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WILBUR I. KAYE

BECKMAN HERITAGE PROJECT

Transcript of an Interview
Conducted by

Arnold Thackray and Gerald E. Gallwas

at

La Jolla, California

on

11 and 27 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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(Signature) Wilbur I. Kaye
Wilbur I. Kaye

(Date) September 26, 2002

WILBUR I. KAYE

1922 Born in Pelham Manor, New York on 28 January

Education

1942 B.S., chemistry, Stetson University
1945 Ph.D., chemistry, University of Illinois, Urbana

Professional Experience

1942-1945 Graduate Assistant, University of Illinois, Urbana

Tennessee Eastman Company
1945-1949 Research Chemist
1949-1955 Senior Research Chemist

Beckman Instruments, Inc.
1956-1968 Director of Research, Scientific Instruments Division
1968-1973 Director of Scientific Research, Corporate Research
1973-1980 Senior Scientist
1980-1987 Principle Staff Scientist

1987-present Consultant

Honors

1959 President, Optical Society of America, Southern California Section
1984 Fellow of Beckman Instruments, Inc.
1987 Honorary Member, Society of Applied Spectroscopy
1991 Wilbur I. Kaye Laser Analytical Laboratory dedicated

ABSTRACT

The interview begins with Wilbur I. Kaye describing his early interest in science, and specifically, instrumentation. He discusses his decision to study at the University of Illinois, and his own academic experience there as a Ph.D. candidate in chemistry. Kaye met his wife, Virginia [Ginnie], (who sits in on the interview) at Stetson University, where they studied as undergraduates, and married her prior to his last year of his graduate studies in 1944. Kaye was then recruited to Tennessee Eastman Company, where he set up a physics laboratory, by Dr. William Hincke. Initially, a PerkinElmer, Inc. spectrophotometer was the sole piece of instrumentation in Kaye's lab, but he soon procured more instruments, such as a Baird Corporation Model AB2 and Beckman Instruments, Inc. DU spectrophotometer. Having joined the Tennessee Eastman division of Eastman Kodak Company near its inception, Kaye relates the history of the company to the explosion of instrumentation research, and analysis in scientific research. Near the end of his tenure at Tennessee Eastman, Kaye was one of the first scientists to publish work in the United States on gas chromatography. Kaye began modifying the DU spectrophotometer while at Tennessee Eastman, and took this work with him on his move to Beckman Instruments. Kaye's modification to the DU became known as the Beckman DK spectrophotometer, which contributed to the competition and friction between former employee Howard H. Cary and owner of Beckman Instruments, Arnold O. Beckman. Amid shifting management and company organization, Kaye continued to improve upon his instruments, as well as develop new instruments while at Beckman. Kaye discusses the difference between DK1 and DK2, the DU and the DK, and addresses the Beckman line of infrared spectrophotometers. During the company's shift into clinical instruments, Kaye developed a glucose analyzer, which James C. Sternberg continued work on once the company withdrew support. Kaye developed the DKU, which combined aspects of both infrared and ultraviolet instrumentation. In addition to pointing out some of the history of Beckman Instruments in the second half of the interview, Kaye describes the interface between administration and research components of the company. A true scientist with an innovative mind, Kaye decided stay in the research laboratory, improving and developing new instruments, rather than join the management scheme of Beckman Instruments.

INTERVIEWERS

Arnold Thackray is President of the Chemical Heritage Foundation. He 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. In 1983 he received the Dexter Award from the American Chemical Society for outstanding contributions to the history of chemistry. He served on the faculty of the University of Pennsylvania for more than a quarter of a century. There, he was the founding chairman of the Department of History and Sociology of Science, where he is the Joseph Priestley Professor Emeritus.

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. As the business grew, he served in many roles from new product development to directing clinical field trials in the United States, 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.

TABLE OF CONTENTS

INTERVIEWEE: Wilbur I. Kaye

INTERVIEWERS: Arnold Thackray and Gerald E. Gallwas

LOCATION: La Jolla, California

DATE: 11 February 2002

THACKRAY: Dr. Kaye, please begin by discussing your family background, your pre-college education, and the beginnings of your interest in chemistry.

KAYE: As I read the book you sent to me on Dr. [Arnold O.] Beckman, I was rather surprised by how our backgrounds and history parallel each other (1). I became interested in science when I was about seven years old. I had a small laboratory in the basement of my family home in Lake Helen, Florida. I did many of the things that most boys do, like making chemicals and blowing up everything. Through college my major was chemistry. My mentor was Dr. John F. Conn. After graduation from Stetson University in De Land, Florida, in 1942, instrumentation began transforming procedures in chemistry laboratories. The *Journal of the Optical Society* is an excellent place to follow that transformation. In 1930 there were only one or two instruments either advertised or discussed. Every year thereafter that number has grown geometrically.

THACKRAY: When you applied to Stetson did you already know that you wanted to pursue science as a career?

KAYE: Yes. I realized that when I was in seventh grade.

THACKRAY: Did your parents have any scientific interest? What did they make of your interest?

KAYE: No, but they always helped me along where they could. My father bought me a chemistry set. He also bought me a small microscope. That would have been before 1935, because we lived in Lake Helen at that time. We moved from Lake Helen in 1937.

THACKRAY: You went from Stetson to [University of] Illinois. Why did you choose Illinois?

KAYE: I chose Illinois primarily because I was offered a scholarship. As a senior in college looking around for graduate schools, I had three primary choices: Caltech [California Institute of Technology], MIT [Massachusetts Institute of Technology], and the University of Illinois. Of the three, Illinois offered financial assistance. I remember when I got the acceptance letters from those schools; I was rather disappointed that the only school that offered me a scholarship was the University of Illinois.

THACKRAY: Why were you interested in Caltech and MIT?

KAYE: I wanted to attend Caltech because of Linus [C.] Pauling and the work that he was doing with x-rays. I wrote to Pauling to see if he would take me on as a student. He said that he was just too busy with War [World War II] work to take on any graduate school students.

THACKRAY: Did you have a particular interest in instrumentation when you were an undergraduate at Stetson?

KAYE: I had that interest when I was in grammar school. I loved to look at a variety of microscopic things with my microscope. Even before I got into college, in high school, when I took chemistry, my teacher encouraged me and allowed me to go into the lab after hours to do whatever I wanted to do. A lot of that was elemental instrumentation.

THACKRAY: If you think back to your high school, what instruments were available?

KAYE: There were balances, pipettes, condensers, and things like that, which were, in a way, instrumental. I also took physics my last year in high school, but we didn't have any experimental equipment to fool with.

THACKRAY: Was there more sophisticated equipment available at Stetson?

KAYE: Yes, in the laboratories we had a lot of chemical instruments and what you might call physical instruments.

Dr. Conn, my mentor in college, helped me receive my scholarship to the University of Illinois.

THACKRAY: Illinois was a powerhouse school.

KAYE: It was, but at the time I went there, during the middle of the War, it was a bare skeleton. Most of the male students had gone off to war, so just the girls remained. That was 1942.

THACKRAY: How big was your entering class of Ph.D. students?

KAYE: I graduated from college in 1942 and graduate school in 1945. I went straight through—almost from grammar school to Ph.D.—in a very short period of time. I was barely twenty-two when I received my Ph.D. At that time, the graduating Ph.D. class at Illinois was only about a half a dozen in the chemistry department.

THACKRAY: Who was your Ph.D. supervisor?

KAYE: Dr. [G. Lee] Clark. Clark was responsible for my thesis development. He was heavily involved in war work as well, so I didn't get as much help from him as I would have hoped for.

THACKRAY: What was your Ph.D. work?

KAYE: As soon as I received the letter from Illinois, I found out that the major work being done in the chemistry and physics area was x-ray diffraction. So I did my thesis on x-ray diffraction (2). My thesis was in three parts: one had to do with the oxidation of oils by heat, the second was the discrimination between the different kinds of hydrozones, and the last year of my work at Illinois was on the structure of penicillin. This work was highly classified and was considered very important. I was using one of the few instruments that could apply to the study of penicillin and it made a great impression on me. Here I was working on a very important chemical, and I had no effective tools to do it with, or none to speak of.

THACKRAY: Penicillin is a complicated molecule. What were you trying to study it with?

KAYE: X-ray diffraction. That was one of the few tools that was available to study molecular structure. Dr. Clark was an authority in this area, and the school was, again, one of the few places that taught it.

THACKRAY: What was your diffraction apparatus? Did you build that yourself, did you inherit it, or was it commercial?

KAYE: Dr. Clark had done a lot of work on an analysis of one kind of penicillin against another. There were about four or five different types of penicillins at that time. My work involved discriminating them by x-ray diffraction patterns.

THACKRAY: There were several people doing x-ray work.

KAYE: One reason I wanted to go to MIT was Dr. [M. J.] Duerger's work in x-ray diffraction. And one of the reasons I wanted to go to Caltech was because of Pauling's work with x-ray diffraction. The ultimate structure of penicillin was worked out in England by Dorothy [M.] Crowfoot [Hodgkin]. I never met her, but I did receive her reports.

THACKRAY: I was in England while Dorothy completed her work. I met her in the 1960s.

Would you say that your interest in instrumentation was shared by other graduate students at Illinois?

KAYE: There were about one or two graduate students under Dr. Clark who continued the penicillin work. We were all in one hall of offices. The x-ray equipment was in an adjacent laboratory.

VIRGINIA KAYE: Excuse me, but you might want to tell Dr. [Arnold] Thackray what you noticed while reading the Beckman book. You had the same professors that Dr. Beckman had twenty years earlier.

KAYE: There's a remarkable parallel between our paths, Beckman being twenty or twenty-five years before me.

THACKRAY: Yes. Speed [Carl S.] Marvel and Roger Adams were in Washington much of the time.

KAYE: Yes.

THACKRAY: Did you study under Marvel?

KAYE: I don't remember whether I took any classes under Marvel. I will tell this amusing incident, however. In late 1944, I had completed and submitted my thesis to the thesis committee, of which Marvel was a member. Marvel refused to sign off on it. The reason being that there were a lot of misspellings. He thought that any Ph.D. ought to know how to spell the English language. My wife [Virginia "Ginnie" Kaye] was the one that did the typing, [laughter] and to this day she can't spell words.

V. KAYE: But everybody else passed it. [laughter]

THACKRAY: Had you and Ginnie met at Illinois?

V. KAYE: No, we met at Stetson.

KAYE: We were married when we were in Illinois during my last year of graduate studies.

THACKRAY: Illinois, of course, had a very strong industrial connection. Did you think at all about trying to take an academic position?

KAYE: Yes, I thought about it. In fact, I had every intention, when I left Illinois, to return to university work after I had a little experience under my belt in the industrial side of things.

THACKRAY: Who recruited you to Tennessee Eastman [Company]?

V. KAYE: Dr. [William] Hincke.

KAYE: Yes, Dr. Hincke was responsible for that. This is the same Dr. Hincke who was Dr. Beckman's best man.

THACKRAY: What did Hincke hire you to do?

V. KAYE: Tennessee Eastman], a division of [Eastman] Kodak [Company], did not have a physics laboratory, so they hired Wilbur to setup the physics laboratory.

KAYE: My first publication was a paper on the oxidation of lubricating oils (3).

THACKRAY: I think infrared was just making waves.

KAYE: I guess I should mention that. In this physics laboratory that I set up for Tennessee Eastman], the only instrument that they had was a PerkinElmer [Inc.] infrared spectrophotometer [Model 21], but nobody could make it work. I went in there and figured out that the problem was that the thermocouples had to be evacuated. They wouldn't work in atmospheric pressure. I got that running and made the impression throughout the laboratory that I could make this darn bit of equipment work. We did use it a lot. Gradually I got other instruments and other infrared spectrophotometers and did much of my first work at Tennessee Eastman] with that infrared instrument.

THACKRAY: What other spectrophotometers did you purchase?

KAYE: We then purchased two others: a Baird [Corporation Model AB2] and a Beckman [Instruments, Inc.] 2T spectrophotometer. These three were the first infrared spectrophotometers manufactured in the U.S.

Here is an interesting story. I was looking out the window one day, when in walked the director of research with C. E. K. Mees, whose fame was in photography, in tow. I didn't realize who had just come into my laboratory. I was a little embarrassed to find out who Mees was. There were the C.E.O. [Chief Executive Officer] of Tennessee Eastman along with the C.E.O. of Eastman Kodak and me asking, "Well, who are you?" [laughter] That same thing happened again. Someone came to see the physics laboratory. I had no idea who he was, so I asked, "Well, what is your name again?" He turned out to be a Nobel laureate—exactly which one, I don't remember.

THACKRAY: Someone must have been proud of the lab and the instrumentation for the C.E.O.'s to stop by.

KAYE: Yes. They were visiting, of course, the whole laboratory, which was a lot more than mine, but yes, they were interested in it. C. E. K. Mees told the director after they'd finished their visit that these were some of the best electron micrographs that he had ever seen. This was, of course, a form of photography.

THACKRAY: When you were buying instruments, was it difficult to get authorization, difficult to get the funds?

KAYE: Surprisingly, no. One day the director of the laboratory came in and asked me if I wanted a spectrophotometer, which Kodak had used a great deal in their development of color photography. Did I want one of those? It was a twenty-five thousand-dollar instrument. I said, "Let me think about it." When I did come back to him, I said, "I would much rather have a Beckman DU spectrophotometer than this very elaborate instrument." The reason I said I wanted the DU was because I knew that I could do a lot more towards studying molecules with the ultraviolet spectrophotometer than any other similar instrument. So I never had any trouble. More instruments were thrust upon me than I knew what to do with. That electron microscope was one of them. It was the very first model that RCA [Radio Corporation of America] made. The director, again, said, "Well, we should have one of these, shouldn't we, in the laboratory?" I said, "That would be great to have, so long as we can have the right kind and not necessarily the most expensive."

THACKRAY: How did you know about the DU?

KAYE: There were advertisements.

THACKRAY: Did you see instruments at spectroscopy meetings? The Pittsburgh Conference [on Analytical Chemistry and Applied Spectroscopy] was started in the late 1940s.

KAYE: I went to about a third of those annual conferences and kept going to them for twenty or twenty-five years.

V. KAYE: I thought that you might like to glance at these. These [photographic plates of aluminum beryllium substrates] were hung in the Smithsonian [Institution].

KAYE: I published on aluminum beryllium substrate (4).

THACKRAY: Thus we can date these by the publication. Cellulose acetate from an acetone solution—this is obviously Tennessee Eastman businesses, isn't it? [laughter]

KAYE: Yes, they were interested in it.

V. KAYE: I think they're dated on the back. This one is from 1949.

THACKRAY: Good. Yes, they're dated as being exhibited in 1949 and hung and displayed at the Smithsonian. Here's a polystyrene deposit from a benzene solution. These are neat.

[END OF TAPE, SIDE 1]

THACKRAY: Being in the Smithsonian in 1949, you obviously weren't lost in the backwoods, were you?

KAYE: I had all sorts of instruments by that time. Again, I say, it was thrust on me partly because the director of our laboratory at Tennessee Eastman was anxious to both appear like he was doing a good job to his superiors and felt to do that, he had to have more instruments. This was just the period, 1939 to 1941, that instrumentation *per se* became an important component of any laboratory trying to study molecular structure.

THACKRAY: Were you being encouraged to publish when you were at Tennessee Eastman?

KAYE: I wasn't necessarily encouraged, but I had a strong desire to do so. I went ahead and published a lot of articles and the company was glad that I did. But that wasn't what they hired me to do.

THACKRAY: Were you also maintaining these instruments for other chemists at Eastman?

KAYE: Yes. There was an analytical laboratory as part of Tennessee Eastman's research laboratory. They sent samples to me to see if I could identify them.

THACKRAY: Did you have technicians working with you?

KAYE: Yes.

THACKRAY: A lot of these instruments weren't easy to keep running, were they?

KAYE: I kept them going, but I had help. I'm trying to think of the name of the first fellow that I had hired. He quit and said that he didn't see how these instruments were going to do anything for us. He didn't want to be a part of something that wasn't going anywhere.

THACKRAY: Would you say that the Eastman lab was a leader, or were other major corporations and their industrial labs doing very similar research at that time?

KAYE: Well, Eastman Kodak in Rochester was a pioneer in industrial analysis. It was one of the first. C. E. K. Mees lead the way.

THACKRAY: Mees, literally, wrote the book on industrial research, didn't he? But in Tennessee Eastman it was much more plant-connected, wasn't it?

KAYE: Well, Tennessee Eastman laboratory had just started when I got there. The reason that the lab started was because, when the War ended, Kodak had a contract with the government to do certain parts of the atomic energy project. And here, coming in almost every day, were people looking for a job from this Tennessee branch of Oak Ridge.

First, we received the PerkinElmer spectrophotometer and then the x-ray diffraction equipment. We purchased an electron microscope because Kodak had one and our director didn't want to be outdone by his Rochester colleagues.

THACKRAY: The company itself must have been enjoying prosperous times to keep hiring in people, buying instruments, and believing in their research in this way.

KAYE: Instrumentation—the whole field of analysis—was rapidly expanding.

THACKRAY: When you joined in 1944, how many Ph.D. chemists were in the Tennessee Eastman lab?

KAYE: In the Tennessee Eastman laboratories there were perhaps a half dozen, maybe ten. One of them, surprisingly enough, was her [Virginia Kaye] brother. [laughter] He wasn't in the research lab, but he was a Ph.D. working in the dye development part of the business.

THACKRAY: But of those six or ten, the others were more traditional organic chemists? Were they as interested in instrumentation as you were?

KAYE: No. The analytical laboratory, which was what you call a wet-chemical laboratory, was a separate laboratory from mine.

THACKRAY: Was your lab called the physics laboratory?

KAYE: Yes. There was no label on the door, but everybody knew it was the physics laboratory because it had instruments rather than test tubes, condensers, and whatnot.

THACKRAY: By the time you left in 1955, how many Ph.D. chemists were in Tennessee Eastman research?

KAYE: I don't know that I can answer that, but my guess would be that there were probably twenty or so Ph.D.'s and a flock of master's degree students.

THACKRAY: Did anyone else in the physics lab have a Ph.D.?

KAYE: At the beginning there was no one else, but later on there were six or so. One of the fellows I hired was Virgil Peck. He did a great job for us in the field of microscopy. The use of the electron microscope was an art as well as a science. I enjoyed my work the ten years I was at Tennessee Eastman.

We got a number of other spectrophotometers. We got a Baird [Model AB2]—we already had a PerkinElmer—and then Beckman came out with an infrared spectrophotometer, the IR-2T. I wanted the Beckman instrument because it allowed me to do more quantitative analysis.

THACKRAY: What did the Baird instrument do?

KAYE: Well, it was another infrared instrument, but its fame was that it would allow one to draw a spectrum automatically, whereas the PerkinElmer and the Beckman infrared instruments, at that time, were used for qualitative analysis. The interesting thing about the Beckman infrared instrument was that it was a pain in the neck to keep running because it had about a

hundred vacuum tubes in it, and each vacuum tube had a lifetime of perhaps a year. Thus, if you had a breakdown of one tube every so often, there was very little time that the whole thing worked. [laughter] In addition to that, the Beckman instrument used a wire recorder to record spectra, and this was a deadly instrument because of the wire. The wire was very small in diameter and ran through the sensors, picking up the signal at a pretty good clip. Just touching it, you would cut your fingers.

V. KAYE: Wilbur, going back to people that worked with you, do you remember Wilson Patton, a Ph.D. who later became a vice president of Tennessee Eastman.

KAYE: Wilson Patton was one of the few people who I really respected greatly for their scientific skills. We hired him.

V. KAYE: He took over your job.

KAYE: That was an interesting period and event. This was about the same time that we worked on the gas chromatograph, which proved to be an extremely valuable analytical tool. When I worked on the development of the gas chromatograph, Wilson was one of my assistants. We later received a letter from the editor of *Analytical Chemistry*, commending us on that work because it was the first published work on gas chromatography in the United States (5). When I left to go to Beckman, Wilson Patton took over and was in charge of the physics laboratory.

THACKRAY: You built your own apparatus for the gas chromatography research.

KAYE: Yes.

THACKRAY: Were there published reports coming out of England? How did you pick up on that territory?

KAYE: One day when I was in the laboratory, somebody came in and said, "Have you heard of the development in England on gas chromatography?" At that time I had no knowledge of gas chromatography, but this set me off and I went and did a lot of research in instrumentation. Wilson Patton helped me. We co-authored the paper, which *Analytical Chemistry* cited some fifteen years later as pioneering work (6).

THACKRAY: What use did you think gas chromatography was going to have for Tennessee Eastman?

KAYE: Well, one of the aspects of my job, analytically speaking, was with gases that were used in the synthetic rubber industry that sprang up during the War. The gas chromatograph proved to be the best instrument to study gases.

THACKRAY: I see. I didn't realize Tennessee Eastman had anything to do with that.

KAYE: Well, the synthetic rubber program was started during the War. In fact, these three brands of infrared instruments—the PerkinElmer, the Beckman, and the Baird—were independently developed.

THACKRAY: Tennessee Eastman never had any interest in becoming an instrument company?

KAYE: No. It was what you might call incidental that I got all these instruments and did this work. The company didn't consciously intend to do these things.

THACKRAY: Did it cross your mind that the gas chromatograph could be made and sold?

KAYE: Yes. Well, I have to jump ahead a little bit. The gas chromatograph work was done at Tennessee Eastman near the end of my tenure there.

THACKRAY: Right. During your time at Tennessee you did a number of modifications to the DU.

KAYE: I got the DU while I was working for Tennessee Eastman. One of the things that I found out, rather surprisingly, was that the instrument was more useful than I had realized. The DU was used only in the ultraviolet. If you look at the dial of the DU, it is calibrated from 200 nanometers to 1000 nanometers, yet it wouldn't give you any useful information outside of the spectral region from 220 to 800 nanometers. I looked at that dial and I kept thinking, "Why can't I use more of the dial than I actually do?"

To make a long story short, I modified the DU so that it ran from something below 200 nanometers up to above 1000 nanometers. It gave very rich spectra in these regions. It was so rich, in fact, that you couldn't handle it, really, manually as you could handle the lower

ultraviolet, infrared, and visible regions. When I learned that, I then realized I had to develop the automation of that region so that I didn't have to manually plot out the spectra. This opened up a whole new field of spectroscopy, namely the near infrared and ultraviolet.

GALLWAS: Wilbur, did you do that work when you were at Beckman or at Tennessee Eastman?

KAYE: It bridged that period.

GALLWAS: All right, but it would have started at Tennessee Eastman.

KAYE: Part of the automation was done at Tennessee Eastman and Beckman.

THACKRAY: Jack [John F.] Bishop came to see you in 1953. Is this because he had seen some of the publications that had come out of your work?

KAYE: Yes. He was interested particularly in the gas chromatograph and wanted me to join Beckman Instruments and continue that work. At that time I was happily ensconced in Tennessee Eastman. When Jack called upon me and asked if I was interested in joining Beckman, I just told him right square I was not interested. But when he came back a couple of years later, the story was a little different and I agreed. Jack Bishop was the man who hired me.

THACKRAY: Was Hincke still there?

KAYE: Hincke was still there, but Hincke was in charge of a chemical part of the plant that made fibers. I'm sure that when the Beckman people started looking into my credentials, they must have asked Dr. Hincke about me. That was a good connection.

THACKRAY: It's a small world, isn't it. [laughter] Now, I want to go back. You worked with the DU and modified it. You did gas chromatography work and then Jack Bishop knocked on your door. All of this must have made you much more aware of the commercial potential of innovation in instruments.

KAYE: Yes. If I had only known then what I knew later, I would have applied for a patent on the automatic DU. I could have done that, but Tennessee Eastman had a policy that all patent

material was first to be submitted to them. They'd make a decision of whether they wanted it or not. They said, we don't want a modified DU or DK instrument, so you can go ahead and patent it. I didn't have any knowledge of patent law, and I didn't follow up on that, which was a shame because I would be quite wealthy today if I had even gotten a 1 percent royalty.

THACKRAY: But you did take it to Eastman. The Eastman people looked at it to see whether they wanted to pursue it?

KAYE: Yes. They looked at it first, turned it down, and said this wasn't their business. Instrumentation wasn't their business.

THACKRAY: So Beckman said, "We'll take it." But the DK was developed before you came to Beckman, wasn't it?

V. KAYE: Well, it was started at Tennessee Eastman and finished at Beckman and was named the DK.

THACKRAY: Was it Bishop who took your idea into Beckman?

KAYE: Yes.

THACKRAY: You modified of the DU when you were at Tennessee Eastman, but Beckman picked up on it before you ever went to Beckman.

KAYE: Yes. I had published a couple of papers on this near infrared region, and Bishop, I'm sure, appreciated that. He came and wanted me to join their outfit.

THACKRAY: But when you didn't, he took the idea back anyway. Is that what happened?

KAYE: Yes.

THACKRAY: Did you then go back to the Tennessee and say, why don't <u>you</u> get into instrumentation?

KAYE: Tennessee just didn't seem to be interested. At that time, all correspondence that I had—letters to Beckman—had to pass through the manager at Tennessee Eastman. The fact was that they chose not to. Well, I can, in fact, tell a more interesting story.

There were three companies making infrared instruments at the end of the War: Beckman, Baird, and PerkinElmer. In addition to that, there was the ultraviolet part of the business. This proved that the ultraviolet region was of even more interest than the visible region and that the other side of the spectral region into the near infrared—this was totally new to everybody. But as soon as I published those spectra, it became obvious to a lot of people. Beckman had a real good thing going with its DU. I'm pretty sure at that time the DU was the primary moneymaker in the company. There's an interesting story here. Cary Instruments [Inc.], at this time, came into the picture. [Howard H.] Cary was an employee at Beckman.

[END OF TAPE, SIDE 2]

KAYE: Cary had done some development work while he was working with Beckman, which he then took with him when he started his own company, Cary Instruments. This called for a lot of rethinking of Beckman Instruments. It was losing its sales on its most profitable line, the DU, because it was not an automatic instrument. There had been no particular urge to have an automatic instrument as long as you were operating in the visible part of the spectrum. But as soon as the infrared and the far-ultraviolet regions came into being, it became clear to everyone and they started buying Cary Instruments instead of Beckman DUs. So Beckman was very interested in getting the automation for its DUs. I had done that. After I joined Beckman in 1956, the instrument was named the DK. The DK sold rapidly. It regained the market for Beckman in the ultraviolet and near infrared region.

THACKRAY: So Jack Bishop, to say nothing of Dr. Beckman, really wanted you to come to Beckman.

KAYE: Yes.

THACKRAY: Can we go back, Dr. Kaye, to your joining Beckman Instruments. Jack Bishop kept extending the invitation. What tipped the balance?

KAYE: Well, part of this is a personal bit that has to do with my wife's illness. When she was ill, I would not leave Tennessee Eastman. But when she recovered, that changed the picture a little bit and I accepted.

THACKRAY: I think it was 1 January 1956 that you actually started work with the title of director of research and engineering for the scientific and process instruments division. What did you think you were being hired to do?

KAYE: I appreciated the fact that Beckman needed an automatic instrument. They kept after me until they got it.

THACKRAY: Tom [Thomas] Park was the division manager when you joined.

KAYE: Yes.

V. KAYE: Wilbur and I were discussing yesterday that we have several tapes of Dr. Beckman in various meetings and interviews. Perhaps you'd like them in the historical museum.

GALLWAS: Absolutely, yes.

V. KAYE: We were wondering, really, what to do with them. It hadn't occurred to us until you arranged this appointment. It will take some doing on our part to find them. There's even a recording of Mrs. [Mabel M.] Beckman.

GALLWAS: Those kinds of things are priceless, and particularly, if after Arnold's staff at the Chemical Heritage Foundation [CHF] is able to review them, we'd come back and have another session. I'm inclined to think we're not going to capture what's in this man's head, and with his tutor here in one session.

THACKRAY: I think you may be right, Jerry.

V. KAYE: We welcome you any time. I'm sorry you caught Wilbur when he is on the way down, mentally. He has Parkinson's [disease].

THACKRAY: Do you have any correspondence records?

KAYE: Yes, we will make copies for you of those that are available.

V. KAYE: What type of correspondence are you interested in?

THACKRAY: Hiring letters and things of that kind become exceedingly interesting because they tell you a lot about the kind of the assumptions and the ambience of the period. We are also interested in drafts of papers, research notebook material, any other written text, either longhand or more formal processing. In essence, we are interested in anything that relates to, is part of, or throws light on professional career.

GALLWAS: There are two parts to what we're doing. One, we are supporting these oral histories, selected histories. The second is putting up a pilot exhibit at CHF's facility in Philadelphia, which is a magnificent place, of very early instrumentation. Of course, it will focus on Beckman's instrumentation and will grow much beyond that.

[END OF TAPE, SIDE 3]

[END OF INTERVIEW]

INTERVIEWEE: Wilbur I. Kaye

INTERVIEWERS: Arnold Thackray and Gerald E. Gallwas

LOCATION: La Jolla, California

DATE: 27 February 2002

KAYE: I have a collection of the reprints of all of the publications I've had, which prompts me a great deal in the memory of what happened when. And I have also a number of patents. Over here I have a few tapes that I found and an interview that I made with Dr. Beckman some years ago.

THACKRAY: I'm delighted you've found those items. They will be very helpful.

Going back to your previous interview, you started work at Beckman on 1 January 1956, Dr. Kaye, as the director of research and engineering for the scientific and process instruments division, with Tom Park as the division manager and John Bishop as the general manager. What was your relationship to Tom and Jack and what were your different areas of responsibility? Did you interact with Jack Bishop at all, or did you mostly work with Tom?

KAYE: Both Jack Bishop and Tom Park were my bosses, you might say, at this time.

THACKRAY: Were they involved directly with the technical aspects of what you did, or simply in a managerial way?

KAYE: They were involved primarily in a management role.

THACKRAY: Thinking about Beckman Instruments in 1956, what was the company like? You were no longer with Tennessee Eastman. You were in a different world. What struck you about your new world compared with the old world?

KAYE: Well, Jack Bishop had made me an offer in September of 1955. At that time, it wasn't exactly squared away as to who was going to report to whom. I just went right ahead and assumed my role as both a manager and a scientist.

THACKRAY: Were there other people at Beckman in a comparable role to yours?

KAYE: Shortly thereafter the scientific instruments division and the process instruments division were split, and I had little to do with the process end. Stan [Stanley] Spracklin headed process.

THACKRAY: Why was that split made?

KAYE: I don't remember all the details on that. I think in part what happened was that Jack Bishop made an offer to both me and to Tom Park. I think he was a little embarrassed that both of us accepted the job. He had to figure out how he was going to make the best use of us. Beside that, I didn't know too much about the process instruments side of the business.

THACKRAY: What did you hear about Howard Cary when you joined Beckman?

KAYE: I had known of Howard Cary before I went to Beckman. Our relations were always cordial, but it didn't take me long to realize that Howard Cary and Arnold Beckman didn't get along.

THACKRAY: What was the nature of the disagreement or the animosity?

KAYE: Howard Cary had worked for Beckman for a number of years before this period—at least back to the 1930s—and, at that time, Beckman thought all was all right with Howard. But when Howard left Beckman's employ and went off and made his own Cary spectrophotometers, this didn't set too well with Beckman because he felt that many of the ideas that Howard was commercializing had been developed under Howard's tenure at Beckman.

THACKRAY: How did other people in Beckman feel about that?

KAYE: I think by and large everybody liked Howard and respected his capabilities.

THACKRAY: His organization must have seemed quite a rival, quite a threat, didn't it?

KAYE: Yes. Beckman didn't have a product that would automatically draw spectra and this was essential if you were going to make use of either the far ultraviolet or the near infrared. So, the people at Beckman were madly scrambling to get an automatic recording instrument since Cary had introduced one. Beckman developed the DR, in 1955, which was inadequate.

Beckman made about four prototypes of the DR. They didn't work very well and they didn't sell at all. So I had, while I was still at Tennessee Eastman, already made an automatic recorder for the DU. Beckman produced it immediately, and it worked. That gave Beckman an entry into the near infrared. This instrument was named the DK1. Shortly thereafter Beckman brought out the DK2, which was a more economic apparatus.

GALLWAS: How did those come about?

KAYE: Those were Beckman's efforts at making use of this automatic instrument. They worked right on the DU, so the DK1 was essentially a carbon copy of the instrument I had made at Tennessee Eastman. After I joined Beckman, the DK2 was Beckman's version of the DK1. It did some things that lowered prices.

THACKRAY: When you say it was "Beckman's version," do you mean the modification you developed at Beckman?

KAYE: Yes.

GALLWAS: Well, the DK1 was actually produced at Beckman, but it was your original design.

KAYE: That's correct.

GALLWAS: Then you and an engineering team must have done something to create a lower-cost version.

KAYE: That's correct.

THACKRAY: Was Dr. Beckman himself involved in anyway with this process?

KAYE: Not to my knowledge. He might have worked in the background on the marketing of the instruments.

THACKRAY: The earlier efforts at Beckman, before you got there, who engineered those? Do you know?

KAYE: Bill [William S.] Gallaway was one. There were a number: William [W.] Ward, J. [Joseph] Ashley, and Lee Kahn.

GALLWAS: Bill [William] Henderson?

KAYE: Bill Henderson, yes.

THACKRAY: The company was fairly unusual in having a Ph.D. chemist as its C.E.O., but at this stage, did it really make any difference that Beckman was a Ph.D. chemist?

KAYE: No.

V. KAYE: Going back to the DK, it wasn't too long ago in, I think, *Analytical Chemistry*, that we saw an ad for a man who repairs DKs.

GALLWAS: That would be Tom [Thomas] Black.

V. KAYE: So we figured they were still in use.

GALLWAS: Yes, they are. There are two versions: the early version of the DK1, which had a non-linear scroll, and the DK2 with a linear scroll. Is that correct?

KAYE: Yes.

GALLWAS: There are a lot of DK2s that are still being used in laboratories.

KAYE: And there's a good reason for that. But what is it?

GALLWAS: Well, you designed a good instrument! [laughter]

KAYE: The DK1 and DK2 measured spectra in the near infrared. We got into it with the far ultraviolet region, and then the near infrared. There's something about these instruments and the way they recorded that continued to be in the Beckman instruments, whereas Howard Cary did it somewhat differently. The DK2 was more economical and used a non-linear scroll which gave linear spectra.

THACKRAY: There was an entire series: the DK1, the DK2, and the DKU. Were you the lead man in that territory?

KAYE: Yes.

THACKRAY: There were some issues around the DU power supply in the late 1950s. Is that something you recall? There was also the project to develop the DU2 and then there was the DB spectrophotometer. Do you remember that one at all?

KAYE: Yes.

THACKRAY: This was like acquiring an outside technology. Well, they acquired an outside technology when they acquired you, of course. [laughter] What was the attitude about bringing in technology from outside Beckman?

KAYE: We didn't need to bring an awful lot in. The DK1 I had done at Tennessee Eastman, and when Beckman brought out their first DK, they didn't need to rely an awful lot on any outside help because the DK1 was a carbon copy of what I had done at Eastman. It looked the same and had the same arrangement of components.

THACKRAY: The introduction of automation into the instruments—scanning and data handling—was that to be done across the instrument line?

KAYE: Well, I was involved in most of the things that were happening at that time. It was just my character to work on them. It was scientific and I left out a lot of the management aspects of the business.

THACKRAY: Going back to Howard Cary for a moment, did you ever receive an offer from Cary?

KAYE: Yes.

THACKRAY: When was that?

KAYE: That was some time after I had been well established at Beckman. I remember there was a party that Howard Cary threw at his house. He approached me, but I didn't give him any hope that I would leave Beckman.

THACKRAY: Was it a two-way street? Did people come back to Beckman from Cary?

KAYE: Yes, but it was not extensive.

THACKRAY: Were you considered a traitor if you went to Cary?

KAYE: No.

THACKRAY: Was the issue of people moving to and fro mainly monetary, or was it a difference in the style of the two companies?

KAYE: Both.

GALLWAS: May I interrupt you and return for a moment to the DK1? When you built the DK1 at Beckman, you had—

KAYE: That would have been Tennessee Eastman.

GALLWAS: That's correct, but then it was manufactured as a Beckman product.

KAYE: Yes.

GALLWAS: It would seem to me that the DK had all the attributes of an improvement in the DU. Was there any discussion of why we continue to sell the DU when we have the DK?

KAYE: That's an involved question because it had to do with how the instruments worked.

GALLWAS: Because the DK had the benefits of scanning. It had UV [ultraviolet].

KAYE: But it was more expensive than the DU itself. There is another important point and that is that the DUs gave better answers than the DKs.

GALLWAS: Really?

KAYE: Yes. The DU had established itself as the primary instrument to use when doing quantitative ultraviolet spectroscopy. People were getting different answers from the DU and the DK. They liked the DK because of its automatic recording features, but they also liked the DU because it was more accurate.

GALLWAS: So it had greater photometric accuracy?

KAYE: The DU, yes. Of course, at that time, the question of what the accurate answer was was a sticky point. The DU accuracy was determined by the photomultiplier. There were a number of articles published in the literature right about this time, arguing this very point, which was the more accurate. They didn't give the same answers. It was quite some time later before I realized what the answer was. The DU used a phototube and the DK used a photomultiplier. The photomultiplier introduced some errors. Both of them were marketed at the same time, just to a different kind of market. The people who wanted quantitative, accurate answers bought the DU. Those who wanted the versatility with rapid charting of spectra chose the DK. With the photomultiplier there was a difference, but I didn't realize that for a long while.

GALLWAS: Please go back to the Model B that's in front of you there. Was that built with a quartz prism?

KAYE: I don't believe so. The Model B came out around 1950 and didn't employ a quartz prism.

GALLWAS: So the quartz prism really didn't emerge until the DU [1940], and that wasn't useful until there was a hydrogen lamp and power supply.

KAYE: Well, the Model B [1940] that's shown here is the second of several prototypes, and it had a glass prism.

[END OF TAPE, SIDE 1]

THACKRAY: Who were Beckman's main competitors in the UV spectrophotometer market in the late 1950s?

KAYE: PerkinElmer bought out another company making, essentially, DK instruments.

THACKRAY: Was it very serious competition?

KAYE: Not really. The competition mainly stemmed between Cary and the DK.

THACKRAY: Did you have anyway of knowing who had the larger market share?

KAYE: The Cary instrument was a good deal more expensive than the DK. I feel Beckman lead the market.

THACKRAY: Did you have some responsibility for the infrared business as well?

KAYE: Yes. The infrared business was started during the Second World War simultaneously by [American] Cyanamid [Company], Baird, PerkinElmer, and Beckman. I ultimately bought, while at Tennessee Eastman, each of those instruments, and I recognized immediately the

advantages that some had and the disadvantages of some of the others. The Baird instrument had automatic recording, which was pretty well necessitated when you work in the infrared because the spectrum is so complicated. But Baird didn't really have a great deal of success in marketing their instrument.

THACKRAY: The year you joined Beckman, the IR-4 was introduced, which was the first dual-beam IR [infrared].

KAYE: Yes 1956.

THACKRAY: What was the conversation in the 1960s surrounding single-beam versus dual-beam?

KAYE: Raging marketing ploys for the two instruments. Shortly thereafter, you had to bring out a new model every year because the market became so competitive. The IR-3 was the first automatic Beckman recording infrared instrument. But it was so expensive that it didn't receive very much marketing attention.

GALLWAS: Who was the designer of the IR-3?

KAYE: There were many people involved. Roland [C.] Hawes, Cary, and others.

GALLWAS: Did Bill Ward take over after Howard Cary left?

KAYE: No.

THACKRAY: Can you talk about Bill Ward?

KAYE: Yes. Bill Ward was a very good scientist. Unfortunately, in about 1960 he developed a brain tumor and had to withdraw from the business.

THACKRAY: Who took his place?

KAYE: Paul Gilbert. His *forte* was flame spectrometry that was used for inorganic analysis. Paul was a very interesting man, kind of queer in some ways, but meticulous. Everything he did, he did with finesse. He didn't get involved in the other sides of the spectrum that were available to anyone with a DU or a DK.

THACKRAY: Another name was Joseph Ashley. What did he do?

KAYE: He worked on the development of the DK2. Ultimately he left and moved to process instruments.

THACKRAY: Was your group, in the late 1950s, involved in other territories like gas chromatography, mass spectrometry, or NMR [nuclear magnetic resonance]?

KAYE: Gas chromatography is an interesting field. I had heard, around the late 1950s, rumors about the field of gas chromatography being developed in England and thought, "Why couldn't we bring out something."

THACKRAY: You'd had some interest in gas chromatography at Eastman, but when you got to Beckman, was your group involved with developing that field?

KAYE: Yes, but I didn't get closely involved in the technical side of it.

THACKRAY: In the scientific instruments division in the late 1950s and early 1960s, how did you agree on what you were working on and what you were supposed to do? Did marketing tell you what was needed? Did you tell them what was made? How did that work?

KAYE: Yes, that was a debate. I was a self-starter on these projects. I didn't have to have a lot of input from the marketing department. I saw them all the time and they would suggest that we do this and that. But by and large, marketing followed research at this period of time. It wasn't until quite some time later, probably in the early 1970s, that marketing took over what was to be done.

THACKRAY: Do you that was an impact of [William] Ballhaus taking over leadership of the company?

KAYE: No, we didn't have much interaction with Ballhaus. He ran the show, but not technically.

THACKRAY: Have you any thoughts on why that change came about from research driving to marketing driving?

KAYE: It developed over a period of several years. I wasn't happy with it. By and large, when I came to Beckman, I had a free reign.

THACKRAY: What do you see as your best achievement at Beckman?

KAYE: That's what this book is all about. But what do I consider the best? That term "best" is hard to handle. Some of the instruments that I was particularly proud of were, of course, the DK, the gas chromatograph, the glucose analyzer, the near infrared, and the far ultraviolet.

GALLWAS: I think it would be worthwhile to talk about the story behind the glucose analyzer. Its inspiration was very close to you, wasn't it?

KAYE: Yes. Starting back early, Beckman was very interested in getting into clinical instruments. They saw that the real direction that instruments were going was clinical. I had a personal interest in glucose because of my diabetes. Beckman didn't have much success with medical instruments until we came to the glucose analyzer. There were several other instruments started, largely in the process instruments division. We began to have a little success with the polarographic method of analyzing glucose. Arnold Kadish, an M.D., was there from the beginning. He had a daughter who was diabetic, and that gave him a large incentive to do all that he could. At first we were going to use saliva to test for glucose. Between the parties involved, we started having real success with the polarographic method. I was in on that from the beginning in an advisory capacity.

Anyway, the company, after it saw the early efforts, wasn't interested in glucose analyzers. I was interested primarily in an automatic glucose analyzer, one that could continuously monitor blood sugar, but the company withdrew support. But James C. Sternberg took a personal interest in the analyzer and pushed it through the company with my encouragement.

GALLWAS: Was all the glucose work done at Beckman?

KAYE: The initial part was done under my direction.

V. KAYE: Rudy Duringer of Germany also had a hand in the work.

KAYE: Rudy Duringer was a technician on the project.

V. KAYE: And Jim [James] McDaniel.

KAYE: Jim McDaniel was also a technician.

GALLWAS: You remember them all. [laughter]

KAYE: She's got a better memory than I have.

THACKRAY: At some moment, the clinical, biomedical instrumentation became a separate entity, but it was part of your territory when it began?

KAYE: Yes, I did the direction and then Sternberg got into it. I assigned the project to him. Jim improved the instrument by making it operate on a rate basis.

THACKRAY: It was in the 1970s that the tenor of the company began to shift towards the life sciences.

KAYE: Well, the company wanted to get into the life sciences several years before it actually did, and it developed a number of instruments that never flew. The glucose analyzer was the first successful medical instrument.

THACKRAY: Were you involved with those earlier, unsuccessful efforts?

KAYE: No.

THACKRAY: As the company began to move in the clinical, life science direction, were you an enthusiast for that or was your heart somewhere else?

KAYE: My heart was somewhere else, but I was definitely very interested in the glucose analyzer because of my diabetes. That was the first instrument that led, ultimately, to the medical side of the business. Well, I suspect that I would not have been particularly happy with letting go of the project, but I did, and Sternberg took it up. When I withdrew from the glucose project I turned my attention to measuring stray light in spectrophotometers.

GALLWAS: Let's go back. What about the application of ultraviolet spectrophotometry to the ultracentrifuge? Were you involved in that?

KAYE: I was aware of it but I was not really involved.

GALLWAS: What about the DKU, the DK Universal?

KAYE: There were only about four models of that made. I was kind of shunted off to the sidelines at this time, but with the help of Jim McDaniel I did bring out the DKU, a hybrid between the infrared and the ultraviolet instruments. It was unsuccessful because of its high cost.

GALLWAS: Did it read well into the infrared?

KAYE: Not well in the near infrared, that region from the visible to about 2.5 microns. At first it was the near infrared instrument that we brought out. We needed an instrument that would work in the near infrared, primarily because it was useful for the molecules that had hydrogen in them. I was very surprised when I moved the prism over relative to its original position so that it would scan the near infrared region. I was very surprised at the detail of spectra in that region. It wasn't that the near infrared wasn't known before I got into it.

GALLWAS: Were there any instruments? Didn't you do the first near infrared work?

KAYE: I did the first near infrared work while I was at Tennessee Eastman. There were instruments that went into the infrared. They were the IR-4s, -5s, -6s, -7s, -8s, and -9s. I was primarily active in administration. The research department was at its most active phase during that time with over a hundred scientists.

GALLWAS: But didn't they cut off at about 2.5 microns?

KAYE: It wasn't so much the cut off as they weren't much good in the region below 3 microns. There was a great deal of interest in that part of the spectrum from 2 to 3.5 microns. The IR instruments did not read well below 3 microns.

GALLWAS: What was the interest?

KAYE: Initially it was used as an analytical instrument in a government synthetic rubber program.

GALLWAS: Even though you used a different detector, did you still use a tungsten source?

KAYE: Yes. During the late 1950s and onward, we had to bring out a new model every year. One of the instrumental achievements then was how near infrared would answer questions posed by people like the synthetic rubber industry and the medical profession.

[END OF TAPE, SIDE 2]

GALLWAS: The smaller instruments are the -5 and the -8. I don't know what happened to the -6.

KAYE: What happened to the -6 is interesting. There was a period of time when there was a small competition within the research department to discover the best way of making a low-cost, infrared instrument. Originally we were using sequential numbers to identify the different models. When we came to the low-cost infrared instruments, two were developed. One was double-beam and the other single-beam. It turned out that the single-beam instrument—

GALLWAS: The IR-6?

KAYE: It was originally the IR-5 because in the sequence of instruments, -4, -5, -6, and -7, the first low-cost instrument was assigned IR-5. But, when the IR-5 didn't go over, we still wanted to keep the sequential number of models, one of them, the IR-5, won out so we had to do

something since there were several models out on the market, to salvage the numbering of the instruments.

THACKRAY: You changed title, Dr. Kaye, sometime later on, from director of scientific research to senior scientist. Was that because you wanted less time in management?

KAYE: I think it was that. My title remained director of research long after it meant anything, but it was on the records. Every six months with your paycheck, you were given a little green slip that identified your position, your salary, and the like. I had been shunted aside on much of the development that was going on at that time, but I retained the title.

THACKRAY: Would you have been happier if you had been in the middle of everything?

KAYE: Yes, but I was tormented between the two. I realized that if I wanted to continue up the ladder, so to speak, within Beckman, I had to expand my type of research. But I stubbornly remained busy with developing my own ideas for instruments.

THACKRAY: Did most people move across to management eventually?

KAYE: No, I wouldn't say so. But most of the more ambitious ones did.

THACKRAY: Well, it's good that you didn't.

KAYE: Yes. The instruments were all identified in here. Many of the identifications were made because of my stubborn streak.

THACKRAY: Who supported your decisions?

KAYE: The company respected what I was doing in development. A good example of this is the episode with pure water.

THACKRAY: Please tell us about that?

KAYE: I needed pure water, water that had no particles in it, for measuring particle size. The way that this had been classically done was to sheath the liquid—meaning that there was one stream of so-called pure water and another stream of an arbitrary sample that one measured the particle size in. What emerged out of that was a still that, under certain circumstances, would give ultra-pure water. There were no particles in it at all. Before that time, I had never been able to get a sample of water pure enough to use as a sheath for the sample. This instrument was a still, a very special still.

We gave that away, didn't we?

V. KAYE: Yes. We asked Steve [Steven] Penton to take it to the Beckman Museum.

GALLWAS: Then it should still be there.

V. KAYE: That piece was made in France with special glass.

KAYE: I don't know whatever happened to it, but it was a very unusual still. There had a cold finger sticking into the collector of water. I would distill it into the sample and one day, by accident, the collector of the distillate protruded into the collecting stream of water and never broke the surface. When that happened, all of a sudden the distillate was perfectly clear and would remain such. Later I realized that what was happening was that what I took to be distinct particles were really voids. This work went on for more than a year, and I'm sure this thoroughly upset management. They felt that I was wasting my time. As it turned out, I probably was because nobody else picked it up.

V. KAYE: Well, you did sell it. We toured California with it. [laughter]

KAYE: I tried to sell it. I took it around all over California to show it off to people whom I knew were working in the field.

V. KAYE: Which company bought it? The company was about to go bankrupt, so they bought the piece to bring themselves out of bankruptcy.

KAYE: I believe that it was Optical Technology Corporation.

KAYE: We tried to sell it to the Spinco [formerly Specialized Instruments Corporation] division of Beckman. They made a prototype that looked nice and worked fine.

THACKRAY: Dr. Kaye, thank you for your fascinating insights and recollections.

[END OF TAPE, SIDE 3]

[END OF INTERVIEW]

NOTES

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4. Wilbur I. Kaye, "An Aluminum-Beryllium Allow for Substrate and Replica Preparations in Electron Microscopy," *Journal of Applied Physics* 20, no. 12 (December 1949): 1209-1214.

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6. See Note 5.

INDEX