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CARLSON: All four of my grandparents came from Sweden and became Minnesota farmers. My parents were born in Minnesota. My father left the farm when he was twenty-one and became a barber. He worked in various parts of the western United States traveling by bicycle. He finally settled down in Seattle when he married my mother. I was born in Seattle, Washington on February 8, 1906.

Shortly after I was born my father contracted tuberculosis and arthritis, or rheumatism. To find a suitable dry climate, which my father thought would help his tuberculosis and rheumatism, the family left Seattle and moved to various places in California, Arizona, and Mexico while I was still very young. But the family fortunes declined and finally gave out. We settled in San Bernardino, California in 1912, when I was six years old. I went through high school in San Bernardino. We were very poor and I had to work to help support the family as soon as I could. This was between school hours.

I did odd jobs, washed store windows, did janitor work, et cetera. By the time I was in high school I was the main support of the family, but I continued with school. My mother died in 1923 when I was seventeen. My father's health had not improved; he was practically a total invalid. He could get around some, but not very much. My father and I continued to live together and I finished high school. My churchgoing was confined to sporadic attendance at a few Sunday schools. By the time I reached college age I became pretty much of an agnostic and lost interest in any church.

ERMENC: Did you have any choice in high school as to the kind of courses you could take?

CARLSON: I emphasized the science courses and my grades were good. In English, history, and languages my grades were poor to aver-

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* This is an edited typescript of a tape recorded interview of Mr. Carlson by Professor Joseph J. Ermenc of Dartmouth College. The interview took place on December 16, 1965 in New York City.

age.

ERMENC: What sciences did you take in high school?

CARLSON: General science, chemistry, physics and biology. And, of course, various math courses.

ERMENC: Did you do any project work in chemistry or physics as part of your curriculum?

CARLSON: I worked after hours for a chemistry professor who did some testing of milk for butterfat and things like that. I would wash the laboratory bottles and clean up the lab. After I had taken the chemistry course during my junior year in high school, I was offered a job in the chemistry lab of a cement plant in the nearby town of Colton. It was in the control laboratory where tests were made of the mix of powdered rock to cement. I was at first mainly a bottle washer. Later I was given some of the control chemist's work to do.

ERMENC: How did you get that job?

CARLSON: The dramatics coach at the high school was a sister of the chief chemist at the cement plant. I guess she must have known about my need for earning some money. I didn't apply for it; I know that. It was a small lab. There were only two or three men in it. It was a twenty-four hour operation and there needed to be a man on the job all of the time. As soon as I appeared to be capable of handling the job there were times when I took a shift. I guess the chief chemist wanted to encourage me and help me. He probably went out of his way to help me and to give me as many hours income as he could. I worked there Saturdays and Sundays in my senior year in high school and, of course, during the summer.

ERMENC: What in particular was the test work you were doing?

CARLSON: I did some of the mix chemist's work. Cement is made from a mixture of limestone and clay and the proportions are very critical. The mix chemist in a cement plant laboratory goes out in the mill roughly every hour, picks up a sample and brings it back to run a routine test for percentage of limestone. Then he writes an order for a certain bin to be emptied along with a certain amount of clay and they are mixed together in the plant to form the mix that goes into the kilns. There the mixture is burned or heated to red heat. A clinker is formed which is later ground. This is cement.

ERMENC: Describe any other high school activities or influences.

CARLSON: In high school I missed most of the social life because I had to devote every spare hour to a job of some kind. I had few really

close friends. One fellow, who was my closest friend, was very different from me. He was not a scholar by any means. He became a machinist in a railroad shop. We were close friends just because we were originally neighbors. Somehow we filled a need in each other for companionship.

ERMENC: What sort of things did you do together? Did you make things?

CARLSON: No. Mostly talking and maybe going out for a ride in his mother's car or something like that; just bumming around together.

ERMENC: Going back a bit again to the high school job you had with the chemistry teacher. How was it that you were chosen for the job?

CARLSON: Well, I was friendly with him and I think I was perhaps the only one that needed the job or showed an interest in getting one.

ERMENC: Do you think there was something else?

CARLSON: Well, I was one of his top students. I don't know that I was on top, but I was close to the top and probably the only one who was in need.

ERMENC: How did you arrange to go to college?

CARLSON: I wanted to go to college, but I had no idea that I could afford it. I had little saved; maybe two or three hundred dollars in the bank. But since my father was dependent upon me, there didn't seem to be any way that I could do it. Nevertheless, I took a year of postgraduate work in high school to make sure that I could be admitted to college because I had missed out on a couple of required subjects.

During that year an uncle, my mother's brother, visited us during the summer and he had a talk with me. He said I should try to go to college if I could find any way at all. I told him that I had heard of a junior college in Riverside, a nearby town, which offered a cooperative course: students worked six weeks and then went to school for six weeks. That seemed to offer an opportunity to earn while studying.

I had a dream of going to Cal Tech[1] before that. It resulted from a visit of a group of Cal Tech students to our high school. They gave a program in which they boosted Cal Tech as a good technical school. It impressed me so that I thought it would be wonderful if I could go to Cal Tech. But that seemed clearly out of the question since the tuition and other costs were too high. So I went to Riverside and interviewed the

1. California Institute of Technology, Pasadena, California.

head of the cooperative course there who was a physics professor.

The Riverside Junior College was a small state-supported institution, but there were some rather unusual people on its faculty. Even though there was a chance to earn a living at Riverside, it was still questionable whether I could make the grade. Most students had nobody but themselves to support. Most of them had some reserve funds, which I didn't, or could expect a little bit of help from their families, which I couldn't. It wasn't clear whether I could make it or not, but I decided to try. So I started in the cooperative course. It turned out that the college had a tie up with Riverside Cement Company in Riverside and I was given a job in the lab there.

ERMENC: What year was this?

CARLSON: I started work in the cement lab in 1925. The cement lab was in one of their chemistry lab's two departments: the chemical analysis department and the physical testing department, where I worked. Here, samples of cement were sieved to determine the percentage passing through a 200 mesh screen. Also, samples of cement were made into pats to determine the setting time, the physical strength of the cement, et cetera. I took samples out in the mill and brought them back to the lab for testing. A cement plant runs twenty-four hours a day. Most of the time I seemed to get the night shift. I continued with that job for the three years I was in junior college cooperative courses. Of course, there was always a pair of us who alternated. While one was going to school the other was working.

ERMENC: Did any of your employers encourage you to go on to college or to invent or develop ideas?

CARLSON: I didn't recall any who encouraged me in that way. Their principal encouragement was providing me with the chance to earn some money.

ERMENC: What was the course you took at Riverside Junior College?

CARLSON: I started out as a chemistry major, but my interest soon shifted to physics. Actually, the two majors were not very different in those junior college years. In part, my shift of interest to physics was due to the personalities of the professors. The physics professor had a personality which appealed to me more. He was also the head of the cooperative course. He was a tremendous out-of-doors man. Every weekend, and every other chance he got, he went into the desert or the mountains to camp and hike. He liked to take students along and he took me

along on a number of trips. This was about the only recreation I had in those years.

After working in the chemistry lab trying to make analyses and tests, I found that my abilities didn't run too strongly in that direction. Physics seemed to me to be more precise and more interesting. So my interests began to change: I thought they would center on physical chemistry. Ordinarily the two year junior college course would take four years on the cooperative schedule, but I managed to complete it in three because I took some extra courses. I also managed to save a few hundred dollars.

ERMENC: Where did you live in Riverside?

CARLSON: My father and I rented a small apartment. During the summers I worked full time. I built up my financial reserves to the point where I decided to take the big step of going to Cal Tech and enter as a junior. In order to do that I had to take a couple of three-week math courses in the summer.

ERMENC: Your physics at Riverside must have been awfully good to be acceptable at Cal Tech.

CARLSON: It was pretty good. It wasn't perfect, but it was pretty good. It was classical physics entirely. Modern physics was just beginning to be taught at Cal Tech in the upper class years.

ERMENC: What was given in the math cram courses?

CARLSON: I've forgotten the title of the course but it covered the first two years of Cal Tech's calculus.

ERMENC: Did it go through differential equations?

CARLSON: Oh my, yes! As I recall, we had a special course in differential equations in the junior year.

ERMENC: At that time who were some the eminent men at Cal Tech?

CARLSON: Milliken was still head of Cal Tech. Smythe was professor of electrical principles, or something like that; Fritz Zwicky gave a wonderful course in analytical mechanics; and, of course, there was Bates in physical chemistry. Pauling was there but I didn't take any of his courses. Dr. Thomas Hunt Morgan had just come out there and started a biology department.

ERMENC: It was really pretty well on its way to becoming a nationally prominent institution in the twenties.

CARLSON: It was getting there, yes. It was just in the growing stage. It also had a fairly strong humanities department. I managed to

pass the entrance exam into physics, chemistry, and math for advanced standing and I was admitted as a junior. I continued through the two years and graduated from Cal Tech in 1930 as a bachelor of science physics major.

ERMENC: That was a pretty grim time to graduate wasn't it?

CARLSON: Yes, it was.

ERMENC: What was your thesis?

CARLSON: We had no thesis requirement.

ERMENC: Were you required to do projects in any of the courses?

CARLSON: I did a project in physical-chemistry. I made some measurements with absorption cells of the absorption spectra of an iodine solution in various concentrations. I guess it was to determine the molecular structure of iodine in various concentrations. I'm a little hazy myself on just what it was now, but it wasn't a thesis. I wrote a report and my findings were later incorporated in a paper written by someone else.

ERMENC: What did you major in?

CARLSON: Physics.

ERMENC: Any particular branch?

CARLSON: Physical chemistry was my interest and I had a good course in physical chemistry in my junior year. I also took courses in mechanics, electricity, and magnetism. I had to take some required courses that I missed. They were scientific, German, history, and some literature. I also had to take freshman geology and paleontology.

ERMENC: How did you take to the humanities courses?

CARLSON: I enjoyed them.

ERMENC: Do you remember what they were like?

CARLSON: The philosophy class was very small. We sat around and discussed the ideas of various philosophers of the past. We also had good courses in English and American literature. The history courses were not very good. Those I did outside of class by doing the required reading. I had an economics course from Professor Graham Laing. He was an old time social economist; I guess you'd call him something of a socialist. He was a very interesting personality and we enjoyed his courses. I had McMinn in American literature.

ERMENC: What about extra-curricular jobs? Did you have any jobs in laboratories at Cal Tech as you did in high school?

CARLSON: No. The course was so stiff that there was not much

time for working during the school year. I think this was discouraged by the faculty. But I did do such odd jobs on the weekends as mowing lawns. During the summer I worked in a cement testing laboratory in Los Angeles.

ERMENC: What made the work at Cal Tech so demanding? Were there a lot of written reports which took up so much time, or was it the high standards of learning?

CARLSON: They covered a lot of material and the requirements for study outside class were quite stiff. There was a lot of math, a lot of reading, and a lot of problems to do.

ERMENC: It wasn't report writing that took up time?

CARLSON: Not report writing. As I recall, it was mainly answering the questions at the end of each chapter.

ERMENC: That was 1930, and I suppose around 1929 or '30 you were beginning to look around for a job. What did you have in mind? What kind of job? With whom?

CARLSON: I had hoped for a job in physical chemistry. I realized that I needed a job as soon as I graduated because I had run into debt and I had borrowed money from every available source.

In the middle of my senior year I started writing letters. I think I wrote 82 letters of application for jobs all over the United States. I wrote to any company that I thought might be interested in physical chemistry, and a few to companies interested only in straight chemistry. I got only two or three replies and only one interview. At the interview the man was friendly and sympathetic but I never got the job.

Close to the end of spring 1930 I was without a job but still hoping to get one. About that time Bell Laboratories was still sending interviewers around to the colleges to pick up men for work. I was interviewed and got a job in the laboratories in New York.

ERMENC: What was your academic standing at Cal Tech?

CARLSON: They had a point system. An "A" might count for four points and "B" for three, et cetera. An average student might have something like 200 points for a year's work. I recall I was up in the 300's. I might have been well in the upper quarter of the class. There were a lot of bright fellows at Cal Tech.

ERMENC: What kind of a job did you have at Bell?

CARLSON: I was with a development group which was working on granular carbon for telephone microphones. You know when you speak into a telephone there's a little button in there with granular carbon in-

side. It jiggles around and its resistance varies with sound.

The carbon was prepared from coal from a certain anthracite mine. It was ground and sieved to a certain size and given a heat treatment. My job was mainly the routine testing of that material.

I don't think my personality went over very big with the interviewers and they probably gave me one of the least desirable jobs in the lab at that time. After a year or so on this job I felt it was a blind alley.

About this time some of the men from the Patent Department gave a talk to the staff of the lab on what the Patent Department's function was. This sounded more interesting to me. I thought it would be a chance to get a bird's eye view of everything that was going on in the labs. So I applied for a transfer to the Patent Department. I was transferred to Patent and started working as an assistant to one of the patent attorneys.

ERMENC: Were you the attorney's scientific aid?

CARLSON: No. We were all actually called patent attorneys, although most of us at the beginning were not lawyers. Our job was the preparation and prosecution of patent applications. We did the bulk of the work of the department.

ERMENC: That sounds very unusual to me.

CARLSON: No. It's very usual. I would venture to say that practically every patent attorney starts as an engineer or scientist. In fact, they need that knowledge to be able to handle patent work. Law courses are usually taken later at night school. That is what I did.

ERMENC: When did you start your work in the Patent Department?

CARLSON: Well, I started in the Bell Labs in July, 1930—it must have been sometime in 1931.

ERMENC: How long were you in the Patent Department?

CARLSON: Roughly two years. After 1930 the Bell Laboratories quit hiring additional men. Then, as the Depression got worse and worse, they started laying men off. They laid off 100 or 200 men every week. They cut the working hours to four days a week and cut the pay correspondingly. We were going through very hard times when I was let go in 1933.

ERMENC: Were you doing anything on the outside?

CARLSON: No. You mean in technical things?

ERMENC: Yes. Did you have any other interests?

CARLSON: For the first time in my life I had tried to develop a lit-

tle social life for one thing. At Bell Labs I saw the handwriting on the wall. I realized that I might lose my job any day. In a way I was in more desperate straits than others because my father was still living in California and I had to send him money every month. I was still in debt from college. I knew that if I lost my job it would be pretty tough.

There was another Bell Labs man who had started a little physics-based company. He was attempting to get consulting contracts and also to develop a couple of inventions that one of the men had. The company was mainly on paper at that stage and I was interested in it. We were looking toward getting going and had actually bid on one or two contracts; but that fizzled out.

ERMENC: When your job ended at Bell, what kind of a job were you looking for?

CARLSON: I had to get a job as quickly as possible, so I started walking the streets in New York looking for work. I started on the list of patent attorneys in the telephone book. I started with the A's and made personal calls on every one. Before I got through the A's I had landed a job in a patent law office near Wall Street. I actually received a slightly higher salary than I was getting when I left the laboratories.

I was doing very much the same sort of work that I did in the Bell Laboratories but under quite different conditions. A patent law office doesn't operate quite like a patent department of a corporation. In an office, there's a little more pressure on you and a little more need to please the clients. It was kind of tough. My work was the preparation and prosecution of patent applications.

ERMENC: What was the name of the firm?

CARLSON: It was called Austin and Dix. It's no longer in existence as such. Mr. Dix died and Mr. Austin, I believe, has retired by now. That job lasted a year. It could have lasted longer but, toward the end of the year, Mr. Austin began to get worried because their business was falling off too. He warned me that he might have to let me go. In anticipation of that I started searching for another job. There was only one man in charge of the patent work in the company at the time, since they were just starting up a patent department.

ERMENC: In regular law a lawyer must pass a bar examination. What are the requirements for a patent lawyer?

CARLSON: At the present time one can only call himself a patent attorney if he is a lawyer. He can be a patent agent without having a law degree. I think that's still the case. But at that time one could be regis-

tered as a patent attorney if he had served a clerkship in a patent law office. I had the status of an assistant to a patent attorney. He signed the papers after I did the work.

In 1934 I had completed the necessary years of clerkship. After affidavits to this effect were made out by my employer, I was admitted to practice as a Registered Patent Attorney before the Patent Office. This didn't permit me to practice in court—just before the Patent Office.

ERMENC: Where was the Mallory Company located?

CARLSON: The factory is in Indianapolis, but the executive office and patent department was in New York.

ERMENC: What did the Mallory Company make?

CARLSON: They made electrical and metallurgical products. They have a tie-up with Samuel Ruben who invented the dry electrolytic condenser which was used in early radio sets. I guess it still is. He also invented the magnesium-copper sulfide rectifier, a number of other electrochemical inventions, and, some years later, the Mallory mercury dry cell.

My work in Mallory consisted of the same sort of patent work: the preparation, filing, and prosecuting of patent applications through the Patent Office. I kept that job from 1934 until the end of 1945, when I resigned. At that time I was head of the patent department.

ERMENC: When did you become interested then in better copying methods?

CARLSON: When I was a child I somehow felt drawn to the graphic arts. I know that one of my greatest desires when I was only ten years old was to have a typewriter. One of my aunts gave me a toy typewriter with which you could actually type. It was a rubber stamp affair. You've probably seen them. I had a lot of fun with that. Somehow the graphic arts interested me all through that period.

When I was going to high school I was the janitor of a newspaper office and, of course, I saw and worked around linotype machines in the printing and typesetting departments. I also worked for a small printer who had a little printing press. I ran the press for him. For part of my pay he gave me an old press that he had abandoned. This gave me the idea of publishing a little newspaper for amateur chemists while I was still in high school. That was when I was taking chemistry. I set the type and I printed one or two issues of the paper, but I gave it up after that. I was impressed with the tremendous amount of labor involved in getting something into print. That set me to thinking about easier ways to do that and I got to thinking about duplicating methods.

I started a little inventor's notebook when I was still in high school. I would jot down ideas that would come to me from time to time. They were not very good ideas but at least things that I thought of. Then I sort of forgot about the duplicating field while I was getting my education and getting my jobs.

Around 1935, when I was working for Mallory, we had constant need for extra copies of patent specifications. They were sent to associates in foreign countries, to companies, to inventors, and others. We would need a dozen or more copies of every specification. To make twelve copies with carbon paper is pretty difficult. Often it involved two typing operations. A specification would be typed and then someone would just have to sit down with a typewriter and retype the whole detailed specification of many pages. Then typographical errors would creep in and two girls would have to read them back to each other. It was a very tedious thing.

When we needed copies of drawings we had to send out to a photocopy firm. Their representative would come in, pick up the drawing, take it to their plant, make a copy, bring it back. It might be a wait of half a day or even twenty four hours to get it back. So I recognized a very great need for a machine that could be right in an office, where you could bring a document to it, push it in a slot, push a button, and get a copy out. I set for myself a spare-time project of trying to fill that need.

ERMENC: What date can you put on the inception of this idea? When did you seem to have a pretty firm objective?

CARLSON: 1935.

ERMENC: That was after you got out of college.

CARLSON: That was when I was working for the Mallory Company and recognized this need.

ERMENC: It was in high school when you started jotting down ideas for development. Who did you discuss these ideas with? Who encouraged you to do this sort of thing?

CARLSON: Nobody as I recall. I don't recall discussing them with anybody. I was pretty much by myself.

ERMENC: What would you do after jotting down the idea? How would you follow it up?

CARLSON: I never followed up most of them.

ERMENC: You just put them down? You didn't go to the library to try to find out more your ideas?

CARLSON: Not at that stage. Not when I was in high school. I

might review them from time to time and try to think how they could be made more practical. But, really these were not very good ideas. I just put them down to have them on record. It was almost obvious in most cases that they wouldn't work so I didn't follow them up.

ERMENC: Youthful hopes?

CARLSON: Just something to stimulate my further thinking.

ERMENC: This amateur chemist's paper you described a while back—to whom did you direct this paper? Was it just the high school group, or had you thought of taking in a wider audience than that?

CARLSON: It wasn't directed to any of the local people. Somehow I had got on the mailing list or subscribed to two or three other papers of this kind published in various parts of the United States by some other kids I suppose; so I got the idea of doing that too.

ERMENC: How did you go about trying to develop a copying process?

CARLSON: At first I did as much thinking as I could about the problem. I jotted down my thoughts in my inventor's notebook. But mainly, in the beginning, I started reading. I know I spent many evenings and weekends in the Science and Technology Division of the New York Public Library. I got out everything I could find on printing and duplicating.

It became clear to me quite early that what I wanted was a copying process and not a duplicating process. In other words, I didn't want something requiring a stencil or a master. I wanted a machine in which you could put a letter or a specification or a drawing and have a copy made. That's copying, as distinguished from duplicating.

I thought it would be difficult to think of any chemical method where you could place a treated sheet of some kind against an image and take an image off by some chemical reaction because originals come in various forms: some are written on a typewriter, some are drawn in India ink, others are written in pencil and ink. They have no common solvent. The only thing common with the different inks, pencils and papers is that they reflect light in different ways from the image areas and from the background areas. It was quite clear to me that a new copying process would have to be essentially a photographic process. In other words, light would have to do the job of transmitting the image from the original to the copy. That, of course, led to photography.

I recognized quite early that if conventional photography would have worked for an office copier it would have been done before by the big

companies in the photographic field who certainly would have explored that possibility pretty thoroughly. So I deliberately turned away from the conventional photographic processes and started searching in the library for information about all the different ways in which light will affect matter. I soon came upon photoelectricity and photoconductivity.

ERMENC: How long a time did you spend in coming to this conclusion?

CARLSON: I think that within the first six months I had centered on the idea of trying to use photoelectric effects. Specifically I thought of photoconductivity as being the most practical of those for my use.

ERMENC: This would be about '35?

CARLSON: Yes, about '35 or '36. The first thing I thought of was an electrochemical method. I thought that if a layer of photoconductive material could be placed in contact with a sheet of paper that had been wetted with a chemical, the paper would change color if electricity flowed through the sheet. I thought of making this kind of a sandwich to produce electrochemical images. I made some very crude experiments, to see if passing an electric current through a sheet wetted with certain chemicals would produce a discoloration. I found that it would, but that it didn't make a very good image.

ERMENC: Where did you do these experiments?

CARLSON: In the kitchen of my apartment. I had married.

ERMENC: When were you married?

CARLSON: I was married in 1934 just shortly after I went to work for the Mallory Company. That was my first marriage. I later divorced and re-married.

ERMENC: What was the next step in your development of the electrochemical method?

CARLSON: I realized after very few experiments that it wasn't a very practical method. After thinking about it I realized that it was even less practical than straight photography would be. So I started casting about for some other way to approach this problem. I read everything I could find about phenomena where light could cause electrical effects. I read about sparking and learned, for example, that the breakdown field for sparking was altered by light. I tried to figure a way to apply that.

But the big break came when, in the course of my reading, I encountered a brief article in an electronics magazine about a process invented by a Hungarian inventor and physicist, Paul Selenyi. He had not been working in quite the same field. He had been working in what is

known as facsimile transmission. That is a process for sending pictures over telephone wires or telegraph wires or even radio channels by scanning. In other words, an original might be wrapped around a drum and as the drum rotates, a spot of light scans the image and sends out signals which are picked up at the other end of the line. A transducer there at the receiving end will convert these signals into dots on a sheet of paper to make the facsimile. There had been facsimile systems before Selenyi's which used chemically treated paper. An electric current flowing through it would cause a discoloration; something like the idea I had been working on.

But Selenyi had taken another step. He had developed, essentially, a triode in air. It embodied a heated cathode enclosed in a metal cup which had a small hole in it. Then there was a drum coated with hard rubber or some kind of insulating varnish that rotated very close to that hole. The heated cathode created ions within the metal cup and the metal cup would give an electrical bias which varied with the signal, and that let varying proportions of ions through a little opening. They were deposited on the rotating, insulating drum by a bias field that was applied. So it laid down an electrostatic image on the drum in a scanning pattern. Then, after the image had been scanned, he simply dusted the drum with a fine powder and the image became visible. I got the article in a German technical journal where pictures which Selenyi had produced with his process were shown—that is, facsimile pictures.

ERMENC: Do you remember the name of the journal?

CARLSON: Selenyi wrote several articles. One was in Zeitschrift fur Technischer Physik, or something like that. I think there was another journal. Later I encountered one in Wireless Engineer. I also wrote to him directly and he sent me one or two publications of his. He worked for Tungsram which is I guess the General Electric of Hungary.

ERMENC: Do you know what Selenyi had done finally with this process and what might have limited him in going ahead with it?

CARLSON: He had developed a prototype of a facsimile recorder. I think he was trying to get commercial support for it. He wrote me that he would be interested in my getting him a licensee in this country. That is as much as I know. I don't think he got beyond this stage.

ERMENC: Did he ever build a machine that you know of?

CARLSON: He had a laboratory model and he demonstrated the making of facsimile pictures. They were quite good.

ERMENC: Is he still living?

CARLSON: No, he died a few years ago. Selenyi's work gave me the idea that you could create a visible image from an electrostatic image. Instead of using electric currents to cause a chemical reaction, one could use electricity to lay down an electrostatic charge.

I did a little theoretical reasoning at this point. I figured that photoelectric currents are generally very tiny, small currents, while electrochemical effects, such as I had been trying to use to cause a chemical discoloration of paper required large currents. I reasoned that these two methods were not very compatible. If electrostatics is used I thought a pretty high voltage electrostatic charge could be placed on a surface with very little current. The current for the electrostatic method could then be very small but the charge and essentially the energy control could be quite large. To do this electrochemically you would have to raise the voltage greatly and that would be incompatible with electrochemical effects. For one thing, if you tried to use high voltage, the paper would most likely burn.

At about that point the whole idea sort of fell into place. I conceived of using high voltage. I would use a photoconductive layer that was of high resistance in the dark, essentially an insulting layer which became conductive when exposed to light, and use that in some way to produce an electrostatic image. These ideas fitted together into the xerography concept. I called it "electrophotography" at that time.

ERMENC: What was the date? How did those results come out?

CARLSON: Well, I did my first experiments on the electrochemical method between 1935 and maybe '37. I got the basic concept for electrophotography or xerography in 1937.

ERMENC: You've described your contact with Selenyi and the way the ideas came together. Now when did you actually start to see whether these ideas would work?

CARLSON: I got the basic idea for electrophotography or xerography in 1937. It was so clear to me at the very beginning that it was a wonderful idea. I was convinced, even before testing it, that it was pretty sure to work and that, if it did, it would be a tremendous thing. I immediately began worrying whether some other inventor might not get ahead of me. So my first step to forestall this was to file a patent application. Being a patent attorney that presented no difficulties for me. I simply wrote up my concepts as completely as possible in a patent application and filed it in the Patent Office in the fall of 1937.

Then I started my experiments. However, at that time I had very lit-

tle time and energy available for them. My job in patent work was quite demanding and was becoming more so all of the time. In addition to that, I had started going to New York Law School at night in 1935. Night law school is pretty tough. You have a lot of homework: cases to study, briefs to write, et cetera. During the school year I was pretty busy. Even so, I would steal weekends from studying to work on my idea.

I realized, first of all, that I needed a suitable plate. I knew it had to be a photoconductive insulating material. It was required to be an insulator in the dark, which, on exposure to light became at least slightly conductive, and which would become insulating again as soon as the exposure stopped. One of my principal references in this experimental work was a book by Hughes and DuBridge called *Photoelectric Phenomena*. It had come out in 1933 and was one of the most complete books on photoelectric phenomena that I found. From this book I learned that there were very few materials that fulfilled my requirements. But there were a few. The most promising one seemed to be pure sulfur. Sulfur is an excellent insulator and is also photoconductive.

My first step was to go to a chemical supply house and purchase some chemically pure crystalline sulfur. Then I got some small zinc plates from an art supply store. I attempted to coat the plate with sulfur by melting it onto the plate and spreading it as thin and uniformly as I could. I worked in our kitchen over the gas burner. I put a crystal of sulfur on the zinc plate and held it over the burner and heated it. Then I tipped the plate and spread the sulfur around. I had a few sulfur fires and filled the apartment with sulfur fumes. Eventually I got a crude plate. That was the first step.

Then I realized that I'd have to figure out a way to give the surface of the sulfur an electrostatic charge. I didn't know quite how to do that. I thought that I probably needed a high voltage electrostatic machine of some sort. I didn't have enough money for that so I tried to make an electrostatic generator. I didn't have very good success with that. Then I began work with the powder, to adhere to the electrostatic image. To start with I decided to use the conventional lycopodium powder that's used in physics electrostatic experiments. I realized that it probably wouldn't be suitable in a final process but at least it would serve for a demonstration.

From my first experiments I didn't succeed in producing any images. But at this point I wasn't able to charge the plate properly. I don't know why I didn't try frictional charges by simply rubbing the surfaces because, at the beginning, I had thought of friction as a way to charge. But

I didn't succeed. As time went on, with the little time I had available to work on the idea, it was clear that I wasn't making much progress. During the summer of 1938 I was able to spend more time on work but even so I hadn't yet succeeded in reducing the invention to practice.

In the fall of 1938 I felt that I just had to do this; I had to show somehow that the idea was workable. But I felt that I needed someone more expert, someone more experienced in laboratory work than I was, to give me a little help. About that time I ran across an ad in the magazine *Electronics*, I think, of an unemployed physicist who was in need of a job. He was in New York and I got in touch with him. He turned out to be just the man I needed. His name was Otto Kornei and he was a refugee from Austria. He had come to this country with his wife and small child. He had very little money and he hadn't yet succeeded in getting a job. He was pretty desperate for an income. He hadn't seemed to be able to land a job. This was still in the Depression years and at that time physicists could be unemployed. He was glad to meet me and I was glad to meet him.

I told him what I had in mind: that I had a new invention and that I proposed to get a room somewhere where we could do some experimenting and that I would like him to help me, to work for me. He was not too pleased with the prospect. He really wanted a better job than that. But he was desperate. We finally agreed that he would work for me for six months—not entirely full time—while he spent some of his time searching for a better job.

I rented a small room in Astoria, New York and fitted it out with some modest laboratory equipment. He started work on October 1st. I had shown him what I had done toward making sulfur plates. He carried on from where I had left off. He made better plates than I did and was generally more skillful in the lab than I was. He made some developing powders. He found that it was quite simple to charge the plates by simply rubbing them with a cotton handkerchief or a piece of fur. We found that cotton cloth worked about as well as anything. Within less than a month after he started, on October 22, 1938, we made the first image.

I would visit him on Saturdays, which was the only free time I had, and we would work together in the lab. On that particular Saturday we set up, we polished, and we charged the plate in the dark. It was sufficient just to lower the shade on the window. The room became sufficiently dark because sulfur is not a very sensitive photo material. The plate used was a 2" by 3" zinc plate with a coating of sulfur melted onto it and polished smooth. Mr. Kornei took a glass slide, a microscope

slide, and with India ink he wrote the date "10/22/38 - Astoria" on it.

He rubbed the plate, laid the slide on it, and exposed the combination to a photo flood lamp for 3 or 4 seconds. He then turned the lamp off, took the slide off, sprinkled the plate with lycopodium powder, and then blew the loose powder off. The remaining powder remained sticking in the form of an image.

ERMENC: Was it a pretty sharp image?

CARLSON: Pretty sharp, yes. It was a pretty good image. Later we used dyed lycopodium and transferred the powder to a sheet of waxed paper and then melted it. That fixed the image on the waxed paper. That was the first permanent image.

ERMENC: Once you got the image and demonstrated the principle, I suppose you went ahead to actually make a machine to see how, in fact, you could do these things?

CARLSON: Eventually, but first he continued trying to make better plates. We tried another material, anthracene, that was known to be a photo-conductor. He developed a method of coating plates with anthracene. Anthracene is a little difficult to work with because if you heat it in the open air it evaporates. Kornei found that by using an evaporating dish and suspending a plate over the top of the dish on a cover and heating it, the anthracene would evaporate from the bottom of the dish and condense and form a layer on the plate. He made some good anthracene plates which were photographically faster than the sulfur. We switched to anthracene after that.

We tried different powders. We got hold of every kind of fusible resin that we could get, such as copal, dragons blood, gum damar, and fusible resins. We mixed them with carbon and with dyes and melted them. Then we ground them up into very fine powders. So we tried to develop a good developing powder. We tried to improve every step in the process. We also experimented with the making of printing plates. We transferred a resin image to a sheet of zinc and fused it on the zinc. We then etched the plate in acid and got a raised type image that way. We also transferred images to offset plates, both the grained zinc plates and aluminum, and also to the paper Multilith master plates. We found this made a good lithographic image for offset printing. We did various things of this nature. Once in a while we would get a wild idea and try it out.

At the end of six months we had the process pretty much set as it was first used in practice, except that we had found anthracene to be better

than sulfur. We had also improved the developing powder. All in all we had learned quite a bit.

Kornei made a little demonstration kit for me to use, to try to interest somebody in supporting the work from then on. At the end of six months he left me. He had located a better job. I was alone again; that would be early 1939. I graduated from law school that spring, I believe. I devoted most of my spare time to try to improve the process still further and to discover other materials which would work. I also tried to interest some company in taking it over. I filed an improved patent application as well as one or two other patent applications on other ideas I had, which were offshoots of the basic idea. After talking to a number of people in industry, I found that my little crude demonstration did not impress them. A technical person could usually understand it but few of them saw the potential in it. Business men were not very impressed with it. It was hard to find anyone who could visualize what could be done toward the engineering development of the process.

ERMENC: What particular areas of business did these people represent? Were they people in the copying business? In the photographic business?

CARLSON: There was no copying business at that time outside of that for Photostat and photocopying. But these were not office copying processes. They were usually done in a plant or in a dark room that was set up, and not in offices as such. I didn't approach these people. I did contact the people in the business machine, and office equipment business, and a number of people in other technical fields who might have been interested in my development. I also talked to people in instrument companies and to the people in one company which was in the photographic field. My main contacts, though, were in the business machine field. Nothing came of these contacts.

ERMENC: Would you tell me what their reactions were?

CARLSON: Some seemed interested but never quite to a commitment. In some cases, after an investigation, they told me they weren't interested. They usually didn't say that the process was of questionable value. They usually said, "we just don't have the facilities or the funds to proceed with it," or "we're too busy with other things." Sometimes they just kept putting it off, not definitely saying yes or not definitely saying no.

ERMENC: Did you have to give them an estimate as to the amount of money you thought would be necessary to develop it to a commercial

process?

CARLSON: I don't recall doing so in dollars and cents. That would have been difficult to foresee at that stage. At any rate, I thought that a more spectacular demonstration was needed. I thought that the best thing to do to give a businessman an idea of the potential of the process by making a working machine, a machine in which you could actually put a sheet, push a button, and get a copy. This was something that had to be done anyway, and now I thought that I would have to do this job also.

I set about making a drawing of my conception of what such a machine would look like, what it would be like mechanically, based on what we had learned in the laboratory from the manual experiments. I took it to a model making firm of machinists in Brooklyn who had some experience in making inventor's models. It was called The Precise Instrument Company. A couple of old-time European machinists had built it up to quite a big business. They must have had thirty or forty men working for them. I think they gave the job to the youngest apprentice in the shop. He did his best, but after the machine was completed, it didn't work.

ERMENC: How much did it cost?

CARLSON: I've forgotten the exact figure but it was between $500 and $1,000 I think. That was a lot of money for me and the country was still in the Depression years. It was quite an expensive thing and I was left with the model which didn't work.

I took my model to a little one-man model shop in lower Manhattan. It was called the Trinity Model Shop, and the operator's name was Milligan. He was an old-time machinist, a very skillful man, and he grasped the problems right away. He practically rebuilt the machine and made it much better. It was still the same general machine, but he improved it. After he finished his part, I put some developing powder in it, got everything else all set, put a letter in it, or a piece of paper with some drawings on it, turned it on and got an image on the drum. We hadn't yet completed the transfer and fusing section. I think we had the transfer section working, but not the fuser, so I didn't get a permanent image. I got the image where I could see it. It was quite impressive to see it turn up in just one rotation of that drum. That took about two seconds. I was quite delighted.

After making a few more experiments with the machine it quickly broke down. The mercury switch blew up and the developing chamber didn't work very well. I couldn't seem to get it going again. It turned out that the machinist needed to do a little more work on it. Just at that

time we were getting involved in war work, the machinist got himself a good fulltime job and couldn't help me anymore. I was again left with the machine in an unworkable condition, and no chance of getting any mechanical help on it because the war was taking men into more important and urgent work. However, having seen that it worked, I filed a patent application on the machine. I went back to demonstrating with my little manual plate during the war years when I approached a few other companies for support.

ERMENC: Do you have any pictures of your machine?

CARLSON: I don't have early pictures. I have some pictures of my model as it now stands. The model is the original model except for the light housing, which was lost. I had another one made that was practically identical to it. I have some pictures of myself going through the steps of the original manual process that were made just for the record years later.

ERMENC: This patent on the machine was simply on the machine? You had patented the idea earlier?

CARLSON: Yes, that's right. These were both patent applications at that stage. Actually I had filed on a related idea before the main electrophotography patent. That wasn't very practical. I was issued a patent for this in 1940. When it was issued, a market research man from a large business machine company saw the write up of it in the New York Times. He called me on the phone and said that he thought his company would be interested in it or in anything else I had along that line. He arranged an interview with his boss who was the head of the market research group. It turned out that his boss wasn't as enthusiastic as he was. We had a few meetings and he arranged for me to give a demonstration to some of their technical men. I gave one using my crude plate again. They witnessed the demonstration, but didn't show any particular enthusiasm. In fact, I don't think any of the technical men said a word during the demonstration. They just saw it and left.

My talks afterward were solely with the head of the market research group. He said that he would let me know when they reached a conclusion. Then he kept putting me off for maybe a year or so after that. I finally realized that they weren't going to do anything. In fact, I think he told me that they were too involved in war work to take on my development.

ERMENC: What year was this?

CARLSON: It was during the war. The first contact was made in

1940 when my first patent was issued. Our contact continued for perhaps a couple of years.

ERMENC: Do you remember the date when the New York Times described your patent?

CARLSON: I have the clipping. I believe it was in October. It was the week after the patent was issued. I can't recall the date but I could give it to you if you need it.

ERMENC: You were describing your meetings with the market research executive.

CARLSON: They practically told me that they weren't in a position to do anything with it at that time. I don't recall that they gave me a complete turndown, but they postponed it in such a way that I didn't expect much from them.

ERMENC: Did you ever hear what the technical men had thought of it?

CARLSON: I talked with one of the men years later who was a physicist. He said that he had witnessed the demonstration and was somewhat impressed with it. He thought it was quite an interesting thing. As a matter of fact, the company didn't have much of a basic science department at that time. They were mainly machine makers and designers. I sometimes wonder whether they could have done much for me. I wonder if they had the laboratory facilities to carry on the basic development that was needed at that stage. They do now, but I don't know whether they did then.

ERMENC: Following this abortive meeting, what was the next step? You went to other companies?

CARLSON: Well, I had contact with a number of other companies and individuals who might have been interested, but no offers came from any of them.

ERMENC: Did any of them encourage you to go ahead on your own even though they couldn't do anything for you? Or were they all more or less noncommittal?

CARLSON: Some technical people seemed to be excited. In general there was postponement because of war activities. I began to feel that I had to wait out the war before I could do anything. My basic xerography patent was issued in 1942. That didn't create any great flurry of interest. Finally the machine patent was issued in 1944.

In early 1944, in the course of my patent work, I had some meetings with an engineer from Battelle Memorial Institute. He was being con-

sulted as an expert witness to help the Mallory Company with some patent applications they had on a bearing material. We were having difficulty getting a patent issued. The patent office didn't think there was any invention in it and we were considering a court appeal to get a patent issued. I had some meetings with the Battelle expert to discuss the problem and what he could do for us as a witness.

He was an interesting fellow, interested in new ideas. I showed him a copy of my basic patent and told him about the idea. He seemed interested and took a copy back with him to Battelle where he showed it to some of his associates.

The next time I saw him was at the hearing we had in Washington before the court on this bearing alloy. There were several of us down there: another patent lawyer, an engineer, this young man from Battelle, and I. In the evening before the hearing, we met in the hotel room to discuss our plans for the coming day. After we had gone over the plans, Russ Dayton, the Battelle expert, asked me if I would care to take a walk with him and I agreed. He told me that he had showed my patent to his associates at Battelle. They were interested in doing something with it. Shortly after that I did visit Battelle. I talked with some of their scientific and technical people as well as their business people. I gave them a demonstration, explained the physical principles involved, and some of my thoughts about its commercial prospects. They were interested and offered to enter into a royalty sharing agreement with me. They would put up some of their own money for research in their own laboratories to improve the invention. They were not committed to doing anything beyond what they felt was worth doing. This satisfied me because they were doing something. We entered into an agreement in August 1944.

At that time, Batelle was starting a new department to do graphic arts research. They had gotten a Ph.D., Dr. Schaffert, who was also a printing man, to head this department. He was assigned to the job of developing my invention. The development work was begun as a small effort in late 1944. Not much was done until the beginning of 1945. From then on, the number of man hours spent grew from month to month. They repeated my experiments and established that they were what I said they were. They went on from there and made improvements.

ERMENC: Do you know how Battelle came to decide to develop your invention? Was it an unanimous decision, or was there some division of opinion?

CARLSON: There are two companies. There is Battelle Memorial Institute, which is a research organization. They had also set up a little

company with no organization. It was just a vest pocket company with a small amount of capital to sponsor new ideas. They were looking for new ideas to sponsor. Their scientists were readily able to understand what I was talking about. This was not always true of the men from the industry with whom I talked.

I think it must have looked promising to them because they worked out a favorable contract with me. It put them under almost no risk except for the small amount that they were to spend in their own labs. This was the reason that they felt it was worth going ahead. I think the director of Battelle probably made the ultimate decision, but I think it was probably largely based on the advice of the technical people, and probably with the approval of the whole board.

ERMENC: Do you care to talk about the terms of the agreement with Battelle?

CARLSON: The first agreement was essentially an agency agreement. I appointed them as the exclusive agent for my patents and inventions in this field and they promised to put into the package any inventions they made. They promised to use diligence to obtain licenses. They agreed to do some research, although there was no dollar value put on the amount of effort they were to put into research. We were to share any return on royalties that would come out of the commercial development on a sixty-forty basis, sixty percent for Battelle and forty percent for me. However, if they were to spend anything above $10,000 on development, my percentage was to drop 1% for every $1,000 over $10,000 that they spent, until my percentage was reduced to 25%. It was not to go below 25%. However, I had the option of contributing 50% of their development cost above $10,000 to restore my original percentage.

ERMENC: Was this considered a pretty fair agreement in your opinion as a patent lawyer?

CARLSON: Considering the desperate straits I was in and the relative strengths of the two parties, I think it was a pretty fair agreement.

ERMENC: At what point did you relinquish active control of the process?

CARLSON: I relinquished active control in 1944 with my first Battelle agreement. I later modified my agreement with Battelle in 1947, I think, in order to give them an exclusive license. The main purpose of that was to help my tax situation. Royalties under an exclusive license are considered capital gain rather than income. Before that it was an agency agreement. I suppose I gave Battelle a little more in 1947 than I

did in 1944, but it didn't really alter our practical working arrangement at all.

ERMENC: Did the working arrangement that you had established at that time remain the same?

CARLSON: Yes. In other words, I relinquished complete control to them. I was simply to receive my share when they collected anything.

ERMENC: Were you obligated to act as consultant to them?

CARLSON: No. I was obligated to assign any future inventions in this field to them, which I have been doing. They have been turning them over to Haloid or Xerox.

ERMENC: What sort of inventions have you turned over to them?

CARLSON: Improvements in xerography.

ERMENC: Could you say in what areas?

CARLSON: Some charging devices. A rather basic patent on corona charging. Quite a number of ideas that haven't actually been put into use. They have been patented, but there are other better ways of doing them or, for one reason or another, they haven't been adopted. I also have a patent on a composition of developing powder which is quite basic.

I developed some special carrier coatings which have been of great use at Xerox. We have not patented them because that would tell the competitors what to do to get around them. I think anyone who would read about what we have done could probably, by doing a little chemical work, find another compound that would work nearly as well and could avoid a patent that way. So we've kept that part secret.

ERMENC: What improvements did Battelle make on the machine and process?

CARLSON: The sequence of process steps remained as they were originally. The essential steps in Xerox equipment, that is, charging, exposure, development, transfer, and fixing, also known as fusing, have been the same throughout and are still the same today.

The individual steps have all been improved and refined. That's where most of the development work went. A great deal went into the improvement of the plate itself. Batelle started with the materials I had used, that is, sulfur and anthracene. They worked principally with anthracene at first. Then somewhere in the period between 1945-1948 they discovered the photoconductive properties of vitreous selenium or amorphous selenium. That was a tremendous advance because selenium is much more photosensitive than anthracene and the plates yielded higher

quality images. That was one big advance.

The other field where improvement was badly needed was in the development process. I had simply used pigmented resin powder which was sprinkled on and blown off, or carried to the plate in a dust cloud. While that produced images, it also produced some powder deposits and showed up as a gray background. Battelle worked hard to improve the quality of the development. They made a discovery that was very important and is still used in all xerographic machines. That was the combination of powder and carrier. Instead of using a fine pigmented powder alone for development, they mixed the powder with tiny glass beads or round sand particles which had resin coatings on them of a different composition from the powder itself. The powder stuck to the beads electrostatically, and when the beads rolled over the plate they would drop powder on the image but not on the background area. This provided a well-developed image and a clean background. That was called "carrier development" or "cascade development"; it was a big advance.

Another improvement was in the transfer step. I had simply transferred images by either using wax paper, or moistening a sheet of paper with water, or humidifying it in a humidity chamber, and rolling that in pressure contact with the powder image. The image was peeled off by adhesion. Dr. Schaffert invented the electrostatic transfer method in which a sheet of dry paper is laid over the image and an electrostatic charge is sprayed onto the back of the sheet by corona discharge. That causes the sheet to pull the powder away from the plate and when you peel the sheet off, you peel it off with the powder image on it.

Another improvement they made was in charging. I had done most or essentially all of my charging by friction methods: rubbing with a handkerchief, for example, or with a fur brush. In my machine it was done with a plush belt. I had recognized that it might be possible to use corona discharges to charge the plate. I had started some experimenting along that line, but hadn't gotten very far at the time. I signed up with Battelle. I suggested that they work in that field. They did, and they developed an efficient corona charging unit which not only improved the charging step but also made possible the electrostatic transfer step, which Schaffert invented. These were the major improvements that I can think of that Battelle made in those early years.

ERMENC: When you made your presentation before Battelle, what did you see at that time as the prospects for electrophotography?

CARLSON: Primarily the first and most important application that I visualized was an office copying machine. But I also recognized that

here was a basically new photographic process that might eventually be applicable in every field where photography was used. There were also fields, where because of its special properties, the process could do something that photography, as then known, couldn't do.

ERMENC: And what were they?

CARLSON: For instance, in the graphic arts it was possible to produce an offset master directly from the powder image. Also, it was possible to make a resist for etching and printing plates or for designs. Of course, you can do that by a modified photographic process too. It becomes possible to transfer a powder image to any surface eventually. You could put an image on any object much easier than you could by photography. In photography you usually end up with an image on a sheet of photographic paper or film. But with xerography you could put the image on fabric directly, or put it on a manufactured product for cutting.

ERMENC: Can you estimate the cost of development to you?

CARLSON: Well, in cash, not very much. I would guess not over two or three thousand dollars up until the time Battelle took it over. If you count my time, I imagine I might have spent 1,000 to 2,000 hours in actual work in the lab, on the patent preparation and on promotion. And, of course, I spent a good many sleepless nights and odd times just thinking about it.

ERMENC: How long did Battelle work on this?

CARLSON: They started in late 1944 and are still working on it.

ERMENC: How does Haloid come into this?

CARLSON: Haloid approached me and I referred them to Battelle.

ERMENC: How did they find out about it?

CARLSON: I think it was early in 1944, before I had tied up with Battelle, that a man in New York named Nicholas Langer, who was a patent agent, had been mustered out of the Army Signal Corps. He was not working at a regular job but he was making some money by writing technical articles for popular journals. I think he was writing for Radio News magazine. Somewhere he had run across my patent and became interested enough to get in touch with me. He said that he had read my patent and found it very interesting and that he would like to write an article about it. I gave him the information he needed to write the article. It was published in *Radio News*, in a special section called Radio Electronic Engineering. This was a special section for technical people. It wasn't included in the newsstand edition. It was sent only to the sub-

scribers. I think this was the August 1944 issue of *Radio News*.

Eastman Kodak had an abstracting service. They abstracted everything published anywhere on photography, or related to photography, in a monthly bulletin and had included the *Radio News* article. Dr. John Dessauer, the Research Director of Haloid Company, saw the abstract. He got the original article and read it.

Haloid at that time had two main lines of business. They made photographic paper which was used by photofinishers to make conventional drugstore prints—black and white photographic prints. They also had a Rectigraph division, in which they made a photocopying machine much like the Photostat machine. In fact, the Rectigraph preceded the Photostat.

Haloid started up in 1906, I think. It was a small company in the highly competitive photographic field in which the advantage often lay with the big companies. They recognized this and, though they managed by hook or by crook to get their share of the business up to that point, they knew they had an uncertain future. About that time also, Joseph Wilson, the present president of Xerox, had become president of Haloid. He had succeeded his father who was president in the war years. About 1945 or 1946 Wilson became president. He was a very brilliant young man. He had come up through the company—through the sales department—after having Harvard business school training. He was an aggressive, young, imaginative person.

Dessauer and Wilson decided to search for something new for Haloid to do. When Dessauer saw this write-up on my invention he called it to Wilson's attention. They decided to get in touch with me to find out whether there was anything to my invention. They got in touch with me, indirectly, through a small laboratory in New York, a little two-man laboratory called Microtronics. This laboratory developed micro-film equipment and had done a little work for Haloid. Haloid felt that it would be best not to get their name into the picture in the early stages of investigation of my invention. Cameron at Microtronics wrote to me that Microtronics was interested in exploring new photographic processes and wanted to learn more about my invention. I met with them and explained it. I also told them that my invention was in the hands of Battelle and that they should go out to Columbus, Ohio, to see what Battelle was doing. So Cameron and Taubes of Microtronics went to Battelle and looked at it. They reported to Haloid that Battelle was doing some interesting work on the invention. Then Dessauer and Wilson went out to Battelle and became sufficiently interested to negotiate their first license

from Battelle. They took the license and started sponsoring work at Battelle in January, 1947.

ERMENC: What were the licensing terms?

CARLSON: As I recall they made a down payment of something like $10,000 and agreed to minimum annual royalty and a percentage royalty on sales. It was quite a high percentage: it seems to me that it was 8 or 10%. At least 8%, anyway.

ERMENC: Are they the sole licensee of Battelle?

CARLSON: They are. The first license was a limited license. As I recall, either it was non-exclusive, or it was exclusive only in certain areas and certain areas were held back by Battelle for possible license to others. But then Haloid renegotiated its license two or three times. I think their present agreement with Battelle is either the third or the fourth. The last agreement called for Battelle to assign all of the patents to them including mine and Battelle's. A final settlement was arranged in which Haloid turned over some of its stock to Battelle in 1956 and agreed to royalty payments of stock and cash yearly until 1966. Next year will be the final payment. After that Haloid (now Xerox) will have all of the rights and no further obligations.

ERMENC: Were these agreements subject to your approval?

CARLSON: Actually not. My agreement with Battelle gave them the exclusive right to decide.

ERMENC: What has Haloid contributed towards the development?

CARLSON: At first primarily money. They started sponsoring development work at Battelle in 1947. Battelle continued putting some of its own money into the development for a few years after that. Haloid's share of the development cost increased from year to year and became quite substantial. As I recall, in the second year it was about $100,000, and more each year after that.

Haloid didn't do any work in its own laboratory until 1948, and then it was primarily in machine design and development. About 1949 or '50 they did start a physics department and began trying to improve the process at Rochester. It's been growing from year to year. Battelle's own investment has dropped off. I doubt if Battelle now spends any of their own money on xerographic development.

ERMENC: Were any of the investigations you made of any hindrance to your development?

CARLSON: No, I don't think so. No hindrances that I can recall.

ERMENC: I mentioned that some inventors, like von Ohain in his

development of the turbojet in Germany, felt that literature surveys which are done in a field too soon inhibit the development of fresh ideas. How do you feel about that in retrospect? Do you think if you hadn't gone through a survey that you might have come upon this idea earlier?

CARLSON: I don't think it hindered in my case. If anything it helped in pointing out the need for a better way to do it.

ERMENC: In other words anything you did was part of the learning process.

CARLSON: Yes, it was background information that I needed.

ERMENC: Would you say then that you started with technology, that you applied technology, and ended up with technology? Or do you feel that you also contributed toward knowledge of certain phenomenon?

CARLSON: That is probably true, but I wouldn't say it that way. I certainly added no new scientific knowledge as did the transistor inventors, for example. I merely combined a set of facts in a new way.

ERMENC: Did any person or persons offer any impediments to your development?

CARLSON: No, I can't think of any one person whom I would consider a hindrance. There were many hindrances, but they were basic to the situation and didn't involve any specific people.

ERMENC: It was really the neutrality of a lot of these people who didn't want to commit themselves.

CARLSON: Yes, that was true.

ERMENC: How did the name Xerox come to be chosen?

CARLSON: I used the term "electrophotography". In 1948 when Haloid and Battelle were preparing for the first public announcement of the process by means of a presentation and lecture at the Annual Meeting of the Optical Society of America, which took place on October 22, 1948, the Haloid people decided that it needed another name.

To my mind, I don't like it better. Perhaps it has fulfilled a need, more of a commercial need than a technical need. I think the original name is more technically accurate and fits better in technical parlance. I think it was changed largely at the instigation of the Haloid advertising people. They got in touch with a professor at Ohio State University, a professor of languages who came up with this term "xerography" meaning "dry writing" in Greek. Xerox is simply a contraction—a coined word—based on that.

ERMENC: What mistakes were made during the development and

what was learned from them?

CARLSON: I don't think very many mistakes were made, at least no major ones. We were working in a new pioneering field. We had to do much by trial and error but that was the normal thing. I wouldn't consider these mistakes. It is just the way things work; a way of doing things; the way knowledge is developed.

ERMENC: How do you think your development could have been speeded up? More assistance? More money?

CARLSON: Well, I think, given enough money and the adequate technical personnel which, of course, has been hard to find in recent years, it could have been speeded up considerably. Under the circumstances, I think Haloid did all it could with its limited resources. As a matter of fact, they really went out on a limb. They were quite a small company to undertake such a big development.

ERMENC: Would you agree with the statement that given the time, the money, and the drive almost all projects can succeed?

CARLSON: I would say that it's hard to agree completely with so broad a statement. It probably expresses a widely held American philosophy. It would be true of some, or many, but possibly not all. It's not a statement that can be proven. It's more a philosophy or point of view. It reflects optimism.

ERMENC: Do you think that the factors of luck, opportunity and personal idiosyncrasies might be equally important?

CARLSON: I think perhaps they are equally important, but I think they should be coupled with the factors mentioned in the previous question.

ERMENC: Some people in government are suggesting that we should set up something like a national foundation for inventors. Then an inventor who has a good idea could apply to this governmental foundation for support for a year or two years which would include his living expenses plus funds to complete models, hire assistants, et cetera. Something like that might have sped up your invention. How would you have looked on that if that opportunity had been available? Do you wish you could have had such an opportunity?

CARLSON: We now have people in colleges who, if they have an idea for an investigation in science let's say, they will apply to the government and will get funds. Funds for equipment, for machinists that they need, technicians and for research assistants to help them carry on this task. The magnitude of these grants can amount to as much as

$40,000 or $50,000 per year.

ERMENC: If such an opportunity was available to you for the development of your invention, do you think this would have changed the course of your invention considerably?

CARLSON: Well, I think I might have accomplished something in the development stage. I don't think it would have helped before I made the invention. In fact, I think I needed the motivation of, well, partly of poverty, and of need. I think if it was made too easy for me, I probably would have not done as well or not worked as hard on it. But once I had the idea and the original reduction to practice, it might have helped a little in getting from that stage to a good working model.

Of course, there was another factor at that time. Mechanical help was hard to get during the war so, money or no money, I might have had difficulty. But assuming that mechanical help was available, some money could possibly have been used in speeding up the reduction to practice. However, there was one thing of which I was not fully aware at the time. That was the extent of refinements that were probably necessary in order to make the thing commercially practical. Of those things Battelle and Haloid knew or learned in the process of development. I was probably too optimistic in thinking that having the idea well in mind, all I needed to do was to build a good model, make a few minor improvements and have it ready to market. This wasn't the case.

ERMENC: During your development did other ideas come to you which you pigeonholed for future consideration? Maybe some of these were ideas outside of your field? Were any of these in your little notebook?

CARLSON: Yes. I recorded ideas in various fields from time to time, but I never thought very highly of any of them. I haven't felt any of them to be worthy of further effort. There is only one possible exception. In the 1930's, at about the same time as I was working on xerography, I also thought of the idea of a ball point pen and I made some experiments along that line. I decided after my experiments, that it would never work well enough to be worthwhile, so I gave it up.

ERMENC: You didn't think of taking a patent out on it at that time? You hadn't progressed to that point?

CARLSON: I think I made a patent search and found that the basic idea was fifty years old. I hadn't gone much beyond the original inventors. I didn't have much to patent if I had tried to.

ERMENC: Did you have any other evidence of what experts

thought about your development?

CARLSON: Of course, there were people who assumed the role of experts improperly. For example, businessmen who attempted to decide technical questions which they weren't competent to decide.

In one case, I visited the head of a research organization which was the kind of an organization that sponsors new developments. He took the invention under advisement and did present it to some of his technical men. I don't know what they told him, but later he showed me some photographic work that somebody else had brought in. It was beautiful photography but it was conventional photography with a little new twist to it. They had added something, but essentially it was photography with a very minor improvement. "This is something you should be doing instead of working on your silly thing," he said, or something to that effect.

In another instance I wrote to the head of the photographic division of a business office equipment company. He sent one of their salesmen to see me who was an old-time Photostat salesman. After my demonstration of the process to him and telling him what I thought it would do he blamed me for wasting his time and was very scornful of the whole thing.

Another man, who was responsible for many developments and new products for a large photographic manufacturer, turned down the process at Battelle. Battelle was in contact with them, not me. The photographic manufacturer was not interested in anything that didn't sell film.

ERMENC: Do you know of anyone who was working on a similar copying device either in the United States or abroad?

CARLSON: No.

ERMENC: Has anyone come forth since your success with a different copying device?

CARLSON: RCA, which developed a similar but slightly different process called Electrofax. It's my basic invention with one change. They use a new photoconductor, zinc oxide. This is a white material that can be coated onto paper in a binder. In that way, they are able to charge, expose, and develop the image, and fuse it on the paper. There's no transfer step. A series of competitors have come out with Electrofax machines using the RCA principle.

I had previously had the idea of coating paper with a photoconductor and this I showed in my patents. Battelle had worked on zinc oxide as well as a number of other materials in binders as photoconductive and xerographic plates. Now Xerox, through assignment by Battelle, really

owns this basic patent which dominates the zinc oxide paper business. All of the RCA licensees have to pay royalties to Xerox.

ERMENC: Have any other alternatives been considered by Battelle or Haloid that you know of?

CARLSON: Many paper ideas and laboratory ideas that have come up through the years, both in Battelle and Haloid and also outside. Some of them may have special applications and may eventually be put into use. Others, probably in most instances, would be rejected as being not as good as what we have.

ERMENC: How long does your patent hold?

CARLSON: My basic process patent was issued in 1942. Since a patent lasts 17 years my process patent expired in 1959. My machine patent was issued in 1944 and expired in 1961. So my basic patents have expired; but I've taken out 30 or 40 from the beginning up to the present time and except for the first four I think they are all still in force.

ERMENC: Were there any times when it looked like a dead end was reached in your development work? What resolved the impasse?

CARLSON: Not exactly a dead end, but there were times when I gave up trying to sell the idea and just put it on the shelf. Then I'd get optimistic again and continue working, both experimentally and promotionally. There was one time when Battlelle had succeeded in making one good copy but couldn't duplicate their results for a long time. Haloid became rather restive and uncertain about continuing their license. But they stuck with it and further work eventually resolved the problem.

ERMENC: Do you remember what the problem was?

CARLSON: I don't recall the exact details. I think part of it revolved around one man who was very skillful in the laboratory and could do things that other technicians couldn't repeat.

ERMENC: You became familiar with other fields of technology through your own research in the library and through other areas of information, is that correct?

CARLSON: Yes. The principal technological advance from other fields I think was Selenyi's demonstration of electrostatic powder images. This for me was the missing link, the needed idea. Of course, we all drew on accumulated technical knowledge. I can think of no other specific new development that especially contributed to our development.

ERMENC: Can you recall any intuitive solutions to development problems by you or by others in connection with the development of

your process?

CARLSON: I would say that most of our improvement inventions involved some intuitive factor. I think most inventions involve an intuitive factor.

ERMENC: You find it kind of hard to put your finger on it do you?

CARLSON: Yes.

ERMENC: Especially when you worked on it intermittently?

CARLSON: In my first work leading up to the basic idea it seemed to me pretty much like a logical reasoning process. I didn't feel the intuitive factor being especially prominent. Probably it was there even though I wasn't aware of it. Some of the improvements, which were later made, came as a result of some accidental observation in the laboratory. That wouldn't be intuitive—it would just be a matter of discovering something and putting it to use.

ERMENC: What appeared to be the weakest link in the development? How was this minimized?

CARLSON: As to the part of the process that required the most work, I would say it would be the development of the image with powder. There are many ways you can apply powder to a surface for development and most of them have background difficulties, don't develop enough intensity, or the image is fuzzy. That probably required the most effort of any step in the process.

ERMENC: You mentioned some applications that you foresaw. Have there been any applications which you have not foreseen?

CARLSON: The field of x-ray xerography—xeroradiography, we call it. I saw it in my early days as a possibility, although I didn't have the equipment to try it. With the plates I had, it probably wouldn't have worked anyway. That was first demonstrated by Battelle with the selenium plates they developed.

I didn't foresee the Electrofax application, where the photoconductor is applied to the paper and the final print still is on the photoconductor to eliminate the transfer. While I realized that a copy could be made on coated paper, I didn't know of any good photoconductor that would be both good and cheap enough so that it needn't be used again. I figured that probably the photoconductor would be an expensive element and that the only way to use it economically would be in a reusable process. This is the way Xerox uses it. Then RCA discovered zinc oxide that provided a cheap coating which is commercially competitive to some extent. I still think it's the second best way to do copying. I think our

way is better.

ERMENC: Do you have anything to add in describing the period in which this was an individual development and then a team development? Could you say that, from the time Battelle took it over, it became a team development?

CARLSON: I would say mine was an individual development from 1935-1944 except for the six months when I hired Kornei to work for me in 1938-39. Battelle worked practically alone with very little assistance from me from 1944 to January, 1947. Then it was Battelle and Haloid in partnership. From 1948 on, I was a consultant to Haloid.

ERMENC: Can you point out instances in your development where experience was important in solving difficulties? You've mentioned this as a pioneering development. Still I ask this question because, in some cases, pre-conceived ideas by people with a lot of experience seem somehow to put blocks before a development. Yet there are some cases where people of experience, who don't have pre-conceived ideas of how something should be done, can be of great help.

CARLSON: In my personal development, before Battelle, I had very little lab experience in development work. For most of my life after college, I had been a patent attorney. Kornei's lab experience helped a lot in that crucial stage of reducing the invention to practice the first time.

ERMENC: Were there any instances where inexperience was more important than experience? In other words, were there any instances where you went ahead in ignorance and found out a great deal?

CARLSON: I can't think of any such instance.

ERMENC: To what extent did theory and empiricism guide your development?

CARLSON: I would say about fifty-fifty.

ERMENC: Some people hold the opinion that there is no such thing as pure invention—that everything is adaptation. What is your opinion?

CARLSON: Well, of course one must build on what went before, and adapt or adopt as much as he can to solve specific problems. But I think, in the case of xerography, the concept was new and I think that could be true of other cases too.

ERMENC: Did the Xerox development stimulate research for new knowledge? What parts of the process are not completely understood either qualitatively or quantitatively?

CARLSON: In the early stages of my work with Battelle and Ha-

loid, it was largely empirical. We were trying to solve immediate problems and it continued that way pretty much until about 1960. At present, there is a considerable amount of fundamental research going on at Xerox which is leading to new scientific knowledge relating to materials and to the process steps. I would say that no parts of the process are completely understood but the areas where least understanding would be is in 'solid state' knowledge relative to the Xerox plate, and in the physics of development.

ERMENC: Some people believe that technological development is inevitable—a result of an accumulation of scientific and technological knowledge. Do you think Xerox was an inevitable development?

CARLSON: I feel it was a badly needed development at the time, but I know of no reason to think that others would have invented it soon after I did.

ERMENC: Do you think your particular background channeled you into this development?

CARLSON: Probably it did.

ERMENC: How secret was this development?

CARLSON: I kept fairly secret about it until my patents were issued, but my patents told practically all I knew at the time they issued. I tried to make them very complete. As Battelle worked on it, they kept their development pretty quiet for the first two or three years, although they did release a little tantalizing information to some of the graphic arts journals. But, in general, they didn't say exactly what they were doing or what they had. A public announcement was made in 1948, jointly by Haloid and Battelle, which pretty well told what had been done up to that time. Some trade secrets were not revealed. Of course, since then there has been developed a lot of technical know-how that's not widely spread around. But in general the essentials of the process and the machine are pretty well known. We just issued a book this past summer on xerography, written by the staffs of Xerox and Battelle.

ERMENC: Who published it?

CARLSON: The Focal Press in London; a branch of Pitman.

ERMENC: What's the title of it?

CARLSON: *Xerography and Related Processes.*

ERMENC: Did you have anything to do with that?

CARLSON: I wrote a chapter on the history of electrostatic recording.

ERMENC: Did you think Xerox would revolutionize the copying business as it has?

CARLSON: When I made the invention there was no copying business—there was no such thing as an office copier then. The only process known was Photostat or photocopying.

ERMENC: Mimeographing?

CARLSON: That's duplicating. That's not copying. You have to make a master. You must have a stencil. Copying is where you have an original like this and take it to your machine and zip you get one copy.

ERMENC: Yet this has largely replaced mimeographing, hasn't it?

CARLSON: Mimeographing has been replaced due to a combination of processes, like Xerox wedded to Multilith or something like that. Xerox copying itself doesn't meet quite the same needs as mimeographing. It's only when it's wedded to another process that it bypasses mimeographing. You're familiar with University Microfilms, aren't you?

ERMENC: We don't use microfilms very much yet.

CARLSON: This is a company that puts out-of-print books on microfilm. You can order a full size copy made by xerography from the microfilm, bound and ready to use. Recently they brought out Lewis Carroll's transcript of *Alice's Adventures Under Ground* in a bound volume, done by xerography and offset.

ERMENC: What parts have chance or luck played in your development?

CARLSON: Well, that's kind of hard to say. Of course, every meeting or contact or idea has a chance element. Outside of that, I can't think of any outstanding instance of it.

ERMENC: What parts have personality played in this development?

CARLSON: I would say perhaps considerable in my case. I think I've always had a somewhat different approach to things from most people. Perhaps I'm less bound by custom or the status quo than most.

ERMENC: So you think this was one of the motives that led you on a different track? The fact there was no such thing as copying at that time.

CARLSON: Yes, I think so. It allowed me to think outside of the conventional fields.

ERMENC: Did you actually make this distinction early—that you were actually after a copying device as opposed to duplicating?

CARLSON: Yes, it's really quite a difference. If you think about it a little, you'll see that it's very different. When you make a printing plate—whether it's a mimeograph stencil or what not—you can run off many copies. In copying you want only one copy. That's usually the case. So you get an original and you make one copy. If you want ten copies you make one copy ten times, each time the machine goes around it makes one copy.

ERMENC: Were changes made in the original conception to make it more acceptable to Battelle or Haloid?

CARLSON: There's been no change in the basic sequence of steps. They are still the same.

ERMENC: As far as you are concerned, would you consider yourself a professional amateur since you have continued inventing in this field?

CARLSON: I would say I'm an amateur professional.

ERMENC: Kettering said most inventors are amateurs, since they are doing something for the first time and doing it rather clumsily at that.

CARLSON: Certainly fits me, all right.

ERMENC: Who and what sustained your development?

CARLSON: I think I did it myself, mostly. I didn't have much encouragement or help from outside.

ERMENC: What are the prospects that Xerox will become simpler? You mentioned that RCA took a path toward eliminating the transfer function?

CARLSON: Well, I think the chances are good as time goes on. We're working diligently in that direction.

ERMENC: We often try to relate creative processes in different fields. I was interested in a statement made by Eugene O'Neill, the dramatist. He said that he has never had a dramatic idea come to him off the top of his head—that every one of his ideas originated, either directly or indirectly, in some other event or impression in his life. Does this have any correlation to your development?

CARLSON: I can't quite relate to his statement. I don't know quite what he means for one thing. Of course, I don't think inventions of this kind come just overnight as a sudden flash, I think there's got to be a long incubation period. I think first one has to recognize in technology or a field of science where a solution might be found. He must let his subconscious mind work on it for a long period, perhaps years, until the right elements eventually fit together in his thinking.

ERMENC: Looking back on the development, do you see any pattern?

CARLSON: I think in spite of the long drawn out period of development and the delays and all, it was a relatively straightforward thing and rather a classic example of how inventions and developments and patents work. I know Joseph Wilson, the President of Xerox, said as much about the Haloid-Xerox experience in taking over a developing xerography. He said it was a classic example of a new development, the growth of a small company, and the importance of patents.