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 July 1985

CHF OH #0014B

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

RELEASE FORM

This document contains my understanding and agreement with the Center for History of Chemistry with respect to my participation in a tape-recorded interview conducted by Arnold Thackray and Jeffrey L. Sturchio on 23 April and 23 July 1985. I have read the transcript supplied by the Center and returned it with my corrections and emendations.

1. The tapes and corrected transcript (collectively called the "Work") will be maintained by the Center and made available in accordance with general policies for research and other scholarly purposes.

2. I hereby grant, assign, and transfer to the Center all right, title, and interest in the Work, including the literary rights and the copyright, except that I shall retain the right to copy, use and publish the Work in part or in full until my death.

3. The manuscript may be read and the tape(s) heard by scholars approved by the Center subject to the restrictions listed below. The scholar pledges not to quote from, cite, or reproduce by any means this material except with the written permission of the Center.

4. I wish to place the following conditions that I have checked below upon the use of this interview. I understand that the Center will enforce my wishes until the time of my death, when any restrictions will be removed.

   a. ✓ No restrictions for access.

   b. _____ My permission required to quote, cite, or reproduce.

   c. _____ My permission required for access to the entire document and all tapes.

This constitutes our entire and complete understanding.

(Signature) Arnold O. Beckman

Arnold O. Beckman

(Date) 9/27/88

(Revised 24 February 1988)

This interview has been designated as **Free Access**.

One may view, quote from, cite, or reproduce the oral history with the permission of CHF.

Please note**:** Users citing this interview for purposes of publication are obliged under the terms of the Chemical Heritage Foundation Oral History Program to credit CHF using the format below:

Arnold O. Beckman, interview by Jeffrey L. Sturchio and Arnold Thackray at University of Pennsylvania, 23 July 1985 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0014b).

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

The Chemical Heritage Foundation (CHF) serves the community of the chemical and molecular sciences, and the wider public, by treasuring the past, educating the present, and inspiring the future. CHF maintains a world-class collection of materials that document the history and heritage of the chemical and molecular sciences, technologies, and industries; encourages research in CHF collections; and carries out a program of outreach and interpretation in order to advance an understanding of the role of the chemical and molecular sciences, technologies, and industries in shaping society.

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 Dr. Arnold Beckman begins with the National Technical Laboratories in the late 1930s, and includes details on its policies and operations. Beckman continues with the change from NTL to Beckman Instruments, and emphasizes the development of spectrophotometry instrumentation during the 1940s in the central portion of the transcript. Other projects, including mass spectrometers, Geiger counters, pocket electroscopes, and especially the oxygen analyzer, are also discussed. Following World War II Beckman describes his reinvolvement with Caltech. The interview concludes with Beckman talking about air pollution work in Los Angeles, the formation of Shockley Laboratories, and the future of the instrumentation industry.

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.

TABLE OF CONTENTS

1 The State of Spectroscopy and National Technical Laboratories in the Early 1940s

Manufacturers of spectrophotometers. Different types of spectrophotometers. Interest of scientists in spectrophotometers. Little knowledge of business world at early NTL. Publicity for the spectrophotometer. Development and market research. Creation of sales and service support.

7 National Technical Laboratories

Personnel and their responsibilities. Employees encouraged in their own business ventures. Entrepreneurial environment in Pasadena area. J. Bishop and business techniques at NTL. Philosophy and education for business. Employee relations. Company philosophy. Decision to leave Caltech. Business methods. Advice for scientific entrepreneurs.

15 Development of the Spectrophotometer

Funding for the DU and other research. Government role in funding research and development. Involvement in synthetic rubber project. IR and UV spectrophotometers. Problems in marketing spectrophotometers through dealers.

19 Other Beckman Instruments, Inc. Projects During World War II

Manufacture of mass spectrometers, Geiger counters, and pocket electroscopes. Development of the oxygen analyzer. Problems with manufacture of bulbs for oxygen analyzer. Development of quartz fibers for oxygen analyzer. Merger of Arnold O. Beckman, Inc. and Beckman Instruments, Inc. Organization of the Board of Directors of Beckman Instruments. Income and organization of subsidiaries. Attitude toward post-war plans.

25 Post-World War II Period at Caltech

Personal reinvolvement with Caltech. Changes concerning applied science. Chairman of the Board. Fund-raising. Tensions between science and engineering. Impact of changes in federal research and development funding. Classified research.

30 Air Pollution Work and Other Projects

Active in Los Angeles Chamber of Commerce. Growing concern over air pollution in Los Angeles area. Incorrect focus on sulfur dioxide as source of pollution. Discovery of auto exhaust as pollutant. Meeting with Henry Ford II. Member of National Air Pollution Board. Setting up Shockley Laboratories. Development of cermets. Future direction of instrumentation industry.

35 Future Direction of Instrumentation Industry
Miniaturization. Competition in the instrumentation field. Involvement in Japan's entry into semi-conductor industry. Electronic instrumentation and the health care field. Need for new methods of education.

40 Notes

42 Index

INTERVIEW: Arnold O. Beckman

INTERVIEWED BY: Jeffrey L. Sturchio and Arnold Thackray

PLACE: 108 E.F. Smith Hall
University of Pennsylvania

DATE: 23 July 1985

STURCHIO: At the conclusion of our interview on April 23rd, we were discussing the early history of National Technical Laboratories. Today we would like to talk more about some of the developments during World War II at what later became Beckman Instruments. Let me start by asking you to reflect on the state of the art in spectroscopy around 1940. Who were some of the people working in the field? Where were they? What contacts did you and your colleagues at National Technical Laboratories have with them?

BECKMAN: Bausch and Lomb was the principal manufacturer of spectrophotometers. They did not make a photoelectric spectrophotometer of a visual or photographic type. The thought occurred to me after we had developed the amplifier for the pH meter, a very stable, two stage, direct-coupled amplifier, that would work well with a vacuum type photocell because it had a high impedance, which would match the high impedance of the vacuum type photocell. I say vacuum type because gas-filled photocells (which were more sensitive) would give a very uncertain, irregular response curve of electrical output versus light flux input. But, with a vacuum type cell, that was a linear function. So that was what really started me. In fact, we had already developed an amplifier that would work with a vacuum type photocell. The advantages of having a photoelectric spectrophotometer were pretty obvious, because the visual ones were limited to an object that had color. And there are many areas, particularly in the ultraviolet, where many organic compounds absorb light out there but do not absorb light in the visible. That was the origin of what later became the DU spectrophotometer. We started out with a model D. We made three or four experimental spectros that actually plugged into a pH meter to read the electrical output. We realized that was not a very efficient way of using instruments, and we incorporated the electronics directly into the spectrophotometer.

STURCHIO: In addition to Bausch and Lomb, were other scientific instrument companies or instrument suppliers making spectrophotometers that people were using at the time?

BECKMAN: Yes, Zeiss made them. I recall an experience after we had the DU on the market. I visited our dealer in Zurich. He took me on a trip to Basel to CIBA-Geigy or one of the other pharmaceutical manufacturers and introduced me to the chief research chemist there. He looked at me strangely and said there were two things that bothered him. The first was that Beckman was a young person. Over there, the Beckmann thermometer dates way, way back, and so they expected me to be an old man at the time. [laughter] Then he pointed over to a table in the corner and there under a black cloth was a Zeiss spectrophotometer. He said, "We in Germany can't understand how an American came up with an instrument that's superior to the German instrument, but there it is, and particularly developed by a young person." He shook his head in amazement, because Zeiss was a principal manufacturer of optical instruments in Germany.

STURCHIO: Last time we discussed the paper you and Howard Cary had published on the DU in the <u>Journal of the Optical Society of America</u> (1). While you were doing that work, were you looking at other spectrophotometers, like the Hardy that GE had produced? Were you in contact with other people working in that area?

BECKMAN: Actually, we had approached Bausch and Lomb and said, "We don't have any optics. We have the electronics and we suggest we work together in putting out a photoelectric spectrophotometer." We would provide the electronics and they would do the optics. They weren't interested. They said that they had been making a spectrophotometer for years, and they couldn't see a market for a hundred of them. Fortunately, we were small enough where that would be a big market. We went back and designed our own optical parts.

STURCHIO: When you went to ACS meetings around this time, 1939 or 1940, were chemists becoming more interested in spectroscopy as an analytical technique?

BECKMAN: Oh, yes. Particularly as organic chemistry developed during that period, the need for spectrophotochemical methods of analysis were becoming more and more obvious.

STURCHIO: If you and Cary were talking in 1940 about chemists who might be willing to use the new DU, who were some of the people you might have called on?

BECKMAN: As a matter of fact, we were not very business-oriented in our thinking. We were interested in development. We didn't know much about market research, and we didn't have any formal surveys of who might be using this. We were interested in if we could make the instrument and put it on the market. I had no business training and neither did Cary. We were babes in the woods as far as the business world was concerned.

THACKRAY: How did the word get out? Was it similar to the acidimeter, in that one used it and then told a friend?

BECKMAN: We had a one-man sales force. We didn't have any capital. We didn't have the financial resources to build up our own sales force, so we sold through the dealers. I recall our final total was twenty-eight dealers. The principal one was right here in Philadelphia, Arthur H. Thomas Co. Ed Patterson at Arthur H. Thomas encouraged me to go ahead and build the pH meter; he was the one who came up with the high figure of 600 pH meters that might be sold in ten years. When we came up with the spectrophotometer, Arthur H. Thomas, Eimer & Amend in New York (that was before it was acquired by Fisher) Central Scientific in Chicago, E.H. Sargent in Chicago, were dealers. They all bought one of these things and sold it. So that was the way the word got out. Once we got a few of them out, papers began to appear and talks were presented at scientific meetings.

STURCHIO: Speaking of scientific meetings, I noticed in the paper on the photoelectric spectrophotometer that this was due to be presented at one of the MIT conferences on spectroscopy in the summer.

BECKMAN: July 1941.

STURCHIO: You mentioned in a recent article that you and Cary weren't able to go to that meeting because of restrictions on travel, but that it was read by one of the representatives from Arthur H. Thomas (2).

BECKMAN: Ed Patterson, yes.

STURCHIO: Had you gone to earlier conferences on spectroscopy of this MIT series or was this..?

BECKMAN: This probably was the first one, as I recall.

STURCHIO: It seemed like a useful venue for describing the instrument to a group of people who were very much interested in developing spectroscopy. Another way that you did get the word out was an ad in Industrial and Engineering Chemistry in September of 1941 (3). (See following page) When did you think of advertising in Industrial and Engineering Chemistry, and what sort of response did that ad and ads like this one from Eimer & Amend's catalog (4) bring in? (See second following page)

BECKMAN: This had the quartz prism in it at that time. This is possibly our first ad on the market. I imagine this is a result of the fact that at that time we had accumulated enough funds to pay for an ad. [laughter] I don't recall any specific conferences or things of that nature that resulted in discussions of where to place ads. It just seemed like a logical thing to do.

STURCHIO: Do you recall who were some of the first people who bought the DU? Eimer & Amend's catalog listed it at $820, and I'm sure not many individual chemists had $820 in 1941 or '42.

BECKMAN: That price was very close to $795 when we started, but it got up to $820. UCLA claimed that they had the first one, and I think they had the one that plugged into a pH meter (5). That was one of the very earliest models. Then we made another model, Model C, which had a circular turret for the sample cells. That was not a good design so then we went over to the sliding cells--that became the Model D. So there were A, B, C, D--successive versions of this thing. The D also had a glass prism in it. When we decided to go to a quartz prism for ultraviolet, that brought the U in there: the D ultraviolet.

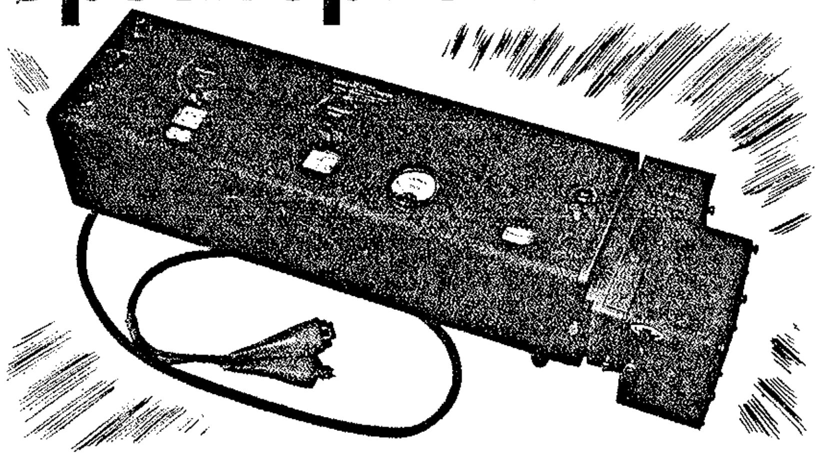
STURCHIO: Here's another photograph (see third following page).

BECKMAN: This is a later model with a gray case, because the

Announcing

AN IMPORTANT NEW BECKMAN DEVELOPMENT

THE BECKMAN Spectrophotometer



Features Many Unique and Far-Reaching Advancements

This precision instrument—the product of several years intensive development by the Beckman research staff—incorporates many important advancements in the design of spectrophotometric equipment, such as:

... *the finest type* of crystal quartz monochromator which covers the full spectral range of interest in spectrophotometry—200 millimicrons in the ultraviolet to 2000 millimicrons in the infrared.

... *precision* optical parts completely protected from dust and fumes.

... *extra long* (100 cm) easily-read Wavelength Scale.

... *direct readings* in both % *Transmission* and *Density*—simultaneously.

... *continuously adjustable* Slit Mechanism to fit every possible requirement.

... *full scale readings* with nominal band widths 2 millimicrons or less over most of spectral range.

... *stray light effects* less than 1/10% over most of the spectral range.

The Beckman Spectrophotometer is entirely *self-contained*, with a unique electronic circuit for rapid measurement of % Transmission and Density. The remarkable versatility of the instrument enables it to meet the widely varying requirements of research and control work with no sacrifice in accuracy or convenience. Regardless of type of Spectrophotometric measurements you are interested in, be sure to investigate the outstanding advantages provided by this new Beckman development!

NATIONAL TECHNICAL LABORATORIES

SOUTH PASADENA, CALIFORNIA

IMPORTANT FEATURES OF THE BECKMAN SPECTROPHOTOMETER

MONOCHROMATOR ✱ Autocollimating type with selected crystal quartz prism provides high dispersion in the ultraviolet. Wavelength scale graduated from 200 millimicrons in ultraviolet to 2000 millimicrons in infrared, easily readable to 0.1 millimicron in ultraviolet to 1.0 millimicron in the red, with scale accuracy better than 1.0 millimicron. Optical parts rigidly mounted in a massive heat-treated iron block, within a dust-proof cast aluminum case. Slits are protected by quartz windows and are continuously adjustable from 0.1 to 2.0 mm. by a unique mechanism that permits very precise adjustment, particularly when slit width is small. Slits cannot be damaged by closing too far.

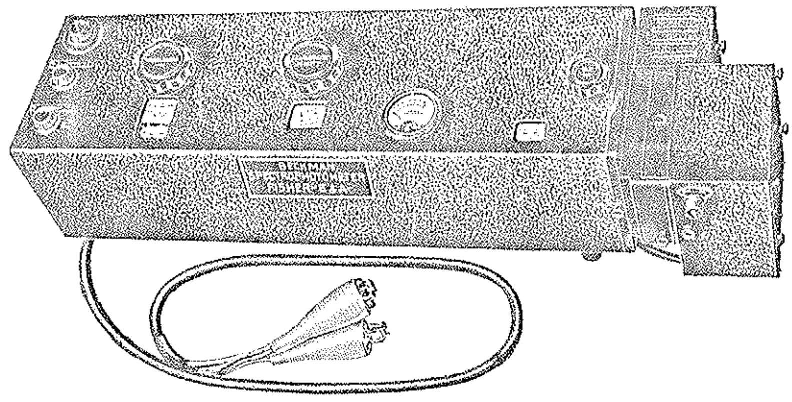
LIGHT SOURCES ✱ Interchangeable light sources permit ready adaptability to all types of spectrophotometric work. Efficient design provides full-scale accuracy even with low light intensities. Lamp housing is well ventilated and heat shielded to minimize temperature changes in absorption cells and phototubes.

SAMPLE HOLDER ✱ Absorption cells are held in light-tight slide holding 4 cells with light path of 1 cm. or less. Interchangeable additional holders are available for cells up to 10 cm. in length. Top of cell compartment is removable for titration and other special techniques.

PHOTOTUBES ✱ Compartment holds two phototubes, a sliding knob bringing either tube into position and simultaneously switching electrical connections. Phototube dark current can be checked at any time without removing cells (or changing setting of electronic meter). Three types of phototubes are available: one having maximum sensitivity in the red, another having maximum sensitivity in the blue, and a third having maximum sensitivity in the ultraviolet.

MANY OTHER IMPORTANT FEATURES ARE INCORPORATED IN THE BECKMAN SPECTROPHOTOMETER. SEE YOUR NEAREST DEALER OR WRITE FOR DESCRIPTIVE LITERATURE.

Beckman Spectrophotometer



14-377

14-377. SPECTROPHOTOMETER, Beckman, Photoelectric, designed for experimental research and control work in transmission measurements. The equipment supplied under this listing permits measurements through a range of wavelengths from 320 millimicrons to 1000 millimicrons. The apparatus consists essentially of a quartz prism monochromator with a light source, sample holder, phototubes, and a built-in measuring assembly to convert the phototube currents into direct reading of percentage transmission.

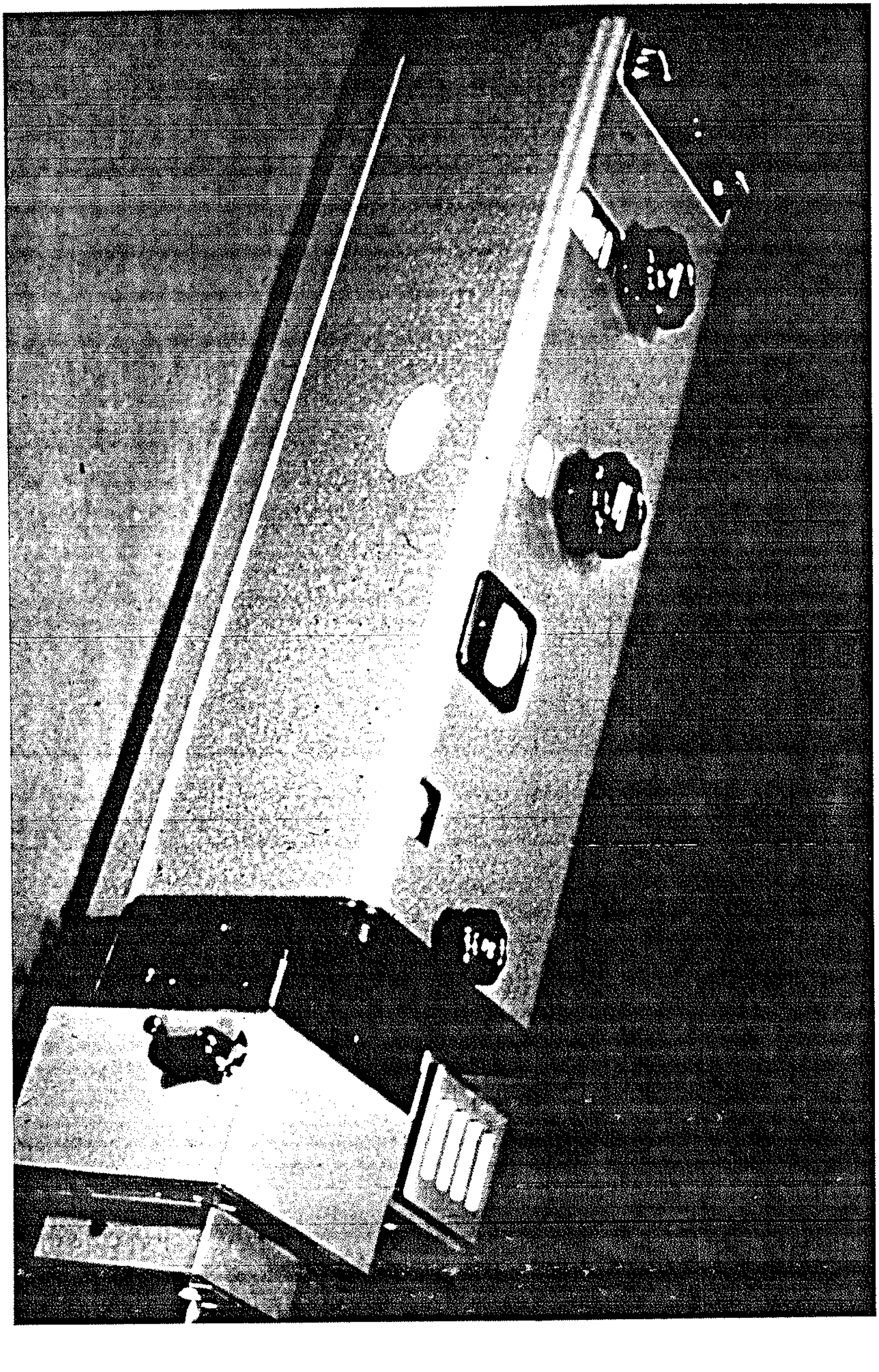
Light from a small lamp is focused on an adjustable slit by means of a mirror system. The light rays strike a collimating mirror where they are rendered parallel and are reflected toward a quartz prism. The prism is aluminized on the back and can be rotated by means of a knob on the top to refract and reflect the light at any desired wavelength. The monochromatic light then passes through the absorption tube into the phototube where it is amplified and measured directly as percentage of transmitted light.

The instrument is self-contained and adjustments are readily made, since all adjusting knobs are located in convenient positions. The wavelength scale consists of a four turn spiral graduated from 200 to 2000 millimicrons. It can be read to 0.1 mmu. in the ultra violet and to 1.0 mmu. in the infra red. A single knob on the top of the case simultaneously adjusts both entrance and exit slits to give a minimum band width. All electrical adjustments of the measuring circuits are made with the left hand knobs and a galvanometer is located on the top to indicate circuit balance. A light-proof compartment holds the absorption cells in a removable carrier, and is attached to the aluminum monochromator case by means of thumbscrews. Two phototubes are held in an adjoining compartment, one tube for wavelengths from 1000 to 600 mmu., the second tube for wavelengths from 600 mmu. to 320 mmu. Either tube is moved into receiving position by means of a sliding knob, which simultaneously switches electrical connections. A shutter coupled with a switch permits the dark current of the phototube to be measured at any time without moving the absorption cell or changing the setting of the measuring instrument.

The spectrophotometer can be used to plot a transmission curve for any material which transmits light. Two variables are involved in the operation of the instrument; wavelength and concentration of solution. Since transmission is a function of both variables, either can be altered while the other remains constant and the resulting per cent of transmission measured. If the wavelength is varied (by means of the wavelength knob) and the solution concentration remains constant, the per cent of transmission at different wavelengths is then determined on the transmission dial. The transmission results are plotted against wavelength and a curve characteristic of that solution is obtained, called the spectral transmission curve. This curve presents a standard picture of the color in question, so that the color can be exactly reproduced.

From several spectral transmission curves, the wavelength can be determined at which optimum transmission occurs for that solution. In a quantitative procedure, this desired wavelength can be employed, thus producing most accurate results. Setting the optimum transmission wavelength as a constant on the spectrophotometer, concentration can then be varied in known amounts, and transmission results plotted against concentration on log paper. If the solution obeys the Beer-Lambert Law, the result will be a straight line, which is used as a standard in spectrophotometric determinations of unknown concentrations. The transmission of an unknown is read on the dial, and referred to the graph where concentration is read directly.

The spectrophotometer is supplied complete with monochromator, tungsten light source, slide absorption cell carrier with twin phototube compartment, four 10x10 mm. Pyrex Absorption cells 50 mm. high, one caesium oxide phototube (range 1000 to 600 mmu.) and one blue sensitive phototube (range 600 to 320 mmu.) Accessories for measuring reflected light, ranges below 320 mmu., etc., are available, and literature will be sent on request. A 6-volt storage battery is required for operation and should be ordered separately under No. 2-470. **Each, $820.00**



earlier ones were all in black.

STURCHIO: You mentioned that at this point you weren't doing any sophisticated market research. But, along with Howard Cary and other members of the technical staff at National Technical Laboratories over the next few years, you did introduce a lot of improvements to the DU. Could you describe some of the improvements made in the early years after its introduction, how the ideas came about, and who developed them?

BECKMAN: We spent a lot of time initially in developing the mechanism for rotating the prism. That had to have a bearing, and the friction in that bearing was a critical point because you could deform the arm that was moving it. We had to work that out and finally got a spherical bearing in there, that was very successful. Another problem was a slit mechanism. We did a lot of development work and testing in our own shop before we got many instruments out in the field.

A major development was to switch over to a power source for the lamp. We had originally used batteries, but they were a nuisance so we went to an AC power supply. We had some headaches getting the bugs out of that, but we finally did it. Another problem was the light sources for the ultraviolet. We finally ended up making our own hydrogen lamp, which later became a deuterium lamp. For the photocell, we could not find a suitable thing on the market that would work in this wavelength region. The reason was that they had glass envelopes which were sufficiently opaque that we couldn't get much light through. Then we came up with the design of a very thin window that would have the mechanical strength to withstand full vacuum and still be very, very thin so that there is relatively little light absorption (6). These were major technical development problems we had.

THACKRAY: Were some of those suggestions coming back from people using the instrument?

BECKMAN: Yes and no. Some of the problems developed in the field. For example, an early slit mechanism would stick after a while. Then we would say that we didn't have a good enough design on it. Also one very amusing one. In getting the wavelength scale, we thought everybody would want a linear scale. We had a very clever man who had architectural training. He worked out that if we put a cosine function on there, we could come up with a linear scale. We did that and then found out that the customer didn't want a linear scale because it crowded all the wavelengths in the ultraviolet down together. This is a case where we assumed the customer wanted something which he didn't want. We had to learn that that lesson applied to many things. We could go off the deep end by just thinking in our own laboratory. We had to get out and work with users to find out what they really wanted and what the weak points were in our design.

THACKRAY: In this period, before World War II, how did you interact with users? Were you visiting labs, attending ACS meetings, or did people come to you?

BECKMAN: I would go to meetings. I was quite active in the American Chemical Society and the Instrument Society of America, but I would go mainly with the dealers because we were selling to dealers. We did not have our own sales force. This led to increasing problems over the years. We were not getting the input from our users back to us. They would tell our dealers and our dealers wouldn't tell us. Finally, we decided as a policy that we had to have our own direct sales and service force or else we would not be able to do our job. This was a very difficult decision because we had some very loyal dealers like Arthur H. Thomas. I hated to change that. We had some other dealers that were not so loyal. This was in the days of the Robinson-Patman Act and some of the other non-discrimination acts, and our attorney said that we had to deal with all of our dealers exactly the same way. We couldn't differentiate and select dealers. That would leave us open to a charge of discrimination. So that was one of the tough decisions, that I had to break off relations with Arthur H. Thomas, and E. H. Curtin in Houston (they were one of our good dealers). We did that around 1960 and this had to be kept secret. We announced it through a full page ad in the Wall Street Journal because we wanted to send an announcement simultaneously to all of our dealers. Our lawyer was very apprehensive about even the differences in time, to let one dealer know twenty-four hours before the other. This was rather a shock to the investment people because we were listed on the stock market at that time. They would say, "What's wrong with this company? They have all of the dealers selling their instruments. They're going to pull that away from them. They are now going to have those twenty-eight dealers as competitors and they don't have their own sales force." A lot of people thought we had lost our reasoning. It was tough going there for a while but we finally built up. It was the right decision, because without direct contact with our users, we would not have been able to develop the products that we have over the years.

THACKRAY: To go back to the early period again, around 1940, did you begin to make one trip a year to all of the dealers? Did you set a rhythm in this that endured through that twenty year period?

BECKMAN: No, this was the job of our sales manager, Tom Herrington. He made the trips to the dealers and would take the orders. We gave volume discounts: if you bought one instrument you might get 10%, but if you bought three you might get 15%. Tom Herrington's job was to go around and try to get them to order large enough volumes so that we could put these through production in more economical batches. That was his job. I used to go around also and visit the dealers on an institutional and a social basis. I did not get involved in the day-to-day writing up of the orders. That was his job. I tried to get around to tell the dealers what was coming on, try to learn from them what the customers wanted in the way of changes.

THACKRAY: With twenty-eight dealers this implied plenty of traveling.

BECKMAN: Yes. I did a good deal of traveling in those days.

STURCHIO: Could you talk about the structure of National Technical Laboratories around 1940? Howard Cary, was Vice President for Development from 1937 to 1946. That sounds as though you had quite an administrative staff, but it was a much smaller operation. Could you talk about the process from designing the DU, to actually putting it into production, to shipping it out the door? What happened in your Pasadena office on a day-to-day basis?

BECKMAN: We had a small development group. Howard Cary was in charge. Kenyon George was another one. Doug Marlowe was another. That was just about it.

STURCHIO: Were they also from Caltech?

BECKMAN: Kenyon George wasn't. I don't think he necessarily had a college education; he was a very skilled machinist. Doug Marlowe had some college education but I'm not sure if he was at Caltech. George Shull was at Caltech and he worked with us. He didn't work much on the spectro but was more involved with the glass electrode.

STURCHIO: Were those all the technical people at that time?

BECKMAN: No. We had mentioned the man who designed the cosine wave for us, but I've forgotten his name now. He was not a chemist. He was more of a mechanical design engineer. George Shull was a chemist. He left us later on to go to work for Howard Hughes, and he developed the glues that put the Spruce Goose together. I'm overlooking one or two others, I'm sure.

STURCHIO: You had a small nucleus of people working on new developments. You had a one man sales force. You must have had people working in production and other areas as well?

BECKMAN: Yes. We started out on East Colorado Street with Henry Fracker and Bob Barton. These were two students who worked part-time. Bob got his doctoral degree and then went to work for Shell up at Emeryville. Fracker was a Caltech graduate who had gone to work for Bell Labs for a couple of years and then he came back to Caltech. He thought he wanted to go on for graduate work, but he just stayed for a while.

When we started production, we had a couple of people come in the door. One was Frank Camarra. He was formerly an employee of Western Electric Company in Hawthorn, Illinois, a cable lacer. At that time he had a job with Mason Cables. He was visiting someone, and came in to get two or three weeks work. He stayed with us for forty years. [laughter] He is still living up in Oregon now. Another one was Frank Traphagen. He was a very skilled machinist. They were the production force. We used to call them "Frank and Frank." Frank Traphagen died about fifteen years ago.

Then we had Johnny Simpson, a high school graduate, who lived just a block away from our place on East Colorado Street in Pasadena. We brought him in and he did everything. He swept the floor and I taught him glassblowing. I blew the early glass electrodes myself. I taught him the glass blowing, and he developed into a very highly skilled glassblower and stayed with us for many years. Finally, he left because he wanted to set up his own business, which I encouraged him to do. That is now known as Glass Instruments in Pasadena. Johnny was a very fine chap, and I could understand his desire to stand on his own feet.

He thought he had an angel who was going to set him up in business. The idea was that he was going to make individual letters of the alphabet filled with neon. These would have magnets, so that grocers could make their own neon signs. They started out and I said, "Fine, Johnny, but have your successor trained." Sid Parker was his name. He was the son of the chemistry stockroom man at Caltech. He had Sid trained, but he said that Sid wanted to join him. So I said, "Well, that means the next in line is Miss Morry Duchane." He stayed on to train her for a few months and then went off to start his business. Things went all right for a while with his business, and then he began to get into technical trouble. His letters began to develop black spots around the electrodes as the metal would plate off. Finally, their angel disappeared, leaving Johnny Simpson and Sid Parker holding the bag. We kept them afloat because we gave them enough glass blowing. They knew our business, of course, so we would use them as our source of supply until they could build up their other business. He still remains a close friend. Sid Parker died but Johnny Simpson is still living out there. I mention this because I encouraged people to go off on their own so long as they did it open and above board, and get their successor trained. Morry Duchane was with us for twenty or twenty-five years. She's retired now.

THACKRAY: Was this a unique example of contracting out or did it set a pattern that you then followed with other aspects of your business?

BECKMAN: That's about the only situation where an employee set out to set up a business in which we could use the products.

THACKRAY: Were you yourself very directly involved in the selection of people for these different areas?

BECKMAN: Yes, in the beginning I was because we didn't have a personnel department. I was still teaching at Caltech. I was carrying this on as a part-time activity, nights and weekends, until 1939. By that time the business had gotten to the size where somebody had to be in there running it full-time. That's when I made the decision to leave Caltech and do that. And that's when we changed our name from National Technical Laboratories to Beckman Instruments. You know how we got the name National Technical Laboratories; that grew out of National Postal Meter. I think we covered that earlier (7).

Thackray: One other thing about the people. It's something about the Caltech or Pasadena context. Were there many people trying to start up their own businesses? Would you say it was an entrepreneurial atmosphere or not?

BECKMAN: That was before the big surge in that direction. There were some, but not many. John Bishop came into my office in South Pasadena one day and I had never heard of him. He's a graduate from the Harvard Business School, and he said that Beckman Instruments was one of the case studies they used. He was interested in the company and wanted to know if we had a job for him. I was quite impressed by him. So we hired him and he stayed with us for many years. He left later on and set up his own successful business. We're close friends. He makes electronic amplifiers, and I think from time to time we have purchased things from him.

THACKRAY: When did he first come to you, approximately?

BECKMAN: We were in South Pasadena, which means that it was after 1941 and before 1954. It was in the early fifties (8).

THACKRAY: So by then Beckman Instruments was catching the eye of someone in the Harvard Business School?

BECKMAN: Now here was a case where I brought him in and put him in charge of a department. It was called Electronic Specialties. He was responsible for it, so he did his own hiring. My method of operation was that I wanted to keep small units. I felt that there's an advantage and flexibility in doing this. We split up into different divisions from the very earliest days, and gave a lot of authority to the managers in those divisions. We had the Helipot Division and the Spectro Division, and Jack Bishop got us into analog computers.

THACKRAY: During the postwar period, as business was expanding very rapidly, were you reading business textbooks in your spare time or were you operating intuitively?

BECKMAN: I was operating intuitively. I've never studied any books on management or had any course in management. I learned the hard way--from making mistakes. Now, this is hard to describe, and I hate to say this to put the public on. Sure it was my thing, but some would say, "What the hell is Beckman doing there if he's not business-oriented". Really, I'm not business-oriented. I do look at balance sheets and profit and loss statements and know what you have to know, but the fun, the heart of the thing, is in the technical aspects. I know that I have to have this to be financially successful, but that's for the controller to worry about. I'm concerned more about solving the technical problems and coming up with something that's useful for the advancement of science. It's that aspect of it that predominates.

THACKRAY: Well, Beckman Instruments is a very vivid example of the way in which brilliant ideas are paramount in the long haul. There's been a lot of commentary and discussion in recent days about the short-term orientation of American business, and the emphasis on quarterly, semi-annual, and annual reporting.

BECKMAN: That, I think, is one of the consequences of the emphasis on the schools of business administration. This is one of the serious problems of business. They look at the short-term, the quarterly reports, or even the monthly reports, and it's hard for a manager, the CEO, to look resolutely at the long range and suffer the consequences of adverse short-term results. I think this is being realized more and more now by some businesses. But we went through a period (in fact, our own company has gone through that) where we looked at the monthly reports and got all concerned when one group was up or one group was down. I think that our business generally has begun to realize that we have to have the long-range goals in mind.

THACKRAY: How does that carry implications for the way people are educated? What is it that we should be doing?

BECKMAN: Well, I'll tell you a story that General Dorio told me. He founded the Harvard Business School. He said that one problem we have here is that our graduates feel they're a failure in life if they aren't vice president at least two or three years after being on the job. He told a story about one of their graduates who was called into the office of the president of his company one day. The man said, "John, I want you to know that I'm going to make you a vice president as of today." John didn't say anything and the president said, "John, did you hear. You're vice president of the company now. Haven't you got anything to say." And he replied, "Thanks, Dad." [laughter]

I was aware of this thing, too, that when the Harvard Business School grads came out, they thought they were a somewhat superior breed of people. Business training is only part of it. There's an understanding of employees and motivation, things that are not emphasized at many schools of business administration. Early on, I was really lucky to have an employee relations manager like Don Strauss, for example, who's been with me over the years. It's paid off. We don't have any unions. We've had fifteen union elections over the history of the company and our employees have turned the union down every time. I feel happy about this because when employees feel they have to have the union to represent them, then the management has somehow been negligent in coming up with things. We try to give them all the benefits and provide a work atmosphere that employees are happy with, and they are. We have annual service award banquets and I'm amazed each time at how many long service employees we have. That's proof of the long range aspects of running a business. It's very expensive to lose an employee. Any employee, no matter what his background, learns a lot and has to be taught a lot on the job. When he leaves, you lose all that experience. That's one reason why we like to hang on to our long-term employees.

THACKRAY: These questions of entrepreneurship versus management technique are very interesting.

BECKMAN: They're not complicated. I speak now having no training in business administration, so I could be naive. I think it is really a matter of being honest. Be honest with yourself first of all, and then honest with your employees. I'll give you an example. One of our alumni, who lives in Honolulu, was with us for twenty-four years. Just last week she came back after she had been off for several years. She came back to the plant and brought leis for me and for Don Strauss. She was just in love with the place. She had tears when she left. This is the kind of attitude we get. It's a family affair, a loyalty that you can't buy, and you can't generate it by phony techniques. There has to be an empathy there, and part of it is that we try to be all the same way.

When I go out on the company floor, I talk to anyone. I'm glad to talk to them. There's no status symbol--well he's the number one guy, he's the number two guy, etc.--none of that. We have to have lines of authority, of course, but everybody can get the boss's attention. Over the years, I used to talk to employees from time to time, when we were modest in size. Here again I might be naive, but I told them the thing to do was to do your best. I said, "You're going to make mistakes. If you don't make mistakes, then I'm going to believe you're not doing much. Making mistakes is part of the job. But, there's one kind of mistake that will not be accepted, and that's a mistake based on any kind of dishonesty or lack of integrity. The one thing you must not do is anything that makes me ashamed to walk into any of our user's offices. You can make mistakes but they have to be honest, legitimate mistakes."

That's our philosophy. The users know that, too. They know they can depend on us regardless of whether we have a hard and fast written agreement. If something is wrong, we correct it. For example, we give a year's warranty on all of our instruments: if it fails within a year, then we'll repair it at our expense. That puts pressure on our employees to come up with high quality, because it's very costly to go out in the field and repair an instrument. So our employees feel the necessity of maintaining the highest quality in construction of the instrument, and in the design, too.

THACKRAY: When you were making the decision to leave Caltech and to go full-time with this venture, did you have any role models?

BECKMAN: I don't recall any role models. This built up gradually. It finally got to the point where in 1939 I said somebody had to run the show full-time. It was a case of whether I tried to do that or whether I went out and hired a professional to do it. That was a very difficult decision to make because Caltech at that time, particularly the chemistry department, was pure science oriented. The scientists looked down a little bit on anybody working on applied science, and anything that savored of commercialism was frowned on particularly. That's why I was careful. None of the pH meter development was tied into Caltech. It had nothing to do with my research. It's a little different from the present burst in entrepreneurship you see in the genetic engineering field. The pH meter was just a chance development that I did as a favor for Glen Joseph. My own research was quantum theory of photochemical reactions; it had nothing to do with pH. Nevertheless, I felt even if I went out and called it Beckman Instruments when I'm still on the faculty of Caltech, there might be some interplay. It was only when I decided to separate entirely from Caltech that I changed it from National Technical Laboratories to Beckman Instruments. That was a tough decision because I enjoyed my association with Caltech and also I had a feeling, which is an extension of this attitude of the pure scientist, that anybody engaging in commercialism was somehow a second-class citizen. It was a real tough decision to make. I thought also, am I prostituting my scientific training by leaving academia and going into industry? Well, I've overcome that feeling over the years. Many friends say that I've done more for science by making thousands of instruments available for others then I would have done with my own two hands in a laboratory. I hope they're right. I don't argue with them too strongly on that.

THACKRAY: That shows your good judgment once again.

BECKMAN: But having no business training, there are a lot of things I didn't do it in an ordinary way. I looked on business in much the same way as I looked on research. You encounter problems, they are tough problems, and you wonder how to solve them. Pretty soon you learn that the thing to do is to break the problem down into components if you can, finally get small enough pieces that you know how to attack. Well, it's true whether it's a financial problem or a technical problem. First, I learned that the most important thing is to recognize what the problem is, because it's so easy to think you know what the problem is. Then you start to solve that and you find out that it's not the problem at all, but something else. So, I stress that you must know what the problem is before you start attacking it. Then break it down into pieces, solve the individual pieces, and get on with the work. Have absolute intellectual integrity. Don't kid yourself or allow yourself to be the victim of wishful thinking. Those two are the fundamentals of my concept of how to run a business.

THACKRAY: I'm sure that out there in college chemistry and engineering courses today there are many students thinking that one day they would like to be pioneering their own business. What would you say to them in terms of training, in light of your experience?

BECKMAN: First of all, I think they should train in the areas of science or engineering that interest them. They must have the competence, first of all. Then they must have some level of common sense. It's easy to engage in wishful thinking--that's the dangerous thing. You have to be realistic. That involves such things as, if a person has an idea, is there a market there, what's the competition like? A person has to go through that step. There are also practical things. I've seen many small companies fail because they utilize their initial capital, which is really small, in the wrong ways. They'll fix up a good office, they'll spend it on typewriters and desks and things like that. They'll use up all of their capital on nonproductive things instead of using it for their working capital. It's amazing how often that phenomenon appears. They want to put on a prestigious front, and I think that's a mistake. We didn't. We started with tin sheds and built up gradually. So, make use of your available working capital, whatever it is, to the extent that you can in producing something that has a profit attached to it.

STURCHIO: That gets us back to the next question. I wanted to ask about the technical matters. In developing the DU I gather you must have financed that out of earnings from the pH meter.

BECKMAN: Yes.

STURCHIO: And a year or so later when you began working on the IR-1 and the Helipot, which were more or less simultaneous, did the money for that come from contracts from the government? You must have had to do more development work and put on more production?

BECKMAN: No, we didn't have development contracts: we were selling products. No, this wasn't in the good old days when the government said "Here's a lot of money to go ahead and do something." No.

STURCHIO: So they were production contracts?

BECKMAN: On the Helipot for example, which was strictly government, they didn't give us one dollar for development. They said we'll buy the things from you when you get them. So we had to absorb all that development cost out of our accumulated earnings from pH meters.

STURCHIO: Did the cost of the contract cover your cost of gearing up to meet vastly expanded production?

BECKMAN: Not in that era. Later on, as we began to get more sophisticated, we got the government or even a private company to put up money. For example, we got IBM to put in several hundred thousand dollars to build a plant for us so we could make more little resistor devices for them. But that's after we got a little more sophisticated. We never had grants from NIH in those early days. It was purely our own money being risked. As a matter of fact, we weren't alone. The whole trend of industrial development changed during the war. When the war came on there was an urgency on the part of government. They then became much more generous and were willing to put money into these things. That came later.

STURCHIO: You started to tell us earlier about getting involved in the infrared developments with the synthetic rubber project.

Could tell us again how that came about and how you were invited to the Detroit meeting in 1942?

BECKMAN: One of the persons in charge at the Office of Rubber Reserve was the one who called this conference. We had Brattain there, and Barnes was there, and others. At this meeting the Office of Rubber Reserve made the decision, first, to use the infrared techniques, and second, they thought that Bob Brattain's design was the one to go on. That was a single beam thing...very, very crude, it had no relation to an infrared like you saw later. It's parts were assembled on a surface plate to get a flat surface.

STURCHIO: Here's a description of that (9).

BECKMAN: That's 1941. I'd forgotten this article. How they adopted this, I don't know. Bob Brattain was, I won't say an agressive person, but he was pretty well convinced that what he was doing was the right thing. I imagine he conveyed this impression. There it is [pointing to Figure 6 on page 7 of Brattain and Beeck article--see following page]. It's all laid out on a flat base on here and had the advantage of being on a crystal. We had mass with enough weight that it wasn't so susceptible to motion by displacement by vibration. Also, these were all freely located, you see. We'd move them around, just fasten them down to the base. That was the advantage on that. I recall a detail also that he had a straight slit on one of the things we put in, and we curved the slit so we could get a little higher resolution out of it. We made a hundred of these things, as I recall. That was the order we got from the Office of Rubber Reserve.

STURCHIO: Bowling Barnes and Van Zandt Williams had built a machine at American Cyanamid around the same time. They had the problem of analyzing butane and isobutane mixtures. The thermocouple output was tied to a deflecting galvanometer so that the output was then photographically recorded from the galvanometer beam. It occurred to me that one way of making the output more accurate (and this is something that Barnes and Williams mentioned in an article(10)) was to get some kind of photoelectric mechanism in there. This was the same problem you faced with Glen Joseph and the pH meter, wasn't it?

BECKMAN: We used a galvanometer on this. It was a Leeds and Northrup HS galvanometer with an optical beam one meter scale out there, and that was the recorded thing on this, too. There was

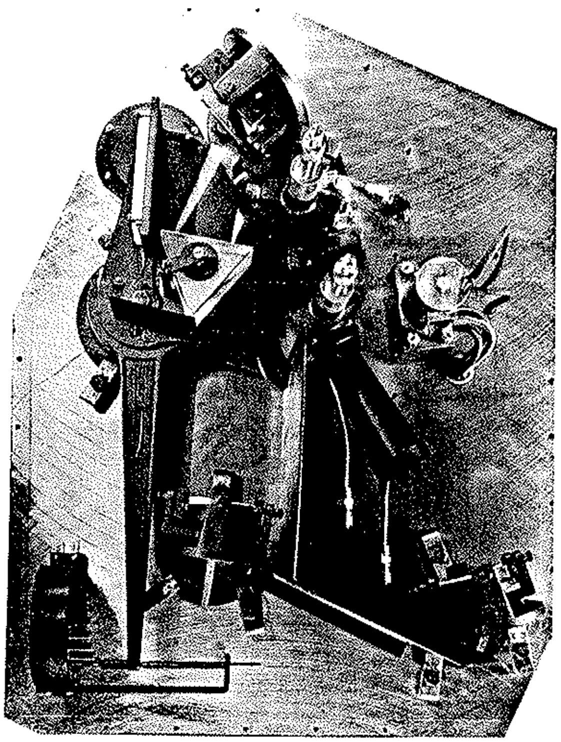


FIG. 6. Versatile analytical infra-red spectrophotometer assembled on base plate.

VERSATILE SPECTROPHOTOMETER[2]

Figure 4 is the light path of the more versatile spectrophotometer for routine analysis.[2] The principal differences between this instrument and the one described above are in the slits and the prism mounting. The prism P_1 and the Wadsworth mirror M_4 are mounted on a turntable. The angular position of this table and, consequently, the wave-length of the radiation arriving at the thermopile T are controlled by a tangent arm resting against stops. There are eighteen such stops which are adjustable from outside the spectrophotometer case. Since one of these is used for checking the focus at the sodium D line, this allows the instrument to be set rapidly and reproducibly to determine the transmission of a sample at seventeen spectral positions. This instrument and the simpler spectrophotometer were designed for work in the 2μ- to 15μ-region. However, other spectral regions can be covered by using a material other than rocksalt for the prism and windows.

Since the different wave-length settings of the prism require different slit widths and since more complicated analytical procedures may require smaller spectral slit widths, slits S_1 and S_2 are adjustable from outside of the instrument case. Also, slit S_1 is curved sufficiently to compensate for the curvature of the image caused by the prism. Furthermore, the change of the refractive index of the prism, with temperature, causes a shift in the wave-length of the radiation reaching the thermopile, and when small spectral slit widths are used, this shift will effect the accuracy of analysis. Consequently, the holder for the Littrow mirror M_5 is built so that it rotates M_5 to correct for the change of the refractive index of the prism. This refinement is not necessary with the large spectral slit widths used in the simpler spectrophotometer described first.

RADIATION SOURCE

The parts of the special Nernst glower source are shown in Fig. 5. The water jacket (6 cm high) on the left is made from standard sizes of brass tubing. If the source is operated in a fire-restricted area, a rocksalt window is sealed into the recess in the flange. Under these conditions, the screw in the top can be removed periodically to replenish the air (oxygen) inside the jacket. The right-hand view shows the Nernst glower and its starting heater assembled. The glower and the heater are located, with respect to the stainless steel studs, by the two mica washers. The studs are spaced in the heat-resistant insulating disk so that they fit a standard four-pronged base. This disk for holding the studs also serves as the bottom end of the jacket. The assembled lamp on its base block is shown in the general view of a spectrophotometer given in Fig. 6. All holes which affect the position of the Nernst glower with respect to the base block are jig drilled, allowing the source lamp to be replaced without readjusting any other parts of the spectrophotometer.

The ballast resistance, the transformer for the heater, and a relay are in a separate box. The circuit is such that closing the switch to 110-volts a.c. impresses the proper voltage across

[2] Now being manufactured by the National Technical Laboratories, South Pasadena, California.

the same problem though, how are you going to measure electrical output down in the 10^{-8} or 10^{-10} ampere region?

STURCHIO: Did you eventually design it so that you could use an electronic amplifier in the circuit?

BECKMAN: We had the amplifier sitting in our pH meter, prior to this time. Later on, we realized that this a good way of measuring the output of a photocell, then that was applied to this.

STURCHIO: To the IR as well as the DU?

BECKMAN: Yes.

STURCHIO: Was that during the war or after?

BECKMAN: Well, that was in '41-'42, during the war.

STURCHIO: I see. We spoke a bit earlier about the visible and UV spectroscopy community. IR was a newer technique, at least as far as using it for analytical purposes?

BECKMAN: Yes, that was used to extend the range. The quartz prism in the DU would work in the nearer ultraviolet and the near infrared, but this extended the infrared range out much farther.

STURCHIO: Had you and your colleagues had contact with Barnes and Van Zandt Williams, Dudley Williams, Norman Wright, and some of the other people working on IR spectroscopy at that time?

BECKMAN: There were meetings at technical conventions. And then this, of course, was a rather ironical situation. Bowling Barnes's instrument was a double beam and ours was a single beam instrument. We were pretty cocky and proud of ourselves having got the contract for this thing. Well, that was a very costly contract from the standpoint that our users were all subject to secrecy classifications. They were not allowed to publish. Bowling Barnes's users were not. So he could go out and sell to the general public, and he did sell. These people could publish. The literature then, for a period of two or three years till the end of the war, in fact, was entirely related to the use of Bowling Barnes's instruments. In the meantime, people didn't know that we had actually made more spectrophotometers than Bowling Barnes had: we had nothing in the literature! We had one heck of a sales problem when the war was over. People wanting to use new instruments referred back to the literature to see what so-and-so used, and it was a Barnes instrument. We had an uphill battle for years. In fact one time we had to make a decision, were we going to be in the infrared business or not? I made the decision that, yes, we were going to be a leader in the infrared business, and we spent a tremendous amount of money on development and subsequent models of the infrared spectros. But again, this was a business matter that we didn't anticipate when we got into infrared.

STURCHIO: Barnes went to Perkin-Elmer right after the war, and so I guess it was Perkin-Elmer instruments you're talking about. Did they also sell through dealers or did they have their own sales force at that time?

BECKMAN: They had their own sales force, and they sold through some dealers also at that time.

STURCHIO: In a sense, they were competing for your prime market, which was the dealers.

BECKMAN: Another factor began to be recognized here. Dealers, by and large, were not able to handle sophisticated instruments. They could handle a balance, or something like that, but when you come to spectrophotometers, that involved more expertise than their salesmen really had. So, the more costly, more sophisticated instruments really were not suitable for dealer handling. That was being recognized by Perkin-Elmer. There were some sales in there, but it was sort of a vague, gray area.

THACKRAY: When do you think that problem really began to be recognized? Was it already emerging before World War II, or was it a problem that really grew under conditions of war?

BECKMAN: I don't really think it had much to do with the war. It was the nature of the instruments. They could handle a pH meter because they had a little portable thing, and if you had a service call at worst you could pick this instrument up and take it back to the shop. You couldn't do that with a spectrophotometer. That's when you had to have a trained service force. It was when instruments of this degree of sophistication began to be used that the need for trained service people began to be recognized. And a few dealers, such as Arthur H. Thomas, did provide these trained people, but many dealers didn't. When anything went wrong they blamed the manufacturer. Even though they were given a very substantial discount to provide that service, they weren't doing it. They'd say, "Well, the manufacturer is at fault on this." That was one of the reasons it began to be a very costly situation and we realized we had to change.

THACKRAY: This was all in the background of your 1960 decision?

BECKMAN: That's right.

STURCHIO: This is a photo we had of the innards of the IR-1 (see following page). One can see visually how it's a much more complicated instrument than some of the earlier ones.

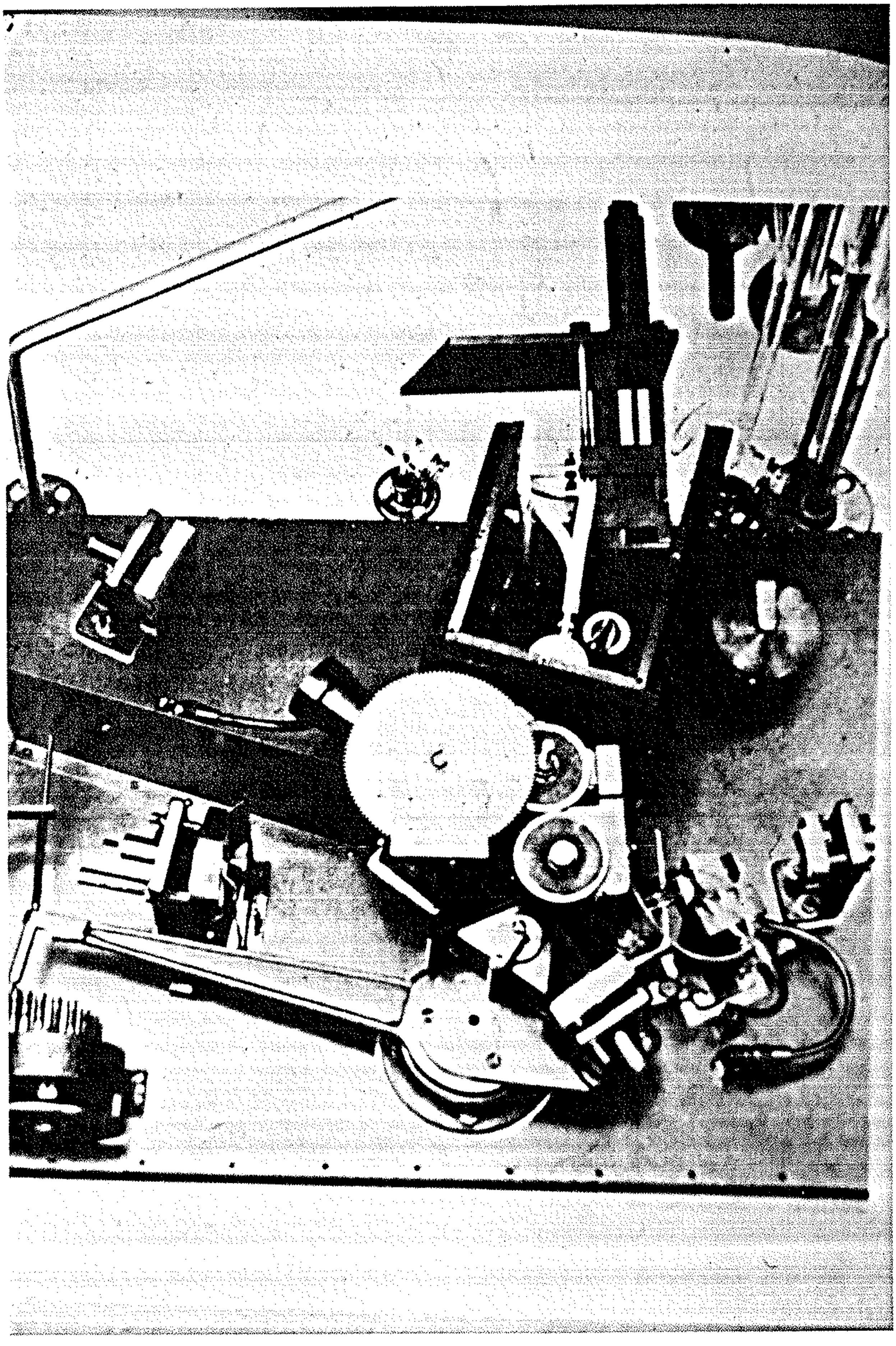
BECKMAN: Yes.

STURCHIO: We've spoken so far about the Helipot and the IR-1, both of which were developed during the war and sold to the government for synthetic rubber and use in the development of radar. What other projects did Beckman Instruments have during World War II, related to wartime work for the government?

BECKMAN: Those were the main ones.

STURCHIO: You mentioned some work for the Manhattan Project?

BECKMAN: Oh, yes. We did make some mass spectrometers, and we made Geiger counters, too. I had forgotten about that. Also, we



made a pocket electroscope, a thing for measuring dosage. That was made in another company, Arnold O. Beckman, Inc. That was another subsidiary. We made oxygen analyzers that Pauling had developed (11). Our board of directors didn't want to take that on because it was under secrecy wraps. They said they didn't want to buy "a pig in a poke." I got permission to do this as a personal venture to help out my friends at Caltech. A lot of interesting stories are connected to that, did I ever tell you about that?

STURCHIO: No, please do. We would like to hear more of that.

BECKMAN: Holmes Sturdivant, who was an assistant to Linus Pauling, came over one day and wanted to know if I would provide him with 100 wooden boxes for an oxygen analyzer. Then I found that the Navy had asked Caltech (Pauling was involved) to come up with an instrument for measuring the partial pressure of oxygen. They hoped to use it on board submarines. They had a demonstration instrument to show that they could use a phenomenon noted by Faraday based on the fact that oxygen is paramagnetic, almost one of the only common molecules that is paramagnetic. All the others are slightly diamagnetic. If you measure the paramagnetic susceptibility, why you could measure oxygen, for all practical purposes.

It's a very simple instrument, you just had a magnetic field with a field gradient. They put a little glass ball in there, two of them, like a dumbbell, because the mechanical forces are much greater than the magnetic force. You balanced that; and it was suspended on a quartz fiber and would rotate slightly in and out of the magnetic field until the magnetic force was exactly balanced by the torsional force in the quartz fiber. Then you had a little mirror on that and a beam of light, and you could have a spot of light on the scale--very simple. They were going to make a hundred of them. We provided them with the boxes. They came back later on wanting to know whether we'd make the instrument. They found out that making a hundred instruments to predetermined specifications was a little bit different than making one demonstration model. [laughter]

They had a group of people, none of whom had ever worked before, girls primarily, doing this thing. This is what I brought to our board of directors, and they said "No, we don't want to do it." I set up a little place on State Street and I took over the group from Caltech. They were fine people, but none of them had ever worked before and they came in and started to make these oxygen analyzers. One of the main problems we had to solve was, what kind of background music were they going to have? They knew that industry had background music, so were they going to have popular music or classical music, and things like that. In the meantime, they couldn't make these things.

This oxygen analyzer involved making little bulbs about an eighth of an inch in diameter, very thin walls of glass, and joining them together so that they were perfectly balanced. Making those bulbs was quite a problem. I didn't realize this after all, I taught glassblowing among other things at Caltech, and I didn't give a second thought to it. These were made by blowing a bulb on the end of a very small capillary, which is the connecting link between the two. The surface tension of molten glass at that small radius is such that it takes all the force that you can generate with your lungs to overcome it. As soon as the bulb gets any radius, it just blows out like that. Caltech had one man, Herb Sargent, who was a Ph.D. candidate in organic chemistry. He worked with the techniques to learn just how to coordinate his muscles, and he could clamp down, bite down on the blowing tube. He could get maybe one out of 100 or one out of 200 that wouldn't blow out. That was the source of supply. [laughter] He was leaving Caltech in about three or four months, after he got his degree, so he was not a very reliable source of supply. This caught me completely unaware.

I went down and realized what was wrong. We had a mismatch in the capacity of this small bulb and that of your lungs. What you had to have was a blowing machine which had a small bulb or small source of compressed air in which the pressure would drop down automatically. So I went down to the shop one weekend and made probably the world's smallest glass blowing machine. It was nothing more than a sliding valve with which you could transfer a small volume over from a tank of oxygen or air. Of course, since the volume was small, as soon as the bulb began to grow the pressure went down. So we went over to about 100% yield, 99% yield. That was one of the things that I had to do. That was used in production for years and years. Well, this was one of the things we had to learn.

Then, we had to balance these things. Balancing was very difficult. The mechanical force was something like 100,000 times what the magnetic force was and we had to balance this very, very carefully. We did this initially by putting little dabs of glass. We finally got down to the point that that wasn't it. We got the final balancing by vaporizing on silver chloride. Just a little of that, because you couldn't touch the thing, you see. We got so we could balance these bulbs beautifully.

The other thing was making the quartz fiber strong enough. We learned the techniques on that. You make a fork out of fused quartz, fused silica, and then just kind of compress that a little bit to seal the fiber. We had to learn just enough to put the right tension on that. Then we had to draw these fibers down precisely. The way they were making the quartz fiber at the time, they took a small quartz rod, maybe half a millimeter in diameter, something like that, heated it up in an oxy-acetylene flame, pulled it out, and probably it would pull apart. Somewhere in there then, was the quartz fiber of the right diameter. You couldn't see it. To do it they'd take a label, wet it, pinch it, make a V out of it, and where they thought it was they'd squeeze the label together. If it fell to the floor, then they didn't have it. If they had the fiber in there, then it would swing. That was the way they actually got the fiber. We had to learn how to actually draw them. We put up a bicycle wheel and spinner out there. These were some of the problems we had. We finally made them, but we didn't supply many to the United States Navy because they were still arguing over the specifications when the war ended. We sold quite a few to the British Navy. The sales just didn't amount to anything.

Then in 1955 an article appeared in the Saturday Evening Post by two doctors from Johns Hopkins (12). They had been concerned over the high degree of blindness in premature babies. Something like one third of all babies would be permanently blinded, and during a piece of chemical detective work they found out that the incidence of blindness was highest in the so-called "best" hospitals, those that had the best equipment. They found that they were using these infant incubators, running up a high oxygen content to help them breathe. But when they got over 40% oxygen, it caused retrolental fibroplasia and permanent damage. What you had to do was keep the oxygen down below 40%. Well, that article appeared in the Saturday Evening Post, not in a technical journal. It brought our sales right up because pregnant mothers would ask the hospital, "Have you got one of these oxygen analyzers?" So that was the story of the oxygen analyzer.

THACKRAY: Was that after the war?

BECKMAN: Yes, it was after the war, 1955, that this article appeared in the Saturday Evening Post.

THACKRAY: How long did Arnold Beckman, Inc., continue?

BECKMAN: That carried on for several years. When we began to sell oxygen analyzers in some quantity it created confusion among our customers. What's Arnold O. Beckman, Inc., and what's Beckman Instruments, Inc.? We finally decided we ought to fold the thing, so I told the board of directors that I was going to stay out of it: "If you want to buy it, well then you come up with what you think is a fair price and I'll take it." They did, and I forget what it was, about a million dollars, something like that, some round number. That was a good, fair price, and we merged the two at that time.

STURCHIO: Was that the first entry of Beckman Instruments into the health field?

BECKMAN: Oh no, the pH meter had always been in the health field. I recall we rushed a spectrophotometer airmail to a hospital in Oakland where Mrs. Kaiser was very ill, and they tried to save her life with that.

STURCHIO: That just shows my own shortsightedness. I always think of pH meters and spectrophotometers in chemical laboratories, not thinking of the early clinical applications.

BECKMAN: Yes, both of them are widely used in hospitals and medical research.

THACKRAY: Let me ask you a couple of things about the period before the end of the war, and then we may go into some of the postwar topics. The first is that when you became president of Beckman Instruments you mentioned that you had a board of directors. Who were some of the early members of that board and how was it organized?

BECKMAN: They were members of the National Postal Meter Company. It consisted of Lowell Stanley, who was the accountant and treasurer of National Postal Meter, and who continued on almost until his death (he died just a few months ago); A.T. Jergins; and Fred Ehrman, who was a partner of Lehman Brothers at the time.

There was a group of investment bankers, five or something like that, of which Lehman Brothers was the lead firm. They had bought Jergins Oil Company and in the course of buying that they found here was an asset called Beckman Instruments. Well, they came over to see what it was. We were struggling on in South Pasadena. The other members of this investment group were not interested, but Fred Erhman was and he stayed on. The rest of them bowed out. He came on our board, and that was my first introduction to finance, something to invest in. Up to this time we had gone entirely out of earnings. He saw our growth curve, which is still about 50% a year out of earnings. He said, "Could you grow any faster if you had more money?" I said, "Yes, because we have a lot of research we'd like to do but we can't afford to take this on when we're making decisions such as, do we dare to go to NMR or anything like that?, where we're limited solely by the available funds." We could borrow money, but I was reluctant to borrow and have more than 25% of our total equity tied up in loans. That was an arbitrary number. He said, "Why don't you go public?" We went public in 1952, I think, and that was a result of Fred Ehrman.

Then there was Ray Waestman, who was a lawyer for the Jergins Oil Co., and I.H. Lyons, who was president of the National Postal Meter Company. That was the board.

THACKRAY: So it was very heavily influenced by the...

BECKMAN: Oh yes, they owned it, you see. We never did have a board for Arnold O. Beckman, Inc.

THACKRAY: In 1944 when the war was in full swing but the promise of the end of it was there, what was the breakdown of your products in terms of dollar volume, and what were you thinking about the postwar world?

BECKMAN: I'll have to get that number, my memory is not good enough. I should point out that we had split off Helipot. Much of Helipot had made 40% of our profit. As time went on we got concerned because we couldn't make them fast enough. We were concerned that the government might force us to stop making pH meters that didn't carry triple A priority and put all of our production into Helipots. We split off Helipot into a separate corporation. Later on we folded it back into the company after we moved out to Fullerton after 1954. So, I'll have to give you both Helipot and Beckman Instruments. Remind me to get that information.

STURCHIO: Clearly, you were making thousands of Helipots, but only about a hundred IR instruments. Do you recall about how many pH meters were being made then, in order of magnitude?

BECKMAN: I could give the actual numbers. I'll get you those. I think that we sold eighty-seven meters the first year. A hundred...I'm guessing now. In connection with our fifty year history, we did bring out those figures.

THACKRAY: As you looked at the likely shape of the postwar world, what business implications did it carry?

BECKMAN: You're making the assumption that I did look ahead. I was enjoying myself. I have to confess that I did not sit down and come up with any long-range projections. Later on, after Fred Ehrman came on the board, we did that because he brought in some of the more conventional investment reasoning.

STURCHIO: Was that a direct consequence of going public?

BECKMAN: I think so. I think if we hadn't gone public it was just a happy little family affair, drifting along, trying to stay in the black. But he brought in a larger-scale look. When you have outside shareholders, you have a different type of responsibility.

THACKRAY: Going into the postwar world, when did you begin to get actively reinvolved with Caltech? By 1964, you were chairman of the board.

BECKMAN: I went back on the board in 1953, I think. I maintained close personal.... We lived right there. We went to parties together, went to seminars. I never lost contact. I lost official relationship with them. But of course through the Pauling Oxygen Analyzers I was the biggest contributor to their patent royalty fund for a while. Not a lot of dollars, but still it was the largest one, about the only one there for a while. So I maintained close relations. I recall I was back on the board in 1953 with Norman Chandler, owner of the LA Times. I became chairman around 1964 or 1965.

THACKRAY: The early period of Caltech, the Millikan and Hale era, has been studied from an historical point of view, but the era immediately after World War II has not yet been subject to historical scrutiny. Who were the great architects of this era? Who do you see as the key?

BECKMAN: Caltech was going through a change in thinking. I mentioned earlier that Caltech was a pure science institution, and its people did not get heavily involved in applied research.

There were a few who would do consulting work. Dr. Lacey, for example, who was Dean of Graduate Studies. We didn't have chemical engineering; we called it applied chemistry in those days. Dr. Noyes encouraged him to have some limited contact, primarily with the Petroleum Institute. He and Bruce Sage were studying the thermodynamic properties of those hydrocarbons under high pressure and temperature. That was one.

In the Electrical Engineering Department, Dr. Sorensen was concerned with the high-voltage transmission of electricity. Also, the Metropolitan Water District would come along and Dr. Franklin Thomas was involved with that, but these were special large-scale projects of interest to Southern California. The Institute thought it had an obligation to help out the local community if it could.

Then World War II came along and the physicists, in particular those who had gotten into chemistry, got involved and they set up a facility in the Royal Hotel. There were a lot of physicists--Smyth, Du Mond, Ernest Watson, who was Millikan's man Friday. The chemist Pauling was involved. They went out and really put together what we later called a "think tank". This is the first time that Caltech really got wholeheartedly behind a practical, applied type of activity; this is when the tide turned. They found that they got paid handsomely for their consulting work, which may or may not have had something to do with their acceptance of that role.

THACKRAY: In the years immediately after World War II, was there serious thinking about going back to the way of doing business in the 1930s, or did people see this as an irreversible change?

BECKMAN: You have to keep in mind that I was not on the faculty of Caltech at that time. I was a little bit of an outsider. I think the change was permanent, for two or three reasons. First, they saw that science by itself, for the sake of knowledge, was really not a complete story. Until you used science in some way for the benefit of mankind it wasn't complete, so I think that was brought home to them. Also, they learned that applied science wasn't necessarily dull and involved a low level of intellect, which some of them had assumed before. Another was they found it highly interesting to be involved in the problems of industry as well as government. I think for many of them the experience opened their eyes to a new world, the world of applied science.

THACKRAY: Just staying with Caltech for a minute, in your period as chairman of the board there, what were the major opportunities and problems that you faced and the major thrust of that period?

BECKMAN: For one thing, that was the first time Caltech had to put on a fund-raising drive. Up to this time money had come in primarily by personal solicitation by Millikan; he was a great fund-raiser. There had been no formal fund-raising. So we put on a drive.

As I recall it was either 16 million dollars or 19 million dollars, in 1958, I think. I had a boat built up at Sausalito, one of the first large fiberglass boats, 40 feet long, and I was sailing it down over the Thanksgiving holiday of 1958. This was right around our fund-raising drive. I was chairman of the drive and we had a meeting scheduled at Caltech and I had to kick this thing off. As I recall, it was on a Monday evening. We were sailing down with the boat and we were becalmed. We realized we were not going to get there by 1:00 p.m., so finally we pulled over at Goleta, which has an old, partly abandoned pier sticking out there. I hopped off and went over to the highway to hitch a ride so I could get in. This was my first experience in hitchhiking: it's not that easy! All these cars go whizzing by and whizzing by. Finally, a fellow stopped in an old T-shirt. He was a plumber and had been up at Camp Roberts, hoping to get a job. He was coming by and he stopped to pick me up. I told him I had to get back to Santa Barbara to get to a telephone. It turned out he and I had a little in common. We were both former Marines. He also was in AA, which I was not, but anyway, we got along well enough and he took me home. When I got to Santa Barbara, I called the vice chairman, Ed Valentine, whose family owned the Robinson store there. I told him I was not going to get back in time for this thing, would he go over and hold the meeting? So he did that.

The next day, or day after, the LA Times had a column about Arnold Beckman hitchhiking down from San Francisco. He wasn't able to make it in time to kick off this thing. Right along in the next picture was Zsa Zsa Gabor, something about her, so I never lived down that hitchhiking experience. [laughter] The main thing now, of course, is the modesty of that campaign compared to what we have now. If we don't have a hundred or two or three hundred millions dollars, why, we don't call it a campaign. That was one of the first things we had.

Caltech was expanding gradually. There was always a problem on campus as to whether engineering was being slighted with respect to science. That's a perpetual problem.

THACKRAY: What could you do with that problem except listen sympathetically?

BECKMAN: Sit down and talk with them. That was when Bob Cannon was brought in as Director of the Engineering School. This is a problem that the Provost has to handle--how to allocate the funds between the two. That will always be a problem, and to a lesser extent within the scientific divisions too. The biologists think they don't get as much as they should compared to what the chemists get. The chemists have always fared pretty well at Caltech, and I think that goes back to Arthur Noyes and the fact that they have had a good chemistry deptartment there. I'm trying to think of other developments in that period, 1964-1974 when I was chairman. Do you recall anything from then?

THACKRAY: No, since it's not a matter of historical record.

STURCHIO: If you look at the way in which research monies were allocated nationally and where money for R & D was coming from, the period of the late 1960s was the heyday of Federal, and especially defense-supported, research. After about 1968 or 1969 there was a relative decline in the amount of federal funds for research and development.

BECKMAN: It was during the Johnson term when they really went down.

STURCHIO: How did the change in the complexion of support for research and development affect Caltech, as it must have?

BECKMAN: Caltech has always been able to raise a fair amount of money. People like Millikan, when he was there, had success because it had the reputation. Also, Southern California around the Los Angeles-Pasadena area was building up with affluent people, people who wanted to contribute. Caltech did not have the fund-raising problems that others schools had. We'd find individual donors who'd come in and underwrite a project. As I recall, we were not overly concerned with fund-raising, the way we are now, for example. Millikan and Noyes and others still had a good deal of weight in Washington, and later with the National Science Foundation: they had clout back there. I don't recall ever really staying awake nights worrying about how we were going to keep the doors of Caltech open.

STURCHIO: Caltech never came to depend solely on Federal funding, the way many other research universities did?

BECKMAN: It got up to around the 50% point. That was one thing I was concerned about. We formed a special committee on it. When we were getting up over 42 or 43% we were asking, are we getting too dependent on it? It's about 54%-55% now. It's a little difficult to do the accounting, because Caltech runs JPL, and that's bigger than Caltech. They have 5000 people up there with a budget of $200 million or so, I've forgotten the figure. That began to have a strong effect on Caltech, and there was concern expressed by some of the faculty as to whether or not we were doing too much and if the influence of JPL would deflect Caltech from its basic science approach. That question comes up every now and then. Particularly during the late 1960s and early 1970s when you had all this student activism, there was a lot of concern at Caltech whether or not any defense research would come in. They had a flat rule that no classified work would be done at Caltech. For a long time they had it at JPL. Now, in the last few years, an increasing amount, probably about 25% of JPL's activity, is defense or in a classified group. But the pressure, the concern on campus, has died down.

THACKRAY: Do you know if that rule is still enforced on the Caltech campus? Presumably in World War II, there was no such rule.

BECKMAN: Under the pressure of the patriotism drive, they set this think tank up, took over the Royal Hotel, and just devoted themselves to it. It was a case of "get on and win the war." It's only after that they began to get concerned about it.

THACKRAY: When did that concern develop? Was the rule that there should be no classified research already there in 1964 when you were chairman of the trustees?

BECKMAN: I don't think so. I think that came later because this was a rule by the faculty. The board did not do it; this is a faculty function. The board stayed out of academic decisions. I think it came out later on. Millikan was there till 1946 and then DuBridge came in and this came in under DuBridge's regime.

THACKRAY: There are a couple of technical areas at least that we should probably get back to. One is the air pollution area and the other is Shockley and the transistor...

BECKMAN: debacle

THACKRAY: ...area

BECKMAN: On air pollution... I got involved in that because I was active in the Los Angeles Chamber of Commerce. I was president in 1956 and head of their so-called science committee. In the effort to get butadiene, the local gas company had a project that was cracking oil, and then quenching it. They had a plant down on Vignes Street, not too far from the City Hall. The City Hall had just built a new building. One day when they put this plant on stream all the air was filled with phenols, aldehydes, all these things from cracking, and they had to shut down this building. A hue and cry came up about that and Paul Jeffries, retired president of Union Pacific, was finally put in charge of a special committee to determine what to do about it. There are many plants down in the city of Vernon, which is entirely surrounded by the city of Los Angeles. Bethlehem Steel was there and I think Atlantic Richfield had a refinery, and a lot of seriously polluting industry. They were in a separate city, so that neither the county nor the city of Los Angeles could invade the perimeter and make them enforce it. So a bill was passed in the state legislature, Assembly Bill 1, by a Mr. Stewart from Pasadena, which created air pollution control districts, with county boundaries on them. This gave them authority to invade any part, even municipalities, to enforce air pollution regulations. That was the beginning.

Now they had to set up a district. They did and they brought in a chemical engineer from the University of Illinois as the first director. He was under tremendous newspaper pressure. I have some old clippings from those days. Even the ministers got up and talked about those murderous sulfurous fumes from Hades coming up. They all assumed it was sulfur dioxide that was causing all the trouble. The reason was that they passed an air pollution law which specified that the effluent from a stack cannot have more than two tenths percent SO2 by volume. This was written in, not on the basis that we had SO_2, but because the lawyers write a law based on precedent. The only real air pollution case in the law books was up in British Columbia. What was the name of that town? Anyway, there's a smelter up there and this was an international suit that involved Canada and America. The sulfur dioxide from this had killed all the vegetation for quite a way around it. The litigation finally ended on that, that they would limit the sulfur dioxide to two tenths of one percent by volume. Well, that was written into the California code. Then everybody assumed that sulfur dioxide must be the pollutant because that's the only pollutant mentioned in the code. That's when the press took it up--these murderous sulfurous fumes and stuff like that.

Dr. Lou McCabe, the director, the poor guy was under pressure, and suggested that we bring out some of the surplus ammonia plants that we had and react with this sulfur dioxide to make ammonium sulfate, which would be a source of revenue and make the thing supporting. I was chairman of the scientific committee of the Chamber of Health, when we had a little problem with Illinois. We'd go on an inspection trip up to Gary and you can smell SO_2 far below two tenths of one percent by volume. So I knew it was not sulfur dioxide and said "Lou, we're going to make a booboo here. You'd better analyze the air and see what is in the air before you start to bring in facilities to remove it."

Haagen-Smit was over at Caltech and running the microchemical laboratory, which I got as a service for the biology department, primarily. I persuaded Haagie to make a couple of analyses. We got a group together: Gordon Alles, Haagie, McCabe, and Dr. Vivian from USC. We passed about 500 cubic meters of air through some liquid nitrogen traps and got a little drop or two of smelly brown stuff and gave it to Haagie to analyze. He said this is peroxy organic material, peroxy butanes, I believe. At this time the Western Oil and Gas Association had retained Abe Zuram to study the air pollution problem and Abe said there was nothing to that, that Haagie didn't know what he was talking about. We arranged for Abe to come over to Caltech to give a lecture on this down in the chemistry lecture room. Then I got Haagen-Smit to sit down next to me. Abe said he had studied these peroxy compounds and they were not eye-irrating, didn't smell, or anything like that. It's unfortunate that a chemist of Haagen-Smit's caliber could be misled like this. Haagie was a Dutchman, and a damn good chemist. He just said, "Well who's telling me I don't know what to do?" I got him to agree to take a year's leave of absence. Probably by this time he didn't want anymore to do with it, because he was working on root growth hormones and a lot of other biological work involving very good microchemistry. He finally got his dander up and said, "OK".

We got him released from Caltech for a year and set a laboratory in Los Angeles. That's when he worked out the photochemical step. He found some light was captured by nitrogen dioxide and finally transferred over to oxygen to form ozone. That was how that got started. In the meantime, the press was full of stuff on sulfur dioxide. The oil industry was saying there was nothing to it, and the problem was the backyard incinerators. At that time, we did have backyard incinerators. I lived then about 1400 feet up at the mouth of Eaton Canyon, and Haagie used to come up to my house early in the morning when the incinerators were working. It was a very interesting phenomenon. The smoke would come up from these incinerators and then it would spread out laterally and disperse. There was stratification.

We had a meeting at the Huntington Hotel. Steve Royce had just had the dining room decorated. We had a meeting with Charlie Heinan, director of research for Chrysler. They were trying to say that automobiles are not doing this thing, it's just the incinerators. To show that, they had a gallon can, and they stuffed some newspaper in it and set fire to it while we were around the dining room table. The smoke came up and Royce had just spent a lot of money to have this decorated. They had the fire department in there.

Well anyway, Governor Young appointed a special air pollution committee, and I was chairman. Reed Brantley was on it, Haagen-Smit, Gordon Alles, and another one whose name I've forgotten. We met a lot of times and we finally came back with a report, which was the air pollution "Bible" for a long time. On the strength of that, Hal Kennedy, who was the Los Angeles county counsel, wrote what is now the basis of existing laws. We set up the alert system stages; I spent hours working on that. That was how air pollution regulation got started in California.

I recall another incident. A group of us were with Fred Hartley. We flew back in his plane to Detroit one weekend to try to inform the automobile manufacturers what would lie ahead of them. We couldn't meet with them in a group, because of antitrust laws. We had to meet with them individually and I recall meeting on Sunday night with Henry Ford II. He had a little bit of a drinking problem at that time. He said, "What is this, another delegation trying to tell us how to run the automobile industry?" That was the attitude he had. We tried to tell him, "No, we're trying to tell you what you're facing in the way of regulation, and the requirements you're going to have to meet." They wouldn't do it. There are two approaches in our report that we recommended: getting away from the internal combustion engine and going back to the steam boiler type, like the Stirling engine. We felt that this deserved study and he had two choices: either come up with a nonpolluting engine or clean up the dirty exhaust. They chose the latter. I suspect they did it because they could see a big repeat business on the afterburners, which is unfortunate. Anyway, that was part of it. We tried to get the message to automobile manufacturers and they turned us down. I think if we had spent the amount of money we spent on afterburner research on developing a nonpolluting engine, we'd have a nonpolluting engine now. Whether it's a Stirling engine or a hydrogen fueled engine, or Lord knows what, we have to come to that. That was another episode.

Later on Nixon appointed me a member for four years on the National Air Pollution Board. That's when Ruckelshaus was the head of it. Even at that time, sulfur dioxide was still ranked in the minds of the people as being the main pollutant.

THACKRAY: Automobile testing has just reached the Philadelphia area. It was just made mandatory this last year.

BECKMAN: I just had to inspect my car. It cost $42.00, $6 just for the certificate. Of course, we make smog analysis instrumentation. We have the probes that you just stick up the exhaust, so, I guess I shouldn't complain. [laughter]

STURCHIO: Before we end, would tell us the story about Shockley and the transistor?

BECKMAN: As I recall, Shockley called me up one Sunday night when he was still at Bell Labs and wanted to know whether I would go on the board of a company he was thinking of starting. He was thinking about leaving Bell Labs and starting a company to make transistors. I asked him a little bit more about who else was going to be on the board and it turned out that he was going to have a board composed of almost everybody who was in the instrument business, all of whom would be his competitors. I said, "You just can't do that, you'll have such a thing as conflict of interest, Shockley. If you want to do this, come out and talk it over." He came over and we found out that he wanted to go into business and make some money--anybody could make some money. He had nothing but contempt for business men. We said, "Ok, we'll back you." We set up the Shockley Laboratories in Mountain View, that's where he wanted to go.

I was guilty of not being severe enough on Shockley. Shockley claimed, I guess it's true, that I helped him out when he was getting his Ph.D. at Caltech, that I made some thermocouples for him, or something like that. He was very friendly to me, so we set him up. Shockley was unbelievably naive. He knew nothing at all about business and worst of all, he was convinced that there was nothing to it. He insisted on running the show his way. That's where I was guilty of letting him have too much leeway. That was my method of operation, as I told you earlier--I set up an operation, put somebody in charge, and let him run. Well, he attracted the best people in the country--Bob Noyce, Gordon Moore, and others. Pretty soon they came to me and said, "We can't work with Shockley, either you get rid of him or get rid of us." With one of my misdirected feelings of loyalty, I felt I owed Shockley and should give him enough of a chance to prove himself, and I said "I'm not going to fire Shockley at this stage of the game." They left and formed Fairchild. If you know the history of the whole semiconductor industry...have you ever seen this genealogical chart [produced by Semiconductor Equipment & Materials Institute, Inc.] that Dr. John Linvill gave me? It starts with Shockley and then Fairchild, all composed of people who were employed with some other company who got an idea or were dissatisfied or something, and they go and start their own companies. That's the whole thing; it is a dog-eat-dog industry. Anyway, it started with Shockley.

For example, he was going to have his pay scale, not on the basis of profit made, but on the number of publications that a person made. That was his idea.

STURCHIO: It's a novel idea.

BECKMAN: Oh yes, it was! Another one was that he got the idea that he couldn't trust anybody, so he was going to make everybody take a lie detector test so that he would know whom he could trust. It's just unbelievable now that I look back, and I am surprised that I had the tolerance, maybe that's too kind a word, to put up with this. It was a critical thing; I didn't know enough about Shockley at the time when this group came up to me and said either it's Shockley or us. If I had known what I know now I'd have said goodbye to Shockley, and I probably would have been very much involved in the semiconductor business, but I didn't. We carried on for a while and finally it got to the point where we could see that he was just incapable of running an operation. So we sold it to the Cleavite Company and got out of it.

THACKRAY: When was that?

BECKMAN: After about a year or so. I'd have to look back and see the date.

THACKRAY: In fact, Beckman Instruments has become reinvolved in that world indirectly, with the way in which computers have become linked to instrumentation.

BECKMAN: Yes, in the applied field. This is all from Helipots. The Helipots developed and then we made all kinds--little ones, and big ones, all sorts of things. But then people began to use them, particularly the military, at high temperatures, higher than they were designed to go. They would char the organic insulating material. Then we developed the cermets. These are glasses, sort of ceramic, metallic things, and it's a whole new technique. You don't have to wire them on the thing, you applied it with a silk-screen process, and it could be run at high temperature. So that was a whole new product line.

That led to making resistor elements. In making them they're very, very low cost because you just use a silk-screen process and put all these on a substrate of alumina, or whatever you're going to use, then you fire them and that's it. That's just the way they make integrated circuiting now, they put a whole bunch on a wafer. It's the same sort of thing, except not to that degree of sophistication. This cut the cost down--instead of $4 or $5 for a helipot, it was a matter of a few cents. It opened up a market, so we began as one of the biggest producers of resistor elements and capacitors. These things are for automatic machine insertion. This is entirely automated, even in our own plant--or nearly automated, we'd have to silk-screen them by hand. Then you'd fire them in a furnace and then these things would be cut up and would go into automatic testing equipment. We had a laser that would go through and trim out and form out of this matte. It would trim the individual resistors right down to the precise value according to the customer's blueprints. It was done automatically. They were laser drawn, trimmed down, you'd come up now with a unit with say, ten or twenty individual resistors, each going out to its terminal, trimmed down to a tenth of one percent, something like that. These were all put into tubes for automatic injection in the assembly. We made millions on this. A far cry from chemical analysis, but still a logical development, step by step. That we sold, along with the Helipots, to Emerson Electric after we had merged with Smith Kline. That was a very sad thing for me personally, because I had invented the original Helipot. But that's the way life is, the company decided to concentrate on health care, which is a philosophy I don't necessarily subscribe to. I think that when you're in research and development, you never know where an idea is going to come from. Look at the Helipots--we started out to make hydrogen ion measurements. If anybody had told me we were going to be in the electronic component business...we didn't even know, so we had not even measured the linearity of a curve, because we didn't use it with a scale. All we wanted was a fine study. It was the Radiation Laboratory that measured it and said, "Hey, these are precision products." We didn't even know it. That developed into over a hundred-million dollar business.

THACKRAY: If we may go, as we get to the end, towards crystal gazing, rather than retrospection, what do you see as some of the directions that the instrumentation industry is moving into?

BECKMAN: There are several things that are clear. First is the miniaturization. You're going to have smaller and smaller instruments with all the advantages; they're more rugged when you do that. Another thing is the use of microprocessors. All instruments are going to feed directly into the computers, and everything will be automatic. For example, we have a Fourier transform spectrophotometer that has lasers, and an interferometer built into it (13). We control the position of the mirror to approximately a millionth of an inch, 300 times a second. Here's a very sophisticated, complex instrument. There isn't a single manual control in the whole thing. So that's what's coming. We're going to take the user out of the loop, and design a robot type of thing. I see that extending to everything, even pH meters. I'm surprised the market still keeps up with pH meters. We're coming up with new models all the time. We've got another one which I haven't yet seen, I just had it described to me. It's a little thing which incorporates the electrode and everything all together in a box about this large, I guess. I'm waiting to see one. Anyway, the market continues, as a result possibly of new applications, new models that have applications that the older models are not suitable for.

THACKRAY: Coupled with this increase in the complexity of the instrument, has there been a shift in the source of the ideas, or is it still very much a very user-driven input?

BECKMAN: I think all those factors are there. For example, going back to the Helipots, we had customers who'd want to run these alongside a hot exhaust pipe, and you couldn't use them again. That's a case where it's feedback from the customer. That led us over to cermets. Once you got this silk-screen technique, that lowered the cost so much that it opens up getting into resistors and AC/DC converters. That could be made from this silk-screen thing. Once you start a new technique, another thing will open up. Sometimes a guy will sit back in a corner and come up with an idea out of the blue. I don't think I can assign quantitatively what fraction of the new products are going to come this way or that way; they're all at work. And the pressures of competition, of course, stimulate you to come up with something new all the time.

THACKRAY: Looking now internationally, where is the competition coming from in instrumentation?

BECKMAN: In chemical analysis instrumentation, the chief competition is Japan. They're coming in more so than Germany, or England, or Switzerland. I'm surprised a little bit that the Germans haven't been doing more. Japan is really on the make in many fields. Maybe I am responsible to some extent for Japan being in the semiconductor business. Did I tell you that story?

STURCHIO: Please do.

BECKMAN: Back in 1956 I was president of the Los Angeles Chamber of Commerce, and Tokyo was celebrating its 500th anniversary that year by having a World Mayor's Conference. Mayor Norris Poulson of Los Angeles was ill and he couldn't go so he asked me to go as his deputy, the President of the Los Angeles Chamber of Commerce as his deputy. So I went over there.

The protocol is that you have to take a gift over there, so what to take? Believe it or not, the Chamber of Commerce decided that the thing to take was a transistor radio. At that time they were brand new; there was only one on the market, made by Zenith. That was all they had. We got one of these modest-sized radios and we put it in a beautiful leather case, embossed the seal of the city of Los Angeles on it, and I took it over.

When I got over there, (I won't bore you with the details), I was late. I missed the grand opening ceremony because the plane was late. They had a protocol officer come on and find out what the gifts were that the guests were going to give. Then they determine the pecking order. I was hoping that I was going to be pretty well down the list because I didn't know what the procedure was, particularly since I missed the opening things, and I missed the instructions that they may have given to the other people. Then when I saw some of the gifts that others were giving, for example, the mayor of Teheran had a beautiful bowl made of woven strands of silver and gold. Here was this radio, and believe it or not, this was selected as the number one thing and I had to go the head of the procession, first of all, and also give him this radio. It was because of the novelty, they had never seen a transistor radio, never heard of it before. I often wonder whether that was what kicked off the semiconductor business! It was only a short time after this that Toshiba, with whom we had a joint venture operation, became the world's largest manufacturer, at that time, of germanium transistors. All of these little pieces fit in, you see, and nothing is very, very important in itself, but still they happened. I don't know, for example, to what extent, if any, that stimulated their technical people that they'd better get into radio. I'll come back to computers now. It's the same way, these little dips and slips we make go into computer things. Here again we had no idea of ever getting into the cheap electronic component business, but we're there.

THACKRAY: Yes. Those aspects of unpredictability are very interesting.

BECKMAN: I don't know, I can predict a little bit, in the near term here. For example, in the health care field, we're going to have diagnostic techniques that will rely more and more on the chemical identification of hormones and neurotransmitters and things like that, that either induce diseases or are the by-products of diseases. As you know, we can pick up cancers of different types. Are there metabolic products that we can pick up? This is just in its infancy. It's going to grow. The whole nature of medical diagnosis is going to change, in my opinion. That means the nature of instrumentation. We are going to have instruments that will do these things, and not only measure these things, but will measure several of them. They'll have a computer in there that will tell what the significance is and come up with a diagnosis, with a therapy. All this is going to be done by unskilled people, technicians, something like that, so it's going to revolutionize the field of medicine. I think that's for sure. With some of the things developing now I can feel very certain about that.

Now what goes beyond that I don't know. I gave a talk a year or so ago at Illinois on industry-university relations and tried to point out that I think our universities are antiquated. We still stick to a four year course. Why? No reason, other than that's the way we've always done it. It's ridiculous to think you can cram an education in four years that's going to last very long. I found out in my own case--I was an educated guy back in the 1920's--bachelor's, master's, doctorate, chemist, scientist. I had never even heard of penicillin, streptomycin, a transistor, an integrated circuit, any of these that are commonplace now. My point is that what we should do in school is teach people how to learn and then make it easy for everybody in all stages of life to go back into the educational system and continue the learning process. But then when I look back and see that was fifty years ago, we have more people doing research now than ever before, so that discoveries are bound to increase in number. There's no reason to believe that research has suddenly become sterile. Just as we didn't know what a transistor was, I don't what "x" is going to be ten, twenty years down the road. But we know there's going to be something just as...maybe we'll be harnessing the internal forces of the nucleus, who knows? So we can project only a year or two into the future.

THACKRAY: Well, very good. Jeff, have we covered what we wanted?

STURCHIO: I think we've covered most of the territory we wanted to today. It's been a long, eventful way back from Cullom, Illinois, where we started, when you were here in April. I'd like to thank you again for taking the time to talk to us.

BECKMAN: There's a lot of irrelevant material in here.

THACKRAY: There are a lot of "nuggets" in there as well, though. I think the story is a fascinating one and, as Jeff says, it's a long way from Cullom. I think an important aspect of all this is making it more known to people what it is possible to achieve, that this is an inspiration.

BECKMAN: There's more to science than just the science itself. Science is carried on by human beings, with all the frailties of human beings and all the other problems woven into daily life.

NOTES

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

2. A. O. Beckman, W. S. Gallaway, W. Kaye, and W. F. Ulrich, "History of Spectrophotometry at Beckman Instruments, Inc.," Analytical Chemistry, 49 (1977): 280A-298A; at 282A.

3. Industrial and Engineering Chemistry: News Edition, 33 (25 September 1941): 1055.

4. Fisher Scientific Company and Eimer and Amend. Modern Laboratory Appliances for Chemical-Metallurgical-Biological Laboratories, (n.p., 1942), p. 825.

5. See "The Beckman Photoelectric Spectrophotometer," National Technical Laboratories Bulletin No. 79, 20 May 1941; and Beckman et al., "History of Spectrophotometry..." (note 2): 281A-282A. UCLA had two DU's by 1942; see National Technical Laboratories, "Users of the First Hundred Beckman Quartz Spectrophotometers," internal memorandum, 1942. Beckman Instruments Historical Collection, Fullerton, California.

6. See H. H. Cary and A. O. Beckman, "Quartz Photoelectric Spectrophotometer," (note 1); Cary and Warren P. Baxter, "Hydrogen Tube," U.S. Patent 2,463,743, issued 8 March 1949 (application filed 29 December 1945); and "Beckman Quartz Spectrophotometer, "National Technical Laboratories Bulletin 89A, October 1945.

7. "Arnold O. Beckman," Videotaped interview with A. James Diefenderfer and Arnold Thackray, Eminent Chemists Videotapes Series, American Chemical Society, 21 February 1985, pp. 28-29 of transcript.

8. Harrison Stephens, Golden Past, Golden Future: The First Fifty Years of Beckman Instruments, Inc., (Claremont, CA: Claremont University Center, 1985), 57, 59.

9. R. Robert Brattain and O. Beeck, "A Rapid Infra-Red Analytical Method for Hydrocarbon Mixtures and a Routine Spectrophotometer for Plant Control," Journal of Applied Physics, 13 (1942): 699-705.

10. R. Bowling Barnes, Urner Liddel, and V. Z. Williams, "Synthetic Rubber: A Spectroscopic Method for Analysis and Control," Industrial and Engineering Chemistry: Analytical Edition, 15 (1943): 83-90, at 85.

11. Linus Pauling, Reuben E. Wood, and J. H. Sturdivant, "An Instrument for Determining the Partial Pressure of Oxygen in a Gas," *Journal of the American Chemical Society*, 68 (1946): 795-798; see also a preliminary communication in *Science*, 103 (1946): 338.

12. Steven M. Spencer, "Mystery of the Blinded Babies," *Saturday Evening Post*, 11 June 1955, 19-21 and 97-106. See also Stephens, *Golden Past, Golden Future* (note 8), 36-37.

13. See "Introducing the FT 1000/2000 Series of Infrared Spectrophotometers," Beckman Bulletin 7728 (1983); and R. J. Obremski, R. P. Gluch, and J. L. Bernard, "FT-IR Primer," (Irvine, CA: Beckman Instruments, Inc., 1983).

INDEX

acidimeter	3
air pollution	30, 32
Alles, Gordon	31, 32
American Chemical Society (ACS)	6
American Cyanamid Company	16
Arnold O. Beckman, Inc.	20, 22, 24
Atlantic Richfield Company	30
automobile pollution	32
Barnes, Bowling	16, 17, 18
Barton, Robert	8
Basel, Switzerland	2
Bausch and Lomb, Inc.	1, 2
Beckman Instruments, Inc.	9, 10, 11, 13, 23, 24, 34
Electronic Specialities	10
Helipot Division	10
Spectro Division	10
Bell Laboratories	8, 33
Bethlehem Steel Company	30
Bishop, John	10
Brantley, Reed	32
Brattain, Robert	16
British Columbia	30
business management	12
butadiene	30
California Institute of Technology (Caltech)	7, 8, 9, 13, 20, 21, 25, 26, 27,28, 29, 31, 33
Camarra, Frank	8
Cannon, Robert	28
Cary, Howard	2, 3, 5, 7
Central Scientific	3
cermets	34
Chandler, Norman	25
Chrysler Corporation	32
Ciba-Geigy Corporation	2
Cleavite Company	34
Cullom, Illinois	38, 39
Curtin, E. H., Company	6
Detroit, Michigan	16
Dorio, James	11
DuBridge, Lee	29
Duchane, Morry	9
DuMond, Jesse W. M.	26
Ehrman, Fred	23, 24, 25
Eimer and Amend	3, 4
electrode, glass	8
electroscope, pocket	20

Emerson Electric Company 35
entrepreneurship 12

Fairchild Industries 33
Faraday, Michael 20
Ford, Henry II 32
Fracker, Henry 8

Gabor, Zsa Zsa 27
galvanometer 16, 17
Geiger counter 20
General Electric Company (GE) 2
George, Kenyon 7
Germany 36
glass blowing 8, 21
Glass Instruments Company 8

Haagen-Smit, Arie 31
Hale, George Ellery 25
Hartey, Fred 32
Harvard University Business School 10, 11
Heinan, Charles 32
Helipot 15, 19, 24, 34, 35, 36
Herrington, Thomas 7
hormones 38
Houston, Texas 6
Huntington Hotel 32

Illinois, University of 30
Instrument Society of America 6
instrumentation industry 35
internal combustion engine 32
International Business Machines, Inc.(IBM) 15

Japan 36
Jeffries, Paul 30
Jergens, A. T. 23
Jet Propulsion Laboratory (JPL) 29
Johns Hopkins University 22
Johnson, Lyndon 28
Joseph, Glenn 13, 16
Jergens Oil Co. 23, 24

Kennedy, Hal 32

Lacey, Wiiliam N. 26
Lamp, deuterium 5
Leeds and Northrup Company 16
Lehman Brothers, Inc. 23
Linvill, John G. 33
Los Angeles Chamber of Commerce 30, 37
Los Angeles Times 25, 27
Lyons, I. H. 24

Manhattan Project 19
Marlowe, Douglas 7
Mason Cables 8
Massachusetts Institute of Technology (MIT) 3, 4
McCabe, Louis 31
medical diagnosis 38
Metropolitan Water District 26
microprocessor 35
Millikan, Robert A. 25, 27, 28, 29
Moore, Gordon 33

National Air Pollution Board 32
National Institutes of Health 15
National Postal Meter Company 9, 23, 24
National Science Foundation 28
National Technical Laboratories 1, 5, 7, 9, 13
Navy, British 22
Navy, United States 22
neurotransmitters 38
Nixon, Richard M. 32
Noyes, Arthur A. 26, 28
Noyce, Robert 33

Office of Rubber Reserve 16
oxygen analyzer 20, 21, 25
ozone 31

Parker, Sidney 9
paramagnetic susceptibility 20
Pasadena, California 7, 8, 9, 10, 23, 28, 30
Patterson, Edward 3
Pauling, Linus 20
Perkin-Elmer Corporation 18
Petroleum Institute 26
pH meter 1, 13, 17, 19, 23, 24
photocells 1, 5, 17
Poulson, Norris 37

quartz fiber 21, 22
quartz prism 4

research, classified 29
Robinson-Pattman Act 6
Royal Hotel 26
Royce, Steven 32
rubber, synthetic 16
Ruckleshaus, William 32

Sage, Bruce 26
Sargent Company, E. H. 3
Sargent, Herbert 21
Saturday Evening Post 22
Semiconductor Equipment & Materials Institute (SEMI) 33
Shull, George 7, 8

Shell Oil Company 8
Shockley, William 30, 33, 34
Shockley Laboratories 33
Simpson, John 8, 9
Smith Kline, Inc. 35
smog analysis instrumentation 33
Smyth, Henry D. 26
Sorensen, Royal W. 26

spectrometer, mass 20
spectrophotometer 1, 3, 18, 19, 23
  spectrophotometer, D 4
  spectrophotometer, DU 2, 5, 14, 17
  spectrophotometer, Fourier transform 36
  spectrophotometer, Hardy 2
  spectrophotometer, IR-1 15, 19
  spectrophotometer, photoelectric 1, 3
  spectrophotometer, Zeiss 2
spectrophotometry, infrared 16, 17
spectroscopy 2, 3, 4
Spruce Goose (airplane) 8
Stanley, Lowell 23
Stirling engine 32
Strauss, Donald 12
Sturdivant, Holmes 20
submarines 20
sulfur dioxide 30, 31, 32
Switzerland 36

Thomas, Arthur H. Company 3, 6
Thomas, Franklin 26
Tokyo, Japan 37
Toshiba 37
transistor 33, 37
transistor radio 37
Traphagen, Frank 8

Union Pacific Railroad 30
Univ. of California, Los Angeles (UCLA) 4
University of Southern California (USC)

Valentine, Edward 27
Vernon, California 30
Vivian, Robert E. 31

Waestman, Ray 24
Wall Street Journal 6
Watson, Ernest 26
wavelength scale 5
Western Electric Company 8
Western Oil and Gas Association 31
Williams, Dudley 17
Williams, van Zandt 16
Wright, Norman 17

Zeiss, Inc., Carl 2
Zenith Corporation 37
Zuram, Abe 31
Zurich, Switzerland 2