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UI Centennial Project

Interviewee: Willaim Cumming Rose

Interviewer: Maynard Brichford

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Maynard Brichford: This is a tape-recorded interview made on March 10, 1966 at the home of Professor William C. Rose on Florida Avenue in Urbana, Illinois. We should begin by, let's see, I believe you were born in April 4, 1887 at Greenville, South Carolina. Could you discuss a little bit your family background?

William Rose: Well, my father was a minister, and he moved from South Carolina to North Carolina when I was four years of age, so I've always considered myself a North Carolinian.

MB: Presbyterian minister, was he?

WR: Presbyterian minister, yes. And my early education was at private schools since at that time there were no public grade schools in the state of North Carolina. The last year before I went to college, my father was not well satisfied with the instruction I was getting. So he took me out of school and prepared me for college himself. And each morning I worked in his study while he was busy with his own affairs. And when I thought I was prepared, I went notify him and he gave my lessons.

MB: He'd attended college, your father had?

WR: Oh yes.

MB: Where did he go?

WR: He went first to Davidson. And that was during the Civil War and the college closed. After the war, he went to the University of North Carolina.

MB: That's a tradition of the Presbyterian educated ministry. He had the advantage of a good education.

WR: And then of course he had his theological training at Union Seminary in Richmond, Virginia. At that time, it was in some other Virginia town. Forgotten what it was. It was later moved to Richmond. So he was an excellent teacher and gave me a concentrated course of studies in various disciplines so that I had no trouble whatever when I arrived at Davidson College, although having no diploma from the school, I was admitted on the condition that I could make the grade.

MB: How did you become interested in chemistry?

WR: My sister had been a student at Congress College in Greenville, South Carolina. When she came home at the end of the first year, she brought an elementary textbook on chemistry by Professor Hira Remsen of Johns Hopkins University. I happened to start reading that book and became so fascinated that I decided that was what I wished to do. So I had never had the problem that many young people do to tell me what my life's work would be.

MB: Let's see, you took quite a bit of chemistry.

WR: I took all the courses that were offered at Davidson College. And on graduation went that fall, Yale University.

MB: 1907, was it?

WR: That was the fall of 1907. I only knew then that I wished to be a chemist, but having a course under Professor Mendel, I soon decided that biochemistry was the branch I wished to specialize in.

MB: Professor Lafayette B. Mendel, could you recall a little bit about Professor Mendel?

WR: He was one of the pioneers in biochemistry in this country, having been the son of an immigrant, German immigrant. Who, as I understand it, had not yet a college or university education. Mendel made a wonderful record as a student in Yale, went to Europe and studied to become a biochemist, and returned then at a young age, in his early twenties, was made the first professor of biochemistry following Harlene Chittenden, who had been the first American biochemist. When I was there, Chittenden was director of the Sheffield Scientific School, but he still gave two courses, each of which, these were electrical, each of which I had the privilege of taking. My thesis was worked out under Professor Mendel. And I received a PhD degree in 1911.

MB: That was a degree in biochemistry?

WR: In biochemistry.

MB: Was this the only place that you could obtain a degree in biochemistry or were there other?

WR: It was the outstanding place. There were a few other institutions that were giving courses in biochemistry and had some graduate students, Harvard among others. I know there were courses also at Johns Hopkins, but 1907 was a young theory for biochemistry. It was not given in all schools.

MB: Where did- Mendel was the first American biochemist trained at Yale. Chittenden went to Germany. Did you mention where in Germany Chittenden went for his, do you recall?

WR: Chittenden had his first training under old Dr. Samuel Johnson, who was in charge of and had founded the first American agricultural experiment station in New Haven. Mendel obtained his interest in the chemistry of living things under Dr. Johnson. He then went to Strausberg.

MB: This was, it was Chittenden.

WR: Chittenden. He went to Strausberg, but didn't like it so well, and finally went to Heidelberg, where he worked on the direction of Professor Kuhne, K-U-H-N-E.

MB: I think we have pictures of some of these gentlemen that you gave us where they tied them in.

WR: So Chittenden came back and formed the first department of biochemistry in this country, and Mendel was probably his most outstanding student. Mendel later studied at the University of Freiburg and the University of Breslau.

MB: How many were taking graduate work for the doctorate in biochemistry at Yale when you were there? Were there a number or?

WR: Yes, there were probably each year about a dozen graduate students.

MB: You had an assistantship in physiological chemistry at Yale?

WR: After the first year I was given an assistantship, the only assistantship available in Mendel's laboratory. At the magnificent salary of $300 a year, which was the going rate at that time.

MB: You could live in New Haven then for $300 a year?

WR: Well, legally. I made some money on the side. Dr. Mendel was very kind in helping me with simple analytical jobs for which money could be collected whenever one came to his attention.

MB: Was this period of time that you had the summer job at the-

WR: Oh, yeah, the first summer, that is the summer of 1908. I served as a biochemist for the Connecticut Hospital for the Insane and did analytical work in various fields in which I'd never worked before, such as analyze coal for the hospital. And they based their order from the analysis of samples of coal that I made at that time. They wanted their water supply, analyzed. I'd never made a water analysis, but I made one in Middletown.

MB: Good work experience. From Yale, then you went to the University of Pennsylvania in 1911?

WR: 1911 in the Department of Biochemistry, headed by Dr. A.E. Taylor, a very unusual man who had been a professor of pathology at the University of California before he became professor of biochemistry at the University of Pennsylvania.

MB: He had studied abroad too, I presume.

WR: Yes, he had studied under the direction of Emile Fischer at the University of Berlin.

MB: How did you come to go abroad yourself for additional work?

WR: After the second- no, I guess it was during the first semester of my second year, Dr. Taylor asked me if I would like to go abroad for a semester in the summer. I explained that I would, but I couldn't afford to do that. Whereupon he suggested that he would see if the university would continue to pay my salary. They paid me in advance for the last half year of the session 1912-13, and I went abroad in February, studied with Professor Franz Cano at the University of Freiburg until August.

In the meantime, an offer had come from the University of Texas where a department of biochemistry was being organized for the first time in the College of Medicine. The offer was made to me, and at first I declined it because I thought I had an obligation to return to the University of Pennsylvania. Later, when Taylor heard that I had declined it, he insisted that if I were to go, I'd do so because he thought the opportunity was one that was well worth my

considering. So I went there as associate professor of biochemistry in 1913 and remained there until the summer of 1922.

MB: Was that in Austin or in Galveston?

WR: It was in Galveston.

MB: Medical school.

WR: The medical school was in Galveston and I was on the medical faculty. In 1922, I came to the University of Illinois. I don't-

MB: Care to go into any of the circumstances of how Illinois was able to lure you away from Texas?

WR: I was asked to visit the University of Illinois and be looked over and looked over the possibilities myself, which I was glad to do. I was not too favorably impressed by Dean Babcock's attitude and probably would not have come to the University of Illinois except for the interviews I had with Professor Heston Bipar and other members of the chemistry [unintelligible], all of whom I found to be very delightful.

MB: Was Roger Adams here that year?

WR: Yes.

MB: He was here too.

WR: Roger Adams was the head of the Organic Division at that time. W.A. Norris was head of the department. Norris happened to be in Europe at the time I was here. But he had authorized the department to proceed with the selection of a biochemist to succeed Dr. H.B. Lewis, who went from here to the University of Michigan.

MB: And then the department decided that you were the man they wanted and then contacted you and asked you to come up here.

WR: Asked me to come.

MB: What was your first impression of Champaign-Urbana? [Unintelligible] Galveston? I don't know.

WR: Well, uh.

MB: Sort of a small town, wasn't it, in those days, even compared to Galveston?

WR: Yes, it was somewhat smaller than Galveston, although Galveston has, because of its storms, has not developed since. But as a matter of fact, I suspect the population of Galveston now was less than it was in 1952. But I was most favorably impressed by Champaign-Urbana and particularly the university. I knew already the reputation of the chemistry department, and that was one of the strong factors that led me to come here.

MB: And the reputation of the chemistry department, who was it? You'd heard of these gentlemen, that is, Professor Noyes and Professor Adams and so forth, had professional meetings, and Illinois had the-

WR: Yes, I didn't know these men, but because my field was different. But I knew that the Department of Chemistry at the University of Illinois was generally regarded as one of the two or three top departments in this country. That was well known even in 1922. Professor Noyes, I think, was largely responsible for the reputation of the department up to that time. We've been very fortunate in this chemistry department because we, after Noyes, had another equally distinguished gentleman, Dr. Roger Adams, to head the department.

MB: And we've had, I guess, strong divisions, too. Your division of biochemistry and Professor Rodebush in physical chemistry.

WR: There were seven divisions that existed at that time, all headed by men that were very able in that especially.

MB: And to why you had some misgivings initially with the university administration. As the head of a division of the chemistry department, you did not run into any great problems with the administration.

WR: No.

MB: You could concentrate on your research and teaching.

WR: The fact is the administration and officials throughout my career here have been very cooperative.

MB: You became, let's see, did they change the name of the division in 1936?

WR: Yes, when I came here it was called the Division of Physiological Chemistry, a term which originated in Germany since what we now know as biochemistry was generally taught, placed in the departments of physiology. So they called it physiological chemistry. In 1936, on my recommendation, the University of Illinois changed to the more modern term of Division of Biochemistry. I changed my title to professor of biochemistry.

MB: I suppose there are a variety of ways in which biochemistry is taught. At Wisconsin, it's in agriculture. Here, it's in chemistry. In some places it might be a separate department even from chemistry.

WR: Yes, in the vast majority of universities, it's in the College of Medicine, even in this country. Illinois was rather unique in having a strong division in the Department of Chemistry, as well as

having a department of biochemistry in medical school in Chicago. I don't recall any other place where that was true except the University of Iowa also had a division of biochemistry in the chemistry department. Now the biochemistry of Iowa has been put into the College [Emeritus?].

MB: Let's see, research professor- let's see, from 1942 to '45, you served as acting head of the chemistry department.

WR: Yes, during the absence of Professor Roger Adams.

MB: And please, you may have indicated this was not a lifelong ambition to be head of the chemistry department.

WR: No. I much preferred the teaching and research aspects over the administrative aspects.

MB: Then in 1953, research professor of biochemistry. This then involved freeing more time for-

WR: This freed me from administrative work in this division, and most of my teaching duties were given to others so that I could devote the remaining time to research and the preparation of a number of manuscripts, which I knew had to be published, and I had not found time to leave them and get them ready.

MB: And this was accomplished prior to your retirement, 1955.

WR: Yes.

MB: It was June 10th and 11th, 1955, your former students and colleagues held this symposium.

WR: Yes, they returned and had a very nice celebration with a program symposium on certain phases of biochemistry.

MB: Let's see, was it Professor or Mr. Decker who spoke and a couple others who spoke?

WR: Well yeah.

MB: Well, I don't think.

WR: Let's see. I think Dr. Doisy, Dr. Griffith, [Inaudible] H. Griffith. And there are two more.

MB: They were your students.

WR: They were.

MB: Or had studied.

WR: No, they were just. Doisy was one of my students. And I'm mixing this a little. There were a couple of other symposiums that we had at different times, and I'm not sure which is which.

MB: I wonder if at this point, seminar, if you could talk a little bit about that. In other words, you were in Professor Mendel's seminar, and then you had your own seminar. How did the seminar work? This changes over the years. I'd be interested in how it worked in your case.

WR: In Mendel's seminar, the graduate students were required to give a short talk every week, limited to about 10 or 12 minutes. The seminar was conducted for two hours without an interruption, so that each student could give his 10-to-12-minute talk and still have a little time for discussion of each paper. That was the center of Mendel's teaching. Most everything that we acquired, aside from one or two courses in biochemistry, was taught to us in the seminar. So when I came here, I tried to duplicate as far as I was able the procedure followed by men limiting the talks to reporting on papers by students. At first, I always assigned the paper and the student would then read it and give a brief outline.

MB: Assigned the paper. Now, this was a paper for him to prepare or a paper that had someone else prepared?

WR: A paper that had been published already and he was to read it and-

MB: Criticize.

WR: Criticize and report it. First of all, just outline what was in that paper for the benefit of the others. Later on, I permitted the students to select their own papers, which has a certain advantage because generally they have to read several papers to find one that's suitable while if I read them and just picked out one, they didn't always read the others.

MB: Yes, as they go along, they become, too, probably more interested in a certain line of research, and they'll want to read papers.

WR: Yes, and some of the students have told me that they would have to read, at least superficially, sometimes six or eight papers before they found one they thought was worth a 10-minute talk in the seminar. Naturally, they wanted to pick out a paper upon which they could give an excellent report. And they were graded by me on the basis of the quality of the paper they selected and the quality of the report they gave.

MB: So it's a premium on finding a good paper, then.

WR: Yeah.

MB: Well, this certainly is an effective way to familiarize them with the literature in the field, I would think.

WR: I also encourage the students to do most of the talking. The discussions were just guided by me, but where necessary, but most of the comments were made by the other students.

MB: I suppose it's pretty hard to pick out outstanding students. You've mentioned, let's see, Wendell Griffith was one of your students.

WR: No. He was one of H.P. Lewis's.

MB: Was Doisy?

WR: Doisy was one of H.P. Lewis's as an undergraduate. Doisy went to Harvard to get his doctorate degree. Griffith got his doctor's degree under Lewis the year I came.

MB: And then there is- who is the other-

WR: Wendell Stanley. He is a Nobel Prize winner.

MB: Was he one of your students?

WR: No, he was a student of Professor Roger Adams and never even took a course in biochemistry while he was here. His interest was acquired after leave. Dr. DeVigno, a Nobel Prize winner, had graduate courses under me. He received his degree in organic chemistry, his bachelor's degree, majoring in organic chemistry. Came back as a graduate student and worked under the direction of Dr. Mull in his major but acquired his interest in biochemistry in connection with his [unintelligible] from our mind. His degree was actually acquired at the University of Rochester. He didn't stay here for his degree.

MB: It appears to some extent that biochemistry was not something that a lot of these people had an interest in as he started work. In other words, later on they wound up particularly the Nobel winners, eventually wound up in biochemistry.

WR: Well, [unintelligible] had an undergraduate course with Lewis in biochemistry, and Doisy did also. So most of them had biochemistry, but Wendell Steiner is an exception.

MB: The Hectone lecture on Synthetic diets, when was that? That was the 30s, was it?

WR: Well, the Hectone Lecture is an endowed lectureship under the auspices of the Chicago Institute of Medicine. And they have this one lecture every year. I happen to be invited- I've forgotten the date now- to give it. And my topic was on the role of amino acids.

MB: From your papers that you have deposited in the archives, from 1931 to 1936, there is a series of papers on the existence and the-

WR: Identification, isolation, identification of three of them. This came about by the fact that I had become somewhat interested in determining which amino acids were necessary for life and which were not. The reason for that being that certain amino acids can be synthesized by the animal organism. If so, they need not be present in the food. Mendel and Osborne had shown that at least two, lysine and tryptophan, could not be synthesized by the animal and therefore were needed in the diet. But after trying to prepare hydrolyzed proteins from which separate amino acids had been removed, it seemed more logical to just prepare the purified amino acids, put them together in the proper proportions, and feed them to the animal, then one could drop them out as they saw fit. But when that was done, the animals were unable to maintain their body weight. They went down very rapidly.

Now, my experiments were not the first of this sort in which the animals had failed to maintain themselves or to grow on mixtures of purified amino acids. But the diets I had were the most complete with respect to the amino acids. I put in every normal amino acid in the food, and still it wouldn't maintain that body weight. Some individuals had suggested that this was due to the fact that the animals just didn't like the taste of amino acids and wouldn't eat the food. This didn't seem to be a reasonable explanation to me because you could boil up a product with acid until it's broken down to a mixture of amino acids, remove the mineral acids you use for this hydrolysis, and the animals would eat such a mixture and maintain their body weight and eat and grow. Therefore, I interpreted my failures in the early experiments involving the eating of

mixtures of amino acids due to the fact that there was some amino acid in products that hadn't yet been discovered.

We set about to isolate that. We found that if we added to this mixture of purified amino acids a crude preparation obtained from a project by a certain technique that I won't go into, that the animals would grow. By about three years of hard labor were required before this was isolated and identified and give them the name creatine. Some of this delay was due to the fact that these crude fractions also contain isoleucine, a known amino acid, which in our synthetic ration was probably not there in adequate amounts. But we had no way of knowing this since if you added more isoleucine, as we did, that wouldn't promote growth because creatine was still missing. And both had to be there.

MB: These were experiments on rats and dogs.

WR: Rats.

MB: Rats.

WR: Because of the relative small food consumption of a small animal like that. These amino acids are very, very expensive. And many of them we prepared in the laboratory from proteins. Others were synthesized in the summer by organic students under what was then known as organic manufacturers, where rare chemicals were made by the organic division of the chemistry department, the student labor, and sold to people who needed them. They plenished many of the synthetics.

MB: This was in the 1930s. The university had a business operation.

WR: Yes. This was a revolving organization where the funds were put back into organic synthesis. And incidentally, the department benefited because the students would make rare chemicals that were very expensive that were needed for research by members of the staff.

MB: I think it'd be good training for industrial chemists, too, because these people then can go out and take charge of an industrial research.

WR: And very, very excellent training for every student. He did a lot of things that he never would have done otherwise.

MB: Your results, the results of your research done are published in the Journal of-

WR: *The Journal of Biological Chemistry* as were most of my papers through the years. That first paper proving the structure and proving the identity and describing the isolation of [creatine?], appeared in the *Journal of Biological Chemistry*, as I recall, in 1936. Is that right?

MB: I think.

WR: A date is on the reprint, but I think that's right.

MB: Was threonine equally essential for man as...

WR: Yes. Later we got into the amino acid requirements of man. And threonine and seven other amino acids are necessary for man because they cannot be synthesized with human organism. Laboratory animals like the rat and the dog require 10 amino acids. The other two being histidine, which H-I-S-T-I-D-I-N-E, which isn't needed by adult man, and arginine, A-R-G-I-N-I-N-E, which also isn't required by man. An animal can make some arginine, but he can't make it fast enough to meet the requirements as well.

MB: Your essential interest was in the feeding experiments was in the chemical chains, then, rather than in the nutritive.

WR: Yes. The purpose I had in mind in finding which amino acids were necessary was to get some ideas of the type of chemical reactions which the animal body could and could not bring about. Once we had determined which amino acids were required, we then made a large number

of experiments in which a single amino acid would be removed and a cluster related chemical compound would be fed in its place. If it can be transformed into the amino acid the armor would then draw. If that reaction were not possible in the body, the animal would still have the amino acid deficiency and wouldn't grow. By means of such studies, we demonstrated that most essential amino acids could be replaced by their alpha-keto analogs, alpha-K-E-T-O analogs, which differ from the amino acid only in having a ketone group in place of amino.

MB: You also worked, I believe, on pepsin and creatine?

WR: I was in my earlier life. Pepsin, in isolation of the development of a simple quantitative method for measuring the pepsin content or the stomach content, worked out while I was a graduate student here. This I found very useful later while I was in the chemical laboratory of Middletown Hospital for the Insane, where doctors were much interested in finding out whether there were changes in the enzyme content of the gastrigens of these patients. That was a relatively simple task that developed one of this.

MB: Your association with creatine was prior to carbiozone,

WR: Creatine was a- creatine was a creatine, creatinine metabolism. It happened to be the topic assigned for my thesis. I was a graduate student at Yale. And following my graduation math, I worked on creatine creatinine for a number of years thereafter.

MB: Right after the isolation of threonine, you received an honor. Was that, I presume, right afterwards or very.

WR: Sure, about a year.

MB: The National Academy.

WR: I was an elected member of the National Academy of Science. This work had already attracted quite a little attention since it was reported before scientific meetings quite a while

before we had it out in pure form. So that probably the work with these unknown amino acids, unknown at that time, was responsible for my election to the national candidate.

MB: I have a list of these various honors. 1938, I believe, is the Hectone lecturer at the Institute of Medicine, Chicago. 1947, the Grocery Manufacturer's Award.

WR: Yes. And then I had the Willett Gibbs Medal of the American Chemical Society, 1952. And the Charles Spencer Award for the American Chemical Society in 1957.

MB: The Osborne Mendel Award at the American Student Nutrition in 1949.

WR: That was created, and I happened to be the first recipient of that award. Named after my old teacher, Dr. Mendel, and his close collaborator, Dr. T.B. Osborne, of the Connecticut Agricultural Experiment Station, who incidentally was the son-in-law of old Dr. Samuel Young, who first started all this interest by-

MB: John Johnson? Samuel Johnson?

WR: Samuel Johnson. Did I say? Oh, I meant Johnson, Dr. Samuel Johnson.

MB: Relationship there?

WR: Yeah, there is. Osborne was a student in the laboratory of Johnson, and in the course of time, he married his daughter. Later, he and Mendel collaborated on some of the early feeding experiments in which rats were used as the experimental animal.

MB: 1961, the 20th anniversary award of the Nutrition Foundation.

WR: That was an interesting experience. When the foundation celebrated the 20th anniversary of its founding, it picked out a few men who had received grants and had conducted work that was published with the aid of foundation funds and conferred certificates and gave us checks, each a

check for $1,000. The others were a rather innocent group. One wonders why I got into it. Dr. Beadle, Chancellor of the University of Chicago. Dr. Elbehem, E-L-B-E-H-E-M, who at that time was President of the University of Wisconsin. Dr. Vincent Devignol, Professor of Biochemistry at Cornell and a Nobel Prize winner. Dr. Stair, professor of nutrition at Harvard University, and myself.

MB: Received these honorary degrees, Davidson, Yale in 1947.

WR: 1947, and University of Chicago in 1957 or '6.

MB: '6.

WR: '6, I guess. And then the University of Illinois very kindly conferred the Doctor of Science degree on me in 1962.

MB: And served as president of the American Society of Biological Chemists in 1940.

WR: Yes, for two terms. Nowadays, you're going to be president for one year, but that rule was not in effect when I was president. And then I happened to be president of the American Institute of Nutrition in 19-

MB: '45, '46.

WR: It was one year.

MB: A fellow in 1959. Another topic, the World War II period. World War I- so many people seem to have experienced both, like Roger Adams was in both. What were you doing in World War I? You were in Texas at that time.

WR: I was in Texas teaching medical students. I was offered a commission in the nutrition division of the Army. But the University of Texas refused to release me. And Dr. Murlin, who

was colonel, in charge of the nutrition work, that's M-U-R-L-I-N, refused to accept me unless I could be released by the University of Texas. So I taught medical students the year round. We graduated students in three years by running through the summer during the First World War.

MB: And World War II.

WR: And World War II had to serve as acting head of chemistry while Dr. Roger Adams was running service in an advisory capacity in Washington.

MB: You had certain advisory responsibilities too, did you not?

WR: Yes, I was called in frequently. I didn't have a full-time position, but I was called in frequently by the United States Public Health Service and the Army and the Navy when they became interested in the type of diets. And the Food and Nutrition Board of the National Research Council was created largely to take care of problems that arose in connection with the war, particularly adequate food for the civilian population, while so much of the high protein food was being reserved for the military. We tried to find suitable substitutes for animal proteins so that more of the meat could be reserved for the military personnel. And we also had to take certain precautions to make sure that in these civilian diets, there are adequate supplies of vitamins. That Food Nutrition Board has continued after the war, but was started because of the war.

MB: I guess a lot of our scientific efforts started out during the war and have been continued afterwards. The National Advisory Health Council was a post-war activity.

WR: No, that was. I don't know exactly when that was organized. That was an agency of the United States Public Health Service and was responsible for the final action on all grants given by the Public Health Service in support of research. Whether it had been in existence before the war, I'm not sure, but certainly its amount of money it spent in support of research was greatly increased during the war.

MB: You reviewed project grants or requests for grants or projects there.

WR: Yes. Each grant was considered by a small group of scientists in that particular field. And then the results of their findings from all such small groups were referred to the National Advisory Health Council for final action. We usually approve what the small group had recommended, but not always.

MB: In terms of supportive research, during your years at the university, I suppose you noticed a change from private and legislative support to federal government support?

WR: Yes, and the most amazing feature of it is the ease with which funds can now be obtained as contrasted with the period in which I was acting. In 1934 or 5, when I knew there was an unknown component of proteins that ought to be isolated, at my own expense I went to New York and spent three days visiting foundations and begging for help. Although I had letters of introduction from one of the directors of the Rockefeller Foundation, Dr. W. E. Carter, who had been dean of College of Medicine at the University of Texas when I was on his faculty. And Carter had furnished me with the names of foundation directors who might be willing to support my research. But after three days effort, I came back without a cent. All money had been committed, they told me, and they had no more funds to provide. The trip, however, had some benefit because a year later, two members of the Rockefeller Foundation, not Dr. Carter, but two others that I had not known before, came to the office on the campus here and asked if I still needed money.

MB: Your answer?

WR: My answer was emphatically yes. But that first grant was made on condition that the university would help too. And the university was to provide more than the foundation. Fortunately, the trustees approved. Later on, they never asked again for the promise of financial support of the university, and for many years, the personnel of the Foundation supported my research by liberal grants. But the most I-

MB: When did this start? Do you recall when this initial grant was roughly?

WR: It was about 1935, but it might have been a year before that, '34 or finally. And I'm not absolutely sure, but it seems to me that they offered me $1,200 on condition the university would give me $1,800. And President Kenley was then chief administrator. I showed him this letter.

MB: This would be prior to 1930 then.

WR: Was it?

MB: Yeah.

WR: Well, he was president, so it must have been earlier than that when they began. But they supported my work very liberally later on and gave me as much as $7,500 a year, which now would look like just a drop in a bucket for the fact I'd have received $100,000.

MB: This is very important then, that is, support from the Rockefeller Foundation for your work.

WR: Oh, it was absolutely indispensable at that time. I couldn't. Later on, when we began the human work of amino acids, it was supported by the Nutrition Foundation.

MB: The federal government did not support, I suppose, by the late '40s, it was-

WR: I received grants from the Army, but it was for specialized work and not directly related to this amino acid work being supported by the two foundations.

MB: I think we've mentioned three members. It occurred to me immediately, that is Professor Noyes and Professor Abb, Professor Rodebusch, but I suppose you worked with them constantly. Did you meet with them as division heads?

WR: Yes.

MB: And probably other heads, too. That is Professor [Marble?].

WR: And the division heads that were most active, of course, were Professor Rhodebush, Professor Marble in organic chemistry, Professor Baylor in inorganic chemistry, and Professor Clark in analytical chemistry. In the meantime, water chemistry, which when I first came here was part of chemistry, had been transferred to the water survey.

MB: One other thing. Was it John Young, this pioneer biochemist? I wonder if you could tell us a little bit about him and your interest in him and possibly how he was-

WR: Well, I was a graduate student at Yale. Professor Mendel gave a talk before what was known as the biology term on a manuscript to which his attention had been called and which had apparently been in the library of the University of Pennsylvania for about 100 years without having been read. The author was John R. Young, a medical student, who wrote this thesis as part of his requirements for graduation. Someone in Washington had called members' attention to this work which dealt with the chemistry of gastric digestion. No one knew anything about the young man, and Mendel reported to this biology club the content of the very remarkable thesis in which Young discovered by experiments in frogs much that was later rediscovered by experiments in higher animals and man, one by Dr. Eilman, who was an army surgeon.

MB: Beaumont.

WR: Beaumont. Beaumont, I'm sorry.

MB: The man with the glass stomach.

WR: [Liquor?] in his stomach. It wasn't glass, but he had suffered a gunshot wound, which made a hole through the body wall and the wall of the stomach. And it healed in that fashion, leaving the hole so one could incite food directly in the stomach and watch it digest. But Beaumont

discovered things that Young had already discovered, but unfortunately no one knew about Young's work. It was published as a separate monograph, but in very limited numbers and partly distributed only to his immediate friends. Somewhat later, about 1918, as I recall, Dr. Howard A. Kelly of Johns Hopkins University, a well-known surgeon on the Hopkins faculty and one who had been much interested in the history of medicine, investigated the origin of this young man, whom he'd become interested in because of his work. He finds from the records at the University of Pennsylvania that he came from Elizabethtown, Maryland, which now is known as Hagerstown, Maryland.

So he goes out to Hagerstown on the train and finally finds through records in the courthouse that this young man was the son of an old fellow named Dr. Samuel Young, who had his medical training in Scotland and settled in Hagerstown there the years. He still could find out very little about the young man, John R. Young, but in looking through the weekly newspapers of that period, he found a notice of the young man's death slightly less than a year after he had completed his work for the MD degree and attended practice with his father in Hagerstown.

This lecture of Dr. Mandel's while I was a graduate student and this paper of Dr. Kelly's, which came later, excited my interest. And so I finally borrowed a copy of the thesis from the Ridgeway Branch of the Philadelphia Public Library. And always in my classes here at the University of Illinois, I take one lecture to discuss Young's work, his contribution, because I thought students ought to know what a student in 1803 or 4 could accomplish with very limited facilities.

MB: Was he in agreement with his professor?

WR: No, he disproved most everything that his professor had said, which was one of the remarkable thesis. And though he was working under the direction of-

MB: Rush.

WR: Dr. Benjamin Rush, he dedicated his thesis to another member of the faculty. Currently, one gets read between the lines that the feeling between the student and Dr. Rush was not as cordial

as it might have been. Every year, I gave this, took part of an hour's lecture period to discuss Young's work with my class this year. Eventually, the History of Chemistry Club here on this campus asked me to discuss this work before them, which I did. And following that, they arranged to have a facsimile reproduction of Young's thesis with the understanding that I would write an introductory chapter which I did.

This was published in this monograph by the Illinois Press. And later on, the University of Pennsylvania became aware of it and asked for permission to have a 2000 copy edition made for distribution to the members of the International Congress on Nutrition, which was meeting in Washington, D.C. A very nice introductory page was written by the President of the University of Pennsylvania, and each delegate to this International Congress received a copy of the monograph.

MB: Interesting story. Were most of your students or a good many of your students here pre-medical students who later went to the medical school in Chicago?

WR: No.

MB: Or most of them, where did most of your graduate students go in what form?

WR: Most of the graduate students went on either in biochemistry or in some related biochemistry.

MB: Into research, chemical research.

WR: Into research or teaching, yes. Undergraduate course included quite a number of pre-medical students. But when a student decided he wanted graduate work in chemistry, he was either majoring in some branch of chemistry or related fields, or was deciding to go into biochemistry himself.

So my graduate students, in addition to the majors, were students from other fields of chemistry, all from the College of Agriculture, animal nutrition department or agronomy department, all from bacteriology and zoology and human physiology, all of whom would frequently send students for minors, but would take graduate courses as part of their minor requirements. But those that did research with me were often going into biochemistry as a profession.

MB: One other thing, you mentioned that your neighbor across the street for a good number of years was Arthur Willard, president of the university.

WR: Yes. Of course I've had his neighbors over there.

MB: Chase was the first.

WR: Chase was the first, and then Daniels, and then Willard, and Stoddard, and Henry, and I've known all of them and been on very friendly and cordial terms with each of them. Dr. Willard, I happened to meet, I think, among the earliest members of the University of Illinois faculty. Soon after we moved here, I think probably within the first week, he and his wife called on my wife and me. And we became very close friends. I don't know how this, what the stimulus was for this early call, except that I met him somewhere. And the fact that it was smaller in those days, we usually tried to go see all new people with promptly to make them feel at home. And probably this was the occasion that Dr. Willard visited in our home.

MB: I remember we had some things in the archives about the president's house and all the criticism early in the 1930s. I think President Chase moved in, and he moved right into a storm of criticism about the house.

WR: I understand yet, as I understand it, he wasn't responsible for the house at all. The plans had already been made and contract let, I think, before he was elected president.

MB: That's one of the occupational hazards of being the president of the university.

WR: And some interesting stories arose about the cost of waste baskets and ashtrays, all of which were greatly judged.

MB: Stockard was also president during a key period after the war.

WR: Yes.

MB: Did you have much contact with the Graduate College, the deans of the Graduate College, Carmichael and Ridenour?

WR: Yes. I served on executive faculty of the Graduate School on several of the committees. particularly during the term of office of Dean Ridenour.

MB: It was quite a change between Carmichael and Ridenour, wasn't there, at least in terms of money and outflow?

WR: Yes. The amount of outside funds coming to the university was steadily increased in these periods. And I think probably more effort was made to get outside money than had been in the earlier years.

MB: In the area of biochemistry, what were the texts, standard texts used for students, say, when you were a student?

WR: Well, when I went to Yale, the standard textbook was a translation of a Swedish textbook by an author named Hammerstein, who was Professor [inaudible]. Later on, there were some others, one of which was by Dr. Hawk, who was the first biochemist here on this campus, who had had his PhD degree in Mendel Laboratory.

MB: He was at Illinois.

WR: He was at Illinois for several years before Lewis came in. He had this training under Mendel. And in the laboratory, both with his publication called Wallace, he outlined the experiments that Mendel was dictated during his thesis at Yale, which didn't sit too well with Professor Mendel, but he never raised an objective vision. There were no other textbooks that I can recall right now.

MB: Later on in the '20s and '30s did you have textbooks?

WR: Yes, later on there were textbooks.

MB: Did you ever try to write one?

WR: No, I never did try to write a textbook because it seemed to me it was an endless task.