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MYSELF AND MY JOURNEY DOWN THE RIVER OF TIME

*44 Years Spent with the Kodak Company and
54 Years Spent on the Study of Photography*

WHEN you come to the turning point in a road, it is natural to look back to observe the path by which you've come and, if you're asked to talk at that time, to talk about the experiences you have had in getting there. And so when Dr. Staud told me that he would like to have a meeting of the Scientific Committee of the Laboratories at which I could talk to them for a little time, it seemed to me that the best subject I could choose would be myself and the history of the Laboratory in relation to the work that I have done. So I'm going today to try and tell you about my journey to the point which I have now reached, where it is time for me to go, and the time that I have spent in the Eastman Kodak Company—forty-four years of it—and the time that I have spent on the study of photography—fifty-four years of it.

I started in England, the son of a Wesleyan minister. I had a very good education; my father was an educated man with a good library, and as soon

as I could read I started to read his library through. I went to the ordinary schools, and I was ten when I first met in my schooling the thing that changed my life.

My schooling up to that time had been the usual schooling; little bits of history, little bits of grammar and English, a little arithmetic, and, as was usual in England at that time, quite a little Latin grammar. Then at the school I went to, which was founded by Queen Elizabeth I, a schoolmaster in the afternoon one day did something that I had never met before—he made a chemical experiment. What he did was to prepare chlorine, and it impressed itself enormously on me, because for the first time in my life somebody talked about something which was a fact instead of about something that somebody else had written. Chlorine was a greenish-yellow gas, he said, and I saw the greenish-yellow gas. He said it had an abominable smell, and I agreed with him. He showed how it was made, and that was most fascinating, and the first thing I wanted to do was to repeat it; I wanted to see if I could make chlorine too. I went to him afterward and asked him if I could make chlorine, and he finally agreed that if I came back in

the evening of the next day he would let me actually handle the apparatus and make some chlorine, which I did. By that time I had fallen in love, completely in love, with science. This was different from all the other stuff; it was about real things—things that you could touch and handle, that you could measure. I began to see at once, it seemed to me, what a marvelous thing science was.

From there I went to a school in England which didn't approve of science. It believed in classics and mathematics. It was a very good school in the sense that you had to learn, it drove you hard and taught you hard, but, in spite of their classical views, they had a laboratory. Now, laboratories need keeping clean, and before long I had made a bargain with the science master that if I kept the laboratory clean, I could have a key to it. I could escape there from the hoards of ruffians outside who wanted to play football and things of that sort, and I could enjoy myself in the laboratory.

So from the age of about ten on, chemistry—the operation of chemical experiments, the handling of chemicals—was the thing that I was chiefly interested in and what I spent all my time on. This science

became to me a sort of religion. I was brought up in a religious atmosphere; my father was a minister, my school was a religious school, I had lots of training in the Bible, and I'm bound to say that I classified most of the Bible with the Latin grammar as a thing that had little relation to fact. But at the same time I saw a purpose in being on earth and that was to increase knowledge. And so it seemed to me that the thing that was worth while for a man was to study science. I didn't mind what kind of science you studied; it didn't seem to me that that was what mattered; all science was one. What one had to do was to add to the total of scientific knowledge, and when you went, as you would in the end, you could feel that you had done something in adding to that total.

When I began to grow up, when I got to about twenty, I began to see something else about science and that was its application to economics. I ought to explain that at this time I lived in London, and London was a place of very poor people. All the world was poor at that time, It was quite different from the world we are in today here—not different from the world as a whole probably but different

from the United States today.

In London at that time a study had been made of the economics of London by Charles Booth. Booth studied the income of a large number of Londoners. He set a standard which was what he called the "poverty line." The poverty line, as expressed in pounds sterling, was a pound a week for a family. It was the amount which would just feed and clothe and house, at the minimum level of cost of housing and clothes and the minimum amount of food, a small family. And when Booth set this standard of poverty at a pound a week, a third of the population of London were below the poverty line! Now that's a thing that impressed me very much. A little story of that time has always seemed to me one of the most pathetic things I have ever heard. The story is of a child who goes to the little shop and says to the man behind the counter: "Please, sir, I want a farthing's worth of bread (a farthing is a quarter of a penny) and please, sir, would you mind cutting it with a hammy knife." When it's of interest to you whether your bread is cut with a hammy knife to give it a flavor or a clean knife, you are very poor. If you are as poor as that, if you are in a situation where one-

third of the population is as poor at that, then there is only one thing that any decent man can do—he looks at the situation, he compares the lot of his fellows with his own, and he says, “It isn’t right that I should take anything more than the absolute minimum.” Then what he does is to become a socialist. He says we’ll divide everything evenly, we’ll go fair shares for all.

So I naturally became a socialist. I got into the socialist movement a little; I began to meet other socialists, and I joined the Fabian Society. The Fabian Society was a group of intellectuals who were trying to organize things so that the socialists could take over the government of England. They succeeded eventually—to the great loss of the English. I was brought into the Fabian Society partly by H. G. Wells, the novelist. H. G. Wells was himself a scientist; he was a zoologist, and he had written a great many scientific novels and a good deal of sociology. He was an enthusiastic socialist, and he was trying to lead the Fabian Society members into what Wells thought was the way they should go. It didn’t suit the Fabian Society, and they wouldn’t do what Wells wanted. But the socialist movement went on and was

growing and eventually, as I say, was successful, but when socialism was tried, it did not succeed, and today we realize that there are better ways of dealing with poverty than the socialist way.

At that time I read the writings of Francis Bacon, who wrote some very interesting essays and who had some very interesting ideas, and I was much impressed by Bacon's ideas. Bacon was a very remarkable man. He was Lord Chancellor of England. He was a scamp, he was tried and found guilty of speculation, but he was also one of the greatest intellects that ever lived. He had two great ideas. One is that knowledge comes from observation and experiment; we take that for granted nowadays, but it was quite a new idea in 1600. People thought that knowledge came from discussion and from reading the books of the ancients, and the idea of going and trying a thing was quite a new idea. There is a story which came to me somewhere—I always thought that Bacon wrote it but I have never been able to find it—about the time when in the monastery they were discussing the question of how many teeth there were in the mouth of a horse. The argument went on for a long time. They looked it up in the books, they read the

ancients, they quoted the Fathers of the church and argued, and then there arose a young monk who suggested they should go out and look in the mouth of a horse and see how many teeth he had. They pointed out at once, of course, that that wasn't the orthodox way of obtaining knowledge; you didn't obtain knowledge that way—you did it by reference to the authorities, and they fell on the young monk and drove him out. Well, Bacon believed in the opposite method; he believed in looking in the mouth of the horse.

Bacon had another idea, which was less obvious and took longer for the world to learn. That was that the science that people were acquiring, that was growing in the world, might be applied to increase the wealth of man. Perhaps, as we did more science and learned more about it, we could improve methods of production. If you could improve the methods of production, you could increase wealth. What Bacon thought of, in fact, was applied science, industrial science.

Those two ideas of Bacon's struck me very strongly indeed. I thought that the important thing was the acquisition of knowledge but that perhaps the next

most important thing was the application of the knowledge to industry, so that we could relieve the poverty that was strangling my country. I could easily think of places where the production of wealth could be made easier, it would take less of man's time and more wealth could be produced. And I saw that in fact this was the road to wealth—to apply science to industry. And so it very soon seemed to me that it was much better to increase the total amount of wealth than it was to redistribute it.

In 1900, I was a student in the Chemistry Department in the University College, London, under that very great teacher and investigator, Sir William Ramsay. I had come to University College, London, from St. Dunstan's College, Catford, which was a technical school for senior boys, at the same time as another St. Dunstan's student, S. E. Sheppard. Sheppard was the son of a farmer at Catford and was a most brilliant student of not only science but of all the scholastic subjects. When Sheppard and I met Sir William Ramsay, he felt that both of us were well advanced in chemistry in our school work and arranged for us to start at once carrying out some elementary research instead of doing the usual lab-

oratory course. (That is a thing that still occurs in England. If a boy has done well enough at school, the university is likely to put him straight into research work.) I was assigned to work with E. C. C. Baly, who was photographing the spectra of the newly discovered rare gases (they had been discovered by Ramsay and Travers at University College) on a ten-foot Rowland grating spectrograph, and with it I started to photograph the red end of the iron arc spectrum, using plates which were bathed in Alizarin Blue to make them sensitive to the red. I simply followed the textbooks there. Looking back on the results, I can only conclude I wasn't very good at sensitizing plates, and in view of my interest ever since in optical sensitizing it's curious how bad those plates were.

Before going to University College, my chief interest in chemistry had been in organic chemistry, but Ramsay's students were expected to study physical chemistry, then a comparatively new subject, and Sheppard and I went to the lectures on physical chemistry and soon found ourselves greatly interested in all the work that was going on around us in the laboratory. Those were great days in University Col-

lege. After the discovery of argon by Rayleigh and Ramsay, Ramsay and Travers had identified the noble gases of the atmosphere: helium, neon, argon, krypton, xenon. They had a large quantity of residues from the evaporation of liquid air from which the oxygen and nitrogen had been separated. The lightest portion of this material consisted of a mixture of neon and helium. In order to separate the two, Travers put in a compressor and prepared liquid hydrogen, with which he was able to freeze the neon from the mixture and so obtain for the first time pure neon. I recall seeing about a liter of neon confined over mercury with a large Rhumkorff coil producing a current through it, so that it was glowing with a bright red light. Norman Collie, the professor of organic chemistry, with that perspicacity which is characteristic of organic chemists, said, "Some day they will use that for street lighting." It seemed highly improbable to the rest of us with less imagination, for we had in that bottle all the pure neon in the world.

Sheppard and I, however, didn't join in the general work of the laboratory. We had become very interested in photography. Our curiosity was aroused by the fact that we could find no satisfactory explana-

tion of the photographic process in any of the chemical textbooks nor, when we turned to the photographic literature, could we obtain much help. We read, for instance, Abney's "Instruction in Photography," in which he writes equations that don't add up. We wanted to know what happened to silver bromide when it was exposed to light, what it was that made it developable, and particularly, being chemists, what the development reaction was and the factors that controlled the rate of development—what physical chemists call the "kinetics of the reaction."

Sheppard was a very good searcher of scientific literature, and before long he turned up a paper in the "Journal of the Society of Chemical Industry" which had been published in 1890—that's ten years before the time of which I'm talking—by a physical chemist who was in charge of the laboratory of the United Alkali Company, Ferdinand Hurter, and an associate of his in the company, W. C. Driffeld. This paper, which we afterward found was quite well known and which was entitled "Photochemical Investigations and a New Method of Determination of the Sensitiveness of Photographic Plates," came as

a revelation. Here were people who had really studied photography scientifically. They had found a way of measuring the amount of image produced by exposure to light followed by development and of measuring the rate of development of the exposed image. So, in the work of Hurter and Driffield, we had a model for the attack on the nature of the photographic process.

The apparatus that Hurter and Driffield had used had been very primitive, and we believed that the advances in physical chemistry since 1890 would enable us to improve on their experimental methods. The first thing to do was to repeat their work with such improvements in the apparatus as we could achieve. Ramsay was enthusiastic about this program, and he arranged for Sheppard and me to work on the subject and to present the work to the University of London in the form of theses as part of the requirements of a B.Sc. degree. We erected our apparatus in the chemical laboratory of University College. We adopted an acetylene burner as a standard light source and had an explosion one day which ever since has given me a great respect for acetylene. We used primitive thermostats made out of zinc pails, and

to measure our densities we borrowed a Hüfner spectrophotometer, the only one in England, from Professor Starling, the great professor of physiology at University College. We understood that Starling had borrowed it from Guy's Hospital, and beyond that we didn't inquire. With this apparatus, we repeated a good deal of Hurter and Driffield's published work and, in 1903, submitted it in notebooks to the examiners. On that work the B.Sc. by Research of London was awarded to us and, incidentally, it was the only time it was ever awarded. I think that the senate decided that if we could get it so easily they had better not do it again.

We made very little progress beyond that already put on record by Hurter and Driffield, but there was one thing we had done—we realized that since the value of gamma for a characteristic curve is proportional to density, the exponential development equation for the rate of development given by Hurter and Driffield could be used to calculate the growth of contrast during development. That is, since $D = D_{\infty}(1 - e^{-kt})$, $\gamma = \gamma_{\infty}(1 - e^{-kt})$. At this point we introduced a new constant, γ_{∞} , the greatest slope of the straight-line portion of the characteristic curve obtained on

continued development. Sheppard and I published our first papers in 1903, when we were 21, in the *Photographic Journal* on gamma and on gamma infinity.

Both Sheppard and I were by then intensely interested in the work on photography, and we were convinced that it would give us results of value. We were anxious to carry on work for another three years, which was the requirement of the University for the London degree of Doctor of Science. It was not usual at that time for a man to go straight on to his doctorate work after taking his B.Sc. As a doctorate of London is rather hard to get, it was usual to go on doing other things for a number of years and then come back and submit accumulated work for your doctorate. However, Ramsay agreed to our plan. Photographic research requires a good deal of space because, in addition to the ordinary workrooms, there have to be darkrooms. As there was no room in the crowded chemical laboratories of University College, Ramsay agreed to let us work in our own laboratories at home and, at the same time, to supervise our work as if we were at college and then let us submit our work finally as internal students. We therefore fitted

up laboratories in our houses in the suburbs of London. We designed and obtained much improved apparatus. Our exposure instruments used Hurter's sector wheel, but the wheel was completely enclosed and was very much more convenient to use as well as probably of higher precision than that of Hurter and Driffield. The light source was an acetylene lamp supplied from a generator outside the building. The exposed plates were developed in a thermostat in vertical tubes, the plates being rotated, to try and obtain even development, which was an advance on the original method, but which didn't really give us even development. Adam Hilger Ltd. made spectrophotometers for us on the general principle of the Hüfner instrument, and these we used for density measurements. The instrument which I used is now in the George Eastman House and can be seen there.

A description of our apparatus was published in the *Photographic Journal* of July, 1904, in a paper which was the first of a considerable series covering the work that we did on sensitometry, statics and kinetics of development, including the chemistry of the ferrous oxalate developer and the equilibrium conditions for ferrous oxalate, the microscopic struc-

ture of the developed image, the theory of fixation, and the effect of oxidizing agents on the latent image. You can see that we covered most of the chemistry of the theory of the photographic process. We didn't do any work on the physical side except for a little sensitometry. Sheppard and I published eleven papers between 1904 and 1907. We offered them for the degree of Doctor of Science in 1906 and published them in a book, "Investigations on the Theory of the Photographic Process," which has been known ever since as "Sheppard and Mees."

At the end of our academic work, Sheppard was awarded one of the 1851 Exhibition scholarships for two years, which he spent at Marburg in Germany and in Paris. I discussed my future with Ramsay. I had no scholarship, and it was necessary for me to earn a living. He insisted that I should go into industrial research, which I was quite ready to do. I argued with him, of course, for an academic career, like most students, but Ramsay had a very clear vision of the part that science should play in industry, and he insisted that I had worked on photography and must go into the photographic industry. That wasn't so easy! After one or two of my approaches were turned

down by photographic manufacturers, I thought of a friend in Croydon, S. H. Wratten, who was the manager of the firm of Wratten and Wainwright, running a small factory making dry plates. He had coated some of the special plates on flat glass that Sheppard and I had needed in our work. I went to him and asked for a job. He discussed the matter with his father, who owned the business, and instead of a job they offered to incorporate into a company and give me a share in the business for a small sum and at the same time make me a joint managing director with Wratten. I think it was a most extraordinarily generous thing to do to a youngster who wanted a job; I have never heard of it being done by anybody else.

The day I was due to start at Wratten and Wainwright was the 3rd of April because Wratten said he wouldn't have me on the first of April! I went to see him, and he said, "I've just got a letter from a man who wants some plates. He's a German importer of plates made at Farbwerke Hoechst sensitized with their new sensitizing dyes. They bathe the plates in these sensitizing dyes, and they are having a lot of trouble with spots and things. Would you try and

bathe plates? I know you have bathed a lot of plates and written papers about them." I said that I knew enough about bathing plates to know that it wasn't a way to manufacture plates. (Incidentally, the same thing came up in 1913, when Charles Gaumont wanted us to manufacture bathed film. Mr. Eastman sent the formulae to Mr. Wilcox, who was the manager of Kodak Park, and told him to get a bathing machine set up. I flatly declined to use it on the ground that I knew that I couldn't bathe film and get decent quality in it.) I thought about Wratten's problem—I think most of the night, and then I got the idea that, instead of bathing the plates, perhaps you could bathe the emulsion. The next day I went down to the works early in the morning and told Wratten my idea and we set to work at once. We took the emulsion and shredded it very fine, bathed it in a solution of the dyes, rinsed it in ice cold water to rinse the surface dye off, melted the emulsion, and coated it in total darkness in the coating room. I won't go into the story of how we coated in total darkness; it was quite a hectic time, because we had never done any coating in the dark. The next morning, however, the plates were dry. I went down to

the works and picked them up. I lived seven miles away, and I had my testing apparatus, spectroscope, and so on, in my little laboratory at home. I took the plates home and tested them—they were perfect. It being Sunday, I went to Wratten's house and told him we had got it. We went down to the works, the two of us, took the plates out of the drying rooms, packed them, and on Monday morning I took them up to London and sold them to Mr. Fuerst and got the money. (Even in those days I was very keen indeed on making things pay when I did them.)

After a little time, we improved on those plates. We mixed the dyes, and so on, and, in June, 1906, we put on the market the "Wratten Panchromatic Plates," which were a very great success almost at once. At the same time, it was obvious we needed light filters. We started to make several kinds. We found a new dye in Germany called Filter Yellow K, and with that we made filters which became the "Wratten K Filters." We made green safelights for working with the panchromatic plates in the dark-rooms and generally developed this whole field of truly orthochromatic photography, making panchromatic plates, filters, and so forth.

One thing that I invented about that time was the technical booklet. As far as I knew there hadn't been any technical photographic booklets; there may have been, but I didn't see any.

I had very little money for advertising. I had a total of four hundred pounds a year for advertising, so it occurred to me one day that it would be much easier to advertise the sort of thing we were making if, instead of talking nonsense, we talked sense about it. So I wrote booklets which gave people information as to how to use things and, instead of advertising plates, filters, and so forth, I advertised the booklets. At first I gave the booklets away, but I didn't do that very long. I told people to send stamps for a booklet and that paid for the cost of the booklet. I very soon got a lot of these technical booklets, which, of course, you are so accustomed to that you probably take them for granted and think that they came from heaven or were invented by the Eastman Kodak Company.

Then I went on with a certain amount of research, mostly not scientific but technological. I worked on screen unit processes, for instance. At that time the Lumière Autochrome Plate had come out, and peo-

ple were inventing new methods of making screens and coming to us and wanting them coated with panchromatic emulsion. They came to us because we made panchromatic emulsion. In 1909 I worked on resolving power and worked out a system of measuring resolving power, which is still used, in which you copy a screen and see how fine a line structure the emulsion will resolve.

So I was getting along quite nicely at Wratten and Wainwright; the company was growing and we were expanding our business steadily and making more profit. But I was still anxious to return to the study of the theory of photography. However, with the work I was doing at Wratten and Wainwright—I was largely running the company—I had no time to do much research. My time for research started at 7:00 in the evening, except for the weekend, when I could spend Sunday at it.

In 1909 I was invited to visit the United States as a special consultant to the American Bank Note Company. I was glad to do it for I could very well do with the money. Wratten and Wainwright was making money fast, but it was using money even faster, and it was in the usual state of a growing

business. I went to New York to work with the Bank Note Company on methods which would make photographic counterfeiting of notes as difficult as possible. My wife and I spent two months in New York while I worked on their problems. I took the opportunity to write to Mr. Eastman, who, of course, I regarded as the greatest man in the photographic world, to ask whether I could visit Kodak Park and the Eastman Kodak Company. He was very nice about it and was very cordial when I came to Rochester. He talked to me for about half an hour about American football, which was something in which I had no interest whatever. He had me shown through the Kodak Park factory by Mr. Haste, who was the manager there. After I returned to England, in January, 1912, at Wratten and Wainwright I received a telephone message from Mr. Thacher Clark of Kodak Limited. Mr. Clark was Mr. Eastman's personal assistant and correspondent in Europe, and he told me that Mr. Eastman would like to repay the visit I had paid him; he would like to visit our factory in Croydon. That afternoon Mr. Eastman came, and we showed him the factory, dwelling especially, of course, on our small laboratory and on

the work we were doing on panchromatic plates and color filters.

After Mr. Eastman got back to London, he called me up on the telephone and asked me to come to see him in London the next day. I said that I couldn't go because I was going to Hungary to lecture; he asked when I was coming back, and I told him in two weeks. He said, "Well, that's too long, I'll be back in America. I think you had better come and see me—I want to ask you if you will go to Rochester." I replied that I thought that was more important than the lecture, and I sent a cable to Hungary. I still have the invitation card written in Hungarian for the lecture which I didn't give. I went to see Mr. Eastman the next morning, and he greeted me with, "Well, will you come?" I said, "I will if you make it possible." He asked, "What have I got to do to make it possible?" and I replied, "Buy Wratten and Wainwright." He said, "That's all right." And it was all right; we didn't have any difficulty in arranging terms.

So I came here in April, 1912, primarily to work out the plans for the Laboratory.

The building site they gave us was occupied by the old Building 3. Building 3 was the original

emulsion building, and it had a sacred well in the back of it in which there was supposed to be specially good water which enabled you to make excellent emulsions. It wasn't for some years that I discovered that when you put some chemical into the old pond in Kodak Park, in twenty-four hours it could be found in the sacred well! The new Building 3 was a beautiful steel building and to me at that time the pride of my heart and a model laboratory. I learned one very important thing during the building of Building 3—that I knew nothing whatever about designing or building laboratories, and I never tried to design or build another. After that I left it to somebody else; they couldn't do worse than I did and they might do better.

The Laboratory was organized on the basis of a group of scientific divisions and a manufacturing division. We decided to make the Wratten plates and the Wratten filters, etc., in the new Research Laboratory. It was a very good thing to do because it gave us the opportunity of selling to the public anything new that we worked out in the emulsion line. We could make it and sell it as a Wratten product, which didn't commit the Kodak Company very

deeply if it went wrong, and if it went right we could rapidly expand it into our Kodak products. So, for a good many years, for the next twenty years or so, we used the Wratten Department largely for trying out things, and at the same time, of course, we sold a lot of plates and a lot of filters and, incidentally, made a good deal of money.

In the Photographic Department, I had J. G. Capstaff, who had come from Wratten and Wainwright primarily to make the Wratten filters but who very soon started research on photography and, before long, on motion-picture photography and color photography; and J. I. Crabtree, still with us today, who took charge of photographic chemistry. S. M. Fernald, a local commercial photographer, came to the Laboratory in charge of the studio and did all the testing of the Wratten plates as well as experimental work on the graphic arts.

The Physics Department of the Laboratory was, of course, based on sensitometry. The head of it was Dr. P. G. Nutting from the Bureau of Standards, and he had a group of men whose names some of you will remember very well: L. A. Jones; Orin Tugman, who was here for only four or five years

and then went as professor of physics to the University of Utah; Emery and Kenneth Huse; and Rex Wilsey. That about represented the physics group at the very beginning. Inorganic chemistry was organized under Dr. McDaniel, who didn't stay with us very long. The physical chemistry was under Sheppard, whom I asked to come from England when I knew I was coming here. I hadn't worked with Sheppard since we had left University College, but he was pleased to come here and of course you know his career. Later all the chemistry was under Sheppard. We had no division for organic chemistry. It was one of my major errors not to provide for organic chemical work at that time, and it can only be explained by my obsession with physical chemistry.

I knew nothing about running a research laboratory, of course; nobody did. In England I had worked for a little time with Mr. Rintoul, the research director of Nobel's, whom I have always regarded since as one of the great original directors of industrial research laboratories. He knew how to run a laboratory and I learned a tremendous lot from him. He knew a lot about organization and plans for it, and so on, and built a most effective group at

Nobel's. I also went to see Dr. W. R. Whitney of the General Electric Company, who had been running a laboratory for about twelve years when I visited him in 1912, and I learned from him not how to run a laboratory but how not to run a laboratory. Dr. Whitney's policies were almost entirely negative, but they were very useful. He warned you against the things you shouldn't do, particularly that you shouldn't try and run the laboratory—that you should leave the men to run the laboratory and just see that nobody interfered with them.

Following Rintoul and Whitney, I set up a form of organization which was based primarily on conferences. We had a conference practically every week on all the different sections of the work. The conferences were extremely stimulating affairs and to this day are, of course, a major factor in the Laboratory. Through the years we have changed the subjects of the conferences. Some of them have died and others have been started, but the conference system has persisted as the main factor, I think, in the organization of the work of the Laboratory—in planning what we do and in the discussion of the progress of the work.

Our finance was very simple. We spent what was necessary, and Mr. Eastman gave his approval. It wasn't until 1919 that we had a budget, but we did have, and this is interesting, the first budget in the Eastman Kodak Company. I'd had budgets at Wratten and Wainwright; I couldn't afford not to, for we hadn't any money and I had to see that we came out at the end of the year on the right side. When I came to Kodak, I found that there was no such thing as a formal budget of the kind that is standard now, and, in 1919, I went to Mr. Eastman and told him that I would like to present a budget for the year 1920 from the Research Laboratory. He asked, "What for, don't you get enough money?" I said, "Yes." Then he asked, "Don't I always give you what you want to spend?" I said, "Yes." "Well," he said, "what do you want a budget for?" "Well," I said, "you might run into bad times or something and change your mind in the middle of the year and I don't want you to do that. I want you to make up your mind at the beginning of the year as to whether I can spend the money and then let me spend it." I was very thankful because 1921 wasn't such a good year. In 1921 Mr. Eastman asked me to

cut my expenditure and I asked him if he wanted me to diminish the budget for the next year. "No," he said, "I want you to cut it down now." "Oh," I said, "we can't do that, Mr. Eastman, for we agreed to what we would spend this year." He said, "So we did," and that was the end of that. We have always had a budget since, and we have always kept inside our budget, and the budgets have grown tremendously and still look quite healthy.

Our early work was mostly photographic science. It was concerned with the theory of the photographic process. There was one thing that I had asked for when I had discussed the Laboratory with Mr. Eastman. I told him I wanted to publish the scientific work done in the Laboratory, and he asked, "Well, what do you want to publish?" I said, "Everything that we do that is scientific," and he said he would approve of that. I said, "If I can submit our papers to you that will be all right; anything you don't want me to publish I don't want to publish, but I don't want to submit our articles to anybody else, such as the Patent Department, Sales Department, Legal Department, etc. If I do that, they will all be afraid that a paper they don't understand will do harm and

they will say no." So Mr. Eastman agreed and we have always published our work. We adopted the principle of numbering our communications from the beginning. We left out all acknowledgements; we don't thank anybody in a scientific paper. We give our papers communication numbers, and we have published nearly 2,000. We publish all our communications from French and English companies in the same series as those originating in Rochester.

At the end of the year, we published a volume containing abridgements of the scientific publications. In 1915 I started to get out an abstract bulletin, and I think that among the things that we have done, our communication system has been a great success. Everybody respects our communications, and I am sure that you all appreciate the possibility of publishing your work as you do it, of getting credit for it in the scientific world, and of keeping in touch with the great stream of production of scientific knowledge.

The rest of our work is, I'm afraid, a longer story. I won't be able to get through it, but I will go on for a time.

In 1914 I worked out a method of making x-ray emulsions with Mr. W. G. Stuber, who adopted it and made x-ray plates and then later x-ray film along those principles.

In 1923 we made our first contribution of importance to the new products of the Company.

Mr. Capstaff had been working on home movies and invented a controlled reversal process in which the reversal exposure given depended upon the density of the bleached image. From that came the whole of our Cine-Kodak process. That was the first example of the present pattern of research and development of the Laboratories, in which we developed a new product practically as a whole and then saw it onto the market and turned it over to the sales folks, who made a success of it.

At that time, I became responsible for the mechanical development department. I started it because the Camera Works appeared to need very much a research department which would work on mechanical things. It was a success because of the man who took charge of it—Dr. Chapman. He had come here as a lieutenant in the Air Force in 1918, and after he had listened to my complaints about the Air

Force for an hour I was so impressed with his patience that I decided he would be just the man I needed. In 1919 he became the head of the Development Department and continued there until '23 when the department was a complete success and running very smoothly, and he became assistant to Mr. Lovejoy. Newton Green, who had been Mr. Capstaff's right-hand man, was the next head of the Development Department. Of the first two heads of that Department, one of them is president of the Company and the other is in charge of the whole of our mechanical production.

Meanwhile, Building 3, the original laboratory, was growing crowded. We had also put in a section of organic chemistry. I went to Mr. Eastman originally in the beginning of 1914 and told him I had made a mistake and that we ought to have an organic chemical laboratory. Mr. Eastman said, "Did you expect to have it when you came?" I told him, "No." He said, "Did you understand from anything I said that you could have it?" I said, "No." He said, "All right then, we won't." The next year, however, things changed in Kodak Park, and Mr. H. LeB. Gray, an organic chemist, asked Mr. Haste if he

could join the Research Laboratory. Mr. Haste and I went back to Mr. Eastman and impressed on him that we needed an organic laboratory and Gray was put in charge of our first organic laboratory.

In 1918 we started to make synthetic organic chemicals to supply chemicals to all the research laboratories, universities, etc., in the country, so that people could get the chemicals that they had previously bought from Germany. Dr. Hans Clarke, who had been working with Gray in the organic laboratory, took that over and organized it, and he had as assistants Ivar Hultman and Bill Hartman. That department has grown ever since. It was part of the Laboratory originally and still is.

In 1926 we started to work on cellulose acetate. In 1932, as a result of the work Hans Clarke and Carl Malm had done on cellulose acetate, we were able to start making cellulose acetate at Kingsport.

The Laboratory was continually getting more crowded; we had taken in Building 4 plus others around Kodak Park. Wherever we could grab a piece of space we grabbed it. But hadn't nearly enough room to do the work that was coming to us. At the same time, I was very concerned about

the question of sensitizing dyes. In 1928, as a result of publications in Europe, we found that we could probably make a very large number of new sensitizing dyes. I mean that the number of sensitizing dyes was quite probably unlimited. We had to test them, and testing sensitizing dyes, even if you organize the work as efficiently as possible, takes up a lot of room and a lot of people. It was clear we had to have a laboratory which would deal with emulsions, which would be able to coat emulsions and make emulsions and which would coat sensitized emulsions and test them and so enable us to make progress on sensitizing. I asked the Management Committee twice, I think, for it, and I shall never forget the time when they agreed to it. It was in 1930 or late 1929, and I put the thing up again to them, and the man who gave us the laboratory was L. B. Jones, the Sales Vice President. He said, "If Mees says he wants a laboratory and we haven't the money for it from any other source, take it out of my advertising budget." In those days the advertising budget was a very big budget, but, of course, we weren't in that state of poverty. The Company built the laboratory without cutting the advertising budget.

We built Building 59, the front of this present building, and we moved here. In addition to the expansion in our space and the fact that we could do more work, we still had available Building 3. I had no intention whatever of letting it go, in spite of some suggestions from the engineers that it would be nice to pull it down or that it would do for something else. I pointed out that what we needed was an emulsion research laboratory and, supported particularly by Mr. Lovejoy, I managed to get that approved, and we started the Emulsion Research Laboratory, with Dr. Staud in charge of it, and intended especially for the testing of sensitizing dyes. Progress in sensitizing dyes came almost at once. In 1930 we made the first supersensitive panchromatic film for motion-picture work. In 1931 the new Emulsion Research Laboratory made the Verichrome film. We designed the Verichrome film and, with Mr. W. G. Stuber's assistance, we got it out on the market in very short time; of course, it has been one of our most successful developments. It would have come eventually, I suppose, but without the new laboratory it would have been a long time before we could have done it.

In 1930 I realized that the new dyes that we could now make would solve the problem of making Mannes' and Godowsky's proposed color process work. The thing that was preventing its development was the difficulty of the wandering of the sensitizing dyes; now that we could make all these different dyes, some of them were sure not to wander. So we asked Mannes and Godowsky to join us here, where they worked happily with us for ten years, and we all set to work and made the new color process work. I say "we" because everybody came into that; Mannes and Godowsky did a great deal of the work, but a great deal of it was done by our own people and particularly by the Emulsion Research Laboratory, which had all the responsibility for the sensitizing, coating, and so on. Since then, the Emulsion Research Laboratory has been very active and has developed one product after another. At the same time, the rest of the Laboratory has gone on growing and expanding until we are where we are today.

When the Emulsion Research Laboratory was established, I was so impressed with the mistake I had made originally in not providing for work in organic chemistry that I decided that the head of

the Emulsion Research Laboratory should be an organic chemist, because I believe that organic chemists might have a good deal to add to the progress of photography. I think that the proper expression as regards that is: "Man, you said a mouthful!" because since then the organic chemists have developed the whole system of addition agents, and so forth, on which our present emulsion technology is based.

Through all these years, work continued on the theory of the photographic process and is still continuing. In 1942 I was able to put the whole thing together in a book, a very fat and heavy book, with the aid of a great many of my associates in the Company who wrote chapters and sections of the book, and we published "The Theory of the Photographic Process." That achieved one of the two objectives I had when I started—the whole theory of the photographic process had been put into a form where it made a comprehensible whole. At the same time, the development of the Laboratory and its relation to the Eastman Kodak Company showed, in the photographic industry, that science could be applied to the technology of the business with advantage to all sides. Science has been applied to the photographic

industry, and the Research Laboratories are now firmly established as a major department of the organization of the Eastman Kodak Company. And I can go to Honolulu.

November 9, 1955

