Dr. Floyd S. Daft

An Oral History

Director, National Institute of Arthritis and Metabolic Diseases
National Institutes of Health Bethesda, Maryland

Interview with
DR. WYNDHAM D. MILES Historian,
National Institutes of Health
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DR. FLOYD S. DAFT

Dr. Floyd S. Daft was born on 19 May 1900 in Griswold, Iowa. He received a B.A. degree in 1921 from Simpson College and a Ph.D. degree in 1926 from Yale University. After receiving his degree, Daft was engaged in research work at Harvard, Yale, and the University of Rochester, and at the Carlsberg Laboratories in Copenhagen, Denmark.

Dr. Daft first joined the NIH staff in 1937 and the Commissioned Corps in 1945. He has directed and conducted nutrition studies, particularly on the B vitamins, and his investigations have contributed importantly to the understanding of dietary deficiencies causing anemia and cirrhosis of the liver. He was a pioneer in the study of an unidentified substance later shown to be folic acid.

Dr. Daft served as Director of the National Institute of Arthritis and Metabolic Diseases from 1953 until 1962.

During the course of this interview Dr. Daft discusses his family, childhood, and early education, followed by his training at Yale and his year at the Carlsberg Laboratory. His time spent in research with Cecil Drinker at Harvard and with George Whipple at Rochester is also discussed. The interview concludes with his decision to come to NIH, and his initial research work building on the discoveries of Joseph Goldberger and W. H. Sebrell.

Name: FLOYD S. Daft

Born: Griswold, Iowa, May 19, 1900
Family: Frances Pauline Miller Fisk Daft (Wife)
Education and Degrees:
Simpson College, Indianola, Iowa: B.A. 1921
Yale University, New Haven, Connecticut: Ph.D. 1926

Abbreviated CV

1926-27: Travelling Fellow, Yale University: S.P.L. Sørensen, Carlsberg, Copenhagen, Denmark

1927-29: Asst. in Biochemistry & Physiology, Harvard University, Research & Teaching

1929-31: Eli Lilly Fellow, Yale University

1931-37:Assistant & Instructor in Biochemistry and Experimental Pathology, Univ., of Rochester School of Medicine, Research and teaching.

1937-present: National Institutes of Health, PHS

Organizations

Member, Staff, NIH-Civil Service
AppointmentL1937-1945.
Commissioned Officer, PHS, since 1945
Asst, Chief, Div. Physiology, 1946-1948
Chief, Lab. B&N, EBMT, NIH, 1948-50
Asst. Dir., EBMI, NIH
Actg. Dir. NIAMD, NIH, 1950-51
Assoc. Director, NIAMD, NIH 1951-1953
Director, NIAMD, NIH 1953

Dr. Floyd Daft, Former Director
National Institute of Arthritis and Metabolic
Diseases
National Institutes of Health,
Bethesda, Maryland

Date: October 14, 1964
Interviewer: Dr. Wyndham D. Miles

Q: Dr. Daft, would you start off by telling me when and where you were born, and anything you wish to about your childhood?

DR. DAFT: I was born May 19, 1900, in southwestern Iowa, in Cass County on a farm. I went to a country school, and from there went to Indianola, Iowa, which is 18 miles from Des Moines, where I attended Indianola High School.

Q: This country school, by the way, was it one of these old-fashioned one-room schools?

DR. DAFT: It was, yes. They were not as good, by present standards, but they met the needs of the day very well.

Q: One teacher for everything?

DR. DAFT: One teacher for everything up through the eighth grade. These were scattered in the midwestern states. In Iowa, they were every two miles, so that people walked to the school up to two miles.

Q: Goodness gracious. That's quite a lot of schools across the state.

DR. DAFT: Yes. It required, of course, the teachers were local so that they, in those days, could find enough teachers. I suppose it would be impossible today. It has slowly changed. I think that all of these small, one-room country schools have been abandoned now, and they bus them in to small towns in central locations.

Q: Did you have to walk to school?

DR. DAFT: Oh, yes. Yes, we walked to school. It was about a mile, a little over a mile and a quarter.

Q: What did your father grow?

DR. DAFT: He raised thoroughbred Hereford cattle, and then the usual things that you do grow in Iowa—corn, alfalfa, oats, wheat. But there is a great emphasis on corn growing in Iowa, and then we fed that to the thoroughbred Herefords, which the Herefords are a beef cattle, a very fine-looking white-faced animal. I stayed there 'til I was 13 years old, and then my brother was attending Simpson College in Indianola, Iowa, and I went up there and went through high school while he was going through college, and stayed in Indianola and went on to Simpson College, graduated from there in 1921.

Q: Did you have any science courses in high school?

DR. DAFT: Yes. This was a very good high school, I think really an extraordinary high school in many ways. The science courses were minimal in number. I believe that the only course that I studied was physics. I'm certain that we had no biology courses and no chemistry course, so physics must have been the only science that was taught by the high school.

Q: Did you have a laboratory?

DR. DAFT: A physics laboratory, yes.

Q: You actually did experiments?

DR. DAFT: Oh, yes. The high school physics courses gave you a number of rather simple experiments in mechanics and electricity and in various areas, and gave you not a bad fundamental training in basic physics, which makes it a little easier for college physics, and that's about all you can say for it. It gives you an introduction to physics.

Q: Was the teacher that you had in physics adequate?

DR. DAFT: He was good, yes. He was, I would say, somewhat more than that. We had some extraordinary teachers in other subjects, in languages, in English, particularly, mathematics, very, very good. The superintendent was quite an extraordinary individual. I think he's the one who made the school. I don't know how this would compare to high school courses today. They were divided up into groups according to ability after the first six weeks period in school. Then each class was divided into about four groups, depending on their ability, and they were taught somewhat accordingly. I believe that one of the newer plans—is it the Amidon plan? I'm not certain which one of the newer plans has gone back to that a little bit so that you're competing, in a sense, with people who have the same interest in studying, let us say, not necessarily the same abilities, but at that age it depends a great deal on interest rather than innate ability, probably. I think it's very much better for the student, probably, than putting them in those that are going faster or going very much more slowly. He stimulated these students in a great many ways.

Q: I asked about your science teacher because in speaking to other people, I have found out that sometimes their science teacher is also the coach of the football team.

DR. DAFT: So was ours.

Q: That's not the thing they simply would expect a science teacher to be doing. (Laughs)

DR. DAFT: That's right. As a matter of fact, I taught science in high school one year, later, of course, considerably later, and I was also supposedly the coach of all athletics. I had never played varsity football, never played varsity baseball. I'd played a little basketball. So I coached the basketball myself and got a local man down to coach the football. I went with the teams and so forth. But that is very common in high schools, I think, even today. Coaches also double in some of the subjects and probably do neither one as well as they might, but that's to be expected under the circumstances.

I did not decide in high school exactly what I was going to do. I had made up my mind that I was going on for graduate work of some sort after college, but it was not until college that I actually decided on chemistry.

Q: Where did you go to college?

DR. DAFT: I went to a small college, Simpson College, in Indianola, Iowa. It has now, I think,

about 900 students or something like that, in those days around 400 students. It's a denominational; it's a Methodist-related college. I've forgotten the exact number—I heard it recently—of small colleges through Iowa, but it's an extraordinarily large number, something like 90 colleges in the state of Iowa. Many of them were church related, related to different schools, several of them quite good. Grinnell is perhaps the best known. I think Harvard University took quite an interest in Grinnell at one stage, and helped establish courses, and I think even furnished some faculty and so forth.

Q: Why did you go to Simpson, just because it was convenient for you?

DR. DAFT: Principally, yes. I suppose that the main reason that I went to Simpson was because my brother had gone there. As I say, I left my parental roof when I was 13, and went to Indianola to live with my brother, who was going to college there. He was eight years older than I, and didn't enter college until he was 21, graduated when he was 25. I had become established in the town of Indianola. As most people in those days, finances were an important problem. The students going to Simpson today—and this is somewhat typical of Midwestern colleges—they consider about $1,750 a year as the minimal requirements without incidentals. That gives a small allowance with books, $1,000 tuition, and about $700 for board and room. In those days, tuition was much less, scholarships were available. I either had a fellowship or a scholarship all the time I went to college. I think probably at no time did I pay tuition, and I worked in the summer and also during the school year, both in high school and college. This was an accepted practice in those days. They've changed considerably now from that time until this. Anyone who really wanted to get an education didn't have to depend on parents for the finances.

Q: What did you major in at Simpson?

DR. DAFT: I majored in chemistry. I took all of the courses they had in chemistry. I became interested in pursuing chemistry further during my course in general chemistry. I think the thing which perhaps was most interesting and stimulating was the fact that we learned all the facts of chemistry during the year, all about the atoms and the fact that it wasn't possible to break them up, they were the smallest particle of matter that could possibly be, and at the end of the year, you found that none of it was true, that radium had come into the picture. The challenge of this, I think, was probably a big factor. We had a very good science teacher in college, and from this small college has

gone out during this particular period quite a large number of people who have majored in science and had gone into medicine.

Q: Do you recall his name?

DR. DAFT: Yes, it was Professor Baker. He died only a few years ago. I've seen him several times during the last ten years. I went back one year. They gave me an honorary degree, and he was still on the faculty at that time. Then I gave the commencement address there a few years ago. So I have seen him a number of times, and he was given considerable honor locally and throughout the state.

Q: What kind of courses did you have in chemistry? What I'm doing is visualizing a small college here. I'm wondering how deeply they went into chemistry.

DR. DAFT: They gave you four years in chemistry. They gave you a course in general chemistry, a course in qualitative analysis, a course in quantitative analysis, and organic chemistry.

Q: How about physical?

DR. DAFT: No physical chemistry. The courses were not up to the standard of the training which one would receive at that period in larger colleges. I was roughly a year behind when I went in graduate work, in comparison with the training of individuals who had gone to Yale and to other schools and had majored in chemistry. But though limited in scope, nevertheless, the courses were well taught, and they not only gave you a fundamental basis for the science, but you had very close contact with the professor, and you became interested in it. I think the fact that it aroused interest in students was one of its very important things, and I don't believe that most of the people who majored in chemistry in this college considered themselves very greatly handicapped by having gone to a small college. I think that there were other parts of the training which were sufficiently valuable that it made up for deficiencies in the curriculum itself.

Q: How many did they have on the chemistry staff?

DR. DAFT: One.

Q: Just this one professor did all these courses?

DR. DAFT: Yes, the one professor taught the four courses and used assistants. I was an assistant in chemistry, I think, about two of the years I was in college. So they depended on student assistants and one professor of chemistry. Of course, that has changed a great deal now even at Simpson. They've strengthened the departments very greatly. But the smaller colleges at that time had relatively small faculties. They had one professor of geology and astronomy, one professor of biology, which included botany and also animal physiology, and this was true quite generally.

Q: When did you become an instructor, when you were a senior and you'd had all these courses?

DR. DAFT: I believe that I started instructing in general chemistry either the middle of my second year or possibly my junior year, but I think probably the middle of my second year, and mostly in the area of making preparations, supervising the laboratory, things of that sort.

Q: How were their laboratory facilities?

DR. DAFT: They were adequate.

Q: Did they have a number of labs or just one?

DR. DAFT: They had more than one. There was a science building there which is still standing, and they're going to leave standing because the building itself is fundamentally quite good, but it has enough sentimental value to a great many people, so they're going to leave it standing. There was one science building which included one floor for physics, one for chemistry, one for biology, astronomy, and, I think, mathematics. But as I mentioned, there were quite a number of individuals came out of this school at about the same period: Max Dunn, who recently retired as, I believe, assistant dean, he was professor of biochemistry at UCLA, quite well known in the field of amino acids and proteins; quite a number of physicians, Clifford Barborka, a gastroenterologist who has been on many of our panels, and I could go on with quite a number of individuals. The percentage of this period who went on to Ph.Ds. was larger than it was for most of the big colleges of the east.

So I have still quite a warm spot in my heart for the small colleges. I think they perform a function very surely, and so I stayed in fairly close association with Simpson. I'm the chairman of their development council at the moment. I meet with people a great deal and follow with considerable interest the development of the school.

Q: Did you go right to Yale after you left Simpson?

DR. DAFT: Yes.

Q: What made you choose Yale?

DR. DAFT: Again, it was necessary to have an assistantship. I applied at half a dozen or so universities, and was offered a teaching assistantship at three of them. Professor Baker had gone to Illinois, and he did not have his doctor's degree at that time, although he took it later at Illinois. Illinois was one of the schools that offered me a teaching assistantship, Minnesota was a second, and Yale was the third. I think that I was motivated partly by the feeling that an experience in a different part of the country might be very beneficial from a general cultural point of view, one might say. I'd lived in the Midwest all my life up to that point. And I knew that Yale had a very good chemistry department, although certainly no better than Illinois. Illinois had an extremely good chemistry department, and Minnesota had a good one. So in choosing between the three, I probably was influenced more by location in the east than actually the excellence of the department, but I really had only three choices. I might have applied to a great many more, but of the ones that I had chosen to which to apply, I had a choice of three.

So I did come immediately to New Haven and spent five years in that area. It took me four years to get my doctor's degree.

Q: You mentioned that you didn't have any physical chemistry in college. Did you have any problems now at Yale?

DR. DAFT: Yes, in a sense, there were problems in the fact that my training in chemistry was not comparable to a great many of my graduate school classmates, so the staff at Yale suggested that I take some really undergraduate courses my first year. So I took organic chemistry. I think at that time most college-trained people, except from the larger schools, had not had physical

chemistry. So they really taught a beginner's course in physical chemistry, as well as advanced courses at Yale, and most students coming there took them. I did take one course in organic chemistry which was really designed for undergraduates as a beginning course. As I say, I was a teaching assistant there for about a year and a half, I think, of the four years I was there.

Q: What courses did you teach and assist in?

DR. DAFT: I assisted in physical chemistry, in general chemistry the first year, entirely in general chemistry. I then stayed out one year to earn some money, and taught in high school at Branford, Connecticut, which is just a few miles from New Haven, where they gave physics and chemistry in alternate years at this high school. The year I was there happened to be the physics year, so I taught general science and, I believe, one course in biology and physics, and was the Director of Athletics, so called, at this school.

Q: It sounds as though these fellowships or whatever you had at Yale didn't pay very highly.

DR. DAFT: They paid well for those days, but they were barely sufficient. I would say really insufficient to carry a man through, even with what he would earn in the summer. As I remember, they paid about $650 or $700. So I stayed out for a year, saved some money, and I taught in high school the year 1922-23. Came back to Yale in the fall of '23 and served as a teaching assistant that year in physical chemistry. I started out as a physical chemistry major, and transferred to organic chemistry largely because of the faculty. I felt very much happier with the organic chemistry faculty at Yale at that time than I did with the physical chemistry faculty. So it was a question of either shifting schools, which is a little difficult to get the physical and chemical training that I wanted, or change to organic chemistry. I chose to change to organic chemistry at that time.

Q: Who did Yale have teaching organic then?

DR. DAFT: They had two very good men. The man with whom I worked was Professor Treat Johnson, who was the head of the department, and they also had Professor Arthur Hill, who was very good and has turned out a great many chemists. Professor Johnson was one of the few chemists in the world at that time who was interested in perimidenes and hydandaenes. Perimidenes have come to notice in recent years because of the fact that they make up a portion of the nucleic acid molecule, so it was a very important field of chemistry, although not recognized by a great many people at that time as having the importance that it actually has.

Q: I've heard of Professor Johnson. What sort of a fellow was he? Can you recall any of his characteristics or idiosyncrasies?

DR. DAFT: Yes. He was quite a colorful character in many ways. He lisped a little, which perhaps added to the color of his personality a little bit. There were many famous stories about him, one which might not belong on the tape, but I think it's not bad enough so that one might not repeat it. But I think the most famous story about Treat Johnson was the student he had who was working on a new perimidene, and he was adding ammonia to it to make it alkaline, and then precipitating it out with hydrochloric acid, crystallizing it out, and he worked on it for several weeks, and he couldn't identify this new compound. Professor Johnson went into his laboratory one day and smelled the compound, tasted it, said, "Young man, that's ammonium chloride. Throw it in the sink and piss on it."

But he was quite an interesting character. They had a new chemistry laboratory at that time, Sterling Chemistry Laboratory, had just been built. It was occupied first, I think, in the fall of '22, and this brought together the faculty from the separate schools of Yale. One leading to the Bachelor of Arts degree, and the other to the Bachelor of Science degree, and they had never lived together before. So there were a great many frictions in the department, particularly between organic chemistry and physical chemistry. He was an extremely colorful character in these altercations with the other side of the house.

I could go on with anecdotes *ad nauseum* about our days in chemistry at Yale.

Q: What was your thesis on?

DR. DAFT: Leading up to that, I went from a teaching assistant in the middle of my second year to a research assistant to Professor Johnson, so that I was working quite closely with him. They had a few research assistants, very few in the school, I think two or three, something like that. I was offered this choice which relieved me of the duties of teaching entirely, of assisting in the teaching, and gave me the opportunity to spend full time on research and, at the same time, have roughly the same income as I'd had before.

At the end of my second year, I was offered the Cheney Fellowship. This was given to Yale by the Cheney Soap Mills, who had a big plant near Hartford, in Connecticut. The understanding was that I would not attempt to finish my graduate course in one year, but would spend two years on this fellowship, and worked on proteins of silk. So this gave me my first training in protein chemistry,

and it was a considerably different training than many of the individuals with whom I was associated, who were spending most of their time strictly in synthetic organic chemistry. The topic for my dissertation was "the protein constituents of sericin" which is one of the proteins of silk.

We had a competitive examination which was also a necessary examination for your Ph.D. there, perhaps your most important examination, which was usually at the end of the second year. I think I took it during my third year, probably. No, it was usually in the second year, and gave a fellowship for your final year or years. I think it was a one-year fellowship. I won that fellowship, but it paid less money than the Cheney Fellowship, so I continued on the Cheney Fellowship, and the man who was second in the competition took this particular fellowship.

So I graduated then in 1926, and was offered a continuation of the Cheney Fellowship for one year for study abroad. This fellowship paid $1,000 a year, which was the highest paid one at that time in the Yale Chemistry Department. It seemed to us in those days representing affluence. But they made it for one year, and one year only. They upped it to $2,000 and permitted me to study abroad, so I chose Professor S.P.L. Sørensen at the Carlsberg Laboratory at Copenhagen.

Sørensen was the second Director of the Carlsberg Laboratory. The first one was Kjedahl, second was Sørensen, who was the one who invented the pH scale, and although that was not his most important contribution to science by far. His most important contribution was in the field of protein chemistry and the structure of proteins, but he's probably best known by more people for pH than for any other thing which he accomplished. The third director of the laboratory was Linderstrøm-Lang, who was Sørensen's first assistant at the time I was there.

So I went to Copenhagen on this fellowship in the fall of 1926, and spent a year with Sørensen, working on protein chemistry. I worked for a time under his direction continuing the work on proteins of silk, and then we transferred, before I departed in a year, to the problem of protein structure. I spent the rest of the year with him on that subject.

Q: Did you have any difficulty with languages over there?

DR. DAFT: I had anticipated some difficulties, and so during my last year in New Haven, I took some conversational lessons in German. I had had a year of high school German and a couple of years of college German, so I had a little basis. With conversational lessons, I managed great fluency in German with a very small vocabulary. To my surprise, however, I had very little use for the German when I

got to Copenhagen, because everyone spoke English almost. All of the educated people there speak at least one foreign language, usually two or three. Most of them speak German, English, and French, and almost all of them speak German and English. They were interested in practice for their English, so I had very little opportunity for using any other language, except I lived in a Danish *pension* and there many of the people did not speak English. Some of them spoke German, some spoke only Danish.

I studied Danish while I was there just for the interest of it, and I took no formal lessons. I studied with a young man in the *pension* and learned enough Danish so that I could spend an evening with a Danish family speaking Danish by the end of the year. My accent was atrocious, of which I wasn't very conscious, but I finally asked the people if I had any accent, and they almost went into hysterics and said that I certainly did have, but with a little patience, they were able to understand what I was trying to say. So I enjoyed it thoroughly.

Q: What sort of a fellow was Sørensen?

DR. DAFT: Sørensen was a very serious individual, but very, very kindly. He came from a very poor family in Denmark. I remember toward the end of the year, driving through the countryside with him, and he pointed out little farm plots and said, "That is the type of a home where I was born." He came from a family of what was known as a one-cow peasant family, and I don't know exactly how much acreage they would farm, but it was very small. He, mostly through fellowships, had gotten an education, which was quite unusual for a person with that background. He was extremely serious in his attitude toward work. It was a very, very exciting atmosphere. You had a feeling of being in the forefront of science. When the Germans came in, they were looked over very carefully and with great anticipation of what other people were doing in the same field, and everyone was discussing all of the newer developments in certain fields of chemistry. It was the most stimulating and exciting intellectual, scientific atmosphere to which I had ever, up to that point, been exposed.

He made himself available to his students; he took a relatively small number of students. The year I was there, there was a Russian professor there for a few months, there was a French student who has become one of the best known biochemists in France, Professor Roche, who was quite important in biochemistry and quite important politically, both, in France, in scientific politics. There were two or three English students at one time or another there.

Dr. Linderstrøm-Lang was an extraordinary individual. I don't know that you ever met him or not. He was one of the best loved of foreign visitors that we had in scientific circles until his death a few years ago, and quite an extraordinary individual in every way, a man with a very well-developed sense of humor, a little on the side of practical jokes. When I'd been in Denmark about a week, Professor Sørensen had a big dinner for the scientific community, and somebody got me aside during the dinner and told me that the Danish custom was to go up and shake hands with the host and hostess after the meal and say thank you for the meal in Danish. So I practiced that all evening. It was a tremendous success. Professor Sørensen and Mrs. Sørensen both were greatly pleased that I had learned Danish customs so rapidly.

So Linderstrøm-Lang overheard this, and got me aside during the evening and coached me on what I was to say to the professor as I left. So I learned that very carefully, also, and made my adieus to him and repeated what Linderstrøm-Lang had told me to. Somebody stopped me and said, "Do you know just what you said?"

I said, "Well, not exactly."

"Well," he said, "you just said to the professor, 'I have had a very boring evening.'" And this is somewhat typical of the little jokes that Linderstrøm-Lang was able to play on me during the year, but all in the spirit of good fun. He was a most charming, delightful individual.

But Professor Sørensen, as I say, made himself accessible to the relatively small group of students who were there. I remember he was quite ill and confined to his bed for a week or two, and told me he had a telephone beside his bed, and he said if anything came up in the work that I needed counsel on, not to hesitate to telephone to him or to come up and see him. He lived in quarters next to the laboratory.

He created a most stimulating atmosphere, and I had in every way a tremendously interesting, delightful year there.

Q: How were the laboratory facilities?

DR. DAFT: They were excellent. It was not a new laboratory, and I imagine that by modern standards you would say that they were not the best in the world, because it was an old building and so forth, but it

was entirely adequate. It was designed for the type of work—it had been redesigned, let us say, by the professor for the type of work in which he was interested, so that everything was available which was needed in the research going on at that time.

This was not a period where you were using a great deal of instrumentation. Instrumentation in chemistry has come since that time. You depended on relatively simply analytical tools, the kjedahl, not even a micro-kjeldahl; you were using macro-kjedahls and various chemical determinations, but they were entirely adequate by those standards. I would say it was an extremely good laboratory of its period.

Q: Did Professor Sørensen teach other students, special students?

DR. DAFT: No. He was a professor in the university, but it was more or less an honorary title. He was Director of the Chemistry Department of the Carlsberg Laboratory. There were two departments, the biology department, also. The Carlsberg Laboratory is somewhat unique in its history, and was quite extraordinary for a small country such as Denmark. The owner of the Carlsberg Brewery gave the brewery to the Carlsberg Foundation, the directors of which were mostly college professors, so that all of the income from the brewery went into the support of science and art. The Foundation maintained two of the museums in Denmark and supported in its entirety the Carlsberg Laboratory, and continues to do so today. All of the proceeds from Carlsberg beer today go into science and art into the Carlsberg Foundation.

So he was actually Director of the Carlsberg Laboratory, which was entirely a research institution. He had the title of professor in the university, but taught very few courses. I think he gave an occasional lecture and that was all. He was entirely a research man. It was a small Rockefeller Institute, and I think that is perhaps the most valid comparison for its period of anything of which I can think, because for the size of the country, this was a large Foundation. As the Rockefeller Institute was given by private capital, the money for the Rockefeller Institute, so was the money for the Carlsberg Foundation in Copenhagen.

Q: Where did you go after you finished your year in Denmark?

DR. DAFT: While I was in Denmark, I became friends with a professor from Harvard, who was in the Harvard School of Public Health. So very early in the year, he suggested to me that I return with him to Harvard. It sounded like an interesting opportunity, so I made my plans accordingly

and came back to the Harvard School of Public Health with him. This was Professor Cecil Drinker, quite a gifted physiologist. He was spending his sabbatical year with a Nobel laureate there, a Professor Krogh, and so I spent the last two weeks of my stay in Denmark in the laboratory of Professor Krogh, learning some of the techniques which Professor Cecil Drinker had been using during that year, and would continue his research in physiology at Harvard.

Professor Drinker was a very wonderful person. He had been, during the First World War, at a very young age, acting head of the Department of Physiology in the medical school. The head of physiology at Harvard was Canon. Canon and a great many others were away during the war, so Drinker, at a very early age, was thrown into the head of the Department of Physiology at Harvard, acting head. When Canon came back, he was made professor of physiology and, I believe, dean of the School of Public Health. I believe he was dean there.

DR. DAFT: I became rather ill in Denmark during the latter part of the winter. The Danish climate, as you probably know, is although not very cold, it's very rainy and very dark. This is far enough north so that although this isn't the Land of the Midnight Sun, nevertheless, you can read a newspaper on the streets of Copenhagen at any hour at night during their longest days, and their shortest days are correspondingly dark. Then I'd always had a little trouble with bronchitis, so I got some sort of an influenza virus infection, a cold, what have you, and I was ill for perhaps a month. I was told by the Danish physician that I had tuberculosis, which made me somewhat unhappy.

Professor Drinker asked me to come down to his hotel. He was living there with his family, in the latter part of my convalescence, which I did. He was able to assure me that if I'd had tuberculosis, I wouldn't have recovered with the rapidity with which I did under those surroundings. So I was very much relieved, had very careful tests, of course, when I came back to this country. There was a great deal of tuberculosis in Denmark, and the Danish physician had had a great deal of experience with it, but he was basing it on certain physical signs. He had never obtained a specimen of sputum or anything of that sort. Although he was very definite in his opinion, nevertheless, fortunately he was wrong.

I came back to the School of Public Health with Professor Cecil Drinker and spent two years there doing three things. One was doing some teaching in biochemistry with Professor [?] and mainly

doing research. We were working very largely on a blood substitute which had come into very great prominence during the First World War. This was before human plasma was available, and they were using [?]. We were working on certain problems of [?] very largely. Professor Drinker was working on lymphatics at that time, and I participated in that to a certain extent.

But I also was permitted to take any courses that I wanted to in the medical school at the same time, so during that period and a little later, I was able to get training in all of the pre-clinical medical subjects. I had been going further over into the biological end as time went on. I think it was started, undoubtedly, by accepting the Cheney Fellowship and getting interested in protein chemistry, which is a little more of interest to biologists, and the year with Sørensen and the years with Cecil Drinker. So that it was highly desirable that I learn as much as I could about medical subjects, and it would have been best of all, which I could have done and probably should have done, was taken an M. D. degree. But I was very anxious to get on with research, so I didn't do it. But I did take there and, a little later, at Rochester, I think I got all of the pre-clinical medical subjects.

Q: What exactly what were you at Harvard? An instructor or some sort of a special Fellow or what?

DR. DAFT: I was on the teaching staff at Harvard. I had an appointment on the teaching staff as an assistant or an instructor in biochemistry and physiology. The biochemistry was, in a sense, a second appointment because my teaching was actually in biochemistry, but my research was in the Department of Physiology at the School of Public Health.

Then about the beginning of my second year there, I was approached again by my old professor at Yale, Treat Johnson, and he said that he was going to have open, the following year, the Eli Lilly Fellowship, and asked me to come back to Yale to accept that, which I did. So I spent two more years at Yale and worked for Professor Johnson from '29 to'31.

Q: What were you working on at Yale?

DR. DAFT: We were working on immunochemistry. There was a great question at that time as to what the active groups were in producing immunological specificity. We were working on tetanus toxin, to try to determine the portions of the molecule which determined the specificity in producing antibodies to the toxin. They supplied enough tetanus toxin from Eli Lilly, [that] we

calculated, to kill a million people, I think it was. That was our basic material with which we were working at that time. We were working partly on the lightweight fraction which has not turned out to be the most important immunologically. It was a very interesting and stimulating two years. We did not make great progress in the subject matter, but nevertheless, I consider it a very valuable two years.

Q: I suppose that would have had some commercial value, wouldn't it?

DR. DAFT: Conceivably, yes. But Eli Lilly gave this fellowship to Yale for pure research, and it was not designed to make a return in the business. If there was, as should be, any commercial return, that was just an added thing which they obtained from it. That was not the purpose of the fellowship. It was given specifically for basic research. Although I had contact with the Eli Lilly people, I made several visits out there, and they were testing some of the fractions which I prepared for us. We were not set up at Yale to test them ourselves. So it was cooperative in that sense, but it was not designed for any specific commercial purpose.

At the end of my two years there, I went to the University of Rochester. This was through a friend, a Professor Anderson, at Yale, who was a friend of Dr. Whipple's at Rochester. They wanted someone with just my training to participate in the work on the anemic dogs. Dr. Whipple, as you know, is Nobel laureate for his work on the use of liver in anemias. He was working on anemia in dogs, produced largely by dietary means, and the liver was found to be specific for pernicious anemia, which was quite different from the anemia with which Dr. Whipple was working.

At about this time or probably a year or so earlier, Professor Minot at Harvard, who had been interested for some years in the problem of pernicious anemia in humans, decided to follow Whipple's treatment of anemia in his patients. They had in the hospital at that time quite a number of cases of pernicious anemia, which was invariably fatal in those days. So he got a medical student, I believe, either a medical student or a young intern, to actually handle the patients for him. Most of the cases of pernicious anemia would try anything, because they knew it was a fatal disease, and they were willing to cooperate in any way. They would drink a glass of blood, thinking that perhaps the blood would help them in the disease.

So young Murphy got this very cooperative patient, and they decided to use liver. It was a choice between liver and kidney which Whipple had fed to the dogs. Fortunately, they chose liver. They got

a patient who ate something like three water-glasses full of raw liver a day. They didn't know it could be cooked at that time. There are remissions in pernicious anemia, so that they weren't certain when the patient got better, whether or not this was really a good treatment or whether they just happened to strike him at the time he would have had a remission anyway. But the patient got much better. Then they were able to go to the other patients and say to them, "He's better after eating three water-glasses full of raw liver a day, and I want you to do the same thing." They actually used raw liver because Whipple had used raw liver for dogs. They used raw liver for two years, I think it was, before they found it could be cooked. But in the meantime, they had treated successfully at least a dozen cases of pernicious anemia, and this was the treatment which saved the lives of pernicious anemia patients. The Nobel Prize was shared between Whipple, for his basic work on animals, and Minot and Murphy for their work on patients themselves.

Dr. Whipple had not received the Nobel Prize by the time I went to work with him in 1931, but he was continuing. He was dean of the medical school and had built the medical school in Rochester. It was an extraordinarily fine medical school as young as it was. It was a very young medical school. I remembered talking with Cecil Drinker while I was there, and asked him what he considered the best medical school in the United States at that time. As any good Harvard man would say, he said that there was one in a class by itself, that was Harvard, and then there were half a dozen that were very fine medical schools, and named them, and to my intense astonishment, included the University of Rochester Medical School as one of this group of the very best medical schools in the country.

I expressed my astonishment. He said, "They haven't graduated the first class yet, but Dr. Whipple has collected together an extraordinary faculty, and I don't think there's any question but what it's one of the top medical schools in the country." And it was a most remarkable faculty. Bayne-Jones was there at that time, [George] Corner; and I could go on with the roster of people that he had assembled to start this new medical school. I went with him, still in the fairly early days. I think the medical school started around 1926, something like that. I went there in 1931 and worked on protein metabolism with him. Again I was doing some teaching in the Department of Biochemistry with Bloor [Walter], and my appointment was instructor in pathology and biochemistry. The research was carried on entirely with Dr. Whipple in the Department of Pathology, and the teaching was in the Department of Biochemistry. So I stayed there for six years, from 1931 to 1937 and had a very interesting experience again, this time in protein metabolism, which sort of rounded out the training I had had in various aspects of protein chemistry, protein structure, immunochemistry, which was related to the

proteins, and then protein metabolism.

Q: What were you trying to do?

DR. DAFT: We were studying how these anemic dogs handled protein. There were some rather extraordinary things which are still not explained, but very simply, it is this: the fundamental use of protein in nutrition is not for energy. We usually consider that fat and carbohydrate furnish the energy, and it's only when you're on an inadequate intake of fat and carbohydrate that you will break down very much protein to furnish energy. The amount of nitrogen which is excreted by the animal in the urine is usually considered a measure of the minimal wear and tear of body cells. There are certain metabolic things that go on that require nitrogen, and there are certainly some cells dying in the products being destroyed, but the amount of nitrogen, when there is adequate intake of calories, is considered to be pretty much this minimal amount. But animals that were anemic and were given a big shot of iron to help them build hemoglobin, suddenly would take nitrogen which they ordinarily would have excreted in the urine, which would have been considered the minimal amount to be excreted in the urine, and make this into hemoglobin, so that the nitrogen would drop off very considerably. So it's somewhat more complex than one believed at that time. We were studying the various nitrogen constituents of the urine under various conditions in these anemic dogs. That, very briefly stated, was the main part of the work.

This then brings me up to the shift to the National Institutes of Health. I was offered, actually, two jobs in the Public Health Service at that time. I had been interested over the years in the Public Health Service, largely because of a Public Health Service officer whom I met in Denmark. In those days, most of the medical officers in the Public Health Service were not assigned to one place and permitted to stay there; they were given what was considered a well-rounded service training. Very few came to the National Institutes of Health as their first assignment.

Dr. Milton Veldee was taking his two years as the examining physician at the consulate in Copenhagen, and he was there with his wife. The three of us lived in the same Danish *pension* for a short time and kept in contact with each other while we were in Denmark and later. He had always talked with me about the opportunities at the National Institutes of Health and what a fine place it was. I, as a great many people in universities at that time, was somewhat prejudiced against

government service. I think that this prejudice was shared a great deal by most of our advisors, including Dr. Whipple, but Dr. Whipple said to me when this came along, "There is one exception I would make to government service, and that's the National Institutes of Health. That is as good as universities, and I think that this would be a very fine opportunity."

The other offer which came along at that time was through Dr. Van Slyke, whom I did not know until much later. He was at Staten Island. They were interested in determining the substance which accounted for the Wassserman reaction. They were working in V.D. The Wasserman reaction wasn't understood, and I don't believe it is today, as far as I know. So the chance there would have been to work on analysis of the materials used in the Wasserman reaction and try to determine the specific substance which was responsible for this reaction.

But at the same time came along an offer from here, and that was to work with Dr. Sebrell in the field of vitamins. I had had no experience whatever with the vitamins at that time, and knew much less about them than I should have. But I came down and talked with Veldee, as well as with Dr. Sebrell, and decided to come here.

As you know, nutrition in the Public Health Service goes back to Goldberger. Goldberger, before we entered the First World War, roughly 1915-16, was not considered a nutrition expert in any sense of the word; he was a greatly gifted epidemiologist. He had been studying outbreaks of disease in various parts of the country, in determining the causative agent, the factors of transmission of the agents, and had made a remarkable record in that area, and was assigned the problem of pellagra, which was an endemic disease in this country at that time, largely because most people considered it an infectious disease. There were some things that made it a little difficult to believe that, but nevertheless, there were several theories. One was that it was moldy grain, bad grain, corn, that is.

When he accepted this assignment, he came to it very objectively and proved to the satisfaction of almost everyone very quickly that it couldn't be an infectious disease in the usual sense of the word, at least. The main reason for that which led him to this was the fact that while the patients in institutions got pellagra, nurses and doctors never did. The only difference he could find was in their diet. He went to great extremes to try to transmit pellagra to himself and to his associates in this work, and he found in the course of his work that there was a counterpart in the animal kingdom, and that was black tongue in dogs. So he had taken the dog as his experimental animal, and was able to study the disease under laboratory conditions, as well as under field conditions. He also carried out studies at the Milledgeville State

Hospital on human patients, and was able, with certain diets, to produce pellagra, to induce pellagra, and with certain diets, not only to prevent but to cure pellagra if it hadn't progressed too far.

At that time, there were tens of thousands of cases of pellagra in the United States every year and a great many deaths from the disease. Dr. Sebrell had joined Dr. Goldberger in the latter years of his life, starting about 1927, something like that. Goldberger died, and Dr. Sebrell was given charge of the nutrition studies in the Public Health Service.

Dr. Goldberger, in his studies, I think was inclined to believe at first that this was a protein deficiency disease. He found that the diets were very low in some of the essential amino acids, and actually treated one patient with a mixture of two amino acids, tryptophan and one other, successfully. That paper is in the literature, indicating the relationship of pellagra to proteins and amino acids. But the vitamins had come into considerable prominence about that time, and quite naturally, Goldberger couldn't overlook the possibility that he was dealing with a vitamin deficiency, and he soon found that he could prepare extracts of foods which prevented pellagra, and that these extracts, which were quite low in nitrogen, were very effective in curing black tongue in dogs, and were also effective in his clinical work. So he invented the term "the PP factor," the pellagra preventive factor, as a vitamin. He left the amino acid work entirely and went over entirely to study the PP value of various foods, doing some work on attempting isolation. It was not until 1937, in the fall, that the pellagra preventive factor was actually isolated, and it was not for several years after that, that it was discovered that the animal and man could make the pellagra preventive factor from the amino acid tryptophan, which Dr. Goldberger had used at one time in his experiments. So that Dr. Goldberger almost discovered that this was a tryptophan deficiency, and if he had come to that conclusion, he would have been correct. In a sense, it is a tryptophan deficiency, because the human and animals can make nicotinic acid, niacin, from tryptophan in the body, and a deficiency of niacin causes pellagra and causes black tongue in dogs.

The job for which I was hired in the Public Health Service, in the National Institutes of Health, to work with Dr. Sebrell, was to isolate the PP factor. I was engaged mostly in library research, library work, getting some background in the vitamins and in the PP factor. At the time that the isolation of the factor and its identification as nicotinic acid was announced, just a few months after I arrived at National Institutes of Health, so I went on with Dr. Sebrell then on working on other of the B

vitamins.

I'll just finish one somewhat interesting part in regard to pellagra and black tongue in dogs. I believe that if the vitamins had not come along at the time they did, that probably Dr. Goldberger would have concluded that he was dealing with the protein deficiency, and would probably have identified it as tryptophan. He destroyed once and for all the idea that this was an infectious disease, although some people of that time who were prominent in the field refused to accept that conclusion, and during their lifetimes maintained that pellagra was infectious in character. But I think for most reasonable people, he destroyed that concept once and for all. Going back and reading Goldberger's studies today, his reports, to me, constitutes one of the most exciting readings in the old field of medical literature. They're beautiful studies in epidemiology. You might consider them classical studies in epidemiology. It has all the excitement of a detective story, the way he proceeded, the very clear logic.

Now, a thing which interests me very much at the present time, we have been working with germ-free animals, and found, to our surprise, that when you use a low protein diet, that germ-free animals grow very much more rapidly than do their conventional counterparts. This was not an original observation for us; other people had made the same observation. But we wondered very much why. It might be there was something very strange about germ-free animals, that they always would grow better on all diets, but we made a test of these diets and found that on certain low protein diets they grow better, and on certain other ones they do not. But to make the long story short, we soon found that specifically on a low tryptophan diet, the germ-free animals grow very much better on these low protein, particularly low tryptopan, diets. Although the rate of growth is very small, it's still about three times as great on the germ-free animal.

Now we're fairly certain at the present time that the bacteria of the gastrointestinal tract destroy about 50% of the tryptophan that the animal or, presumably, human is receiving from these diets. I am reasonably well convinced that the humans on a pellagra-producing diet would not have gotten the disease had they been germ-free, that, in a sense, this is related to bacteria, not as an infectious disease, you understand, at all, but in relation to the bacteria, the non-pathogenic bacteria which we carry around with us all the time. With rats, we can put them on a tryptophan niacin-deficient diet which kills the conventional animal in a short space of time; the germ-free animal gets along beautifully.

Now we intend, just for the continuity of the Goldberger story, of the pellagra story, to prove whether or not this is true not in humans, but in dogs, because we have a wonderful counterpart. We prefer not to have to have germ-free dogs, so we're trying to modify the flora so that we can modify it in the conventional dog, so that the bacteria will not destroy the tryptophan, and that the dog will not get black tongue, even though he is on a black tongue-producing diet. So this is the most recent story in the Goldberger story, and is giving us a great deal of pleasure and interest at the present time.

End of interview