CHEMICAL HERITAGE FOUNDATION

ROBERT J. MANNING

BECKMAN HERITAGE PROJECT

Transcript of an Interview
Conducted by

David C. Brock and Gerald E. Gallwas

in

Fullerton, California

on

19 February 2002

(With Subsequent Corrections and Additions)

ACKNOWLEDGMENT

This oral history is one in a series initiated by the Chemical Heritage Foundation on behalf of The Arnold and Mabel Beckman Foundation. The series documents the personal perspectives of the individuals related to the history of Arnold O. Beckman and Beckman Instruments, Inc., and records the human dimensions of the growth of the chemical sciences and chemical process industries during the twentieth century.

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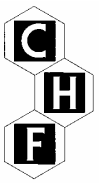

ROBERT J. MANNING

1920 Born in Kansas City, Kansas on 12 January

Education

1943 B.S., chemistry, St. Benedict's College
1948 M.S., chemistry, University of Missouri, Kansas City

Professional Experience

United States Naval Ordnance Test Station, China Lake
1948-1953 Research Chemist

Beckman Instruments, Inc.
1953-1960 Senior Applications Chemist
1960-1963 Regional Applications Chemist
1963-1968 Ultraviolet Product Line Manager
1968-1973 Principal Application Chemist
1973-1986 Senior Scientist, Beckman Technical Education Center

ABSTRACT

Robert J. Manning begins the interview with a brief sketch of his educational background. In 1948, after graduating with a M.S. in chemistry from the University of Missouri in Kansas City, Manning spent time in the United States Navy researching rocket fuel. From there Manning obtained a position at Beckman Instruments, Inc., where he remained until his retirement in 1986. Working in the application engineering department at Beckman, Manning gravitated toward infrared instrumentation, and eventually became the national president for the Society of Applied Spectroscopy. Having spent thirty-three years at Beckman Instruments, Manning has a valuable wealth of knowledge about nuances in development of landmark instruments from Beckman, which he details throughout the interview. In 1960, Manning moved his family to Chicago where he started an applications laboratory at Beckman's offices there. Manning moved back to California in 1963 to enjoy his new position as product line manager of ultraviolet instruments, before returning to the laboratory, where he would spend the rest of his career. Education and information sharing were paramount to Manning, and he traveled the country educating people about spectroscopy, via workshops, lecture series, and summer courses at various universities. Manning concludes the interview with reflections on his career and home life.

INTERVIEWERS

David C. Brock is Program Manager for Educational and Historical Services at the Chemical Heritage Foundation in Philadelphia. He is currently a Ph.D. candidate in the History Department, Program in the History of Science at Princeton University. In 1995, Mr. Brock received his M.A. in the History of Science from Princeton University and in 1992, he earned a M.Sc. in the Sociology of Scientific Knowledge from the University of Edinburgh.

Gerald E. Gallwas was a member of the original team in the mid-1960s that founded and managed the growth of what became the clinical diagnostic business of Beckman Instruments, Inc. As the business grew, he served in many roles from new product development to directing clinical field trials in the U.S., Europe, and Japan. This led to an extensive involvement with professional and trade organizations as well as regulatory agencies. He retired after thirty years of service as director of program management overseeing new product development programs.

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INTERVIEWERS: David C. Brock and Gerald E. Gallwas

LOCATION: Fullerton, California

DATE: 19 February 2002

BROCK: Robert, please tell me about your childhood in Kansas.

MANNING: I was born in Kansas City, Kansas in 1920. I was educated in the Catholic school system—from grade school to high school to St. Benedict's College in Atchison, Kansas. Once I graduated from college, I joined the [United States] Army in the early 1940s. I stayed in the Army for four years in various positions. After I left the Army, I went back to college, the University of Missouri in Kansas City, on the GI Bill and got my master's degree in chemistry. My first job was with the United States Naval Ordnance Test Station at China Lake, as they called it then. Now it's the Naval Air Warfare Center Weapons Division in China Lake. I was in the research department there for five years working primarily on rocket fuel research.

A little bit of my work at China Lake went into the Sidewinder Missile, which is one of the missiles whose development started in the late 1940s. The Sidewinder is still an operational missile today. It's a heat-seeking missile that has electrically-guided controls. To generate electricity for operating the missile's controls, a small generator was built into the missile. This generator has its own rocket motor that spins and produces the electricity for the controls. My contribution was the development of the fuel for that internal generator. I don't know whether or not the generator is still using the fuel that I created, but I was very pleased with my results.

BROCK: Was this a separate fuel system from the propulsion system?

MANNING: Yes, it was entirely separate from the main propulsion system.

BROCK: How did you become interested in chemistry?

MANNING: I had a chemistry set when I was a kid. Also, my father had a friend who was a chemist, and it always sounded as if my father's friend did interesting work. I have always been interested in science.

BROCK: When you were studying for your master's degree, did you have to conduct research?

MANNING: Actually, my master's is in biochemistry. I studied the oxidation of chicken fat in order to look for antioxidants to lengthen the shelf life of chicken fat.

GALLWAS: It is ironic to think of how basic that is, but how important it is also.

MANNING: Yes.

BROCK: Did you employ instrumentation for that research?

MANNING: There wasn't that much instrumentation around when I was in school. While getting my bachelor's degree, the most complex piece of equipment available was a colorimeter. A colorimeter was donated to the college by a local oil company that no longer had any use for it. That was the only instrument of any kind that we had. We had balances and things like that, but no other instruments. Even in graduate school, when I was working on my master's degree, the amount of equipment available was limited.

I ultimately ended up being a spectroscopist at Beckman [Instruments, Inc.], but my total spectroscopy experience in college was one day. There was only one day in which we talked about spectroscopy. My class went to visit the Midwest Research Institute to see their spectroscopy laboratory. The Institute had one one-meter Bausch & Lomb [Inc.] spectrograph. My total familiarity with spectroscopy developed in that one day.

BROCK: Was that the case as well for the five years that you spent at China Lake? What was that laboratory like?

MANNING: There we did have a little bit more in the way of instrumentation. We had a spectroscopy laboratory. There were several spectroscopes, and then shortly before I left there, the lab brought a Beckman DU [spectrophotometer]. But I wasn't permitted to operate it. The DU was reserved for the department head. [laughter] There was also one pH meter in the chemistry department.

I came to work at Beckman through some gadgeteering that I did at China Lake. At China Lake, one of the problems that I was working on with rocket fuels was determining their melting points. I made all kinds of different mixtures to determine melting points. The lab had a hot plate microscope, so I'd put a drop of the fuel—I could only use very small amounts because the fuel was highly explosive—on the microscope slide, put it on the hot plate, and then

watched as the temperature rose to see where it melted through the microscope. Spending hours doing that was not something that I enjoy, so I put a phototube down instead of the eyepiece of the microscope. Then I got a strip chart recorder that had an X/Y recorder. One axis on the strip chart reading correlated to the temperature of the phototube. The other axis detailed the amount of light coming through the microscope. When the crystals melted, the light would suddenly jump, so there was a sudden big spike on the chart, which related to the temperature.

I have always been a gadgeteer. Back in those days I built my own audio amplifiers for high fidelity audio.

BROCK: I take it that you had an interest in electronics.

MANNING: Yes, the idea just occurred to me, use the phototube instead of the eye of the microscope. It worked for that particular application. I described that when I first interviewed at Beckman. The interviewer was Ed [Edwin] Arthur. Ed thought that it was interesting that I was into instrumentation. But there wasn't much instrumentation in the chemistry lab at Beckman.

BROCK: What drew your attention to Beckman and attracted you so much that you interviewed at Beckman?

MANNING: I had heard about Beckman. The China Lake lab had a Beckman DU and I had a friend [William "Bill" Ward] working for Beckman. He and I went through St. Benedict's College together. Bill, I, and John Speier were the three chemistry majors in our class.

As I said, Bill Ward was working for Beckman, so I knew a little bit about Beckman from him. When I was getting tired of living on a Navy base out in the middle of the desert, and I saw the "help wanted ad" from Beckman in the *Los Angeles Times* one day, I answered it and started to work at Beckman.

GALLWAS: Did you come in contact with Roland [C.] Hawes during your time at Beckman?

MANNING: No, he was before my time at Beckman. Hawes worked on the development of the DU and infrared instruments.

At China Lake, there was interest in developing an analog computer that could be used to study the dynamics that take place in rocket motors—to determine why propellant burns at a certain rate depending upon the pressure and temperature.

The creation of that type of computer was a good idea, so the Navy came to Dr. [Arnold O.] Beckman and said, "Would you consider developing this type of computer for us?" Dr. Beckman considered it, and had Roland Hawes look at the problem to see whether it was worthwhile. Roland Hawes thought, "Yes, this is a good idea," and he told Dr. Beckman, "I think we should do it." But Dr. Beckman said, "No. This doesn't fit in with our other business. We would spend a lot of time and effort on this project, but we wouldn't gain much from it, so I don't want to do it." But Roland Hawes wanted to do it, so he began to work with the people from China Lake after that. He set up a personal contract to develop and construct that computer for the Navy. Dr. Beckman did not want to pursue that project, so when he found out that Rollie had done all of this, he said to Rollie, "You're out."

BROCK: Going back to your interview at Beckman, were you applying for an available chemist position?

MANNING: Yes, a chemist position.

BROCK: How was that position described? And when you accepted it, did the description fit the reality?

MANNING: I interviewed with Ed Arthur, who was an electrochemist in charge of the glass shop. He thought I would fit in well at Beckman, but not in the position that he had available. So he called up somebody in the employment office and said, "You ought to talk to Manning; he may have something to offer you." They talked to me, and shortly thereafter I got a call to see if I would be interested in a job in the application engineering department. The application engineering people looked into the problems of customers and figured out ways to correct these problems using Beckman instruments.

There were four of us in the application engineering department in 1953. Al [Albert] Schreiber, who headed up the department, was not a chemist, but he was an operator. [laughter] Al was a good administrator. There were three of us who were chemists: Ken [Kenneth K.] Kendall, Hideo Watanabe, and me.

GALLWAS: Who did you work for?

MANNING: Al Schreiber. Al had a degree in chemistry but had never worked as a chemist prior to that, but somehow or another he got hired. As I said, he was an operator. He was a good talker. In fact, he came in one day and told me, "Go sell your car, get all the money you can, and buy Beckman stock today. It'll never be lower." He was right, and I wish I had done it. [laughter] Al was more interested in the business end of things.

I worked as an applications chemist at Beckman. You, Jerry [Gerald E. Gallwas], were in applications sometime later.

GALLWAS: Yes, after you.

BROCK: When you started in the application engineering department, where did that department fit organizationally within Beckman?

MANNING: It was part of the marketing and sales department. Requests came in through sales and we would handle them. The department was housed at 820 Mission Street in South Pasadena. I had a little lab, 10 feet square, about 50 feet from Dr. Beckman's office. Beckman used to stick his head in every once in a while, just to smell the smells of the chemistry lab.

BROCK: So it was primarily through the sales people that potential applications or customer problems came to your group?

MANNING: Yes, customer problems would be presented to us and we would see if we could devise a solution for them. Initially, there were an awful lot of pH problems—back then we also did metallic ions, sodium, potassium, and so forth with a pH meter, but not real well. Then we got into doing things like measuring sulfides, cyanide concentrations in sulfides, and things like that with pH meters. We did a lot of that stuff. We were also pushing infrared at the time. Beckman had this beautiful, big IR-3 infrared instrument, a twenty-five thousand-dollar instrument in 1953. It was really something. So I started studying infrared and applications of infrared for solving chemical problems. Is there an IR-3 in the museum?

GALLWAS: There's not enough room for them. I wish we had an IR-3, but I don't think any of them survived. There weren't very many of them.

BROCK: Did you publish any of your results from the applications department to use as a marketing piece?

MANNING: Yes, we used to put out application data sheets. We printed those to send to customers. If a customer called in with a problem that we already had studied and had a data sheet on it, we'd just send them the data sheet. My department tried to develop as many data sheets as we could. We even used to give out papers at scientific meetings.

For many years, I went to the Pittsburgh Conference [on Analytical Chemistry and Applied Spectroscopy] for the first two weeks of March. It always snowed. [laughter] The only publications that we really "put out" were through our papers at the Pittsburgh Conference, other local meetings, ACS [American Chemical Society] meetings, and the like.

BROCK: Did you and your colleagues divide the Beckman products amongst you?

MANNING: Yes, sort of. I gravitated toward infrared. My total exposure to spectroscopy in college was one day, and I didn't learn much that day. I had to learn much more after I came to work for Beckman. When I first got into spectroscopy I didn't know UV [ultraviolet] from IR [infrared] from anything else. It took a lot of studying on my part, but I gradually learned enough about spectroscopy to become national president of the Society for Applied Spectroscopy. I was recognized as a spectroscopist. [laughter] But I don't think I ever was a good spectroscopist.

BROCK: In trying to get up to speed on infrared, did you speak to the individuals who developed the instrument to understand more about it?

MANNING: Yes. In order to get anything going, I had to talk to the people who knew about. I'm sure you've discussed Bill [William S.] Gallaway. Bill Gallaway had a Ph.D. from [University of] Michigan in infrared. He got his Ph.D. on the infrared spectrum of ethylene gas. That was his thesis. In order to get a spectrum of ethylene, he had to first build the instrument. Gallaway built his own infrared instrument at Michigan, did enough work on it to get his Ph.D., and then came to work for Dr. Beckman. Gallaway worked for Dr. Beckman for several years.

When [H.] Howard Cary left and formed his own company [Applied Physics Corporation, renamed Cary Instruments in 1966], he asked Bill Gallaway to work for him. So Bill went with Howard Cary and worked with Cary for several years. Gallaway came back to Beckman in the mid-1950s. Bill Gallaway and I got to know each other while he was working for Cary because I was always calling him up to ask him questions about infrared. He knew it both from the standpoint of what it was used for and the instrument, too. I discussed a lot of things with him.

I also discussed a lot of things with Bill Ward. Bill Ward had a Ph.D. in infrared from Ohio State University, and he had worked as an infrared spectroscopist with Texaco [Inc.] back in the New York area. Ward was working for Beckman when I first joined Beckman.

BROCK: Did Cary compete with Beckman in IR at this time?

MANNING: Not in IR, but in UV. I started out mostly in IR, but I was also doing a lot of UV work. During the 1950s, when we were developing new UV instruments, I spent more time with UV than with IR.

BROCK: Did you speak to the people who had developed the UV instruments? Were you in a similar position where you needed guidance?

MANNING: Sure, I used to call the engineers all the time. Off-hand I can't think of any particular people that I talked to. But I needed lots of help in the beginning. I didn't even know which was the red end of the spectrum and which was the violet end. I was a neophyte, but I learned quickly.

BROCK: Did you require assistance with the [Beckman Model] DU or the [Beckman] Model B?

MANNING: When I first went to Beckman, I was just getting into infrared. Then Beckman started developing two new recording ultraviolet instruments simultaneously. They were the DR and the DK. The "K" was for Dr. Wilbur [I.] Kaye, who had developed this instrument when he worked for Tennessee Eastman [Company]. The DR, which was a robot operator for the DU—you don't have any of those either—had connections that turned the knobs on the DU, just like a human operator would.

BROCK: Did the DR use a linear shuttle or have a turret or a cell compartment?

MANNING: It had a kind of linear shuttle with four positions. There was a superstructure that contained the electronics, the recorder, and various servo motors that turned the knobs on the DU.

[END OF TAPE, SIDE 1]

MANNING: The DR also had a recorder, but not a continuous strip chart recorder. It had a moving chart. When a reading was made, a plunger would move over and make a dot on the chart. The strip chart recorder had a three-color typewriter ribbon, so you could run three samples simultaneously. It made red, green, and black dots. The DR was a great instrument, but it was overshadowed by the DK, which was faster. It would take the DR twenty minutes or so to run an ultraviolet spectrum whereas on the DK, it only took two minutes. So the DK became the instrument of choice, although it wasn't as accurate as the DR.

GALLWAS: When was the DB created? Was that later?

MANNING: That was in the early 1960s after the DK. Eric Sundstrom had developed an idea for doing double-beam work on a DU in Sweden—he got a patent on it—so he approached Beckman. Dr. Beckman liked the idea, bought the patent, and hired Eric to get the project going. The project engineer on the DB was Harold Schultz. He and I used to ride to work together, so I heard about the complete development of the DB riding back and forth to work. That was an interesting instrument, too.

The "B" in DB had two meanings. DB stood for double-beam, and it was a replacement for the Model B spectrophotometer. The Model B was a great instrument. Everybody who had one liked it, but it was limited and its wavelength range didn't go into the ultraviolet. The entire case and all the components were cast metal, and the tooling wore out. Manufacturing said, "We can either get new tools at a big expense, or we can get rid of the Model B." So we developed the DB to take the place of the Model B. The DB was supposed to be in the same price range as the Model B. It didn't really turn out to be that way, but that was the design.

BROCK: After you got into applications work in the ultraviolet area, what was the "story" you heard about the development of the DU? What was the "local wisdom?"

MANNING: The DU was a wonderful instrument, and very widely used, very popular. Most of the design work was done by Howard Cary. Howard Cary was a civil engineer who was very good at mechanical engineering and optical engineering. He, Roland Hawes, and another gentleman named Kenyon George played key roles in the development of the DU. I didn't know Kenyon George, but Howard Cary and Roland Hawes always mentioned Kenyon George. Bill Gallaway also spoke about Kenyon George.

GALLWAS: Where do you think the idea for UV came from?

Before you answer that, let me make the observation that the only patent held on the DU system is held by Howard Cary (1); and it's not for the DU, it's for the hydrogen lamp power supply.

MANNING: I don't know. You couldn't patent the idea of ultraviolet spectroscopy. Using a quartz prism was something that was known. Other people had used those instruments and there were a lot of different designs and so forth using a quartz prism. You've got one sitting in the museum, a DU that's using a Model G pH meter as the readout. The monochromator was developed, but they had to have some way to measure the signal, so they used the Model G pH

meter. Later on, the DU had its own electronics, which consisted of the pH meter electronics stuck in the DU box.

GALLWAS: It was a very clever guise.

MANNING: I can see that perhaps there wasn't very much that they could patent initially on the DU. First there was the Model D, and then the Model D Ultraviolet when they added the hydrogen lamp to it. I don't really know too much about the development of the instrument or about who was the major mover behind it.

GALLWAS: Do you have any recollection of the GE [General Electric Company]/[A. C.] Hardy spectrophotometer? Could you tell us about that?

MANNING: Yes. In fact, a gentleman who was the sales manager for the Hardy spectrophotometer worked for Beckman later on. The GE/Hardy was developed at MIT [Massachusetts Institute of Technology]. Hardy was a professor at MIT. Hardy's spectrophotometer used a glass prism, because it was developed for color work—it didn't have to go into the ultraviolet. Since Hardy's spectrophotometer used a big glass prism, instead of the light falling directly on the phototube, it had a hollow integrating sphere that was about 6 inches in diameter, and coated on the inside with magnesium oxide, which is the whitest white that you can get.

You'd put the sample against an opening in the sphere, the light would strike the sample, reflect off of the sample, and bounce all around inside the sphere. There was also a phototube looking into the sphere from the side and it would get its signal that way. The light went into the integrating sphere so that you could use samples that were highly scattering and still get enough light to make a measurement. It was a big instrument, a good instrument. People started using it to do other chemical work and so forth. Instead of putting the sample in the usual spot, where a paint sample, for example, would go here and you'd look at it by reflection, they would put the sample in front of the sphere where the light beam would go through the sample and then into the sphere. They were doing chemical work that way, using liquid samples and so forth, as in the DU. That instrument preceded the DU.

GALLWAS: But it wasn't ultraviolet.

MANNING: It was not ultraviolet. That's right.

GALLWAS: Were there any other ultraviolet instruments at that time?

MANNING: No, there were no other ultraviolet instruments. Coleman [Instruments, Inc.] had a small spectrophotometer; it was called the Coleman Junior Spectrophotometer. It was, again, a glass prism instrument, using visible light only, and was pretty popular, very widely used. But since it didn't go into the ultraviolet, it couldn't do what the DU could do.

A few individuals came to Dr. Beckman about measuring Vitamin A in cod liver oil. You could only make that type of measurement in ultraviolet, so we had to create an instrument that went down into the ultraviolet. Now, using a tungsten lamp in the instrument, you can get down to about 325 nanometers or millimicrons. You could get down to 325 with the phototube and the tungsten lamp, so you could do Vitamin A with a quartz prism instrument that went down to 325. The glass prisms just couldn't do anything at 325, so they didn't really work. The Model B, for instance, could not be used to measure Vitamin A, nor could the Coleman Junior or the General Electric instrument. None of those could do Vitamin A. But they could measure Vitamin A with the DU.

GALLWAS: So through the war period, the DU was all that was available.

MANNING: That was it.

GALLWAS: Were there any "real" competitors before Howard Cary came out with the Model 10?

MANNING: No, that was our first competitor.

GALLWAS: Very interesting. Another big blank has been filled for me.

Moving forward in your career, you became a product line manager.

MANNING: Yes, for ultraviolet.

GALLWAS: That was in the mid-1960s, when I joined Beckman.

MANNING: Yes. I was product line manager from 1963 to 1968.

GALLWAS: Please, tell us about the accessories for the DU including the atomic absorption accessory and the discussion-making process behind the double-beam, single-beam, and adding accessories to the beam.

MANNING: The DU was a great single-beam instrument, but it was slow. You could do very accurate work with a DU.

One of the first accessories made for the DU was for flame photometry. You could use this accessory to look at a flame on the DU for doing things like sodium, potassium, and cesium. In order to get a good signal for flame photometry you had to use a phototube with a little more oomph to it, so the photo multiplier tube was adapted as an accessory. We also had another little gadget. On the GE instrument, as I mentioned, the detector was an integrating sphere. We also had an integrating sphere for the DU. The sphere was used for doing color work, paint samples, and things like that.

Later on we had an accessory for atomic absorption. The DU was not a very good instrument for atomic absorption. Here is a bit about the history of atomic absorption. At that time, one of our big competitors was PerkinElmer [Inc.]. They were primarily our competitor in infrared, but they also had an atomic absorption instrument. It was a big kludge of a thing. It was not very good, but it was the only atomic absorption instrument that was available. So it was decided that we could do atomic absorption with the DU. August Hell, a German who later went back to Germany, was the engineer on the absorption project.

GALLWAS: And was he ever German. [laughter]

MANNING: Yes. He came up with the atomic absorption accessory for the DU. But it wasn't very good.

GALLWAS: Was there any discussion about whether or not you could develop a double-beam accessory for the DU or whether you should make an integrated instrument?

MANNING: There was a lot of discussion. I don't remember much of the discussions about atomic absorption. I remember that we wanted to get into the business. One of the necessities for doing atomic absorption was to have a monochromator—we had a DU monochromator that would cover the wavelength regions of interest. We had the electronics and everything that was needed. All we needed was to somehow get the atomic absorption sample in there. That was what we needed to do, and August Hell did that. But, as I said, it was not a very good instrument.

GALLWAS: Did Dr. Beckman play a role in any of this?

MANNING: I don't recall that he did. Of course he was aware of it and knew how much money was being spent on it. But I don't know that he ever got involved.

GALLWAS: Let me read something to you to see what you think.

MANNING: All right.

GALLWAS: "Fundamental to this whole question is what happened in UV." Did you see this article years ago (2)?

MANNING: Yes. I think I have it in my files at home.

GALLWAS: There's an interesting section in here. "One of Beckman's few business mistakes came early. Among his top engineers in the late 1930s was Howard Cary, who helped Beckman design the DU spectrophotometer device to revolutionize laboratory testing by allowing researchers to analyze chemical compounds in seconds. It was a big moneymaker for Beckman but Cary thought he could redesign and make it better. Arnold said, 'Why?' recalls sixty year old Peter I. Lipman, a patent attorney in Montrose who later worked for Cary. Cary and a handful of other engineers interpreted Beckman's decision as choosing profit over science. That was the beginning of the end as far as Howard was concerned. He took off and started his own company. Jack [John F.] Bishop, who became general manager of Beckman Instruments in 1952 at age twenty-eight, describes the incident as unusual. It was one that Beckman immediately regretted. Cary's company became a key competitor."

Central to understanding this whole issue, I think, is understanding what Dr. Beckman's influence or interest really was. I'll give you a nickel's worth of background. Howard Cary was quadriplegic for a number of years after spinal surgery and I tried to get Dr. Beckman to go see him several times. Dr. Beckman took the same position with Howard as he had taken with Rollie Hawes. "This gentleman cheated me and I won't go see him." Beckman's story was that he was pressing for double-beam but Howard said, "We have too many other things going on, and this is more complex than you think it is, and we don't have the technology to do it." So there are two very different stories concerning this one incident.

What is very clear is that Howard Cary was the design brain behind the DU, although he had plenty of other helpers. There's also pretty good evidence that Howard Cary said, "For this thing to be successful, it's got to go down into the ultraviolet." He had to go create the hydrogen lamp power supply and the hydrogen lamp. So there's overwhelming evidence, I

think, that this was his toy, in that sense. What we don't know, what we don't understand is what happened, because we cannot ask Cary and we cannot ask Beckman. I think that Dr. Beckman has said, "I made a mistake and let Howard go in a weak moment." That's probably true, but I've always wondered about what pressure you must have been under as product line manager to sustain the DU, and its ultimate representation in the atomic absorption accessory, as a single-beam instrument, because the thing was sustainable. [laughter]

MANNING: Let me say that the PerkinElmer instrument was before us. It also was a single-beam instrument, so we didn't have any competition from that standpoint. I think it was the engineers who decided what they had to work with and what they wanted to do, so they went ahead and did it. August Hell, who was German, was told, "Build an atomic absorption instrument, and use the DU monochromator," so he went ahead and did it that way. It was a single-beam instrument. But we'd already been into double-beam instruments. The DK, DR, DB, and so forth were double-beam instruments that came out ahead of the atomic absorption instrument.

I had a hard time with the DB and with atomic absorption.

GALLWAS: I bet you did. [laughter]

MANNING: I had a hard time because I was product line manager and they came to me and said, "Here's an instrument. Sell it."

GALLWAS: You didn't have any applications?

MANNING: We introduced that instrument at a national sales meeting. We told the salesmen that we were going to have a new instrument for them. Everybody was there. We concealed the DU under a big black cloth. "As of now, we are in the atomic absorption business and here is an instrument you can go out and sell." I took the big black cloth off of the DU, the room was quiet, and I heard somebody say, "Geez! It's big, isn't it?" And it was. It took up the whole table. That's what our customers said, too. "Geez! It's big, isn't it?" [laughter] It was not a good instrument. It was a mistake. I gave all kinds of incentives to the salesmen to go out and sell it. I said, "The first salesman in each district who brings in an atomic absorption instrument, I'll pay to send you and your wife off for a vacation weekend at some fancy place. You can wine and dine your wife and make lots of points at home."

BROCK: When did Beckman enter into the atomic absorption business?

MANNING: This was around 1966 or 1967.

BROCK: That was pretty early, wasn't it? I know you said that the Techtron was the first, but the Techtron wasn't out too much before 1966 or 1967, was it?

MANNING: The Techtron instrument didn't come from PerkinElmer, it came from Varian [Inc.]. It was developed in Australia. PerkinElmer had developed their own instrument and shown it at the Pittsburgh Conference and so forth, but it was a big instrument so the DU could compete with it. The DU couldn't compete with the Techtron, which was a beautiful, small instrument.

BROCK: So the decision was made to get out of atomic absorption.

MANNING: The atomic absorption business kind of died because nobody would buy the instrument. [laughter] There were better instruments available for a lower price. So we failed.

BROCK: I would like to take one step back, if we could. I would like to return to the period where you were doing applications work. We talked about IR, UV, and pH as the three areas in which you and your colleagues were working. Over that time, as 1963 steadily approached, were there any other areas beyond those three that you were doing applications work for?

MANNING: No. In our applications work, we were trying to develop things that would be of wide interest.

[END OF TAPE, SIDE 2]

MANNING: The Orange County crime lab had a DR. They could keep that DR working when others gave up. Because of the Orange County crime lab, I ran all kinds of drug samples and published application data sheets showing the use of the DR in determining the presence of heroin, cocaine, and things like that. Later on, similar data sheets were generated for the DK.

The DK was the only instrument that would go into the near infrared. The near infrared is a very useful region of the spectrum to work in. Right now there are all kinds of people working in near infrared, but at that time nobody was. Wilbur Kaye published a paper showing all the chemical groups that would absorb in the near infrared—it was a technical paper that showed that you could see OH [hydroxyl], CH [methyl], and other groups using the DK (3). For the DR, I went into the lab and ran any organic liquid that I could find throughout the whole

near infrared region. I published several hundred curves that we printed up and made available to people.

Sadtler [Research] Laboratories [Inc.], which is in Philadelphia, had published infrared spectra for thousands of different compounds, so I tried to develop as many near infrared curves as I could. One of our DK customers, Anderson Laboratories, in Champaign, Illinois, came to me and said, "We would like to do what you're doing as a business." "We'd like to continue what you're doing, but sell the curves later." They asked me if I would back off and let them take up the job. Sadtler also put a man on it full time and published quite a few curves, but they never got much of a sale from them because nobody was working in the near infrared. It was a great idea, but it was ahead of its time. I was doing it to publicize the fact that there were a lot of things that you could see in the near infrared, so why not use it? We were ahead of our time. Fifteen years later, somebody else discovered near infrared and now it's a big thing. Now, they build special instruments to work in that region.

BROCK: For the ten years that you worked in the applications group, did it remain, organizationally, within sales and marketing?

MANNING: Yes.

BROCK: Do you think that was a good place for it to be?

MANNING: It was a good place to be because the money to operate applications was available within the sales department. It was also a useful sales tool. In fact, in 1959, it was decided that having an applications department was such a good idea that we needed to get even closer to the customers. They came to me and said, "Would you consider moving to Chicago to set up an applications laboratory in the Chicago office?"

A brand-new building had just been built outside of Chicago, in Lincolnwood, Illinois. I said, "Yes, I'll do it, if you give me a little financial incentive. I'd hate to leave California. I promise I'll go for three years, and if my family can take Chicago for three years, maybe we'll stay longer." So I went to Chicago and opened up a field application lab. Shortly thereafter, we opened up another lab in the Mountainside, New Jersey office for the eastern region. Eventually, we had one down in Houston for the southern region.

In the field application laboratories, I worked on customers' samples that the salesmen would bring in. I also went out and made sales calls with the salesmen and discussed problems that customers had in their laboratories. I did that for three years. Then people at Beckman came to me and said, "We'd like you to come back to California as product line manager for ultraviolet." So I left. I really enjoyed the work in Chicago and I would have stayed there longer. Even my family enjoyed it. My daughters learned to ice skate and they loved the snow.

BROCK: You established the field applications laboratory in Chicago. Did you help set up the labs in New Jersey and Texas as well?

MANNING: The plans that I had drawn up for the Chicago office, the layout of the laboratory and so forth, were used for the other labs as well. The one down in Houston was almost identical to the one I had in Chicago. The two labs were identical down to the adjacent seminar room where we could hold lectures and seminars. Also in that seminar room we had lab benches where we could put instruments up and have demonstrations. That worked out real well. After I left, Bob Jarnutoruski took over, and he was there for several years. Leo Clougherty went to the lab in New Jersey. That was how I got away from things for a while.

GALLWAS: But then you returned.

MANNING: Then I came back as product line manager.

GALLWAS: When did you leave California for Illinois?

MANNING: I left in 1959, and returned as product line manager in 1963.

GALLWAS: How long did you stay as product line manager with Beckman?

MANNING: I was with Beckman until 1966. Later on I went back out into the field lab. I was the applications chemist for the western region for a number of years.

GALLWAS: Once you became product line manager, I started working on the clinical side of things, so we didn't have much contact.

MANNING: Yes. I'm a technical man, I enjoy chemistry and spectroscopy. As product line manager, I was spending all my time on the telephone answering customer complaints. "The windows are falling off of my hundred-dollar UV cells that I bought from you. I buy them and the windows fall off."

GALLWAS: Were there a lot of quality problems?

MANNING: We had a few problems with the UV cells. Has anyone mentioned the problems with the DU power supply? The salesmen, for years afterwards, would go in and say to the customer, "I'm from Beckman," and the customer would say "Don't talk to me, I had a DU power supply." [laughter] The DU power supply worked, but it had problems. It would break down frequently and it got very, very hot. People used to put their coffee pots on top of the DU power supply to keep their coffee hot. [laughter] It ran extremely hot. I spent many, many hours talking to customers trying to calm them down about their DU power supplies. I got tired of that, so I got back into the lab.

When I was in Chicago, an infrared spectroscopist, John Ferraro at Argonne National Laboratories near Chicago, came to me and said, "People don't know anything about infrared. Let's offer a short course in infrared here in the Chicago area." I said, "That's a good idea. We can use our little seminar room and invite people to come in." So we set it up. I think John Ferraro was the president of the spectroscopy society in Chicago at the time, so he sent out invitations to the members of that society and also to the American Chemical Society members in the Chicago area. About a hundred people signed up for that course. We charged forty or fifty dollars for the series of lectures. We found out that when people had to commit money, they came to the course. If it were a strictly voluntary course, then people would start and then drop out. This lecture series went on for six months or so, once a week. John Ferraro did some of the lecturing, I did some of the lecturing, and a gentleman named Joe [Joseph Ziomec] from one of the oil companies did some of the lecturing. We taught them as much as we could about infrared in six months. We also had Beckman instruments there for them to try out. It was very successful.

I knew that a lecture series of this type would work because when I was here [in California], before I went to Chicago, we organized the Southern California Society for Applied Spectroscopy. We got together with other spectroscopy societies around the country and formed a national society. Locally, we were having trouble getting members, so we decided to offer a series of lectures on spectroscopy. I was the president of the local society, so I got to organize the course. I don't know how many separate sessions we had, but it must have been about fifteen or twenty. There were sessions on infrared spectroscopy, ultraviolet spectroscopy, emission spectroscopy, x-ray spectroscopy, and so on. We went through the gamut of spectroscopy. The lecturers were individuals who specialized in a particular area of spectroscopy. We held the lectures in a lecture room at Caltech [California Institute of Technology].

One of our lecturers, the x-ray spectroscopist, worked for Caltech, so he got permission from Caltech to set the courses up. We had one lecture a week. We charged forty dollars, as I recall, to get people to come. It was very successful. As an incentive, members of the Society for Applied Spectroscopy paid half price on this lecture series. Members paid twenty dollars and non-members paid forty dollars, but if you paid forty dollars you got membership in the Society for Applied Spectroscopy along with the lecture series. We got a whole bunch of new members for the society that way. The society was very happy, and we helped generate a lot of

interest in spectroscopy locally. People enjoyed the course. It was good. Wilbur Kaye did ultraviolet. I had people from MIT, the University of Illinois, and so forth, professors who came out and gave lectures on their subjects. I invited them to come, but I didn't offer to pay them anything for coming or to pay for their expenses, and surprisingly they came. [laughter] That was a very successful series. It showed me that people are interested in learning. But in order to get them to come regularly, they have to pay. If it's free, they won't come.

GALLWAS: That was good psychology.

MANNING: Yes, it was good. We also charged for our infrared course in Chicago. Later on a few people at Beckman became interested in teaching various aspects of instrumentation to potential customers. So we set up the Technical Education Center, with Ken [Kenneth] Stein as head. We were able to get Beckman employees, who were specialists in the different instrumentation areas, associated with the Technical Education Center. We went around the country and gave courses. For example, we would go to Miami, be there for three days, and have four-hour lectures on different topics. We charged people to come, and they paid. Dale Harms was there; he handled the clinical side. We had courses on electroencephalography, infrared, ultraviolet, and the like. They were short, one-day courses in various locales all around the country. We also had a building by the Fullerton airport that we would bring people to for lectures.

GALLWAS: You're telling me that you got back to having a good time.

MANNING: Yes. I taught the various aspects of infrared in various places around the country. PerkinElmer, Beckman, and Cary would all contribute instruments and lecturers to different locations. For twenty-some years I went, every summer, for the third week of July over to Arizona State University in Tempe and spent a week lecturing on infrared. Howard Sloane, I, and a gentleman from PerkinElmer were the lecturers. We also held summer courses at Fisk University, MIT, Canisius College, the University of Minnesota, and UCLA [University of California-Los Angeles]. We covered the field. The university holding the lecture was the sponsor, but it was the people from the three instrument companies that were doing all of the work and providing the instruments that were to be used. These were typically one-week courses.

BROCK: Was there one person in particular who organized these summer courses? It sounds kind of unusual to me that you would have representatives of three competing instrument companies coming together repeatedly to teach these courses.

MANNING: Word got out, primarily, through infrared. MIT was interested in teaching people about infrared, so at the Pittsburgh Conference they got together with us to talk about doing something like this. I believe that the first course was held at MIT. Then the University of Minnesota found out about it, so they invited us to teach a course as well. Then UCLA and Fisk University wanted to sponsor a course too. Fisk University also had a gas chromatography summer course that the instrument companies would participate in.

BROCK: When did the summer courses begin?

MANNING: In the 1960s. The first one that I went to at MIT was while I was still in Chicago. I dropped out of the one at Arizona State when I retired. They didn't pay the lecturers. They gave us a nominal sum, a hundred-dollar check or something like that for a week of hard work. Beckman would underwrite my going there and provide the instruments for it. It was the same way with Cary and PerkinElmer.

PATRICIA ASHTON: What was the benefit to the companies? Potential sales?

MANNING: Sales, yes.

GALLWAS: Real visibility with customers who were interested.

MANNING: The chemists out in the laboratories didn't learn infrared and some other scientific techniques in college. It wasn't available to them when they were in school. These chemists would have problems, which they could not solve using the methods that they had been taught. But they would hear somebody in another lab like theirs say, "I've got this infrared instrument and it solves all kinds of problems for me." The chemists would then say, "But I don't know anything about infrared, so how can I afford to buy a fifteen- or twenty-thousand dollar instrument if I don't know how to use it?" The class at Arizona State went on for thirty years, it must have been. A professor at Arizona State, Jacob Fuchs, was the one who would set the program up. Fuchs would arrange for a week-course on infrared and a week-course on emission spectroscopy. Beckman didn't participate in the emission course, so Fuchs got other manufacturers to come in and help on that.

BROCK: I would like to shift gears for just one moment. I have one last question about the applications side of things during 1953 through 1963. You spoke about the flow of information from the sales departments to the applications lab and then back out. Did the applications group have any role in developing new products? You were using these instruments all the time, and you were constantly hearing from the outside. Did you help to design the next generation?

MANNING: Yes, we would attend meetings like the Pittsburgh Conference and the American Chemical Society meetings, and we would see what was going on in the universities, listen to the papers, and talk to the people there. We always came back loaded with ideas. "This gentleman over at MIT is doing this, and it looks like there's a chance that we might have an instrument that would be able to help."

To give you an example, Hank [Henry J.] Noebels was the head of applications engineering after Al Schreiber. Hank Noebels went to Pittsburgh one year and spoke with a fellow that he had formerly worked with. The gentleman had developed a microbalance using a meter movement as the device. This entailed a meter with a needle on it, which one would put a weight on to move the needle down, then by dialing in a little current one could bring the needle back up.

GALLWAS: Are you talking about [Lee] Cahn?

MANNING: Yes, Cahn. Hank Noebels came back to Beckman from the Pittsburgh Conference and said, "That balance looks like it would work very well. And we need to measure very small quantities for a lot of different kinds of samples, so I think we ought to look into this instrument." He talked to the engineers, and they also thought that this was a good idea.

[END OF TAPE, SIDE 3]

MANNING: Lee Cahn assigned Hal Schultz to the job of creating one of these little microbalances. It worked extremely well. It was a nice little instrument. We made three of them. I had one of them in the lab that we used all the time. But the powers that be decided that it wasn't a good instrument for Beckman Instruments. As a result, Lee Cahn said, "I think that there's a possibility that we could make money with that instrument," so he left Beckman, started his own company, and began making microbalances. The company was successful, for a while anyway.

GALLWAS: There's a similar story about Turner and the fluorimeter. Both of them used a DU type circuit.

MANNING: Yes.

GALLWAS: Beckman Instruments actually built some Beckman-labeled microbalances. Did we ever sell them?

MANNING: No, we didn't sell them. We only built three.

I mentioned that Hal Schultz was assigned by Lee Cahn to build the microbalances. Hal Schultz left Beckman and went to work for Lee Cahn as his chief engineer. Hal and Lee had a falling out, and Hal said, "I'm going to go into business myself and make microbalances as the Schultz Instrument Company." He developed an instrument that was, according to him, a little bit better than Cahn's. So Lee sued Hal for stealing his idea. Hal's lawyers called me as a witness at the trial. I took the Beckman design for the microbalance with me. My testimony was that, essentially, the design was done at Beckman. It was the same as what Lee Cahn was selling. It was, essentially, the same thing as what Hal Schultz had, and yet it was developed at Beckman Instruments.

GALLWAS: You got out of that one. [laughter]

MANNING: Yes. As it turned out, Lee and Hal settled out of court.

GALLWAS: There were so many products, all of which were useful laboratory products, but none of which generated enough money to make the company rich.

What do you know about Arnold O. Beckman, Inc. and the oxygen meter?

MANNING: It was housed in a little building halfway between 820 Mission and 1020 Mission. That was where Ed Arthur had his office. As I heard it, the idea for the oxygen meter was Linus [C.] Pauling's.

GALLWAS: Yes, that's correct.

MANNING: He thought it would be a moneymaker, so he talked to Dr. Beckman and they got together and set up a separate company to build oxygen meters.

GALLWAS: Did you know any of the people at Arnold O. Beckman, Inc.?

MANNING: I did, but Arnold O. Beckman, Inc. was a separate company and we didn't have much interaction. It's just that the companies were physically side-by-side up there in South Pasadena.

GALLWAS: For a gentleman who only gets paid with water and a book, he does pretty well, doesn't he? [laughter] You filled in a lot of blanks for us, Bob. You really have.

MANNING: My time at Beckman Instruments has been interesting. I enjoyed thirty-three years with the company.

GALLWAS: What have you done since you retired from Beckman?

MANNING: I've done a few things. I was a consultant for Beckman for three or four years after I retired in 1986. My wife was quite ill at the time, so I had to stay home and take care of her. She passed away in 1989, and I moved out of our big house into a condo down in Fullerton. I met a nice lady who had lost her husband. We got together and I got married again.

Five years after I lost my first wife, I had another one. We're living happily together. She had a beautiful house in Peralta Hills. Our next-door neighbor is the former CEO [chief executive officer] of UNOCAL [Corporation], so it's a nice neighborhood. We have one and a third acres, and we can't see our neighbors' houses. We're surrounded by vegetation and trees.

GALLWAS: It beats living in a condo by yourself, doesn't it?

MANNING: Indeed.

BROCK: I have one or two last questions, if that's all right. You were the ultraviolet product line manager from 1963 to 1968. Were you responsible for another product line after that, or did you go directly back to the lab?

MANNING: I went into the lab of the applications department. Bill [William F.] Ulrich was the manager then.

BROCK: While you were product line manager for ultraviolet, what was the governance structure? Did you report to the division manager?

MANNING: We had four or five different product lines, and we all reported to the sales department manager for the division.

GALLWAS: Was Bill [William I.] Slee the sales department manager at that time?

MANNING: Yes, Bill Slee was the manager.

BROCK: Are there any other thoughts or experiences that you'd like to share?

MANNING: When I joined Beckman Instruments, there was only the Beckman division of Beckman Instruments. That was it. Then Dr. Beckman bought Spinco, Specialized Instruments Corporation. He also bought other small companies that had products that didn't last very long. Then the company split up. A lot of our instruments were being used in laboratories, but a lot of them were also being used by manufacturers. We developed instruments for processes and continuous measurement devices for manufacturers of various kinds. As a result, two divisions developed: the scientific instruments division and the process instruments division. Eventually they split up and the process instruments division became a separate company and was bought out by another company.

We also bought the Liston [-Becker Instrument] Company. When we bought Liston, we had problems integrating the two. One time I went back for six weeks to Stamford, Connecticut and worked in the Liston laboratory to learn all about the instruments there. Each instrument had to be specifically designed for a certain analysis. If you were detecting benzene, the detector on the Liston instrument had to be specifically tailored to measure benzene and only benzene. There was a lot of chemistry involved in using the Liston instruments. When Liston was brought to California they also brought a fellow named Bob Chapman from Connecticut who was able to handle it all. They didn't need me anymore, so I got out of that.

At various times I was involved in continuous pH measurements. Beckman also had continuous UV and flow colorimeters. I was involved in setting up the process for the colorimeter because I knew about color measurement. Once I got the instrument going, I got out of it. I've been involved in all kinds of things in my years with Beckman. I would get in and then get out because somebody else would take over after I got it started.

GALLWAS: In my early days I had a lot of questions about color measurements. I went to you and you taught me everything I know about color. I think about you every time I open [Adobe Systems Incorporated] Photoshop. It's very clear you didn't teach me everything I need to know. [laughter]

MANNING: Yes. Color is quite interesting.

BROCK: Thank you.

[END OF TAPE, SIDE 4]

[END OF INTERVIEW]

NOTES

1. Henry H. Cary, U.S. Patent # 3,022,704. Issued 27 February 1962.

2. Arnold O. Beckman, William S. Gallaway, Wilbur I. Kaye, and William F. Ulrich, "History of Spectroscopy at Beckman Instruments, Inc.," *Analytical Chemistry* 49:3, (March 1977), 280A-300A.

3. Wilbur I. Kaye, "Near-Infrared Spectroscopy: A Review," part 1, *Spectrochimica Acta* 6, (1954): 257-287.

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