Dr. Albert J. Dalton

An Oral History

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Interview with DR. WYNDHAM D. MILES Historian,
National Institutes of Health 6 November 1964

Transcribed 1 December 1988

History of Medicine Division
National Library of Medicine
Bethesda, Maryland

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Date-Place of Birth: November 9, 1905, New London, Connecticut

Education:

B.S.: 1927 – Wesleyan University

M.A.:1929 – Harvard University

Ph.D.: 1934 – Harvard University

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Date: November 6, 1964

Interviewer: Dr. Wyndham D. Miles

Q: Dr. Dalton, if you would just start off by telling me where and when you were born, and anything you want to about your childhood?

DR. DALTON: I was born in New England, Connecticut, November 9, 1905, at 13 Terrace Avenue. My father was Irish, born in Brooklyn. His folks came over during the potato famine, so you could say that I'm half potato famine Irish, and the other half, my mother was born in a small town near Steban, Norway, and came over from Norway at the age of 11.

Q: If it was the potato famine, that was pretty long ago, wasn't it?

DR. DALTON: Yes. My dad was born in Brooklyn. His father came over during the potato famine. We lived in this house the early part of my life up through at the beginning of World War II, when my dad went in the service, in the Navy. He was a chief petty officer, I remember. He was stationed part of the time at the New London Naval Hospital, set up after the U.S. got into the war, and then was moved to Newport, Rhode Island. During that wartime, we moved around quite a bit. I remember we went over to Greenport, Long Island, with my uncle. In those early days, I was, of course, a Boy Scout. I can still remember very vividly the Liberty Bond drives we Boy Scouts went on during the war.

Q: Incidentally, how did they work? I've seen photos of those things, but just what did you do?

DR. DALTON: We canvassed any place or any part of town we wanted in our off-school hours and weekend, and there were three different drives that our Scout troop took part in. We actually had our little subscription book, and had people sign up for them, and gave them a copy of the statement that they had agreed to buy so many dollars worth of bonds.

Q: Do you recall how many you sold?

DR. DALTON: I remember doing over what we were committed to try to get each time, but one time I thought I did particularly well. I don't know whether she was an aunt or a great-aunt or what, but she worked as a maid in a house up in one of the important parts of town, up on a hill, and she arranged for me to interview her boss. So I got in to see him, and he said he had already bought about as much as he could, but he figured he might be able to stand buying another $50,000 worth. The other drives, I probably managed to pick up around 1,000 or so. This particular time really did my morale a lot of good.

Our Scout troops was listed as number two with the Methodist Church in New London. Actually, it was number one because the troop number one had been formed and disbanded, and then we started up after they did.

Q: When you said the date, World War I, I was thinking it was pretty early for the Boy Scouts in this country.

DR. DALTON: Oh, yes. We were one of the earliest in that part of New England, and we pioneered in establishing a Boy Scout camp. I remember going out with the troop leaders after I'd been in the Scouts for a few years, and we had to set up a parade ground. The dynamiter had been out and blown out stumps of trees, and we had to go out there the day after and pull the stumps out, and then clean up afterwards. I don't know what the gas was that was still lingering around in those stumps, but most all of us, as a result of some nitrous compound or something, that night we all had very severe cases of diarrhea from working around where the dynamite gases were still collected. This was on Gardner's Lake, just about 15 miles out of town, out of New London.

I served as tent leader at that camp for several years, and I remember going out fishing one day with a priest who happened to be out. It was on a Sunday. He was a real fisherman, and he knew the lake very well. We went out, and I had a rod that I had actually never caught a fish on before, and we went over an area where there were supposed to be bass, and I hooked one. It must have been a pretty big one, because it snapped the pole right off. The priest said, "No sense in staying around here. We've disturbed the bass in this area. You did a good job of that.

So we went on further. I had another pole. We went across the other side of the lake, and I caught a tiny little perch about four inches long. Coming back toward camp, I put that on the line. The priest suggested I do it. We went past an area in the lake where there were a lot of old stumps of trees right in the water. The lake level had risen, and I didn't think this would be much of a place for catching bass, but I hooked into one. The line went up along the side of the reel and down on the axels, just a wobbly old reel, you know, but luckily the line held. I pulled that bass in hand over hand, and it broke water at least four times on the way in. I finally got it in, and my mother still has the picture of me with that bass. It was about a three-pound bass, the biggest bass I ever caught in my life. I haven't caught one as big as that since.

My last year at the camp, I wanted to get off a week early to go visit my folks, who were at a Methodist camp meeting in W_______________, which was about 40 miles from the Scout camp. I had never asked for any time off before, when other tent leaders had. The camp director refused to give me it. He said, "You're asking for time off at a very awkward time. I just can't let you go." So I said, "Okay, I'll just quit." So I quit. I went to the camp meeting, and I decided by quitting the camp, I'd quit Scouting, too, and I never did go back. I had got Life Scout and Star Scout, and was on the way to Eagle Scout, but this disagreement with the director of the Scouting for the town, I just quit. We became good friends again later after I got in college, but that was the end of my Scouting career.

As I say, before the Scouting period, I lived in Greenport for one winter, the winter of 1917 or '16. We were in the war, so it had to be 1917-18. I remember that winter very well, because we had very heavy snow, about two feet, followed by hail and sleet and melting and freezing. We had about a half an inch or more of good solid ice on top of that snow, so you could skate everywhere, across the country, drifting over fences and so on. You could just go skating wherever you wanted. I remember that very wonderful sensation of being able to skate to school and skate wherever you wanted to go.

I also remember coming back at the end of our stay, came back from Greenport to New London, on the old Orient ferry. We got out in the middle of Long Island Sound out around Plum Island, beyond Plum Island, Plum Cut, it's called, and got a U-boat scare. A U-boat had fired off a torpedo and had missed, but the German torpedoes in those days, they would make their run, and if they didn't hit anything, they'd come to the surface and function as a floating mine. So they had a trawler out, a minesweeper, trying to find that torpedo. We stayed out in the middle of Long Island Sound for four or five hours, and it was winter. I mean, it wasn't deep winter, but it was late winter. There was a big strong wind, and the boat, of course, was a tub and was rocking in all directions, and there was no heat except from the stack in the cabin. People were seasick in there. You'd go in to get warm, and you could stand it just so long, you'd have to go out and freeze to death for a while. We kept this up for four or five hours, I remember.

Q: Did they ever find the torpedo?

DR. DALTON: I don't know. They finally gave us permission to move ahead, and we got into New London without any further incident.

I remember grammar school was split up. Part of my time was spent in New London grammar school and part in Greenport. In Greenport, I had the one chance—no, I never did take the New York State Regent Exams. I left before I had to take them, I think.

Then back to New London in the grammar school, which in those days, of course, was just a two-year school with a lot of emphasis on English and grammar. I remember distinctly one teacher, Miss Bush, a plump, gray-haired lady, a very good disciplinarian. She just had complete control of that class and complete respect from everybody. This was a public school. Of course, in those days a lot of kids who were actually going on for higher education went to private schools, but this was a public grammar school. She had quite a range of capabilities and attitudes in the class, and she was able to keep good discipline and able to teach us a lot. I learned practically all I know about the English language, I learned in two years of English that I had with that woman. I'11 never forget one time flubbing an assignment when I was called on, and she looked over her glasses at me and said, "You can do better than that, Albert." I ended up making English my minor subject in college, as a matter of fact, I think partly as a result of the influence of that woman.

There was one girl in my class, Elizabeth Hitchcock, her name was. She was the youngest daughter of the superintendent of one of the public high schools, the one I eventually went to. She was very bright, and I did a lot of competing, but never could quite make the grades that she did in grammar school. Then we ended up in high school together. At that time when I went there, it was called a vocational high school, the kind of school that boys and girls went to learn a trade of some sort. It's not a typical trade school as we have them today, but it was in that general area. It had woodturning, lathe work and machine lathe, forging, and mechanical arts, mechanical drawing, and so on. I took all of those subjects, carpentry, and four years of mechanical drawing.

While I was in the school, its name was changed to Chapman Technical High School, and you could get college credit, actually take an academic course. By the time I was a sophomore, this had been arranged for.

That first or second year, we had three teachers from Wesleyan University in Middletown, Connecticut, who had just graduated from college and had come here to teach in high school. One of them was named Longacre, and his stature fitted the name, because he was about 6'8", in that range. I had him for my first year in algebra, and I learned all the algebra I know the first year. The second year, the teacher I had wasn't much. This other fellow moved on. Then I had a man by the name of Spencer, who taught physics, and he was from Wesleyan. Then another man who taught history. Why I can't remember his name right now, I remember the others very clearly.

Q: You mentioned physics. Is that the only science course you had?

DR. DALTON: No. I had chemistry, also, with a Miss Morris. I had good teachers in both physics and chemistry and my first year of algebra.

Q: In these sciences, did you have laboratory work?

DR. DALTON: Yes, in chemistry and in physics, we had laboratory work.

Q: Why I ask that, sometimes I run into someone who went to a high school in which they didn't have any laboratory facilities. That's why I like to ask about that.

DR. DALTON: They gave us something in biology that didn't amount to anything. I think there was one microscope. But the chemistry lab was pretty well equipped, and I got quite a bit out of that.

Q: This chemistry teacher you had, did she teach anything else besides chemistry?

DR. DALTON: The chemistry teacher, Miss Morse, I don't think so, I think that's all she taught.

Q: Some people have told me about their chemistry teacher who also coached the football team.

DR. DALTON: Our history teacher coached the football team, I remember. I played on the team. I got a letter in it, at least, my senior year.

Q: Is that right? What position did you play?

DR. DALTON: End. There were a group of us. Maybe some kids went to college before our class, but I don't remember them. If they did, it's not more than one or two. In our class, there was a fellow by the name of Green, who went to West Point, and Ken Holmes, who I grew up with, he and I went to Wesleyan, and a fellow by the name of Walter Farnham went to Wesleyan. He was a good athlete, and he flunked out of Wesleyan. He just didn't put enough time in his studies. In those days, they just didn't fool around. If you went down in your grades, you knew about it all along, and they didn't feel that you had to be warned about it. So if you failed, you were just out.

Another fellow by the name of Wilson went to Wesleyan. There were four of us that went to Wesleyan from that high school because of these fellows from Wesleyan who taught there during our years there.

Q: What did you major in?

DR. DALTON: I majored in biology. First I want to go back and express the fact that in my senior year in high school, I got interested in electronics and amateur radio. I got an amateur radio license in 1922, my junior year in high school, I guess it was, between the junior and senior years.

Q: Was that a sending station?

DR. DALTON: Yes. I got on the air with a spark set, and I remember being heard by the submarine base about 11 miles away up the river from my home. Then when I went to college, of course, I dropped out of it, but I kept up an interest in it. I remember building a little radio set and listening to the Dempsey-Tunney fight at the fraternity house.

Q: What was it, a little crystal set with cat's whiskers and so on?

DR. DALTON: No, this was battery-run with small tubes, receiving tubes available in those days. It was just a little two-tube set with a little tiny speaker about so big. If you got up close, you could hear it, you know. All it was was an outsized earphone. But we heard it. That was 1927, my senior year.

Anyhow, I went to college thinking I was going to major in physics and electronics, but when I got to Wesleyan, I heard about how tough the physics course was. I decided I'd let that go until the second year, and I took a course in chemistry. I did quite well in the chemistry course, so I thought maybe I'd major in chemistry. So I took organic chemistry the second year, and biology the second year. I did all right in organic chemistry, but I realized it wasn't for me. I just had to keep going back to the original concepts and redoing them in order to move on to the next step. I couldn't accept anything that I had learned. I had to each time go back and learn it over again to move on a step further. I did very well in biology. Primarily because I had some background in it. And having two men in biology; H.B. Goodrich, who is very well known (he died just recently), a geneticist and embryologist, and E.C. Schneider, a physiologist, who in the First World War did a lot in connection with the development of the testing for aviators. Those who would have the right attitudes and physique to probably function well as aviators, he was involved in this program of developing the testing method, and I was there at the time when they were, more or less at their best.

I decided I was either going to go to graduate school or to medical school, one of the two. I was president of the pre-medical club in my senior year. I got a scholarship for Columbia. But I got interested, from the embryological point of view, I got interested in cancer and the problem of uncontrolled growth and so on. I was sort of engaged to a girl who felt that staying engaged to me for the length of time it would take to get through medical school and internship wasn't for her, at least this was part of the reason why I decided to go into graduate school.

I spent one year of graduate school at Wesleyan, and then went to Harvard on a university fellowship the next year. This university fellowship didn't pay an awful lot, but it did require that I couldn't work part-time; I had to put full-time in the course work. So while I had to go in debt a little bit, I did manage to get enough course work completed for the master's degree, which I received at the end of 1929.

During these summers after my graduation and the next summer, I went to Woods Hole with Goodrich, and worked with him as his assistant. This was a wonderful opportunity. I took the invertebrate embryology course. Actually, I had three summers in a row there.

Q: Incidentally, is that a regular part of the education degree up there, or is this just something that you did on your own?

DR. DALTON: I did this on my own. Goodrich was one of the staff members, one of the important staff members at Woods Hole Marine Biological Laboratory, and he had me come there as his assistant for one year. No, that was for two years with him. One year I took the experimental invertebrate embryology, and the other year was a straight assistantship. The third year I got a job with the U.S. Government, with the Bureau of Fisheries at their station there, working on the problem of why you can't grow mackerel like you can trout in a hatchery. I didn't solve the problem, but I did get my summer pay. I stayed there at the Bureau, had a room, and then I waited on tables at MBL, a place where everybody got together to eat, anyhow. It wasn't a cafeteria, because we waited on tables. I got my meals for that. Then I made drawings of insects for Professor Bruce [sp?] at Harvard in my spare time, so that I ended up that summer pretty well off, and then went to City College in New York as a tutor.

Q: Where did you get your degree, at Harvard?

DR. DALTON: I got my master's at Harvard in '29. I remember Crozier, he was a professor of physiology at Harvard, when I met him on the steps of the old Agassiz Museum at the end of the term and told him I was going to City College to teach, he said, "Well, you'll never be back." He probably would have been right, but I got to City College in the fall of '29, and that was when the Depression hit. I was teaching at uptown City College the first year, and I also taught in night school.

The reason I quit to teach for a while was primarily financial. My folks had helped me a lot, and I had taken out insurance policies on them for loans and so on. But, I was about as far as I could go. So I stopped and taught. I did as much teaching as I could, night school and summer school, too.

Q: What subjects did you teach?

DR. DALTON: I taught first year biology, which included quite a bit of botany. It was a complete biology and zoology course. I remember one amusing incident that concerns Robert Moses, you know, the world's fair man. He was on the school board or something like this at the time. The biology department was in the habit of charging a laboratory fee to students. This was to take care of the purchase of lobsters and earthworms and different laboratory materials. They charged enough, though, so that they wouldn't lose money, and there was always a little bit left over. This had accumulated over the years so it was something in the neighborhood of $5,000. Moses called Melander [Axel.L.], the head of the department, one day and said, "You're going to be investigated in a few days, and you'd better not have that $5,000 in excess. It had better be spent on something before they show up."

So I was given the job of spending $2,000 on laboratory supplies that were reasonable, charts of life cycles of plants, ferns, mosses, and things like this, special dissecting equipment and so on. Luckily, one of the biological supply houses was right in New York City, and I went down there and spent a whole day looking and ordering and buying, buying them right off the shelves, you know, and signing for them. I ended up by spending the $2,000, and nobody was caught with excess money on their hands.

Q: Incidentally, did you teach the same courses in night school?

DR. DALTON: Yes, I taught biology, and I taught some histology, and I taught some embryology, as well. I taught the embryology courses in night school. I also made histology and embryology slides for the department in my spare time. This was just putting money aside to go back to graduate school.

Q: This was in the middle of the Depression. Were your classes very big then? Did this have an effect on the size of the classes or anything else?

DR. DALTON: Very little. No, the people had the time to go to school, and of course, it was free tuition for people living in Manhattan, except for laboratory fees and things of that sort. That's when I decided to grow a mustache, when I was teaching night school. Everybody in the class was older than myself, and I felt I had to do something to make me look a little older, and I haven't taken it off.

I started on a project for my thesis there, too. I had gotten interested in the problem of embryonic differentiation in the chick with Hoadley [Leigh], who eventually turned out to be my professor. He had done work on this problem by growing portions of the early chick embryo on the [?] membrane of older chick embryos, and examining the growth to see how much differentiation would occur. He had published a series of papers on the subject. B.H. Wilier, who was at Chicago when Hoadley was there, was also working in the field. I think Hoadley, from Chicago, got a National Research Council fellowship, something like that, and he went to Europe and spent some time with Spamon [sp?]. He came back and taught in Chicago, and finished up and got his degree. Then he went directly from Chicago to Brown, where he was one-year assistant professor, the next year associate professor, and then the next year he went to Harvard as associate professor, and a few years later was made full professor. Just moved up, skyrocketed up. Whereas Wilier, who was in Chicago at the same time, working in the same general field, didn't get a Research Council fellowship, and stayed on in Chicago teaching part-time and working for his degree part-time. He didn't finish up 'til sometime after Hoadley. I don't think Hoadley realized it, but Willier definitely did have a certain animosity toward Hoadley for just the fact that he was able to move up so fast. Wilier ended up doing equally good work, and I think he kept at it longer in the lab, working in the lab, than Hoadley did.

But I was sort of in the middle, and my loyalties were toward Hoadley. Wilier was beginning to publish stuff that contradicted Hoadley, so I decided that my thesis was going to be one which would attempt to settle this question of the difference between the two men, and that's what I did for my thesis work on the chick embryo.

I took a course with A.B. Dawson called experimental cytology. Dawson was something of a disciplinarian in the sense that he wouldn't give you a moment's rest. He was always coming into the lab and checking on what you had gotten done since the last time he was in. It might have been only 24 hours before, but he was always around. Hoadley just let you go pretty much. I ended up, by the time I got my degree, I worked on a problem on the [unintelligible] apparatus in the chick embryo, the developing chick liver, with Dawson, but Dawson wouldn't let me have his name on the paper, just let me put a reference to having thanked him for his advice and whatnot, this kind of business. Hoadley was the same way. I got the paper with Dawson published. I think it came out before my thesis did. I was sort of a split personality there for a while, because I had a loyalty toward Hoadley, and yet I began to get more and more interested in psychology.

So my first job after I graduated and got my degree in 1934, the only job that showed up was one at Western Reserve. I was given a break on this because I was married and had a child by this time, so

I was given the first interview. Freddy Wait was a professor out there, he interviewed me at Harvard and gave me the job, $1,500 a year. I had ended my last year at City College, I was making $2,000 straight, plus another 1,000. I was making $4,000 a year at City College. This was sort of a come-down, but this was 1934.

I remember getting out there and reading in the paper that some guy by the name of Grace had been given a bonus for his services to the U.S. Steel for the year of $1 million. This was the period of the Spanish Civil War, also, and a lot of people were volunteering to go over, and a lot of people were soliciting funds for support of the loyalists, so called. Me making $1,500 a year and this guy, Grace, getting $1 million, not salary, this was just a bonus for his services to the company for the year, this was really before Roosevelt had really gotten his income tax thing really going.

It made you sort of have a sympathetic attitude toward Communists, at least toward Socialists in those days. I think this was a period when a lot of men and women in my age group actually became Communists. Things were so bad. I know teachers in the high schools in Cleveland were being paid in script, because the city didn't have the money to pay their salaries, and they would have to go to certain stores and turn in the script for food. I wasn't quite that badly off. Of course, $1,500, while it doesn't seem to be much now, it was equivalent to about $3,000. So we didn't starve. We didn't own a car, but we did get along. I got a raise to the munificent increase of $300 my second year, and then $100 increase each year as token increases for four years, and I ended leaving there at $2,000.

At Western Reserve, I taught dental and medical histology and embryology. Then I was interviewed for a job at McGill, as a lecturer at McGill, and I was accepted at $2,500. I went there in 1938 to teach histology and embryology, again in both medical and dental schools. I was in the department of C.P. Martin, who was brought over from Trinity College in Dublin, as head of the Department of Anatomy. This is what the Canadian schools, I guess, still do to some extent. Their own people on the staff, in some of the universities, anyhow, never get a chance to become professors; they will pull somebody over from the old country. This was done in this case, C.P. Martin was brought over when the chair became vacant. Instead of considering somebody from Canada, they brought him from Trinity College in Dublin. No question about his ability as a teacher, but I'm sure there were others of equal ability in Canada who could have filled the bill. Soddy [sp?] [Frederick] was assistant professor of anatomy there when I arrived. I arrived in the summer, I guess in August, and he was down lecturing in South America. So Martin asked me to line up things in the lab for the histology course, which I did. Soddy [sp?] just arrived the night before the first lecture was to be given in histology, and I had already prepared to give it. I went into the lecture room, and Soddy [sp?] showed up. So, actually, I didn't have to give the first lecture, but I really worked my pants off the latter part of that summer getting things ready. Soddy [sp?] is a real hard worker, very ambitious, and also a very bright, quick thinker. I found out shortly after I had been there just a while that if I wanted to do any research work, I had to do it with Soddy [sp?], because he had all of the equipment, all the space, all of the animal room space, and all the funds. I managed to get the acting dean, who was an embryologist himself, managed to scrape some money together to buy me an incubator, so that I could work on chick embryos, but that was all. So I did work with Soddy [sp?] for the three years I was there.

One summer when he was away, there was a French Canadian, and I worked on a problem and completed it, and wrote it up and got approval from Martin, and published it while Soddy [sp?] was out of the country. When he came back and found out that we had published this all on our own, he was furious, because he was just that way. I'm still good friends of Soddy [sp?]'s, but some of his peculiarities used to get me in those days.

Q: That's rather unusual, isn't it, not being able to do much research like that?

DR. DALTON: If you worked with him, you see, this was fine. Martin used to say that being head of a department that had Soddy [sp?] in it was like sitting on top of a volcano; you never knew when it was going to blow. He had that kind of energy.

Canada went to war in '39. I arrived in the fall of '38. I was told by Martin, when I first went there, that if my work was satisfactory, I'd be moved up to an assistant professorship with a salary increase to $3,000 at the end of the first year. So in the spring if '39, I went to see Martin and asked him if my work had been satisfactory. He said, ""Yes, it has been better than expected." I thought that's all I had to say. Well, fall came along and my first salary check in the fall was the same as before, so I went to see him. I said, "I thought I was going to get a change to an assistant professorship and salary increase if my work was satisfactory." He said, "That's right, but it's too late to talk to me about it now. Nothing can be done about this 'til next year." I said, "Well, don't you remember I asked you about this in the spring?" He said, "Well, that's not the way you go about it. You have to come out and ask about it specifically."

So I was stuck in Canada at $2,500 a year, with prices going up and two children by this time. One of them was only a year old. I remember the cigarettes used to be 25 cents for 25 cigarettes. They had a different kind of package than we had down here. They started taking out cigarettes one at a time from the packages and keeping the price the same, and sticking cotton in the place of the cigarette, until they got down to about 15 cigarettes for 25 [cents]. Then they stopped doing that, and they went back up and filled up a package with cigarettes, and increased the price to 40 cents, I think, something like this. This is the way things went very rapidly.

So I managed to get to a meeting of zoologists, the American Society of Zoologists, in Richmond, in 1940. On the way back, I stopped to see my friends from Woods Hole that I knew here in Washington, then members of that group at the Bureau of Fisheries. One of the women, Mrs. Wagner [sp?], had just been out here to NIH, which was just opening up, and she was a registered nurse and had come out to see whether they had any opening on the staff for nurses at the time. She was told that they had plans for a clinical center some day, but they didn't have actually any use for nurses at the moment right on the grounds, but they were looking for research people. So she suggested that I check.

So I wrote to the Director of the National Institutes of Health, and I got a letter back from Voegtlin, the Director of the Cancer Institute, inviting me down for an interview. So I went to the bank and got money to pay my bus fare down and back and a little cash, and came down for the interview. I said my reason for the trip was to visit the National Institute of Health. I didn't say what for. I was offered a job on the spot. So the next time I went to the bank, I told the banking clerk that I'd been offered a job down there. He said, "Well, I'm willing to bet if we had known that that's what you were up to, we would have never given you the money."

Then I went to see Martin and told him that I was accepting a job at NIH. So he said, "Well, stop in and see me tomorrow." So I went in, and he said, "I suppose that you wouldn't consider an assistant professorship and an increase to $3,000 now, would you?" I said, "No." They could have done it all the time, but the Chancellor had said at the beginning of the war that the salaries were to be held at their present level for the duration, but it turned out that it was not that way.

Q: That would have been a long duration, wouldn't it?

DR. DALTON: Yes, first, Martin said, "Well, if you're leaving, I suppose you will resign as of June 1." I said, "I suppose so." Then I went to see the acting dean, who was a friend of mine, and he said, "You don't resign June 1. You resign as of September 1, because if you resign as of June 1, then you won't get your summer pay." Of course, I had gone up there in August the first summer and worked for them while I was getting paid by Western Reserve. Just one summer, I took a two-week vacation; the rest of the time, I worked at the university. So I felt I had this summer money coming. I went back to see Martin, and he said he couldn't see that, because I'd be getting paid by two places at the same time, and he was against that in principle. I said, "All right. I won't leave then. I'll stay here 'til the end of the summer, but I won't work for you. I'll just live here and draw my paycheck." So he said, "Well, I'll see what I can do." So a few days later, he came down to me and said, "Well, it's all right. You get your summer pay, and you can leave here any time after June 1." We did leave in June and went to New London. Then I came to work here as of July 1. A year or two later at an Association of Anatomists meeting, I met Charlie Leblond, who is now a professor there at McGill, a professor of anatomy. We compared notes. He took my place up there, really. I was warning him about the financial capabilities of the school, of McGill, that it was certainly living up to its name. So I told him about this episode. He said, "You know what happened to me? They offered me a bonus if I were to come up in the summer and work for the summer to get things started." And that bonus was my summer pay. They wrote back later to tell me they were sorry, they couldn't give it to me. So I didn't leave the place with a very good taste in my mouth. That was, of course, just being naive on my part, but I know universities do this all over the place one way or another, to one degree or another, but you are paid what you're worth or what other people think you're worth. You can also use this to increase your salary where you are, usually. But I didn't have that attitude, and I've never had it here.

One of the most pleasant things that ever happened to me in my life, I came on a Cancer Institute Fellowship, a Cancer Institute fellow, and I started working in July. In March of 1942, Voegtlin called me down to his office and said, "Would you like to become a regular member of the staff?” I said, "Yes, that's what I'd like very much." So without any further ado, he said, "Well, sign these papers." I didn't even read them. The next thing that happened, I was a member of the staff and with a grade increase. He didn't even mention this, but apparently I had signed the papers, and he had put through a grade increase without telling me about it. I went down to thank him, and he said, "Well, you're worth it." That was the first time in my life that I ever was given a raise in salary without having to fight real hard for it.

After I'd been here a while, I had to fight a little bit, because I was the first cytologist in government, and nobody knew how to write job descriptions.

Q: Is that right? In the government?

DR. DALTON: Yes. They didn't have such a classification. So I had to write up my own job descriptions, and then somebody in the classification section would have to make a decision as to what job level it was. But, nobody had any know-how about this field of cytology; it just wasn't a thing that existed. Pathology, but not cytology. Those early days here were, for me, quite pleasant. There were several fellows who came in, like Heston and Bryan, came in a year or two ahead of me, and the old gang from Boston, Andervont and Shear and Lorenz, Johnny Hartwell, and the group from down here, Earle and Morris, Thompson. This was an established, functioning place when I arrived, with Voegtlin as the director.

Q: The building was already up and everything was going?

DR. DALTON: Yes. Earle had already begun to function as only Earle could. I don't know whether he had the impression that I had come to work with him, but when I talked with Bergmann, Bergmann gave no indication of this. He had hired me as an independent investigator, responsible directly to him. But he got me some space in one of Earle's rooms, the third floor, southwest corner room in building six, and I had half of that room. That was the idea. I started working, beginning with some of Earl's tissue cultures of material, and then I also started to work with Red Stewart's group, particularly Jesse Edwards and some with Jay White in biochemistry. I wrote one paper with Earle on the [unintelligible] apparatus in [unintelligible] in one of his tissue culture, and he insisted that it had to be in his series of papers. So I never worked with Earle again. I worked indirectly with him, but he had the feeling that anything that was done in connection with his group, that work had to be in this series of papers so people could find them readily in the literature. I worked more with Jess Edwards on hepatomas in mice and rats, and the cytology of these tumors.

Q: What were you trying to do?

DR. DALTON: What we were trying to do was to use classical cytological methods as means of identifying and distinguishing different types of tumors to a more accurate degree or extent than you could do with straight pathology or histopathologic methods. I think we did succeed to some extent in making distinctions, for example, between a spontaneous hepatoma and one induced with chemical azotoluene. You could tell by looking at the cells how they originated, whether it was a spontaneous hepatoma, as in the C3H mouse, which is of unknown origin, not viral, certainly, distinguish between that and one induced with azotoluene. Primarily, the mitochondrion in the cells of two types of hepatomas were different. And giving the detailed cytology of the series of different tumors that were being carried in the institute and new ones which developed. So I worked some with Andervont, some with Jay White, some with Morrie Barrett, and some with Edwards. The techniques of classical cytology just hadn't been used, and no effort by anybody had been used, it--just hadn't been done. A little bit in England in cancer research from people, but not much. I remember in the early war days, Red Stewart's group of Jess Edwards and Lippincott and Murphy and Grady, they sort of functioned as a team under Stewart. We used to have some parties. I remember one time going with Lippincott. We did not have a car at the time, and we started out going to Lippincott's place. She was a nurse in some old folks' home down in Washington; I forget where it was. We had to sneak up the back stairs quietly to their apartment for a few drinks. I remember we came back down after that, we were going to go to eat, and Red Stewart was trying to get my wife to go in his car, and I wasn't about to have it that way. Stewart wasn't used to having people disagree with him, and he took a swing at me. I ducked, and he hit Jess Edwards in the eye. Then I got behind Stewart and grabbed his arm. This was on a Saturday. On Monday, Edwards showed up to report to Stewart on Monday morning with this beautiful black eye. Stewart said, "Where in the hell did you get that, Edwards?" Stewart said, "I've got black and blue spots on my arms, and I haven't the slightest idea where I got them." There was quite a lot of good morale and a lot of hard work done by that group. Then of course, the war split it up. They were all M.D.s, and they all went off pretty early. Stewart, of course, went to Letterman General, head of pathology on the West Coast, and Edwards was overseas part of the time. Morrie Barrett was a surgeon on the Western Front. I forget where Grady and Lippincott went. They all went in the Army, the military somewhere, and none of them came back here. Ed was at Mayo Clinic, Grady's in Philadelphia, Lippincott is somewhere out west. I saw him at a recent meeting. The only one that came back was Red Stewart.

Q: These fellows you're mentioning, how long had they been with NIH? I'm asking that because I'm wondering, while you're talking to me, if it would be worth my while to look them up. If they were only here for a year or two, it probably would not be. But if they could tell me something about the early Cancer Institute, then it would be.

DR. DALTON: They were all gone from here by 1943.

Q: Do you know when they arrived?

DR. DALTON: They were all, most of them, here when I arrived. I think Stewart is mainly responsible for having hired those fellows. It was a pretty smoothly functioning group by the time I got here in '41, so they must have been hired at least a year before.

Q: I see. But it wasn't very long, though.

DR. DALTON: No.

Q: You'd say just from '40 to '43, something like that.

DR. DALTON: Yes. Then with all these fellows leaving and being replaced, I remember Jay White, he went. He wasn't an M.D., a Ph.D., but he volunteered and went overseas. His wife took his place. She was in biochemistry. Then Thelma Dunn came in to pathology. Eschenbrenner came in from another position in the Public Health Service, and took over Stewart's rooms. I began to get itchy feet, too. I felt that I was pretty healthy and probably could do some good somewhere, but I went across the street to the Navy and talked with them there, and then I went downtown. They all were very interested when I talked with them, and then I got word back, "Sorry." It seems that what had happened, when this first exodus occurred, NIH developed an unwritten understanding with the other services to the effect that there would be no more proselytizing of the staff, and I couldn't get out. They wouldn't let me go. So I stayed here during the war and worked on the effect of low oxygen tension on animals. The main thing we did was demonstrate was: number one, that male rats could be exposed chronically to altitudes that it was very difficult for them to take physically with low oxygen tension--they would get gastric ulcers and they had a decrease in testis weight and so on, and the actual loss or cessation of sperm formation. But, then after you do this for six months, they would get more or less adapted, but not quite. As long as they continued to have the stomach ulcers, there would be no sperm production.

Q: What was the purpose of this? Were you doing this for the Air Force?

DR. DALTON: There were stories about what happened to pilots flying the hump to China, that they lost interest in women and they had constant dyspepsia and that kind of thing. Apparently rumors were going around that if you'd do this too much, you'd become sterile. We proved that these rats, at least, would recover after long periods of this. They could eventually become normal male rats again. This information was then sent out eventually as evidence that while there might be some temporary damage, it was not a permanent thing. They never could be sure, at least the senior military men in the Air Force, whether these men took this attitude because they needed every bit of their abilities, physical and mental, to carry on their job, and that going out on the town in between was almost committing suicide because it was such a rough flying job, anyhow.

Q: Did you continue this whole project over the war, or was this just something you worked on?

DR. DALTON: I first started working with a commissioned corps man, Jones, in building two. They had an assignment for the study of the effects of high altitude. They put in that tank over there next to building two, you know. They ended up by studying human volunteers, too, but at the time they didn't have anything big enough, so all the work was on experimental animals, and that's the work I did. I did the histology and histopathology of these animals, as well, and got the first evidence that you could get a sterile heart valve injury from these conditions if they were severe enough. The heart action was so fast and the valves were utilized so continuously and rapidly, that you would get actual physical damage, not bacterial infection or anything. You'd get a swelling and thickening of a valve and eventual malfunction as a result of this. That work was carried on later on by [unintelligible] over there.

I remember Mr. Voegtlin saying, when I went down to see him to let him know that I knew that arrangements had been made to keep me there, among others, he said, "Well, when I took this job, I didn't intend to become head of a women's seminary." He just felt it was essential to keep some of the male staff in the place for the duration of the war.

Q: I can realize that. Gee, if everybody had left, it would have ruined the place, wouldn't it, for some years?

DR. DALTON: Yes.

Q: It would have taken a lot to build it back up again.

DR. DALTON: Yes.

Q: After the war, then, what did you go back to doing?

DR. DALTON: I'd like to go back a bit now to what I talked about, the amateur radio activity. In Cleveland, my wife was about to have our second child, and I was tutoring at night and got a fair amount of money together to take care of the expenses, and I had Dr. Bill, who is head of the Children's Hospital here, was the doctor. After everything went nicely and my son, Bob, was born, I went down to thank him and to pay his bill. He said, "Don't pay me anything. You have to pay the hospital bill. When you get rich, donate something to the hospital." So I had this extra money left over, and my wife, I think, bought a phony coat and I bought a receiver and parts for a transmitter, and then got back on the air. This was in 1937. In '38, I moved to Canada and I managed to wrangle an arrangement to get on the air there. But then the war came, and I had to quit. I came down here in '41, in July, and I got on the air down here and had to go off in December, of course, of '41. So my amateur radio activities had been pretty badly cut up all along. So after the war, in addition to continuing work in straight cytology, I worked with Harold Morris on cytology and histology of changes in the thyroid gland and other organs with thiocresol treatment, that is, the result of prolonged stimulation by a thyrotropic hormone. This work was going on very nicely.

I got my equipment back together and got on the air in about '46. Burt Kahler got one of the first electron microscopes here on the grounds, and I started working with him on tissue fragments and so on, with his electron microscope in '46 or '47.

Then in '48, Granger and Baker had an anatomy meeting in Philadelphia, demonstrated that you could get sections soon enough to see something in the electron microscope, and this was really the beginning of the whole era of the use of electron microscopy and the study of ultrastructure of cells. The next year I worked with Newman Briscoe and Swerdlow, testing out their samples. They had developed the method of imbedding methacrylate in the plastic. They were doing it in order to study the details of different types of fibers, textiles and fibers and so on. They tried out some cells, tissues to study, and I checked them for them in these early days. This was 1948-49.

Then I got my own microscope in 1950. They happened to have some money left over, I remember, that year, and Mider asked me if I would be interested in getting an electron microscope of my own. I said, "Yes, but I don't have any room for it." I only had the one room. So I asked all around building six, all the people on the staff, if they'd be interested, if they had the space, to put in an electron microscope, I'd share it with them 50-50 if they could let me put it in. Nobody could do it, so I just curtained off a part of my room first, and then later I had a filled-in regular wooden partition.

Then I had an animal room next to my regular room. That, incidentally, was Stewart's old room on the third floor. I partitioned off part of the animal room for a dark room. (I'll tell this story, anyhow). We had set our enlarger in a certain position in there, and it was very awkward to work at this way. Marie Felix was working with me then, felt that we ought to widen the dark room by moving it out just another foot. This would fix it so you could have the enlarger in the proper position in there. So I put in a work request for this, and Mider turned it down because he didn't want to see any more space being taken over by photographic facilities. There was a central room, and I didn't really need the space. So he refused to let it go through. He went on vacation, and while he was on vacation, Walt Magruder approved it, and we got it done by the time Mider got back. The next time he came into my room—he was very much interested in what we were doing—he came in, he looked around and smiled. On the way out, he said, "Some changes have been made, haven't there?" There wasn't anything he could do about it then.

From that period of 1950 on, I started right away with the scope and with sectioning with Ben Elliot. He got to be very good at cutting thin sections with a steel knife at first, and then with a glass knife. I remember looking at my first section of a pancreas cell in section and seeing all that network of membrane system. This must be an artifact. I really couldn't believe it. But I did publish, eventually, pictures on the intestinal epithelium and on the pancreas and so on. They sort of held up.

We originally used to take the plastic out of the section, but that did a lot of damage. Ballotti demonstrated that you could get much better results by just leaving it in, and you'd get more faithful representation of what was there. But I started, at the beginning, to look at tumor cells. There was so much that was new, that I just couldn't interpret it. I felt I had to spend some time learning about normal cells, so I went to normal cells, and we had no real techniques developed, no fixations. We had no basis to build on. We'd have to build a base first before we could make any interpretations. We had to learn about artifacts and what were really artifacts and what weren't, what was real and what wasn't in what we saw. There was a group at Rockefeller, a group [?] at Bernard, and a group with Oberlin, and a group with Shustran [?] and Karolinska, and our group here, were the main people doing work in those early days.

My radio background just fitted in very nicely here, because in those days you had to know something about electronics in order to keep a scope running. They didn't have enough knowledge or enough good servicemen to do it, so if something went wrong, you had to know how to repair it. Otherwise, you'd just sit and wait for over a week or two before a servicemen might come around to fix it for you. So my practical knowledge and the certain amount of theoretical knowledge that I got out of the ham radio came in very handy here. Washington was sort of on the outskirts then. New York was the center. Of course, RCA wasn't very far away from New York, from the Canon group. But both geographically and also from the point of view of significance, Washington and NIH wasn't as important in those days as it is now. So I didn't get much help. But we did have one good man, Lee Cochran, who came as often as he could. Unless it was a real emergency, I wouldn't see him for long intervals. But I kept that scope going and getting about 20 angstrom revolutions with it for a good many years.

Dave Scott got active about that time, too, in the mental institute, but his approach and his interests were quite different. He was interested in piece and hard structures, and I wasn't as much interested in that. Of course, we had Burt Kahler to help, too, the physicist.

So that period of time from 1950 to about '58 was the period of time when I just worked on normal cells, studying and learning about them. It was also the interval when we did the work on the [?] apparatus and proved that it was not an artifact, but at least a fair number of cytologists had thought it to be.

Then about 1957, I started getting into some viruses and saw that this was going to be one of the big areas where the electron microscope could really contribute. As techniques improved and the capabilities of the electron microscope improved, we would get more and more information on morphology of viruses, their life cycles, and so forth. So in 1957, I put in a request for a Siemens scope, because I felt that although this was a good machine, it just didn't have the capabilities and versatility that the Siemens scope did. I had seen one in action up at Sloan-Kettering and at Rockefeller, so I put in a request for one. Maybe I didn't give enough ammunition to Mider, who, by this time, he was still Director of the Cancer Institute at that time, but he took it to Shannon’s office. Who was the man, the virologist who died not too long ago of cancer?

Q: Smadel?

DR. DALTON: Smadel, yes Smadel turned it down on the basis of the fact that there was an electron microscope at NIH, which was not being used to its fullest advantage, and until it was, he wasn't going to approve the purchase of another one for anybody. It wasn't personal with me. He said that they thought that I was doing all right with the machine I had, but they weren't going to buy another one. I still have that memo.

So I wrote a letter to Sloan-Kettering to find out whether they would be interested in my services up there. I thought that if they here couldn't see what the future was in this field, then I didn't belong here any longer. So I went to a meeting. When I came back from the meeting, there was a telegram on my desk telling me to stop, that they'd very much like to talk to me at Sloan-Kettering. They don't proselytize, of course, but when a person makes the gesture, why, they'll follow through.

So they offered me $15,000. They couldn't quite at that time offer me a membership, but they promised me that as soon as one became available, I would have it. I didn't argue or talk with anybody, but apparently Mider got word of it, and he called me over to his office and said, "Instead of leaving here, how about going up there on a year's leave of absence and see how you like it? Then make a decision at the end of the year." He said, "In the meantime, you might resubmit that request for the Siemens." So I resubmitted it, and it was approved in four days.

But I went up to New York, anyhow, for a year. Mider's advice was very good, because I just couldn't stand it. I was used to living in Rockville for many years with a backyard. Whether I went in the backyard much or not, the fact that I could step out the back door. I lived in the apartment house across the street from Sloan-Kettering, and all I did was go down in the morning in the elevator, cross the street, and go up the elevator on the other side to work.

Q: Incidentally, you keep mentioning Mider. I thought you were with the Cancer Institute.

DR. DALTON: Mider was Director of the Cancer Institute.

Q: Oh, I thought it was Heller and these other people.

DR. DALTON: No. Voegtlin retired just at the end of the war, around '45 or '46, and then we had then Spencer, Scheele, and then Eagle, and then Mider.

Q: Eagle was the scientific director.

DR. DALTON: Yes.

Q: Was Eagle the scientific director? I thought you meant Heller, the head of the Institute.

DR. DALTON: No. I seemed to have quite a lot of dealings with Mider all the way along. We've been good friends, too, socially, and I learned fairly early that Mider's first reaction to anything is "no." You go in with a proposition with him, and his first reaction is, "No." Then you give him a chance to think about it, and then he may still say no, or he may, after having thought it over, change his mind.

Q: I wondered how you got up to Sloan-Kettering. I have a copy of your *curriculum vitae* here, and I was just wondering how you did it.

DR. DALTON: The organization up there, the physical environment, is quite different from here, in that I had complete charge of people working with me. I could have fired any one of them with two weeks' notice, and I could hire anybody I wanted within the limits of what was available. I didn't like this. It's too much responsibility for somebody working at the scientific bench to have and to do research, and also to have practically the power of life and death over the people working under you, as far as whether they stay on the job or not. This is a little too much. I think it's important for a person to have the right to say who's going to work with them when you're starting off an organization. If somebody doesn't work out after a trial period, they have to have the right to have them removed. But there's too much being a director of the organization instead of just head of a research group.

Q: May I ask you a few questions? Could you tell me anything about the reasons why the cellular biology section was formed in the Laboratory of Viral Oncology?

DR. DALTON: The original background of the whole thing was that Eagle decided, when he was director, to bring in H [?] and Schneider from Rockville. They were a good pair, and they did this work on differential centrifugation of mitochondria and so on, and so he arranged to have them brought in. To do this, he had to establish a new section, and he had it set up in biology with Andervont. I went into that with H [?] and Schneider, and they added Ed Copp. Emma Shelton, I think, was in it for a while. This was all under Andervont. The biology laboratory got to be too big, and so it was divided into two parts. When Andervont gave up his lab chief job, biology was divided. One part remained as the Laboratory of Biology under Heston; the other part became the Laboratory of Viral Oncology with Bryan. The same side of the biology section, which I eventually became head of when H [?] died and Schneider transferred to biochemistry, and I became head of cellular biology.

Q: But that was something that Harry Eagle started, then, right?

DR. DALTON: Right. Now cell biology is really with Manaker, who is head of it.

Q: How about the background for the reasons of the establishment of this new laboratory, the viral carcinogenesis?

DR. DALTON: Sort of the same situation. The Laboratory of Viral Oncology became quite large, all the emphasis on virus and cancer and particularly leukemia. I had become completely involved in virus studies, morphology and so on. The electron microscopy grew because it became very obvious very early that you had to have some other means of identification or determination of the presence of virus other than bioassay, because bioassay takes so long. By this time, we had come to the point where we could identify or recognize at least the mouse leukemia virus.

I had my original two scopes. Eagle [?] came in just about the time I came back in '59, and eventually I got a Siemen's for him.

DR. DALTON: Then we had developed this program with Maloney and Frei and myself on studying bugs, leukemic cases, carrying on from the work on mouse leukemia, using this as a laboratory system. We decided to do the studies on the human and mouse the same way. It's a very time-consuming business and takes a lot of a technician's time and a lot of scope time to follow this. So we added two more RCA scopes to the group, so that it amounted to my scope and Eagle’s [?], research scopes. It was planned to have the other two function at least in good part in guiding the program in terms of if we found particles in fractions of, for example, plasma, then these materials would be used in inoculating animals, monkeys and so on.

This screening program we started out here, but the problem is, if you get a person who's good enough and has the brains and capabilities to do independent work and somebody who's capable of identifying these things, he's not going to be interested in doing nothing but a screening program. They have to have a certain freedom or leeway to work out things that might develop and might attract their interest. Otherwise, they would leave. So then we got the contract with the Pfizer people to do a good bit of the screening. We still do some of that. We do the screening on things that are the most likely, we think, to be interesting to the general program, but the routine screening is being done at Pfizer now.

With the buildup in electron microscopy that was practically essential to any real movement in the program with what became obvious. The need for much more tissue culture work specifically aimed in this area, rather than the type of work being done in Earle's group, the one that Earle headed, in which Katherine Sanford and Virginia Evans are more or less in charge of now, it just got bigger. Manaker's group grew, and Sarah Stewart, while still the only top person in her group, she had a lot of people working for her in her program. This meant that viral oncology was getting too much for Bryan to handle, particularly when he was handed these other jobs, the chairman of this task force, and becoming an assistant director, social director or whatever it is.

So it became necessary to split again, and I was put in charge of carcinogenesis with Dr. Stewart and Manaker, having sections within the group. I'm still in charge of a section which is now called electron microscopy.

Q: You mentioned Harry Eagle a number of times. I'm interesting in finding out what sort of a person he was as a scientific director of the Cancer Institute.

DR. DALTON: He started out in a very difficult situation where decisions had to be made and when he didn't have the knowledge to make them, but he had to make them just the same. He had never been in cancer research, and yet he came in as director. If Ray Bryan ever—well, I don't think he'll ever have a nervous breakdown, because if he was going to have one, he'd have had one when Eagle became director, because the first thing that Eagle said was, "There's no reason why people should be concerned about the relationship of virus and cancer; it just isn't so." He was trying to force Bryan into some other area of work, to get out of virology and get into something else. He was about to cut down his space and budget and personnel and everything else. He also couldn't see much in what I was doing either in electron microscopy. It was, of course, just getting started at that time, but he didn't make any gestures about reduction. He thought there might be some usefulness in it some way, maybe in pathology. But he's a very bright fellow, and in about a year's time, he began to catch on. Then also in a year's time, he began to realize that being Director of the Cancer Institute is a full-time job, and he couldn't do research and be a decent director. So he finally resigned and went back to research. I would say that the latter part of the time when he was director, he was doing a fine job of it, but the first year was rough on most of us.

Q: How about Voegtlin? What sort of a director was he?

DR. DALTON: He was a very good one, I thought. He was a Swiss, a German Swiss, and he was a real autocrat, no question about that. He inherited some people and he hired others. Among those that he inherited, there were some that he thought a great deal of, and others he didn't. Those he didn't think much of, they didn't get very far, didn't move up very fast under his control, but he was a very shrewd fellow. He had a broad background in pharmacology and in chemistry and pathology, too. So that if anybody could function as the director of an institute like this and have 30 people, individual investigators, being directly responsible to him, I'd say was the man. He had Ora Marshino, who was sort of his protectoress, and she sat in front of his door and wouldn't let anybody in that she didn't want in there. I saw her not too long ago since she's retired, and she's working on something of a history of the place, too, in her own way.

I remember the period there when I was working with Murray Shear, when the first chemotherapy work began. I was trying to set up a system for making determinations of the effect of compounds on tumors by examining them histologically. So I set up a dictaphone, and I'd read the slides and give the results to the dictaphone, and the girls down in Marshino's office would type up the records for me. Sometimes when I'd come back from a meeting, I'd stop in the middle or finish one and just keep right on with a dirty joke, and that would go downstairs, and the girls would have the earphones on, and they'd always start laughing. Ora Marshino never did figure out why, because as soon as they finished the thing, they'd erase it. That was quite a bunch of girls. Carol Ann Mullinix[?], who got married and left, and I don't remember the names of some of the others.

Q: How about Mider as a scientific director?

DR. DALTON: He was very good. You always knew where you stood with him. I mean, he made mistakes, I guess, but they were minimal compared with the right decision. Incidentally, now I know he boasts about the number of electron microscopes that are on the grounds. He's a good man. I don't think anybody with Mider ever really suffered because of any bias.

Q: I noticed that you were a lecturer at Johns Hopkins in 1950. What did you teach down there?

DR. DALTON: In 1950, McCordle and I went over. The head of the Department of Anatomy practically fired all of the people there when he moved in, and he didn't have the staff completed at the beginning of the school year, so he asked if there were any people on the staff here who could help out in anatomy laboratory, primarily histology. They finally agreed to let "Mac" and me do this. I had a lot of annual leave left over because we couldn't take any during the war, so we went over three times a week, the mornings, and then came back and worked in the afternoons three days a week.

Then they let us do it the one year, and they turned me down. I asked to go the second year, and they said they were sorry, but they felt that Hopkins should have been able to staff the department in a year's time, and they felt it would interfere too much with my work to continue it. Then they agreed to let us teach in the School of Public Health. I did that for two years, but this was at night and Saturdays, where it didn't interfere with daily work.

Q: Did you ever have any problems getting funds for electron microscopes?

DR. DALTON: Funds?

Q: That's right. They're pretty expensive, aren't they?

DR. DALTON: In the government, I don't think there's even been any real lack of money to purchase them once the understanding of the capabilities of the machine became general on the part of the administrators.

Q: Have you had difficulty getting people who knew how to use them?

DR. DALTON: Oh, yes. See, there was a time when Cornell gave a course in electron microscopy, but it was a relatively short one, and just in the summer for two or three weeks. Still, the only way now to become really capable at interpretation of electron micrographs is just working at it. You can get the fundamentals of the use of the machine and upkeep and how to take a good picture and so on, in a relatively short time. I'd say you can do this in about a year, a year and a half. But the interpretation is something that you just have to learn to develop over a period of time, and we're all still learning in this area. You never quit improving yourself in this. I understand now there's something like 17 RCA machines on the grounds, and they have one RCA serviceman for just NIH now; he's never off the campus. The Siemen's people have a man in the Washington area, and right now there are four Siemen's installed and there are three more going to be installed in the coming months, the next two or three months.

Q: Your colleagues who work for you on these things, what have you had to do, train them yourself?

DR. DALTON: Yes. Ben Elliott, an interesting thing, he came with me in 1943 from the Messenger Service, came in to try out as a histopath technician. He learned the cytological techniques as we developed them in those early days, and then when electron microscopy came in, he learned all of these. So he dissects fixes and dehydrates them in beds of tissues and cuts and stains the sections, and he's a whiz at it. I couldn't ever expect to get anybody better. He's been with me all these years, 21 years now. He has trained other people who come in. In fact, he's training [unintelligible] right now in the techniques. People like him are harder to find than professionals. Really, because many universities now and medical schools, people are getting training in electron microscopy, and, actually, I get a fair number of requests and inquiries about job openings from people who have been trained, and I have two people with me right now who are from Washington University in St. Louis, who were trained there, who are quite capable, and another woman, Dr. Currington [?], just came with me the other day, who is eventually going to work with Paul Coten [?], but is going to be with me until his lab space becomes available.

But the technicians, there's no technician school in electron microscopy, and not everybody can do it. Manual dexterity has to be of a high order in order to run the machine, and you can't make any mistakes with a diamond knife. If you're just the slightest bit rough with the knife, it's shot, and that's $300 to $400 gone down the drain. You can break them easily, you can ruin the edge and they have to be re-sharpened for 100 bucks, and it has to be out of service for several weeks while it's being re-sharpened. Up to now it's a problem of a diamond knife or a glass knife, and there's a trick about breaking the glass knife right so that it will cut well. So you can't be sure, if you hire somebody, they may be intelligent and hard working and apparently capable, but they won't necessarily make a good EM technician. Usually somebody who's good in histopath techniques, cutting paraffin sections and who does this kind of thing well, is almost certainly going to do well with an electron microscope. It's just a more refined approach to the same kind of problem.

Q: When you learned the techniques of this operation yourself, did you have much difficulty? You were the first one around here to have one of those, weren't you?

DR. DALTON: I was the first to cut sections around here.

Q: Was there an earlier electron microscope than yours?

DR. DALTON: The one that I worked with, with Burt Kahler, his was one of the first. I don't know whether his…

Q: He's dead, isn't he?

DR. DALTON: Yes. Whether the fellow over in building two, who retired sometime back, I always have trouble recalling his name, he was a commissioned corps, and I think he went out to the University of Colorado. I'm not sure. He was the one who developed the system of correcting stigmatism in the electron microscope by putting iron screws in around a little collar that fitted into the pole piece. I can't remember his name, but he might have had a machine here a little ahead of Kahler, but one of the very first ones that was put out by RCA when Hillier was in charge of their manufacture. I'm just not sure.

Q: How long had Kahler had his when you started using it?

DR. DALTON: He got his right after the war, almost immediately after the war, around early '46. RCA had made their first machine in 1940 and put them on sale right after the war.

Q: What was Kahler doing with it?

DR. DALTON: Kahler was a biophysicist, and he used it in the study and did some work on viruses. Those were the days when you put the suspension of material on a grid and shadow it, maybe, or stain it in some way and then look at it. There was no sectioning. I remember he was the first, I think, to demonstrate that certain viruses will stain with a urinal salt. It was some virus, I forget which one, that he worked on and it stained up very brilliantly with the urinal salt, but then he tried some others and they didn't react. So there was not a general characteristic of virus to stain that way.

With the creation of the chemopath course and then this new appropriation for Cancer Institute for virus and leukemia, it has become quite hectic in the last few months. You see, the idea is that if you, as an individual, take over part of the job of organizing and getting this thing started, they're going to supply you with a person to do your research for you while you're doing this other job. Well, this doesn't work that way. I can't turn my research over to somebody else. There is not many people that have been at it as long as I have. There's, no doubt, a lot that could do the job better if they had the amount of background I do, but they just aren't around. So I can't just turn it over to somebody else. I have to fit it in odd moments when I can get to it. This whole period since, say, 1950 and continuing into the present has been a really exciting time in electron microscopy. It's probably more exciting than it was when the compound microscope was first put into use.

Q: Is that right?

DR. DALTON: Yes, because we have an increased resolution of 100 times over the best the light microscope can do, and that's a lot. The compound microscope went from 100, that was about 16 times increase in resolving power, from about 100 to 1,600, and the ability to resolve something in the neighborhood of about two-tenths of a micron. We've gone up to a resolution of about five angstroms, with 20 angstroms being the average. That's about a hundredfold increase in resolution. When this sort of thing happens in any of the natural sciences, it's bound to do something, like the shots at the moon getting up that close. They still haven't learned all that they expect to learn out of those pictures. When you're able to resolve cell components practically down to macromolecules, it's a big advance. The new techniques that are being introduced now are labeled antibody technique and radiography in electron microscopy, with a resolution that's down now, I guess the resolution is about around 100 millimicrons. A lot of things happened. We're beginning to be able to follow the movement of viral nucleic acid into the cell and see where it goes, follow the process of the life cycle of the circle.

Q: That's very interesting. I guess along with all that, too, there has had to have been development of new methods of sectioning and so on.

DR. DALTON: The sectioning has gotten to be as good, I think, as we will ever need, really, because if you really try, you can cut sections about 60 angstroms thick and routinely cut them at 200 angstroms.

Q: Is this with a machine?

DR. DALTON: Yes, with a ultramicrotone, thermal expansion system, and the sections comes off on the surface of a fluid, and you pick them up on a grid. At this thickness you're cutting viruses, you can get two or three sections of a virus, or the big ones of 10 or 15 sections through one. So you can get the internal structure of the organization of them. Metal stains have been developed so that you can increase the contrast and selectively stain, to a certain extent, cell components and viruses. I'm sure the day is not far away when we will be really selectively staining different types of protein, depending upon their complexity and organization and available reaction points. You have to still remember, though, that the electron microscope is a tool. It's not going to do anything by itself, and an electron microscopist can't do anything by himself either. Either he himself has to be, in addition to an operator of a machine, he has to be something else to make the machine produce; he has to be basically a cytologist or a biochemist or a geneticist, virologist, and apply the tool, the electron microscope, to the problems in this field.

Some people, like Brenart [sp?], is already saying that we've reached the end as far as the descriptive story of cells and biological material is concerned. We have to go now to development of histochemical techniques and to use of radiography in labeling antibody techniques. This is, in a sense, true. I think that people ought to be working in developing the techniques in these areas, but there's still a world, an extremely large area where simple descriptive studies are still of great value. For example, it's really just a descriptive study to study the structure of DNA. You can see DNA molecules with the electron microscope if you make proper kinds of preparations, and this is purely descriptive, but it's an important thing that needs to be done. I keep turning up things when I'm looking in the scope, for one thing, finding something else with considerable interest, not that the thing I'm doing isn't, but the other thing is another lead-off in another direction to something else. This goes on all the time.

Q: This sounds very interesting.

End of interview