measure its linearity or to put it on the market as an item of sale. It was just a component replacement part for a pH meter--to show how narrow minded we were for not thinking.

Then the development of the spectro, that was a deliberate attempt. I realized that in this amplifier we had a very good stable DC amplifier that would work with vacuum type photocells that give a linear response. That was a deliberate attempt to go on. Next year we went back to Rochester and talked to one of the big optical companies and they were not interested in this thing. So, we had to go back and do our own optical design on the thing, and not only an optical design but a mechanical design. There are a lot of features in here that users do not realize. For example, the friction on the bearing that holds the prism mount would cause just a little bit of deflection of the arm, just enough to bring in a slight inaccuracy. Well, we had to worry about things like that. Consider just the straightfoward mechanical design, for example. You don't think when you look at it now--it's just obvious you had to have a bearing under there--but it took months and months of testing to find a...

THACKRAY: How large was the company at that time and how much specialized help could be called on?

BECKMAN: This is in 1940, when we started to work on this. I think we had a couple of hundred employed. Remind me, I'll go back and look in our annual reports.

THACKRAY: Did that mean most of the people were in production or in sales, I mean how much...?

BECKMAN: We had roughly 10% of our people in research and development. The rest of them were in production.

STURCHIO: Here we have an article in the Journal of the Optical Society of America in 1941 on the DU spectrophotometer.**

BECKMAN: Yes. This was the first publication.

STURCHIO: You wrote that in the spring of that year. I wonder if you could talk a little bit about the degree to which chemists were using visible spectrophotometers as analytical tools then.

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* Arnold O. Beckman, "Variable Resistance Device," U.S. Patent 2,454,986, issued 30 November 1948 (application filed 22 October 1945).

** H.H. Cary and Arnold O. Beckman, "A Quartz Photoelectric Spectrophotometer," Journal of the Optical Society of America 31 (1941): 682-689.

BECKMAN: They were used to some extent, but not widely. At that time, you had a matching field technique that was very tiring and not very precise. That's why this optical company was making these things. There was just no great market for them. They were not very popular and I realized that we could relieve the human fatigue first of all. And second, we would get it to an objective rather than a subjective determination. And also extend the optical range beyond the visual. These are the three things that were obvious to me at the time.

STURCHIO: In this case you were doing something very similar to what you had done with the pH meter: you designed the skill into the machine.

BECKMAN: It's always been our thing. We build complications into the machine. As I mentioned, the newest example is our Fourier transform infrared spectra. Did you ever see that?

THACKRAY: No.

BECKMAN: Well you must come out and see it. It's a marvelous instrument. Not a single control knob on it. We standardized that; it's a single beam type of IR spectrophotometer. We got things so stable that now we can calibrate that at the factory and run it for weeks or months after. You've got lasers and an interferometer built into the thing and very, very complicated things inside. But for the user, it's simple to use.

THACKRAY: By 1939-40, were you drawing on other people at Caltech or elsewhere for the development side of things?

BECKMAN: Yes. Not so much at Caltech because Caltech was not an applied science school. The only one we got from Caltech was the Pauling oxygen analyzer and that we were brought in because they found they couldn't make it. Making these things to pre-determined specifications was quite different from just making a demonstration model. But that's about the only thing I recall we ever got from Caltech. We're indebted more to Rockefeller. There we got the ultracentrifuge from Dr. Pickels. We got the Moore-Stein automated amino acid analyzer, and we got the peptide protein synthesizer through Merrifield, three from Rockefeller.

THACKRAY: Where does that Rockefeller connection trace back to?

BECKMAN: It traces back to Dr. Pickels. He used to be at the University of Virginia working with Beams who had the air driven quartz rotor. And then he went to Rockefeller Institute (just before it became Rockefeller University). Then he came out west and teamed up with three other people from California, Maurice Hanafin (who became president of the company), and two others. The four of them formed a company called Specialized Instruments Company. They developed the analytical ultracentrifuge. Maurice Hanafin came to see me one day. He said, "We've got a product but we don't know how to go about selling it. How do you sell instruments?" We got acquainted and talked and it ended up that

we acquired Specialized Instruments, called Spinco now. That's how we got into the ultracentrifuge. That was entirely through Dr. Pickels.

THACKRAY: What about other, did you have consultants back in Illinois or...?

BECKMAN: No. We have had consultants, I'm sure, mainly when we find someone like Clark. Dr. Clark developed Clark Electrics. We arranged the licenses. So it's mainly through that approach. Someone would develop something, we'd hear of it and then we'd move in.

THACKRAY: Now that sounds deceptively simple when you say someone would develop something and we would hear of it. And I'm sure it's the truth. But, how many years have you...

BECKMAN: Now, you see one of the advantages of selling directly. We have sales and service people, particularly service people. That's the thing we stress, because an instrument is no good if it isn't usable. We spent a lot of our time and money on building up a service organization. These people are in close touch with what's going on, and if a fellow develops something, our people know about it. Once in a while there will be somebody who will come to us and know us from our other instruments and want to know whether we're interested in his device. By and large our people in the field pick up the leads.

THACKRAY: In certain ways, that's been true all along because that was what Glen Joseph was doing. That really has been a motif all the way through. Or did it pick up much more later on?

BECKMAN: Of course there's been a constant change in our instruments. From the days of the original pH meter and the spectophotometer, the microchip has come in. So all of our instruments had to be redesigned and we have a whole flow of new instruments. Now that's an in-house development. We have the techniques and we know what to do. We depend on an outside supplier. We design our own chips, for example, but we have them made by others for us. We don't do that. But we have all that capability.

STURCHIO: If we can get back to the consultants that brought us into this, how did you get in touch with Cary in developing the DU?

BECKMAN: He came to work for us when we were on East Colorado Street, Dr. Swift's dry cleaning place. We had several students from Caltech who came in later. George Shull was one of them. He left us later on to work with Howard Hughes on developing the Spruce Goose; he developed the resins, the glues that were used on that. And there were a few others. Howard Cary was one who came out with us and Henry Fracker was another. Fracker spent a little time with Bell Labs, too, and then he decided to go back to get his degree or do more graduate work at Caltech. After a few months he gave it up and he was kind of floating around, so

he worked with us on some of these things. He was co-patentee on that first patent.

STURCHIO: There was a very close tie between the early DU model and the pH meter. Here's Bulletin 79 from National Technical Laboratories* that shows that you could use the pH meter...

BECKMAN: The first one...you might ask why model D? The first ones did plug into a pH meter to read the output of the photocell. We made a few instruments, I don't think we sold them. I think we just put them out for evaluation. I don't see any model number on here. I know one went over to UCLA. Then we had another model before this one which had a circular turret. It would hold six cells around and we decided that that was no good so we abandoned that and went to this. By the time we got down to that model, it was model D. That had a glass prism. Then we realized that we wanted to work out the ultraviolet. We had to use quartz. So we switched over to quartz and that became the DU for ultraviolet.

STURCHIO: Where did the first DU go?

BECKMAN: UCLA got one of the early ones. I don't know. I'd have to go over the user list. There must be at least 25 or 30 people who claim they bought the first DU. (laughter) Fifty people claimed they bought the first pH meter!

THACKRAY: But you have records in the company that would tell you this?

BECKMAN: Possibly. We do now. But back in those early days the record keeping was not one of our top priorities. Our records were a little bit sparse, but some of these things we can find.

STURCHIO: Here we have just at the end of the war an article in Instrumentation on spectrophotometric analysis.** Was this the sort of thing you were trying to do to build up the business? Or you just wanted the word out?

BECKMAN: Sure. This was to try to build up sales. We wanted the world to know we had instrumentation that would do them some good. Do us some good, too, if they bought the equipment. So this was just getting the story out. You have to keep in mind that back at that time instrumentation techniques were not very sophisticated. And particularly in the industrial sector. They wouldn't go in. They had their big steam gauges. I'm not criticizing them. We had a lot to learn too. I remember the first industrial instruments, we had 2-56 screws in them. That's a

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* "The Beckman Photoelectric Spectrophotometer," National Technical Laboratories Bulletin No. 79, 20 May 1941.

** A.O. Beckman, "Spectrophotometric Automatic Chemical Analysis," Instrumentation, 1 (July-August 1945): 16-17.

small screw. You put them in with a jeweler's screwdriver. That's fine for an instrument. When you got to the point where the smallest screwdriver was perhaps 1/4 of an inch, well we had to learn those things too.

THACKRAY: By this time, by 1945, did you have a sales and service force out?

BECKMAN: No. We were depending on our dealers. We made industrial things specially. We were depending on our dealers, and maybe one or two of our engineers would go around and talk to our dealers and urge them to go into the field, but we didn't build up a sales force really until 1960.

STURCHIO: I'd like to ask more questions about the DU and about the IR-1 and about the connections with the synthetic rubber project in World War II, but those I'm afraid will have to wait for another time. It's been so interesting we've gone on with some of the earlier topics. But just to finish up, we have here patents for a method of soil analysis for locations of oil deposits in 1940, and proportioning in 1941.* So it's clear that the early National Technical Laboratories was involved in a number of other ventures. Do you recall what those arose from?

BECKMAN: Oh, yes. I mentioned the National Postal Meter Company. One of the half-owners of that was the Jergins Oil Company. This is back when they were interested in geochemical prospecting and McCormick and others were doing that. Over a dome of oil or gas, chances are that the concentration of hydrocarbons in the soil above that would be higher than it would be in barren territory. So the question was to analyze the soil and plot a map on that and try to find out where they might go. The trouble was that the methods for measuring hydrocarbon content were on the borderline of what was possible and what was not possible, so that that never developed. This was before we had some of the new techniques available. We were doing it by straightfoward chemical analysis. So Jergins always was interested. I did discover one field for them up on San Marcos Pass. Gee, I had forgotten we had a patent on that.

STURCHIO: And the method of proportioning?

BECKMAN: Oh, yes. A fellow came in the door one day out at East Colorado Street. He wanted help. At that time, there was a law in California, I think they still have it, that when you wash dishes behind the counter as you do in a bar or soda fountain, you had to have a sterilizing rinse. You had to have three tanks, washing, then sterilizing, and then rinse. You had to put

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* A.O. Beckman, "Method of Soil Analysis for Location of Oil Deposits," U.S. Patent 2,348,103, issued 2 May 1944 (application filed 31 January 1940); Beckman, "Method and Apparatus for Proportioning," U.S. Patents 2,351,579 and 2,351,580, issued 20 June 1944 (applications filed 22 November 1941).

in a sterilizing unit which was usually hypochloric. Well, that's nasty stuff to handle and the clerks wouldn't use it until they saw an inspector coming in the door. Then they would really rush up and put the stuff in. So this fellow had developed a means for aspirating hypochloric into the tank at the time you filled the tank. You just had an ordinary aspirator and gallon bottle of hypochloric. He realized that the sterilizing efficiency of hypochloric depends on the pH--how much hypochloric acid is sent in there. So he knew we could come in and devise a way of giving him pH control over this aspirator.

That got us interested in the thing and I realized that the aspirator is not a very precise proportioning device. So I developed this. This was a precise way that depending on how much water goes in, you actually displace the course of the proper amount of disinfectant solution. So, we had gotten on that and we sold a lot of them.* That was not our main business. He sold them and we finally decided that selling this sort of thing was not consistent with selling precision scientific instruments, so we got out of it. One of the things that brought us that attention, to show what happened, we had a salesman in Long Beach, selling a lot of these sterilizing things. I finally found out what he did. He would go around with a centigrade thermometer. This was before centrigade temperature was being talked about. He'd go in and switch this around and say that C stands for chlorine. Your chlorine is clear offset like that. As soon as we found out about that we said we have to get out of this business.

STURCHIO: Here was an example of somebody coming to your door because he knew you were in pH and it was sort of a market response.

BECKMAN: He wanted pH control on his product.

STURCHIO: And so that was another application of pH, which you talked about before as being an important aspect of the early growth of the company.

BECKMAN: As far as I know, this is still being sold by other companies. I know up in San Francisco a fellow named Thompson is still doing it, so that served a useful purpose.

STURCHIO: But it was also consistent with the principles you were using to build what soon became Beckman Instruments. We talked about some of the design principles you were using, but here was something where you wanted to just "stick to the knitting," as it were, to stay in precision scientific instruments.

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* See National Technical Laboratories, "Sterimatic: Automatic Preparation of Sterilizing Rinse Solutions for Fountains and Bars." n.d. Copy in Beckman Instruments Historical Collection.

BECKMAN: Well it's the same sort of thing. In other words, you want a precise chemical under control. I forgot about these things. I'm pleased to talk with you. It provides memories about them.

STURCHIO: Did this sort of thing lead later on to the decision to go into process control in industrial chemistry?

BECKMAN: No. That came through a natural development. We had pH meters and then we had continuous recording pH meters and then the continuous recording spectrophotometers. We had all the component parts. That was one way we got into process control.

STURCHIO: And that gets us all the way back to the beginning and the Keystone Iron and Steel Works, where you were doing process control the old fashioned way. So I think there we had better stop. Thank you very much Dr. Beckman. It has been most interesting and enjoyable.

BECKMAN: Well, I've regaled you with a lot of stuff. And you've refreshed my memory on some things I had forgotten all about!

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