THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

HARLAND G. WOOD

Transcript of an Interview
Conducted by

James J. Bohning

at

Case Western Reserve University

on

19 January 1990

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

HARLAND G. WOOD

1907	Born in Delavan, Minnesota on 2 September

Education

1931	B.A., chemistry, Macalester College
1935	Ph.D., bacterial physiology, Iowa State College

Professional Experience

1935-1936	Fellow, National Research Council, University of Wisconsin
1936-1943	Instructor and Assistant Professor, Iowa State College
1943-1946	Associate Professor of Physiological Chemistry, University of Minnesota
	Case Western Reserve University
1946-1965	Professor/Director, Biochemistry Department
1965-	Professor of Biochemistry
1967-1969	Dean of Sciences
1970-1978	University Professor
1978-	Emeritus University Professor in Biochemistry

Honors

1942	Eli Lilly Award in Bacteriology
1946	Sc.D., Macalester College
1952	Carl Neuberg Award
1954	Glycerine Award
1955	Senior Fulbright Research Scholarship, University Duneden (New Zealand)
1962	Commonowealth Fellowship to Max Planck Institute fur Zellchemie (Germany)
1968	Modern Medicine Award for Distinguished Achievement
1969	National Institutes of Health Senior Research Fellowship, University of Georgia
1972	Lynen Lecturer and Medal
1972	Sc.D., Northwestern University
1976	Senior Scholar, Fulbright Hays Program (Australia)
1979	Senior U.S. Scientist, Humboldt Award
1981	Alumni Citation of Distinguished Citizen, Macalester College
1982	Sc.D., University of Cincinnati
1985	Lynen Memorial Lecture, 13th International Congress of Biochemistry
1986	Selman A. Waksman Award in Microbiology, National Academy of Sciences
1987	Rosenstiel Medical Research Award
1988	Michelson-Morley Achievement Award

1989	Wellcome Visiting Professor in the Basic Medical Sciences Award, St. Louis University
1989	The Distinguished Achievement Citation, Iowa State University
1989	President's National Medal of Science
1990	William C. Rose Award in Biochemistry and Nutrition

ABSTRACT

Harland G. Wood begins the interview with a brief discussion of his role in the restructuring of Western Reserve University's medical curriculum. He then reflects on his childhood and education, recalling that his former Latin teacher (then, his high school principal) first sparked his interest in chemistry. He chronicles his career in chemistry and molecular biology from his college years at Macalester through his extensive laboratory research at Iowa State College, where he first developed his concept of the fixation of carbon dioxide by bacteria; the University of Minnesota, where he continued this research; various other temporary positions; and finally his current work at Case Western Reserve University. Throughout the interview, in addition to discussing research and the influence of various colleagues and associates, he often focuses on the numerous advancements that have occurred during his lifetime and their impact (both positive and negative) on the way laboratory research is conducted. He concludes with his thoughts on the future of science, stressing the importance of continued enthusiasm and motivation in scientists of all ages.

INTERVIEWER

James J. Bohning, Assistant Director for Oral History at the Beckman Center, holds the B.S., M.S., and Ph.D. degrees in chemistry. He was a member of the chemistry faculty at Wilkes University from 1959 until 1990, where he served as chair of the Chemistry Department for sixteen years, and chair of the Earth and Environmental Sciences Department for three years. He was Chair of the Division of the History of Chemistry of the American Chemical Society in 1987, and has been associated with the development and management of the Center's oral history program since 1985.

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INTERVIEWER'S NOTE

Professor Harland G. Wood has described much of his personal and scientific life in two autobiographical publications:

1. "My Life and Carbon Dioxide Fixation," in J. F. Woessner, Jr. and F. Huijing, Editors, The Molecular Basis of Biological Transport (New York: Academic Press,Inc., 1972), pp. 1-54.

2. "Then and Now," in Annual Reviews of Biochemistry, 54 (1985): 1-41.

In this interview the focus has been primarily on personal reflections of people and events in his career rather than on the scientific details which he has already clearly defined in these two papers.

INTERVIEWEE: Harland G. Wood

INTERVIEWER: James J. Bohning

LOCATION: Case Western Reserve University, Cleveland, Ohio

DATE: 19 January 1990

WOOD: That was a pretty exciting time when we put the new medical curriculum through at Case Western Reserve University (then Western Reserve University). There was a lot of controversy that had to be argued out. I had a pretty big part. They had decided ahead of time that they were going to teach on an organ system basis. The first year was to be normal metabolism and cell biology. Then the second year would be pathological and its influence on metabolism and the organ system. They finally convinced me to be chairman of the Phase I Committee. Well, everybody that was on the Phase I Committee spent a lot of time getting the first year curriculum planned. It was sort of a political undertaking to try to figure out what people wanted and how we were going to get them to accept the plan.

I realized, after a while, nobody was going to vote for this change unless they had a concrete example of what it was actually going to be like. There was a Professor [Carl J.] Wiggers here who was a very eminent professor of physiology and in his last years here. He, of course, didn't want to change, and I didn't blame him. Why should you change your teaching in the last year or so? So he was strongly against it, and probably for other reasons too, because there were other people who were against it as well. In fact, Carl Cori, during one of my visits to Washington University, really crawled down my throat. He said, "You're taking biochemistry right out of the biochemistry departments."

We set up a committee called the Kidney Committee, representing that organ system. They planned all the lectures, all the labs, and how it was all going to be put together. I picked someone from Wiggers' department to chair the Kidney Committee. I figured I'd better get some support there if I could. His name was [Ewald E.] Selkurt, and he later became a chairman at Indiana. He did a good job.

We got the plans well organized for the first year, but there were at least two or three heads that were not much in favor of a change. Wiggers was one, and there were a couple of others. So a committee was appointed to decide whether we should start or not. The doggone committee came back and said, "No, we have to wait a year." I was pretty disgusted. I certainly didn't want to spend another year running around trying to get everything lined up. [Thomas] Hale Ham was the chairman of the Committee on Education. He was formerly from Harvard, and a nice guy. I went down to see him and said, "Doggone it all, there's no committee going to say that we can't start. There's going to be a meeting of the faculty, and the faculty is going to decide whether we start or not. I can tell you who's against it right now. There's about seven chairmen for it and a couple against it." And he said, "No, we can't do that." And I said, "Well, the heck we can't. I'm going to call a meeting myself if I have to!"

So I went over and saw Dean [Joseph T.] Wearn, and the dean agreed that we should have a meeting. At the meeting Wiggers spoke against it, and some of the others and I spoke for it. The main thing they said was, "You can't start this until you know it's going to work." Well, it was called an experiment in medical education, and in experiments you don't know how things are going to work. You do an experiment and then you change it accordingly. That was my pitch. Of course, once it was started the second year had to be ready the following year. Alan Moritz, professor of pathology, was chairman of Phase II and he wasn't strong for a change. But he finally agreed that they would put the subjects in order on an organ system basis, but not necessarily correlate them. Well, that is the way we got started, and it was really a rat race because they had to build the labs for the correlated teaching in the basement of the medical school. The benches arrived the night before the classes were supposed to start. We had it fixed so that these benches were all on rollers and could be plugged in to electric outlets and next to the sinks. So Les [Lester O.] Krampitz and I and others rolled them in and got the lab going.

Well, that's starting at the wrong end of my career.

BOHNING: That's all right. We can come back to that later.

WOOD: In your letter you asked about early grade and high school education and teachers.

BOHNING: Yes. You indicated that you were frail as a child and later that gave you an advantage. I was wondering if that was due to an illness of some kind.

WOOD: I don't think I really had a serious illness of any kind. I can't really tell you. I think that I was fragile in some ways. I used to have headaches quite a lot. So I did spend two years in kindergarten and two years in first grade. As I said in one of those papers (1), I was the third in the family and I had two older brothers who were good athletes. It was a big tradition in our family that you had to be out for athletics. My Dad was very interested in it. So in a way, it probably helped me because I got stronger and was two years older than my competitors. I never was as big as my older brothers. My older brother Chester, in particular, was interested, and he more or less coached me, especially in track. I always wondered about this because we had a twenty-acre farm and he brought all the equipment out there I think he acquired during the track meets. There was a discus, a shot put, a pole-vaulting pole, a javelin, and so on. He fixed up a jumping pit, and we had high and low hurdles. So I got to practice on all of these things. As a consequence, I think I got pretty good because of this arrangement. Of course, in football we were always playing touch football and the like.

BOHNING: You had to walk about a mile to the grade school?

WOOD: Yes. Well, I usually ran to the grade school. We had to come home at noon to water the horses and cows. It was crazy. In wintertime the horses were always penned up, and when we took them out, they'd make a break for it because they wanted to run. It used to be a son-of-a-gun. They'd run down the hill, and then we'd have to go down and try to get them back again. By the time we got to school it seemed like we'd been running forever! [laughter] So we were in pretty good shape.

As far as the grade school is concerned, I remember all the teachers, but I don't remember that they had a big impact on me. I guess I was kind of an upstart in some ways. If I liked the teacher I was very good. If I didn't like the teacher, I was not so good. I remember the teacher in the fifth and sixth grades, Rose Staley. She was noted as kind of a tough teacher and wasn't very good looking and I didn't particularly like her. So I would study (I always got good grades), but when I'd get through with the studying I'd start horsing around a bit. She got so she was pretty down on me, so she put her desk so the corner was right in the aisle and she was looking right over at me. In those days we had inkwells and pens you dipped into them. One day the kid behind me took this pen and stuck me right in the seat through the crack where the seat folds up. I went right over onto her desk. I never could explain to her what the heck was going on. My Dad had to come down. She put me in the hall for about two weeks. I had to study there and then come recite to her. It seemed to me it was a little unreasonable.

BOHNING: Did you do much reading during that time or were you more interested in outdoor activities?

WOOD: My father read to us stories from the Youth's Companion that came every week. As a child, I read most of Zane Gray's books and books by [Joseph A.] Altsheler of the Indian fighters, mostly adventure stories. In high school and college I did not read much; I was too busy in athletics. I did the studying that I had to do. I suppose our family was sort of academic, at least for Minnesota at that time. Mother had graduated from high school. I think Dad only went through eighth grade. Neither went to college. In those days they had country schools, and both Dad and Mother taught in country schools. Mother came from a pretty well-educated family. Dad not so much. There wasn't a lot of encouragement by them to go to college. I think the reason my brothers went to college, at least my older brothers, was because they were good football players. People came and encouraged them to go to college so they'd play football. By the time I came along it was understood I was going to go to college. It was just the thing you did in the family. They tried to get my oldest brother Chet [Chester] to go to Carlton College, which is a good college. But my folks didn't want him to go out of town. So he went to Mankato State Teachers College and Huron College instead of going to Carlton, which probably was a grave mistake. They got my next brother Delbert to go to Macalester College. All of us went to Macalester College after that. I was the first one, though, that took a little more academic and scientific approach. I really thought I was going to try to be a doctor and get an M.D. degree, and of course one had to take scientific subjects to do that.

BOHNING: How early did that thought begin?

WOOD: I know that it was certainly by junior high school, and it probably was even before that. Well, before that you just took what they told you to take. There weren't any decisions at that time to be made while in grade school.

But a funny thing happened. You had to take Latin to get into medical school in those days, so I was taking Latin in junior high school in 9th grade. Then I went over to high school which was in a different building in a different part of town. The Latin teacher that I had had in junior high school by that time was the principal at the high school. Well, I signed up for Cicero. I used to have to work hard on this doggone Latin. I'm not very good at foreign languages. I took Cicero for about two weeks, and then I got to thinking to myself, "What the heck. Why take Cicero? What good is it going to do me in the end anyway?" So I went down to see Louis R. Kresensky, who was the principal. I told him I didn't really feel like I was doing the best thing for myself, taking Cicero. "Well," he said, "Wood, you ought to take chemistry. Chemistry is very important in industry. It's very important training. Why don't you drop your Latin and take chemistry?" Which is kind of interesting because he was my former Latin teacher and yet he told me that. [laughter] So I dropped it and shifted to a chemistry course.

That probably did have a pretty big influence, because they had a teacher named Marie Lang. She was a good teacher, and I got interested in chemistry. It was a lot easier for me for some reason than Latin, and then I took physics. You had to have those courses to get into medical school. So that's sort of how I got started in chemistry. But my major interest was athletics, football, track and basketball. I made letters in all of them and played throughout high school and college. I was interested in Big Bands and dancing. I was a good student. I made the honor roll but mainly I guess I was just thinking, "O.K., I'll get into medicine." Well, it turned out that the Depression was so bad that it was impossible for me to go to medical school. Maybe it wasn't impossible, but at least I decided I couldn't do it. So I went to Macalester and there majored in chemistry and mathematics.

BOHNING: Had your older brothers done any work in science?

WOOD: No, they hadn't. It's sort of interesting. There's a break in the family right at me, the third of the six children. I got a Ph.D. Then my brother Earl who came after me got a Ph.D./M.D. in physiology at the University of Minnesota and my youngest brother, Wilbur, got a M.D. degree. Louise was between Earl and myself. She didn't go into science, but it was sort of a break right at me.

My oldest brother took shop and mechanical drawing and forge and things like that. He got a kind of a technical degree from Mankato High School. Delbert ("Buck") took sort of a general course. I used to envy him. When I went to Macalester he was a senior and he was the captain of the football team, and I was a freshman. We worked for our board and paid for our room. I was making salads with my brother and peeling potatoes with another star athlete for my board. My brother took a nap after we got through working. I had to go and work in the chemistry lab and then go out for football, while he'd been sleeping for two hours. I thought, "Doggone, pretty soft life he's got here!" [laughter]

BOHNING: When was it that you gave up the idea of medicine and decided to concentrate on chemistry?

WOOD: I don't know. Probably by the time I got to be a sophomore at college. You see, the Depression really hit around 1929 and I graduated from high school in 1927. My Dad was a real estate agent and sold land. He was one of the few who didn't really go bankrupt. A lot of them had loans on farms that they were going to sell and they got caught in the middle. It was obvious that my Dad couldn't send me to medical school. By the time Earl came along, which was about five years later, they could help him some. Actually he got a fellowship in physiology over at the University of Minnesota and took a combined Ph.D./M.D., so that helped as well.

I got married in college, which was very unusual in those days. It was probably the best thing that ever happened to me, because I really got down to business after that. When I was a sophomore, I was crazy about this gal and of course she was taking the easy courses and I was taking the tougher ones where you had to go to lab. I was out for football or out for this or that, and the time to study was pretty limited. She and some of the other gals and boys would come over and holler up at me in my room. I was supposed to be studying, but off I'd go. Well, I damn near flunked a course in math analysis. But we got married that summer.

It was rather amusing. I had to talk to the president about whether we could continue. There were lots of married kids but they lived in Minneapolis and St. Paul. They weren't on campus. He said, "Well, O.K. Just so you don't have any visitors to your room." We just had a single room. Millie's folks paid for her education. It was only $87.50 a semester for tuition and I worked for my board even then. We had a room close by. The chemistry professor liked me, and I think he realized that we were married and needed help, so he gave me a job correcting papers and filling up the reagent bottles. I also was a representative of a clothing store in St. Paul so every once in a while they'd come out to the dorm and I'd get up and give a little talk, and they'd show their wares. I'd go down there on Fridays because I played football on Saturdays. So I got a little money there. Between all of these things I actually graduated out of debt. [laughter] Before then I had been in debt.

BOHNING: Can you tell me something about the chemistry department? How many faculty were there?

WOOD: Well, as I remember it, there were only about three. I remember the person that taught physical chemistry, Chester H. Shiflett. He was working for his Ph.D. at the time and was a good teacher. He was pretty tough. Dean Richard Jones, who was also the chairman or at least professor of chemistry, was a nice guy, but he didn't teach what I considered a good chemistry course. I knew he didn't teach a good chemistry course when I got to Iowa State College at Ames, Iowa (now Iowa State University), and started taking some chemistry. I was very, very chagrined at how little I knew about chemistry. Dean Jones was a nice guy but he wasn't teaching a very advanced course in chemistry.

I think the person that had the most influence on me was a professor of biology. His name was O. T. Walters. I took microbiology and comparative anatomy from him.

BOHNING: Was there any reason why you took biology courses?

WOOD: Well, for one thing he had an in at getting students accepted from Macalester into the medical school at the University of Minnesota. He was known as a good teacher. So when I took some of his courses I was still thinking I might go into medicine. But I was still fully planning on, if that failed, at least going into chemistry for a Ph.D. It is sort of interesting. I applied for fellowships in chemistry at probably ten places. Walters would look at my applications even though they were in chemistry and give me advice as to how to phrase them. Then it was kind of amusing because it turned out to be very decisive. One time when I was in his office, he reached up and pulled down a book and said, "Why don't you apply here?" It was Iowa State College, and this was a book by [Robert E.] Buchanan who was chairman of the bacteriology department (2). Well, I had liked microbiology when I took it from Walters so I said, "O.K." I kind of slopped through that application and sent it off to Iowa State College and that's the only one that was successful. The chemistry applications were not successful.

The fact of the matter is that the day I got the acceptance I rode the streetcar over to the Ag campus at the University of Minnesota. There was an eminent biochemist there named [Ross Aiken] Gortner. And I went over to see Gortner to see if I could get a fellowship. He said, "If you come for a year and make a success of it, I'll guarantee you a fellowship the second year." And I said, "O.K., I'm coming." Gosh, when I got off the streetcar on my return, here was Millie with a letter in her hand. That was the acceptance from Iowa State College--$450 for nine months. Fifty dollars a month. Of course you got your tuition free. So just like that I changed from chemistry. I think that had a big impact, because the microbiology that [Chester H.] Werkman was specializing in was really intermediary metabolism, which in those days was at the forefront of the biological sciences.

[END OF TAPE, SIDE 1]

WOOD: Gortner was more involved with agricultural biochemistry and things like that. I asked him, "What kind of a job can you get after you get your Ph.D. degree?" He handed me a letter from industry which said they would pay $3,600. I said, "That's it." In those days a professor such as Werkman was only getting $4,000 or $4,500. The salaries were always published in the Des Moines paper. Of course, it was during the Depression and the dollar was worth something, that's for sure.

By going to Iowa State I started working on metabolism of bacteria. I was reading about [Otto] Warburg's work in Germany and others who were working on yeast metabolism.

BOHNING: When you applied to Iowa State, was it specifically to work for Werkman?

WOOD: No. I just applied.

BOHNING: Then how did you make the association with Werkman?

WOOD: Well, he picked me. I think what really got me the position was that he wanted somebody who had chemistry and a lot of the people who were applying were biology majors. Here was a guy who had a major in chemistry and mathematics. So I had the qualifications that he wanted and I think that's why I got the fellowship. I never really talked to him about it, but it's logical that that's the way it happened.

BOHNING: Had you done any research at Macalester, or was it just laboratory work associated with courses?

WOOD: I did a little. Of course, it didn't amount to much. In chemistry they had a sort of research lab. Dean Jones wanted me to take weeds and heat them in the absence of air and find out what gases were generated. Mostly I melted his pyrex flasks. [laughter] It was not real research. We had organic labs where we synthesized compounds, but it was more or less a cookbook affair. We didn't get into what was really going on very much.

I took qualitative organic chemistry at Iowa State. They gave you unknowns. That's the first laboratory where I got down to brass tacks. How do you identify a compound? What is the melting point? How do you separate mixtures? They gave you a mixture and you really had to get to work and think pretty hard about how to identify the components. That's when I found out I didn't know much chemistry. I took the course from Henry Gilman, a world-renowned chemist. We had a book by [Oliver] Kamm (3). It was a little book that told you how to identify compounds and separate them. At the end of each chapter there was a set of questions. So the first time I studied those questions like mad. There wasn't a one of them in the first test, and I think I got a D or a D-. So the next time, I didn't study Kamm. I studied what Gilman gave us. Jeez, he asked all the questions out of Kamm. [laughter] I was in tough shape!

This was kind of a rough time too because my wife Millie was pregnant. She was in Foley, Minnesota, and here I was in Iowa having a hell of a time with this chemistry course. So I studied like mad for the final exam. This time (and he was known for this) he gave a question that was like a puzzle. If you didn't get the first part, you couldn't get the second part and the third part and the fourth part. I sat there studying that question, and finally I got it figured out. But it was getting awfully close to the end of the time. So I was writing like mad, and they called for the papers. I said, "No way! I'm not giving you this paper. I know the answer to this question, and I'm flunking. You're not going to get my blue books until I get this down!" Well, they finally let me finish it. To my disappointment, I didn't get a very good grade. It turned out that they'd got the blue books mixed up and there was one blue book missing. So I finally got that straightened out and passed the course.

But Gilman was awfully good to me. In those days if you wanted a compound containing carbon-13, you had to make it yourself. I'd go over to see him, and he'd tell me what he thought ought to be done. He even sent two students to us from chemistry to work on a couple of problems. So he was very good to me, and very upset when I left. I was there for four years as a graduate student, and then after a year as a postdoc at Wisconsin I came back and was there for seven more years. So I was pretty well known as one of the younger scientists around that place.

We had a very good time in research there. Werkman had seven or eight graduate students. He was pretty well supported for those times because they had the Ag Experiment Station and they had an Industrial Science Foundation, and he got money from them. We didn't spend much money in those days, but he did have support from these sources for fellowships. He gave us assignments. In my case he just said, "I want you to work on the propionic acid bacteria." And that's about all he said. He said, "I want you to read every article that's ever been written about the propionic acid bacteria." Well, practically all of them were in German and I had never had any German. Jeez, I'd take the German dictionary out and look up the words, line them up, and then try to figure out what the sentence meant. It took me a long time to figure out if it's got "ge" in front, don't look for "ge."

There was a Dutchman named [C. B.] van Niel who became a very eminent microbiologist. He came to the United States at Pacific Grove, California, and taught a very famous summer school course. Arthur Kornberg and Paul Berg and a lot of others took this course because he was a very good teacher. He had worked on the propionic acid bacteria for his Ph.D., so I had his thesis to work from. Fortunately it was written in English. So I started working on my assignment.

The controversy was: is succinic acid a product of the fermentation? At that time we made our own media. One took yeast and boiled it in water and filtered, thus making yeast extract. van Niel said that the succinate was coming from the aspartate of the yeast extract and that it wasn't coming from the glucose. Well, I set up the fermentations. In those days we did what were called carbon balances. You fermented the sugar and determined how much glucose was utilized and multiplied the moles by six. Then you had to account for all that carbon as products from the anaerobic fermentation. Well, when I did it, I found that I needed the succinic acid to account for all the carbon of the glucose that was fermented.

[A. J.] Kluyver was van Niel's mentor. Werkman brought him to Ames to give summer school lectures. In some ways Werkman was pretty smart. When Kluyver came I was down in a lab in the basement, and Werkman introduced me and said that I had shown that van Niel was wrong. Boy, Kluyver just went straight up in the air! "van Niel is never wrong." [laughter] Well, I finally convinced him. By that time we bought the yeast extract commercially. I cut the amount of yeast extract down as much as I could and I got more succinic acid than the yeast extract that was added. I showed him this and said, "van Niel's definitely wrong, that's all there is to it." Kluyver had to agree, so that worked out fine.

BOHNING: You said Werkman didn't give you much direction initially. Did he have much daily contact with you or were you pretty much on your own?

WOOD: Well, he was kind of a funny person. We each had a weekly conference with him, but I don't remember for certain whether it was exactly like that when I was a student, but I was there for quite a while afterwards. He was possessed by the idea that everyone was against him. He thought the dean wasn't supporting him. When I'd go up to talk to him, all I would hear was his problems. We'd never talk research. I'd start, but that's as far as it got. He was trained as an immunologist and shifted into this field. He never really directed us very much. He was always proud of his students, and made them feel good by his praise. If he had a visitor he would talk about what was done. He had us out to his home. He had a basement recreation room and we'd have seminars down there and talked about our research. But day by day there was very little interaction.

In a way I was almost running the lab towards the end. We'd all talk about our research and then I'd figure out, "Well, we ought to do this." I'd tell them, "You go up and tell Werkman you want to do this. And he'll tell you, 'Oh, I don't think so.' But don't argue with him. Just tell him. Then in about a week he'll come and tell you you should do this." So that's the way we'd get things done. But he was good at getting us equipment.

I found carbon dioxide utilization by the propionic acid bacteria more or less by accident. I grew them on glycerol and calcium carbonate was added to neutralize the acid. I determined how much calcium carbonate was added and then at the end how much was left and how much CO_2 was collected. I collected less CO_2 than the calcium carbonate I put in. Of course, the dogma was that it was impossible for heterotrophic bacteria to utilize CO_2 that require organic compounds for growth. I was fully convinced of that too. I had set up five different species, and the total CO_2 was negative with each. I figured I must have weighed the calcium carbonate out wrong or something like that. But there was a discrepancy. When I did the carbon balance, it wasn't too bad. Of course, if you use CO_2 which is only one carbon, it doesn't influence the carbon balance much. The carbon balance came out a little high, about 105%, and I didn't pay much attention to it.

The other thing one calculated was oxidation-reduction balances. If it's an anaerobic fermentation, for every oxidation there's got to be a reduced product. Well, in these fermentations the oxidation-reduction balance was way off, and I couldn't figure out what was going on. I had found a lot more oxidized products than reduced products. So I was bugging everybody about this. When I was writing my thesis, I wrote up all the other results and then I got to these results. I said, "Gee, I wish I could write something about this. It's a lot of work to do all these analyses." Then all of a sudden it hit me. "My God, if that CO_2 is reduced, that's the missing reduced product." Now the calculation came out right. So I knew immediately that the bacteria were using CO_2. I went over to see Werkman about it. By that time we had the whole thesis typed but I was using a completely wrong way of explaining how succinic acid was formed. I knew everything was wrong in that thesis, but he didn't want to change it. He said, "What the heck. You've got to retype the whole thing and everything else." So we just left out CO_2 utilization. But I'm not sure he believed it.

[Halvor O.] Halvorson was the chairman of the Microbiology Department at the University of Minnesota, and they had on their staff [Erling J.] Ordal who was a good biochemist and microbiologist. When I gave my little talk at the North Central Branch of the Society of American Bacteriologists (4) Halvorson said to Werkman, "I don't believe a word of it." And Werkman said, "I don't either." Now that's inconceivable. Here's a guy with his name on the paper and he said, "I don't either."

BOHNING: What kind of general reaction did you get to your presentation?

WOOD: There was mostly disbelief, there's no doubt about that. Then I gave it another time in St. Louis. It was known that CO_2 could be reduced to formate by E. coli and it also was known that the methane bacteria could reduce CO_2 to methane. (This was all happening about the same time.) But this was different because in our case the CO_2 was going into a carbon-carbon linkage, which is more like the synthesis that occurs in photosynthesis. So it was different. I remember [Selman A.] Waksman, who isolated many antibiotics and received a Nobel Prize Award, got up and said, "I don't see that this is so important. We all know CO_2 can be reduced to formate." I got peeved and said, "Well, that's not so important. The question is how do you make compounds out of CO_2? Now you're making a carbon to carbon bond, and this is more like what occurs in photosynthesis. This is quite a different situation." When we got through he said, "Yes, I guess that's true."

In 1939 I went to the International Congress of Microbiology in New York. At that time carbon-11, the short-lived radioactive isotope with a 22 1/2 minute half-life, was available. I met Martin Kamen and [H. Albert] Barker and Sam Ruben. They had a cyclotron at Berkeley. Of course, our interest was, "Where is that CO_2?" Is it in the succinic acid the way I postulated? If it is, it should be just in the carboxyl positions. So I talked to them and they said, "Well, if you want to come out, we'll do an experiment, but you realize that the whole thing has to be done in not more than five hours. You've got to ferment it, you got to get your succinic acid out, and you got to give it to us to assay the counts."

You see, we were usually doing fermentations for five or six days by growth in the medium. So I went back and grew the bacteria and then I spun them down so that I had the concentrated bacteria. Then I added the glycerol to them, and of course they fermented the glycerol fast because there was a whole pack of bacteria. Then I spun them out and took the solution up in plaster of paris which gave a dry mixture. Then I put it on a funnel and poured ether through under acid conditions. The succinic acid and of course the volatile acids were extracted rapidly under these conditions. Then I added silver nitrate, and the silver salt of succinate is insoluble. I also could add barium and alcohol and obtain the barium succinate. I got so I could do the experiment in about three hours or so. When I went up to see Werkman I told him about this and he said, "No, you can't go." I wasn't even asking him for money to go. I was going on my own expense in my own car. Of course, I was crushed because I thought, "Jeez, here we have the methods and now all we have to do is to go out there and we'll know where this CO_2 is."

Then I found out about Al [Alfred O.] Nier at the University of Minnesota. He was doing work with carbon-13, the stable isotope which is assayed using a mass spectrometer. It just happened that my brother, Earl, who was in physiology at the University of Minnesota, knew about Al Nier. I was telling Earl about my problem when we were at our summer place out at Lake Washington near Mankato, Minnesota. He told me about Nier, and I went up to see Al. I told him what I wanted to do, and he was really pleased because a lot of people were doing work there, but they'd feed CO_2 to a rat and then they'd take the organs and burn them and assay the resulting CO_2. But they didn't have any specific question. He could see that I had a question that could really be solved.

Werkman didn't object to this at all. I never was quite clear why. We never really talked about it. The only reason I can think of was that he didn't want Barker and the others in California to get in on this subject because they would have been able to study it there and we wouldn't have been able to in Ames. Whereas with Nier, he had no fear of a physicist cutting in. Maybe that was the case, but he never told me so. Anyway, we struck up a nice relationship with Al Nier. He's a wonderful person, and he helped us a lot. It turned out that the fixed C-13 carbon dioxide was where we predicted it would be.

[END OF TAPE, SIDE 2]

BOHNING: When you were determining carbon balances, how difficult was that experiment? Was it straightforward? That was almost state-of-the-art then, wasn't it?

WOOD: Well, in some ways it was difficult. For example, you had to determine propionic acid and acetic acids which are close homologues and not easy to separate. They used what was called the Duclaux distillation in those days. Propionic acid is a little more volatile than acetic acid, so if you distilled and collected fractions, by the rate at which the acid distilled you could calculate how much propionic acid was there. This was not a very good method. When I came to Iowa State, there was a fellow named [O. L.] Osburn who was working on a method called the partition method. You shook up your acid solution with ether in a separatory funnel and then you determined the amount of acid that was extracted into the ether. You really titrated the aqueous phase. If two different volumes of ether are used compared to the water phase, a different proportion of the acid is extracted. Then simultaneous equations are used to calculate the amounts. So we steam distilled, then concentrated, and then did the partition. We worked out methods for succinic acid. It could be extracted with ether and then precipitated with barium ion. When it came to the C-13 experiments, the question of how much C-13 was in the propionic acid and how much was in the acetic acid was more difficult to determine. I remember we used an azeotropic distillation. We had a column half-way up to the top of the ceiling (about 2 meters long). The acid was distilled with toluene and fractions collected. It was supposed to separate propionic from acetic acid. It did a pretty good job, but not 100%. But if one determined how much acetic acid was in the fraction containing propionic acid, a correction could be made for the acetic acid. Of course, we made most of our own equipment. I remember we used to make the glass electrodes for determining pH. You sat and blowed bulbs until you got one thin enough to work. They were delicate. You'd get mad as hell when somebody broke it and you had to make a new one. That sort of thing we did a lot. We did our own glass blowing.

BOHNING: This question may be fit for later, but since you're talking about this I wanted to ask. You were doing that as a graduate student. You had to be very versatile as you just said --glass blowing and making your own equipment and doing chemistry, doing a number of different things. Do you think today's students have the same opportunity to be that versatile? Or do you see today as being more focused?

WOOD: Well, I think today things have to be more focused. I was reading an account by [Maurice B.] Visscher who was at Minnesota when I was there (5). He said, and it was true, that in those days you could go to a library and read practically every article. You didn't read just what you were interested in. You sort of browsed through the whole journal. He said now (and this was written eleven years ago) if a person spent all his time reading all the articles in physiology and related subjects, and he read a page a minute, it would take him three years to read what was published in one year. The whole subject about chemistry is so broad compared to what it used to be that there's no way that one is going to be able to be quite as broad as we were. We weren't so broad; it was the fact that the subject was narrower. I read almost every article in microbiology and biochemistry, and I was interested in every article. Nowadays there's no way I could do that. I don't think the students today work quite as hard as we did. After all, at Ames, there wasn't much to do. We were poor, we didn't have any cars, so we did a lot of work on Saturdays and Sundays. They did here for a long time, but they don't now. I can tell you that. I sometimes come over here on Saturdays and it's pretty empty down the hall.

BOHNING: But experimentally, do you think you were more versatile?

WOOD: Well, these students use some pretty complicated equipment and they've got their computers. You can say, "Well, a computer." But they've got to know how to use those computers, and they've go to know how to make sense out of what they're doing and use some judgment too. So it's hard for me to really nail it down. I don't doubt that they're just as smart. But you don't work alone as much as we used to. You can't work alone as much as we used to. You've got special techniques that you apply. If you want to do x-ray crystallography, somebody is isolating the enzyme and making the crystal, somebody else is running the x-ray crystallography, somebody else is interpreting the results, and it's all going into one paper.

Well, sure, they've got to understand something about it, but you just can't do it all. That's all there is to it. So you pick up an article now, and for crying out loud, sometimes you see twelve authors on it. That was inconceivable in the past. Some of it's artificial, I'm sure. The granting situation makes it so people have to have their names on papers. For that reason people's names are put on papers on which they didn't do very much. That's for sure. I do work now with molecular biologists. I don't do the molecular biology, but we can do some wonderful experiments together because we've got the enzymes, we know where the active sites are, we know which amino acids we think are important, and we can tell them, "O.K., leave that amino acid out." And they can do it! We isolate the protein, then we test them and so on. It's fantastic. It was impossible in the past to do what you can today. So more and more we are forced to work as a team. Then if we get this enzyme that's got an amino acid left out and it doesn't work, "Well, O.K., why isn't it working? Is it a component of the actual catalytic reaction? Part of the catalytic process? Or is it because the conformation of this protein has changed?" Then pretty soon you're going to crystallize this protein. Then you're going to send it to somebody to get x-ray crystallography. Then they're going to interpret whether the three dimensional structure is different in the altered protein than that of the original enzyme. All of this is impossible for one person to do, at least in my opinion. But if you're going to stay at the forefront of science nowadays, you've got to do this. There's no way you can avoid it. Especially in enzymology.

BOHNING: By comparison then, back when Werkman assigned you to the propionic acid bacteria, were the others in the group working on related problems or were each of you working sort of independently of each other?

WOOD: We were independent in a way. For example, he'd assign one to the butyric acid bacteria and another one to E. coli and so on, but in general we were doing carbon balances and oxidation-reduction balances. And then when isotopes became available, we were feeding in isotopes and getting out the products and degrading them to determine the location of the isotope.

That was pretty much true even when I came here in 1946 as head of the department. I knew practically every experiment that was going on. We weren't as big, I'll grant you that. But I knew pretty well what was going on. And I could almost walk down the hall and say, "How did the experiment go?" Well, nowadays, heck, there's work that I don't have much of an idea about what's going on, even when I hear it presented in seminar. You know, these molecular biologists and immunologists have their own little language. Many different disciplines are applied in an investigation.

So, it's a quite different situation than it was at the time I was a graduate student. The closeness can't be the same as it was, because at that time you knew what was going on. Now each group has got their own specialty. They meet and they discuss their problems. When you have a general seminar like we have every week, it isn't like it used to be. When we had a general seminar there were plenty of questions popping back and forth. Now they more or less just give their talk. If I popped in questions where I didn't understand something, they wouldn't get through the first sentence. Things are very much different. Progress, I think, is tremendous. That's why there are so many articles. Not all of them are good, but still there is lots and lots of excellent work going on. No question about it. It's hard to compare. It was exciting in those days. It is exciting now.

I think the tragedy now is that the support of grants is so competitive that it's really serious as to what's going to happen in the future. I was surprised that one of the people in heart and lung said they only funded 12% of the approved applications. Now you know, when you have to be in the top 12% of the applications, that's awfully rough.

BOHNING: And that's a budgetary judgment rather than a scientific judgment.

WOOD: Well, the study sections have only got so much money. I'm glad I'm not on a study section anymore. There's lots and lots of good work that's not getting supported. When you walk down the hall you see certain people in the department that aren't getting grants who had been getting grants for a long time who are pretty good scientists. The students see these people and they begin to say, "I don't know if I want to get into this racket. This is a pretty tough racket." If you want to make the professional football team, there's a lot of competition. You don't all make the professional football team. In some ways there's got to be competition in science too. But I think it's getting a little bit too tough.

BOHNING: Does this also affect attracting students?

WOOD: Oh yes. I think students are going into business and things like that more than they are into science right now. At least it seems that way. And there's a consequence. Lots of foreign students are coming in both as graduate and postdoctorals whereas we used to have very few. Now a lot of departments have got a heck of a lot of Chinese and Polish and Indian students. That's all right in a way, but you'd like to see more people from our country being trained in the field. I don't know whether it's going to be true or not that we're going to get behind because we won't have the proper number of trained people from our country, and foreign students will be taking over a lot of the jobs here.

BOHNING: Do most of the foreign students stay here as opposed to returning?

WOOD: A lot of them stay here. They don't want to go back to India, for example. They want to stay here because they can't have the same opportunities for jobs and equipment in India. Some go back, but a good share of them want to stay here. I don't know just how it's going to go. I got a notice the other day that the society is calling a meeting of all the past presidents of the Society of Biological Chemistry to consider this problem and what should be done about it. Well, I don't know if I have any very good ideas how to convince Congress that they should give more money. That's what amazed me when they said heart and lung because if anybody ought to be convinced, the

Congress should be easily convinced, that heart and lung ought to have money. If it comes to more basic things where you can't point to applicability, then it's easy to see why one might not get as much money. General medical, where most of my support for basic research comes from, is always the toughest. That's why I was a little amazed when I heard about the heart and lung situation. Just why that is I don't know. But it's certainly true. A lot of people are having a heck of a time getting support, and they will continue to have a hard time. I've been pretty fortunate. I've sometimes wondered at my age whether they should be giving me money. They still give it to me. Of course, that money is supporting a lot of younger people and training a lot of younger people, so in a way my age isn't quite as important as what's going on. My age has never come up in the study sections as far as I know. If you put in a good grant apparently they don't say, "Well, he ought to quit."

BOHNING: Well, we got sidetracked, but I think we've covered some very good issues. I would like to go back to Iowa State for just one more moment. You have commented that you had studied fermentation of glycerol for no apparent reason.

WOOD: Well, in some ways it was for no apparent reason, but that isn't quite true. van Niel had done fermentation of lactate, glycerol and glucose, and he found (this is kind of interesting in a way) that he only got propionic acid from glycerol. He didn't get succinic acid, he didn't get acetic acid, he got damn near 100% propionic acid, which fits an oxidation-reduction balance. He said that the reason he didn't use calcium carbonate in this case to neutralize the acids (he just put them under nitrogen and used a phosphate buffer) was that it was too hard to get decent balances. In hindsight, I always wondered if he set up the glycerol fermentation with calcium carbonate and he got negative balances and thought, "Oh, oh." Anyway, I set them up with calcium carbonate. I didn't think, "O.K., let's see what happens." I found a lot of succinic acid and the interesting part was that for every CO_2 used there was one succinic acid so it looked like a three-carbon and a one-carbon compound were combining to make the succinic acid. I don't remember of ever just sitting down and saying, "Well, I'll get something very good out of this." I just thought, "I'll do the balances and I'll see what happens." I did show that if you set the glycerol fermentations up the way van Niel did, with the exclusion of CO_2, you do get just propionic acid. But he didn't believe it when I found the CO_2 fixation. So, it makes me think he must not have done the experiment with $CaCO_3$ and glycerol. If he'd have had a negative value, he would have said, "Oh my God, I had it and didn't know it." So I don't think he did the experiment.

BOHNING: It was still the Depression time as you were proceeding through Iowa State. Had you thought about what you wanted to do after you finished your degree?

WOOD: I was thinking, "I'll get an academic job somewhere." At least that's the way I remember it. I don't think I was interested in going into industry. I was enthralled with research and I liked teaching. I didn't do much teaching. I really liked to have a bunch of students around to discuss things with. I think even in those days I felt that was a very important aspect of research and success.

You asked me about people that influenced me. You know, when I look back, I think the ones that influenced me were the ones that I took a negative attitude to, because I didn't want to act like them. I think they had a bigger impact on me than the ones that I looked up to. I know with Werkman I had a pretty good relationship and he obviously was getting a good reputation from the work that was being done there. In fact, he came up fast. He had always told me that I could advance through the ranks. So when we bought a house and I told him about it, he said, "Do you think that you're going to stay here forever?" Well, I was just flabbergasted, and we got into a hell of an argument. And I said, "O.K. I can't order any chemicals. I don't have any graduate students. I have been here for seven years, and in a way I am practically running your research." I said, "O.K., give me a graduate student I can run by myself, and give me a small budget so that I can order stuff by myself." It wasn't that he hadn't supported my work; it was just sort of the principle of the thing. And he said, "No way. No way will I do this. You think you can get a job?" Of course it was ridiculous. I had just turned down two jobs that were paying more than he was giving me.

So I went home that noon and got on the phone and called up Maurice Visscher at the University of Minnesota and I had a job before you could say, "Jack Robinson." I made more money. As I look back, it's probably very fortunate. Werkman kicked me out of a place where I probably couldn't have gotten very far ahead. I ended up in a medical school where people outside looked at me and said, "Well, Wood's in a medical school, he'd be a good chairman of the department," when the fact of the matter is I hadn't had any biochemistry at all. I hadn't even taken a course in biochemistry. But in a way I was fairly well trained because intermediary metabolism was in the forefront, or coming to the forefront, of biochemistry. So in spite of the fact that I hadn't had very many courses, I had done a lot of studying of the experiments of Warburg and others so I knew a lot of intermediary metabolism. I think my experience in research was a pretty good course in biochemistry. It was amazing. You couldn't find a decent textbook in biochemistry at that time. We wrote our own syllabus. We should have published it. It would have sold. There's no question about it. We waited for Al [Albert L.] Lehninger and others to publish textbooks. [laughter] In a way my bad experience had a big impact on me. I decided I'll never run a department where I don't give the people the freedom to do their own work and to get credit. I got credit, but Werkman's name was on every paper, of course. I think that that made me set up a democratic type of department here which, frankly, has carried on almost to this day. It's amazing. We wrote bylaws that included what rights I had as chairman and how they could out vote me. It never really came down to that because if you've got any sense, and know that you're going to get out voted, you know you better not do it.

BOHNING: Is there anyone else beside Werkman who had this "negative" impact?

WOOD: Well, a little bit at Minnesota because Visscher had a big department and he had a lot of prima donnas who were giving him lots of trouble. I decided that we'd better get staff more or less equal here and not allow for prima donnas. Everyone was going to live by the same rules. Each staff member could have so many graduate students, and so many postdocs. We kept things more or less on an equal scale so everybody had an opportunity. This has changed some. The reason it's changed is support is so difficult. We used to have stipends for fellowships from department fellowship grants that took care of all of our graduate students. It didn't come off individual grants. Well, if somebody didn't have a grant, I could say, "Here's the support." Nowadays, if you don't have a grant, you don't have any money to pay for a graduate student's tuition, and you're out of luck. That's pretty tough. There's no way to get ahead. I don't know just what's going to happen in the future. So that has changed.

[END OF TAPE, SIDE 3]

WOOD: One positive influence on me was a dairy microbiologist at Iowa State College named [Bernard W.] Hammer who seemed old to me, but he probably wasn't so old. I was always impressed with this man because he worked in the lab. Most professors didn't work in the lab. I always thought, "I'm going to see to it that I stick in the lab." I admired Hammer for that. So when I got here, in spite of the fact that I was chairman, I always tried to keep my hand in to a certain extent. We had sabbatical leaves, and I always took my sabbatical leave. So I'd take a year off. I didn't monkey around. I worked hard in the lab when I was on leave. It was a rejuvenation and it served a heck of a nice purpose because I could say, "I'm going on sabbatical. Don't assign me to any committees because I'm going to be gone." Then I could get off the committees the year I was gone, and it always took about four or five years before I got swamped with committees again. So I had a break on that basis.

When I came here, I was very upset with the administration. There was only one office in the department. We had half the floor at that time. There were no offices for anybody but me. I said, "What the heck's going on here anyway? We've also got associate professors and other faculty." But they considered there was only one professor in this department. I remember I went over to see the dean and said, "I've got this fellow, Mert [Merton F.] Utter, who's a first-class person, and I want to advance him to professor." And he said, "Professor of what?" And I said, "Professor of biochemistry." He said, "You're the professor of biochemistry. We have to give him a different title." And I said, "That's funny. A lot of schools have five professors of chemistry. You look across the street and there are lots of professors of chemistry." Well, I finally got it through and they made Utter a professor. Then that gradually was adopted at the medical school. There are more and more professors. Dean Wearn wanted to make Utter a professor of biochemical research. I said, "Wait a minute. He can be professor of biochemistry. I'm going to be professor of biochemical research." [laughter]

I was head for nineteen years. At 65 you were supposed to retire as chairman. They had a funny philosophy that the chairman should let the department go downhill so that the new chairman could come in and appoint people of his own interest. I wasn't about to do that. I didn't want to see our department go backwards. It was rating right up in the first ten in the country. Why should I tell these guys, "Now, you've got to get out of here." So I said, "No, I'm resigning as head." Man, they had a fit! They sure had a fit! The fact of the matter is that I had to say, "I'm leaving." I went out and got a good offer for a position at Michigan State University, and I was going. But they finally came around to it, and they made Mert Utter chairman of the department. He was chairman for about ten years. And Dick [Richard W.] Hanson is now an excellent chairman. So we've been lucky with very good people. They've been good to me. They haven't kicked me out.

BOHNING: I read that when you came here, someone had said you were able to actually bring a lot of new people in. Was that part of that old philosophy?

WOOD: Well, you see when I came here they had a Professor Victor Myers who had been chairman of biochemistry. I don't know for sure, but I think he suffered from Alzheimer's disease, because he was lecturing about the cars that he had bought, and reminiscing about the professors that he had met, but he wasn't teaching much biochemistry. It fell to the younger people to take care of this. They made him the professor of clinical biochemistry, and his group taught the pharmacy students and nurses, and we taught dental and medical students, and of course graduate students. He took everybody with him, so we had a clean sweep. I appointed a bunch of young people. Of course, that made it congenial since we didn't have to worry about hurting somebody else's feelings. In a sense it was a new department. Victor Myers died in two or three years, and we took over teaching the nurses. The pharmacy school was discontinued. [Leonard T.] Skeggs was in Myers's department and he joined our department and is a first-class guy. He did a lot of work on hypertension. He devised these automatic devices in which only a small amount of blood is required to run a whole battery of analyses. He worked on membranes and kidney dialysis. From these studies he got the idea, "If we run this blood through a membrane-like tube and have different reagents on the outside, each can be used to analyze a different thing." He made a fortune from that invention.

BOHNING: I'm going back a little bit again. You had that one year postdoc at Wisconsin. When you applied to the NRC was that directed towards Wisconsin, or could you make your choice when you got the NRC Fellowship?

WOOD: No, as I recall I wrote out a research plan with no site stated. I'm amazed that I even got it because as I think back, I wrote down everything I could possibly do in ten years. [laughter] But when I got to Wisconsin, we didn't work on anything that I wrote. Wisconsin was noted at that time for nutritional studies, and they were working on growth factors. That's where [Wayne] Wooley was when he discovered nicotinic acid, which cured black tongue in dogs and pellagra. Ed [Edward L.] Tatum was there; he had his Ph.D. but he couldn't get a job, so he was sort of biding his time awaiting a job. Ed and I worked together on nutritional requirements of bacteria. We were the first to show that vitamin B_1 is required for growth by bacteria. There was an interesting sidelight. We would take (and I don't know why) corn, soak it, grind it up, and then spin it, and then evaporate the extract down and fractionate it. We had one fraction when it was evaporated in the beaker. You couldn't see a thing there. But if you added water to it and added it to a synthetic medium with B_1, the bacteria would grow! They wouldn't grow without it. So Ed then got interested in microchemistry and he went to the Netherlands with [Fritz] Kögl. Kögl was working on biotin. What we had was biotin. Biotin is required in such small amounts that you just couldn't see it. Then he got hooked up with [George W.] Beadle and genetics. It was a very fortunate setup because Tatum knew all about growth factors. They made mutants and determined which ones wouldn't grow if they left out this or that. It was a very fortunate team because of Ed's experience with growth factors. Beadle, I'm sure, wouldn't have done it if there hadn't been this setup.

BOHNING: How did you strike up the collaboration with Tatum on this project?

WOOD: I don't know. He was there and he was a nice fellow. It was interesting working with him though. He would never wash dishes. [laughter] So we struck up a deal. We'd run an experiment and then he'd go and write it up and I'd wash the dishes and get things cleaned up. [laughter] The poor guy smoked too much. He died of lung cancer. His fingers were always brown from smoking. They had a very good department of microbiology at Wisconsin; in addition we participated in physiological chemistry, but it was more or less biological chemistry. I was guided by [William H.] Peterson who was a professor of physiological chemistry. So again it was metabolism. What do vitamins do? And of course finding that vitamins were part of enzymes--coenzymes--was a very exciting business at that time. It was as exciting as some of the things we do now. It seems commonplace at the present, but nobody knew why these growth factors were required and in such small amounts. Well, of course B_1 is part of a coenzyme, and nicotinic acid and niacin are part of coenzymes, as are flavins. All of this was worked out at that particular time. I was disappointed because I had a two-year fellowship and Werkman offered me a job, but he wouldn't wait for a year. I was going to work in Germany at Heidelburg. I think he made a hell of a mistake because I would have learned a lot of enzymology and brought that back to Ames, but he just wouldn't let me go. I didn't dare turn the job down because jobs were at that time too hard to come by. That was 1937. Yes, I didn't dare turn it down.

BOHNING: Had you anticipated going back to Iowa State when you were at Wisconsin?

WOOD: Yes. I guess I wasn't surprised because Werkman knew that we were being very productive at that time. I don't remember whether he talked to me beforehand about that or not. I wouldn't be surprised if he did, though. But I was hoping he would let me put if off a year.

BOHNING: He seems to have been a very controlling kind of individual in some respects.

WOOD: Yes, he was.

BOHNING: Not letting you go to Berkeley. Not letting you go to Heidelburg.

WOOD: Yes. And he was his worst enemy in a way. We had a plant unmatched anywhere else in the world. We'd made the mass spectrometer, we'd made the thermal diffusion column, we could concentrate our own C-13. There was no place else except at Columbia, where because of [Harold] Urey's influence, they were concentrating C-13 (as cyanide). But these guys were physiological chemists and didn't know very much about enzymes. They didn't know much about intermediary metabolism. So Werkman really had a tremendous opportunity. You see, I had been pretty much responsible for putting up the thermal diffusion column and setting up the mass spectrometer; so when I left, things came to a halt. When you think back, that was a tremendous undertaking to build a five-story thermal diffusion column which has to be gastight and soldered together. Krampitz was there. Krampitz became head of microbiology here at Western Reserve, and somehow Werkman's group didn't have the same drive with us gone. Gradually that place just went backwards. I told Werkman, "You're just dumb as hell! All you have to do is sit on your can and let us guys work. You'll get the Nobel Prize just as sure as hell." And he would have, I think. But he let it go.

Let's see, Gus Ehrensvard from Sweden came over to see me once. Ehrensvard was close to the Nobel committee. I didn't know that at the time. We'd been drinking quite a lot and somehow the prize came up. I said, "I'll never take the Nobel Prize with Werkman." I was completely off of him at the time. Ehrensvard kind of stepped back like this. Whether this really had an impact, I don't know. When I went back to Ames after Werkman died for an honorary ceremony, his wife said, "You kept him from getting the Nobel Prize." I don't know. But he sure as hell would have had a good chance if he had kept me, Les Krampitz and Mert Utter there. All three of us were later elected to the National Academy, so he had a good crew.

The discovery of the utilization of CO_2 was a pretty outstanding thing at that particular time. It doesn't seem like much now but then it was. The fact of the matter is it had a big impact on [Melvin] Calvin, because it was the first real chemical proof of how CO_2 could add onto another compound and form a carbon-carbon bond. At that time they were talking about CO_2 going to formaldehyde and formaldehyde condensing, but the addition of CO_2 onto another organic compound was new. Then, of course, Calvin discovered the reductive pentose cycle, but for a long time they used our reaction in their schemes for photosynthesis. That proved to be wrong, but the principle was there, and this opened up a lot of new thinking about how autotrophic growth could occur.

BOHNING: That's another thing. Wasn't Werkman against working with animals?

WOOD: Oh my God, that was a silly thing. We really got screwed there. You see, I kept telling Werkman, "We'll never get real credit for this unless we show CO_2 is used by animals." Today the discovery in bacteria would be recognized a lot more. But in those days, there was a pretty clean separation between microbiology and animal physiology. And I said, "We've got to show this in animals. I'm sure that they fix CO_2. We've just got to show it." But he wouldn't let me do it. He said, "That belongs in the zoology department." Well, a publication came out by Earl Evans (6). Earl Evans became chairman of the Department of Biochemistry at the University of Chicago. In fact, he's one of the people that offered me a job before Werkman let me go. Krebs had done all his work with pigeon breast muscle. The reason they used pigeon breast muscle is because they fly and they've got a lot of oxidative capacity. The breast muscle has a strong Krebs cycle, but it doesn't fix CO_2. The muscle doesn't have the CO_2 fixing enzymes in it. Therefore, whenever Krebs wanted to run the cycle, he had to add a C_4 acid that we now know could be made from pyruvate plus CO_2, yielding oxalacetate. Krebs showed that if you put in malonate it inhibits succinic dehydrogenase, which when you go around the cycle and get to succinic, makes it so you can't make fumarate. Thus the cycle can't continue and intermediates accumulate. So in the presence of malonate a C_4 acid was necessary to allow pyruvate to be oxidized.

Well, they published a paper in The Biochemical Journal. Evans worked on liver instead of the pigeon breast muscle. With liver they found it was not necessary to add the C_4 acids. The pyruvate was oxidized without any addition of C_4 acids. In the presence of malonate, alpha-ketoglutarate and succinate accumulated. "God," I said, "They never mentioned anything about CO_2 fixation; this is as plain as the nose on your face. Liver can make its own C_4 acids from pyruvate, and it's using CO_2 to do it." So I went to see Werkman and I said, "This is just so damn obvious. You've got to let me do this." So, he finally said, "O.K."

Well, we made a very bad mistake. It was too bad or we'd have been the first to show CO_2 is used by animals. I knew how to purify succinate. If you look at the scheme, if citric acid were symmetrical, which we thought then, the CO_2 would be fixed and be in the carboxyls of succinate (you'd have to go through the scheme to see it all). It would be formed from alpha-ketoglutarate. And the alpha-ketoglutarate, if it came from citrate and if citrate were symmetrical, would have the CO_2 in both the carboxyls. So I thought, "Let's throw in 2,4-dinitrophenylhydrazine and the keto-acid will precipitate. Then purify it." Here is where I made a silly mistake. I oxidized the product to get the succinic acid. I knew how to purify succinic acid, and you had to have real pure stuff since it was to be oxidized to CO_2 for the mass analysis. If I oxidized it as the 2,4-dinitrophenylhydrazine which has six carbons in it, I thought, "The C-13 of the resulting CO_2 will be too dilute. I'll just throw that away." So I threw it away. Gosh darn it. There wasn't anything in the succinate. I was just flabbergasted and so disappointed.

Then in about three or four months, a letter to the Editor of JBC [Journal of Biological Chemistry] appeared in which Earl Evans had isolated the alpha-ketoglutarate from a $^{11}CO_2$ experiment. With C-11 they could count it (7). And the fact that there were extra carbons in the hydrazone made no difference. The isotope was there. I thought, "What the hell's going on? How could I miss it?" I got busy. Alpha-ketoglutarate was not available as a commercial compound so I went to Beilstein and I found out how to make alpha-ketoglutarate, and I made it. I found out how to separate it and purify it. And sure enough, the alpha-ketoglutarate had C-13 in it when I repeated the experiment. But it only had it in one carboxyl, not in two. Well, this made it seem that citric acid was not in the cycle, because if we assumed citric acid was symmetrical molecule and an enzyme acted on it, we thought the enzyme couldn't tell the difference between the two primary carboxyls.

I went over to Henry Gilman, the chairman of the chemistry department, and talked to him about this, and asked, "Can citric acid be an intermediate in the Krebs cycle?" He said, "No way." So we all concluded that citric acid as such wasn't in the cycle. It either had to be a phosphorated citric acid or it had to be a derivative so that the carboxyls were differentiated.

This idea held up for about ten years, until [A. G.] Ogston, who was a theoretician, said, "Enzymes are asymmetric. They can tell the difference between the carboxyls of citrate." (8). Of course it turned out that citric acid is in the cycle. But if I had a just taken that CO_2 from the oxidation of the hydrazone to succinate and counted it, I think I would have seen a smidgen of C-13 there even though it was eleven times diluted, and I might have worked it out. But I was so convinced of this symmetrical molecule business that I figured, "No use, get the succinic acid. Get it pure. Then if it's only in one carbon, the C-13 will be diluted by four in the succinate; but if the intact hydrazone is oxidized to CO_2, the C-13 will be diluted." We worked it out and Evans did too at the same time. We did a lot of nice work with Nier on these kind of things. Then went I went up to Minnesota; that was interesting too.

BOHNING: But your association with Nier started before you went to Minnesota?

WOOD: Yes. He was awfully good to us. In any collaborative work like that, you are always delayed because you send stuff up and it takes a while. I remember I went to him and said, "Well, Al, couldn't we make a mass spectrometer down at Ames?" "Oh, sure you can. No problem." [laughter] We had a lot of problems.

[END OF TAPE, SIDE 4]

WOOD: They made the actual mass spectrometer tube at the University of Minnesota and we made the magnet in Ames and lined everything up. Nier was going to come down and show us how to run the mass spectrometer and test it. Well, I got a call from him and he said, "I can't come." He was going to send down a graduate student named Ed Nye, who was very good. I know him now. He's in the National Academy of Science. Then I got a call that said, "Nye can't come." They finally sent an undergraduate student, Donald McClure, down to work with us. He is in the National Academy of Science too. During the Christmas holidays we worked our fannies off. We couldn't get that damn thing to work! Well, they had an electrometer tube in a casing that was supposed to be under vacuum. I looked at the casing and I thought, "Our machine shop didn't make this properly. We don't dare put this under vacuum." So we put calcium chloride in there to take up the water. Finally I said, "Well, nuts, it must be that you've got to have that drier than we're getting it." So we put it under vacuum and the case just caved right in and broke the electrometer tube. My wife was up in Minneapolis where her folks lived, so I drove there that holiday. I went over to see Nier. It had all changed. I couldn't get in his lab. I couldn't get downstairs. There were policemen around there with guns and I thought, "What's going on here?" I had no idea. You see, he was in the Manhattan Project and isolating U-235 for the first tests of whether it would serve to make an atomic bomb. He told us that we couldn't get an electrometer tube. They were all requisitioned. Well, as it turned out, Krampitz went over and found one in the physics building and we finally got the thing working.

Nier, kind as he was, did two things with good intentions that turned out to be detrimental. He figured he was going to make things so we couldn't foul it up. The mass spectrometer has a platinum filament that's heated electrically and it shoots the electrons across and ionizes the gas. That filament has to be lined up properly so when the electron beam goes through and ionizes the gas, the ions will pass through the slits and form a beam. So he made the thing with a couple of brackets with prongs on the filament holder. If the filament burned out and was replaced, you had to slip the holder into the brackets and the filament was lined up right.

Prior to this we found out why our trials did not work. The magnet was wound the opposite from what we thought. You see, they wound the magnet and then they tied it and wound the wire back in the opposite direction. When we looked behind the wrapping, we thought, "Well, it's wound like this." And so we hooked it up accordingly. For weeks, we worked on that mass spectrometer night and day. Finally I decided, "We've done everything. It's got to be this cock-eyed magnet!" So we tore the paper off and found it was wound the opposite of what we thought. All we did was change those poles and the thing worked immediately. But, we were supposed to bake it out to degas the tube. Well, the trouble was when you heated the tube, the tube expanded and those prongs that were sticking out shifted the filament and shorted it out. Well, we'd have the spectrometer working and we'd think, "Let's bake her out." And it always shorted out. We must have taken that filament off and put it on dozens of times. The glass got so glazed from resealing it that we had to put in a new piece. Then it finally dawned on me.

"Damn it all! Let's take those prongs off of there." We took the prongs off and it worked immediately. Well, that screwup was solved.

The thermal diffusion column at the University of Minnesota had a big pipe up at the top which then was reduced to a smaller pipe. The first twelve feet or so was this great big pipe. Nier thought, "That big copper pipe is going to be hard for them to solder." So he designed ours with two columns coming down first, which were joined by a collar to a single small column. Well, that was a fatal mistake, because if these two parallel columns at the top didn't get heated exactly the same, one became a chimney and recirculated the methane gas. So when we started the column, the C-13 increased for a little while, but then it didn't go up anymore. I finally figured out, "Well, just by logic, if that cock-eyed thing isn't heated exactly the same it is no good." So we cut the one column at the top right off and then the C-13 increased just fine. So this held us up too. Every time Nier tried to save us time, he made us spend more. [laughter]

But he was very, very kind to us. Of course, these experiments were extremely fortunate for me because I got the reputation of working with isotopes in the early days. Nier told me, "You know I gave mass spectrometer tubes during this time (which was during the war) to about twelve people. Most of them were physicists. You're the only guy that ever got them to work." We only got it to work because we were so darn stubborn! Of course, it was a simple mass spectrometer. It had "B" batteries, twenty of them hooked in parallel, so one didn't have electronic power packs to monkey with. He specially designed that for us. But when I moved to Minnesota, the physicists were all gone. All the physicists were on the Manhattan Project, and all the mass spectrometers sitting down there, none of them working, and of course they were all with power packs which I couldn't run. I finally got a student who knew something about electronics, and we got one of them going and did isotope experiments there with the physiologist. We worked on glycogen. Baird Hastings had shown that CO_2 is fixed in glycogen. We decided we could determine which of the six carbons of the glucose contained the C-13. I used bacteria to ferment the glucose to lactate and got it degraded that way. We were able to show that the C-13 distribution in the six carbons of the glucose was in carbons 3 and 4, which was in accord with the predictions of the Krebs cycle.

BOHNING: Am I correct in understanding that you built both the diffusion column and the mass spectrometer, and it was your group that was doing it? Did you have much assistance from physics?

WOOD: We did it entirely.

BOHNING: You did it entirely by yourself.

WOOD: Yes. The graduate students and I did the whole damn thing. Silver-soldered it all the way and did everything else. Today I'd like to see anyone get a bunch of graduate students and set out to build something like that. [laughter] Yes, it was a tremendous undertaking.

BOHNING: How long did it take?

WOOD: Oh, I don't know. I imagine it took several months at least for the mass spectrometer. It is interesting. I went to Ames to receive an achievement award from Iowa State, and they told me that that mass spectrometer tube is in the Smithsonian Institution. I suppose it's down in some hole somewhere, but it is unique for its use by a biological group. Well, there weren't that many mass spectrometers anywhere!

Nier, of course, was extremely helpful in this regard. Jeez, I'll always remember Krampitz and I went up there and he sat us down in front of the mass spectrometer and said, "Now, you turn this dial and you see the galvanometer move across the scales, and you change the voltage and then you see what the C-13 is compared to C-12." It got to be about 5:30 and he said, "Well, I've got to go home, but you sit here and just monkey with this thing! Just don't open up the cocks and let some oxygen in." [laughter] Here we were with this complicated instrument we had never seen before and he says, "Aw, you can't do any damage." [laughter] A lot of guys wouldn't have let us touch it! And it was the same when I said, "Do you think we can make one?" "Oh, sure, no problem." A lot of guys would have said, "No, that's impossible." But he said, "We'll get a tube made here and you guys go down there and finish it." We didn't know anything about electrical circuits. We just went up and drew pictures of his circuits without understanding them.

There is that story about the thermal diffusion column. We got the thing built, and pretty soon we got it so it was working pretty good. And then one morning I came in and the damn column was all warped out of shape. I pretty well knew that the cooling water must have turned off or something. You see, the inside rod was heated and if the cold water for cooling stopped, then things would get hot and expand. We got it fixed pretty soon. I was working in the lab where I could hear the water running and every once in a while I noticed it slow down. I wondered what was going on. I finally figured out that this happened when the home economics gals had their classes dismissed. So we went up to the toilet at night. They had three of them there. You could flush two and the water pressure would stay up all right, but if you flushed all three simultaneously there was a problem; the pressure became too low to push the water up the five stories. I went to Werkman and told him, "Get rid of one of those toilets."

He had a separate line put in with a pressure gauge. With that kind of problem Werkman would get it done. He didn't have any grants, but somehow he got the money.

BOHNING: Did he do any experimental work himself?

WOOD: No, no. He never did an experiment when I was there. He was pretty good about getting equipment, but he didn't do experiments. He did a fair amount of reading, and you know, there's a lot of professors that don't do any experiments. It's not unusual. So you couldn't really fault him for that. What you could fault him for was that he probably wasn't at the forefront in some respects of what was going on in his labs. I was there. Mert Utter was there, and Krampitz was there. He had three guys there plus some others who were damn good. When he kicked me out, he just ruined that whole team, and he had a team there that he couldn't have missed.

BOHNING: Did the others leave too?

WOOD: Yes. When I went to Minnesota, Utter came with me a year afterwards. And when I came down here, Krampitz came with me. Krampitz was stuck there and things were going to pot within the three years. Werkman hired a pretty eminent biochemist, Fritz Schlenk. I got a letter from him about a year or so ago. He said, "You were awfully charitable to Werkman in your prefatory chapter 'Then and Now'" (1b). I said, "Well, I don't know. I wasn't so charitable." But then he wrote about his problems. While at Iowa State he was getting grants, and he said, "I never could spend any of the money even from my own grants." Of course that kind of thing ran the place down.

BOHNING: I was curious about one thing. You went back to Iowa State in 1936 but it wasn't until 1939 that you learned about the carbon-11 availability. What were you doing in that three year period?

WOOD: We were working our fannies off trying to show where that CO_2 was fixed. I did show that if you put in sodium fluoride, it stopped the succinate formation and it also stopped simultaneously CO_2 fixation, so it all fit together. But to really nail it down, exactly where the CO_2 carbon was fixed, was not possible. When isotopes became available this opened up a whole new ball game. And of course, we knew what we wanted to look for, and we knew exactly which compound.

It was kind of funny. I isolated propionic acid from these fermentations, but I never did anything with it. I just put it in the frige and froze it up. Well, Martin Kamen and Barker took the propionic acid they isolated and they degraded it. They oxidized with permanganate and got oxalate, and they showed the oxalate had the isotope in it as well as the CO_2. So they said that the isotope was in all three carbons. Boy, I was just mortified! I thought, "My God. Here I sit with that propionic acid and I don't even have enough sense to degrade it."

The other thing they did was make the barium salt, and they pyrolyzed it. That gives diethylketone and CO_2. And again the results indicated the CO_2 was fixed in all three positions. So I thought, "Well, these guys have not really shown it's in all three carbons. It's in the carboxyl, but they haven't shown whether it's equally in the alpha and the beta positions and I better find out about that." I went over to see Henry Gilman to talk to him about how to degrade the propionic acid. We treated it with bromine to brominate the alpha position and thus converted the propionate to lactic acid and then we converted it to CO_2 and acetaldehyde. Then we did an iodoform reaction on the acetaldehyde and in that way we got all three carbons separated. I happened to be at Nier's lab when they assayed these samples. Gosh, there was nothing in the methyl group, there was nothing in the alpha position. It was all in the carboxyl position (9). It turned out their degradations were not specific. The fact that CO_2 is in the carboxyl of propionate is because the succinate and propionate are more and less in equilibrium with each other, and the labeled propionate is derived from the succinate. So I got off the hook that time. [laughter] You know, we did a lot of chemistry, degrading these compounds, which is not done anymore. We buy our compounds. I don't see any chemistry going on of that type. In the first place, we use enzymes that do a lot of what we used to do chemically.

BOHNING: Once you had that facility at Iowa State, were others quick to pick up on that, or were you still one of the few to have that?

WOOD: It never really got picked up. C-14 came along in the 1940s after World War II, and carbon-14 is much easier to use than carbon-13. You don't have to have as sophisticated equipment. There was a sort of rivalry between C-13 and C-14. You could do some things with C-13 you couldn't do with C-14, but C-14 has a long half-life so you can do just about anything with it that you want to do. When we came here we made a mass spectrometer and we could buy C-13 at that time. But gradually it's gone out of use. It's used for nitrogen. Quite a lot of work goes on at Wisconsin where they are studying nitrogen fixation and those kind of things. Well, it's used sometimes now for isotope discrimination. They get 100% C-13 and determine whether there's discrimination in chemical reactions.

BOHNING: When you went to Minnesota in 1943, what kind of relationship did you have with that department?

WOOD: Oh, I had a pretty good relationship. Of course it was an entirely new experience. I was sitting in on neurophysiology courses because we were working on poliomyelitis. We were working with monkeys and cotton rats, infecting them, and it was a whole new ball of wax for me. Our main assignment was that Ephraim Racker had done some experiments with cotton rats and had claimed that the rate of the glycolytic pathways were way down in the brain during polio. It looked like some enzymes in the brain were influenced by the polio virus. Visscher wanted me to work on that. We infected cotton rats and took out their brains and studied glycolysis--the rate they formed lactic acid. We decided that before we did anything with polio, we'd better find out how to measure glycolysis in the brain of normal rats as a standard. It turned out that the methods that Racker used weren't determining anywhere near the capacity of the brain to carry out glycolysis. We did these experiments with cotton rats, and unless they were so morbid they could hardly move, there was no effect on glycolysis. That was our main contribution there. But we learned something about how to work with animals and how to take out organs. You see, as a microbiologist, I was traveling with a different group of people, and I didn't know many of the eminent biochemists. I knew them by their papers, but I didn't have any personal contact with them. Of course, this sort of opened up my acquaintance with these people. We didn't go to the Society of Biological Chemistry meetings when I was a graduate student or postdoc. That got me going to them, and was important in my selection as head of this department.

BOHNING: How did that change come about? You said that you were reluctant to leave Minnesota and come to Cleveland.

WOOD: I certainly was.

BOHNING: Certainly the deer hunting is better there than it is here! [laughter]

WOOD: Well, I don't know if it is, but I had my brothers and my Dad and we had a deer camp. I was always so cock-eyed busy around here, I never really got going on hunting and fishing in this area. The only way I would go is if I'd take two weeks off and go to Minnesota. So that's the way it sort of settled down here. Yes, I was reluctant to leave Minnesota.

BOHNING: So you weren't really looking. They came to you?

WOOD: Yes. They called me up. I know now, but I didn't know this for a long time, that they had interviewed two or three people before they got in touch with me. I think Carl Cori put them onto me. Carl knew about our work and he apparently told them. Wearn was from the East and he was going to Harvard and the eastern schools to find his men. I think they offered it to Ed Tatum but I don't know for sure. I think they offered it to Eric Ball at Harvard and he didn't take it. I didn't know that until I read a biography that Hastings wrote and he told about Eric Ball being offered this job here (10). So that's how it happened, I guess.

BOHNING: What was it, then, that made you come?

WOOD: Well, I thought, "I want to go and set up a department the way I think it ought to be. And here we will have a clean sweep. Nobody's going to be leftover from the old department. We can just set it up and we can run it the way we want to run it. Let's go try it." I guess I probably didn't realize how difficult it would be, but in general it worked pretty well because I had a cooperative bunch of young fellows working with me who were just as anxious to get things going as I was.

BOHNING: Was the university supportive of what you were doing?

WOOD: Yes, Dean Wearn was very supportive and never interfered. We made decisions as a team by vote. We set up a whole bunch of new lab experiments for medical students. I visited schools like Pennsylvania, Columbia, and Harvard and saw what they were doing in labs. Man, the labs were, in my opinion, ridiculous. They were a bunch of cookbook stuff and did not have much to do with the lectures. When I came here, we got busy. The medical students did some isotope experiments. We had to get special permission from the Atomic Energy Commission to do it. We made rats diabetic with allocane. If the students were going to determine sugar, I wanted them to have a reason to determine the sugar.

[END OF TAPE, SIDE 5]

WOOD: We worked our fannies off getting these experiments ready for them to do. Most of us didn't have very much experience with handling rats. I'll always remember [Warwick] Sakami. He was in charge of the rats. He bought the rats to the lab and he was taking the rats out with tongs. Of course, they didn't like that. Jeez, they bit about three or four medical students, and we realized that we better get a little more organized in the future. We put the rats in mailing cartons with a hole in the end where the tail came out. To get blood, you snipped a little bit off the tail and collected the blood from the tail, and it was analyzed. The rats were put in an oven a little bit above the normal temperature so that the vessels were dilated and they would bleed. We got all the ovens we could find around here.

Some of them got too hot. [laughter] We had a hell of a time! Some of the rats died. I think sometimes the kids learned more when the experiment didn't work than when it did. [laughter] It made them think about what went wrong. Not necessarily this one, however.

BOHNING: You said that you had started this new curriculum and then convinced the others on the basis of it being an experiment. When did you consider the experiment a success? How long did it take?

WOOD: Well, when it comes right down to it, I don't know if anybody knows. They set up a committee with staff for evaluating our students as compared to other students. Well, how do you evaluate? Do you evaluate them on how successful they were as clinicians? Heck, a lot of that depends on bedside manner, not good clinical practice. It doesn't necessarily prove they are better. If they evaluate them on the basis of internships and residencies, well, that might mean something. As far as national boards are concerned, our students did well at first. They don't do as well now. I don't know for sure what the cause of that is. Well, do national boards determine whether a guy is a good physician or not? There's a lot of different problems. I think at first there was enough enthusiasm by the faculty because they were doing something different, and because there was a lot of effort put in, that the teaching was excellent. As time goes on, things get sort of routine and I don't think there's the same sort of effort now. Whether it's better training? In some ways it probably is. In some ways they have a little more liberty to work with patients early in the game. They're supposed to be given some time to do their own thinking (whether they go to a ball game or whether they do something else, that's hard to tell), but at any rate, they are treated a little more maturely than they are at a lot of medical schools where it is pretty lock step. Whether the lock step gives them better training is hard to tell. I think students like it better here.

BOHNING: Were there any other places that followed your lead?

WOOD: Yes. There's been quite a few that have followed it one way or another. Not quite the same, but similar in some respects. The main objection of some of the chairmen was that they thought they were losing their authority over decisions. I never worried about it very much because I figured our main effort as far as the department was concerned was our research. We could teach the best courses in the world, but we would never get recognition unless we did good research, which in some respects is true. So I didn't worry too much about that part. I felt we were giving good instruction in biochemistry to the medical students, and I knew we were teaching our graduate students the way we wanted to. Fortunately, Dean Wearn got a bunch of young people in all at the same time. Otherwise, the new curriculum would never have gone through. If they had been teaching for years and were set in their ways, they never would have done it. There was a general feeling that this was a solid move to make. Wearn picked the chairmen with that in mind. He felt strongly that information was important, but attitudes and maturity were more important. I think that is true.

BOHNING: You had commented earlier that you limited the number of graduate students that anyone could have. Is there a specific reason for that?

WOOD: Well, I thought every staff member should work in the lab. And I thought if they had too many students, they couldn't keep track of what was going on, and they wouldn't work themselves. Then our budget was sort of limited. I felt at that time (of course it's not done now) two or three was all anybody could properly direct. Well, it's probably still true that's about all you can direct closely, but I've got ten people now and I don't direct them as closely as I used to, or don't work as closely with them. So a lot has changed over the years and perhaps not for the best.

BOHNING: Also I read that you indicated remuneration from honoraria and so on would be plowed back into the students rather than staying with the individuals. It sort of cooled everybody's efforts.

WOOD: Yes, we did for a long time. I had the feeling that the university ought to pay our salaries, and they ought to pay what we were worth. If you're going to go out and do something else, you shouldn't be going out to do it for the money that you get. You ought to go out and do it because you thought it was good for you or you wanted to do it. It was a socialistic sort of thinking. The fact of the matter is, it hit me more than it did anybody else because I was the oldest and I was receiving the most honoraria. But it served a very useful purpose because when we'd hire a young staff member, we could make him a loan without interest to help him buy his house or help get settled. In a way it was a nice fund to have. If you needed some equipment that you just couldn't get any other way we could get it off of this fund. In those days we didn't have many travel funds to send young guys to the meetings and we'd use it for travel. That held true for all the time that Mert Utter was chairman, so it was almost thirty years. It was interesting.

They had an outside committee come here to give the dean advice on how it would be best to get a new chairman, and they were very opposed to this. [laughter] Well, it didn't work badly. Everybody knew what everybody else's salaries were, and we voted on them! Yes, it was a kind of a funny system, but it seemed to work. And if somebody needed a raise or he was offered a position somewhere else, we sat down and talked about it. And I would say, "Well, if you give this person a raise, it only means that you've got a wedge to get your salary up. It doesn't hurt you a bit that we get this guy's salary up. Because if we do, I can go to the dean and say, 'Look, here's what he's getting.'" So, it didn't work too badly. I don't know if it would work nowadays or not. It worked all right then. But the visiting committee was very upset by this kind of show. They didn't want that at all. But, it worked. It was probably one of the few places in the world where staff actually sat down and discussed the salaries of other people. [laughter]

BOHNING: Yes, that's amazing. It's even more amazing that it worked. But, you had perhaps a unique kind of openness with this group.

WOOD: Yes. We talked everything over. And they were all about the same age, and they had all come up through the same period of time, and they all grew up with this system. I know I was offered a position at Columbia when I was here and I said, "Hell, I'm not going there with that bunch of prima donnas! You'd have a heck of a time running that show!" You'd have to be a dictator. That's all there is to it. You'd have to say, "O.K., here it is." Because you've got guys that you can't sit down and discuss things with and get them to be reasonable about overall decisions. The prima donnas are used to running their own show and they often aren't thoughtful about the welfare of others. So, I never investigated the position or considered the job, partly for that reason.

BOHNING: How did the other departments here react to that?

WOOD: I don't know if they knew much about it. Oh, they probably knew something, but I never heard any complaints. And, things were a lot simpler. I'd write down the salaries on a piece of paper and go over to Dean Wearn and say, "Here's what I think we ought to do this year." And he'd say, "Have you got enough money to pay for it?" And I'd say, "Yes." "O.K." Or, if I'd say, "I've got to have a little more," "O.K." Nowadays things are a heck of a lot more complicated. Take just hiring a staff member. You've got to advertise in Science. You probably have one hundred applications or more, and think of all the hours that are spent now just going through and deciding. Heck, I used to write to Fritz Lipman and Cori saying, "We've got a position here. Do you have anyone that is good for this job?" There might be three or four, and I'd have one or two out here, and it was done. The person was hired. Well, gee, you can't do that kind of thing anymore. And the same with construction or anything else. If I had wanted a wall torn down, I'd go over and see Dean Wearn and he'd say, "Can you pay for it?" And I'd say, "Yes." "Tear it down." Now you go through buildings and grounds and you go through all kinds of committees and it makes it a lot more complicated, that's all there is to it.

Of course, when I was head, the granting situation was so much easier. I was head when it was the easiest time that you could possibly have. You could hire a person and say, "We'll get a grant. Don't worry about that. We'll get a grant." And, you knew if he was pretty good, you were going to get a grant. That's all there was to it. But you can't do that anymore. They almost have to look for a person that's already got his grants, so that they're on safe grounds. And that means that the young people have got a heck of a time getting started.

BOHNING: I know in chemistry there will be academic positions by the turn of the century. Because of the big expansion in the early 1960s there's going to be a large turnover at the end of the century which people are already questioning if we are going to be able to fill. How does that fit in biochemistry? Or is biochemistry the attractive area today?

WOOD: I don't think there's going to be a shortage unless the shortage comes because we're training a lot of foreigners and not training enough American citizens. I think certainly the situation with respect to women has changed tremendously. You walk down the hall now and fifty percent of the graduate students and a lot of postdocs are women, and a lot of them are good. They're in there for business. They're not going to walk off and get married in the middle of their training, which was true in the old days. In the past you hated to take on a woman because they'd be here perhaps two years and then they'd go off and get married. They don't do that anymore; they're here to prepare for a profession. There are going to be a lot of women available who are, without doubt, fully qualified. At the present time, there's no doubt there are more men trained that have got a big reputation than there are women. But in ten years or so there's going to be a lot of women who are fully qualified and have good reputations. So that's going to be a pretty big change, and it's already true to a certain extent. It's certainly true in the training program. We've got more American women being trained than we have American men being trained. I just graduated three women. Boy, they were good. There was no question about it. They were damn good! And, they've got pretty good positions. You know, they're getting started. In a few years, they'll be fully qualified.

BOHNING: You even were dean for a couple of years.

WOOD: Yes, two. Well, that was more or less by accident. When they federated Case and Western Reserve they wanted a dean who they thought wouldn't be biased and they figured I was in the Medical School and wouldn't be biased. And, they were having a hard time getting one. I was in favor of the federation, as far as that goes, so I said, "O.K., I'll take it for a year." I took it for a year, and then they wanted me for two years. And I said, "I'll do it for one more year, but that's the end of it." Well, when the end came, they tried to pressure me again, and I said, "Nothing doing! I'm taking my sabbatical and I'm going!" That was it. I really didn't want to be a dean. But I was enough interested to try to help out. It was a difficult time in a way because they had to get rid of one head of each department when they combined them, and a lot of times they had too many faculty. But in general it went pretty well. The Case school had a good administrative group and they took care of a lot of the work, so I didn't really have to do very much. They ran their show a lot differently and it was probably a good thing I was there because academically I didn't think they were quite as liberal or free as they should be. But, in general it wasn't too bad.

BOHNING: You served on a number of national committees. I'm particularly interested in two areas. When you were on the editorial board of the <u>JBC</u>, there is one story about your famous quote to the board. I guess it was supposed to be a five-year-to-life kind of appointment.

WOOD: Well, it was a pretty closed shop when I went in there. You see, they had about sixteen people. Most of them stayed on for a long, long time. When they took me on, they took on a couple of others. I was assigned almost entirely manuscripts on isotopes, and it was really kind of a tough job because whether it was isotopes going into bone, or any other thing, I was assigned the manuscript. Anything on isotopes. People didn't know much about how to use isotopes, so there were a lot of papers that were coming in that were difficult to referee and to recommend what they do. So I worked pretty damn hard at it. I made up my mind. "I've done my duty. I'm out." Boy, did they raise hell that night when I said that. Cori and Hastings and others. They used to have the damnedest parties at that thing. In some ways it amounted to a drunken brawl, and that's the only direct benefit we got for our work. Nowadays they can't afford to do it. They've got a hundred or so editors, and it's not a close group. Anyway, when they got mad I said, "Listen, if you all died tomorrow, we could form an editorial board," and this really rocked them. [laughter]

At that time there was a kind of a revolt going on. I don't know whether it was simultaneously, but almost at the same time. People were objecting to the editorial board and their having so much control and no turnover. So it wasn't long before the society actually made a rule. All new appointments could be for five years, renewed for five more, but then the editor had to go off for a year, at least, after that. So things changed at that time. The people that were on the editorial board were good, no doubt about that. But in some ways they weren't quite up to some of the modern stuff. I know when I was doing this CO_2 work we published in England because they were a little bit more up on intermediary metabolism, and we were having a little bit of trouble in JBC. I guess I was feeling that a little bit too, that you've got to have turnover. It was a lot of work and it still is a lot of work for the editors.

BOHNING: I'm sure the volume is enormous.

WOOD: Yes. You see, if you turn a paper down, you have to send it to some other editor to confirm it. Well, I had a paper from [Philip] Handler. He was working with P-32 and he was showing that glycolysis does not occur by the Embden-Meyerhof pathway. I spent a lot of time and I must have written four or five pages of suggestions, the principal one being that when you put P-32 on the outside as inorganic phosphate, you don't know what the concentration of P-32 is on the inside of the cell, and what's the transport? So I sent it to Cori and Cori wrote back, "What are you trying to do, run a correspondence course?" [laughter] But it was interesting. Handler accepted that criticism. He never sent the paper back. He sent a letter back thanking the editorial board for the careful review of this work. So it was kind of interesting that he accepted the suggestions.

BOHNING: What kind of rejection ratio did you have?

WOOD: Oh, that varied a lot too. Each year they had the score of who turned down how many. They were sort of proud of the fact that they turned down a lot. I never was very proud of that. I was always trying to fix it so the author could get it published and not to turn it down. I didn't turn Handler's paper down either. I just sent him a lot of advice. Well, I think probably around fifty percent were turned down in those days. Maybe more, but not much more. And, turned down doesn't necessarily mean it wasn't published. It might have been published somewhere else. I think it is a little more difficult now than it was then. Of course there are a lot more papers. The JBC comes twice a week, it's about so thick, and it has big pages. All you can do is read the titles, and if you see a title, you put a little mark beside the title and come back and sort of glance through it and that's about all you can do. Then you pick out the ones that directly bear on your own work. So you are very narrow in terms of what you read, unless you read reviews or you go to meetings. So that has changed a great deal. You sure don't read any physiology or anything like that. We used to read some of it. On the other hand, our students, they must be getting a lot of information when you consider what they've got to cover. They don't master it to the same degree, but a student going through today must be pretty broad and smart.

[END OF TAPE, SIDE 6]

BOHNING: I also wanted to ask you about the IUB [International Union of Biochemistry], because I recall reading that you were involved in revitalizing it.

WOOD: In a way I was. I was elected to the council in 1967. The council, as far as I could tell, didn't do anything except do what the executive committee decided. We didn't know anything about what was going on. And I thought, "That's a funny way to run IUB. There's three or four guys who are more or less running the whole show, and I'm not sure they're running it the way it ought to be run." Then they wanted me to be general secretary of IUB and I wasn't at all sure I should take it, but I thought, "Well, maybe as a general secretary I can do a little something towards a change." The fact of the matter is the general secretary does a hell of a lot of the work. A lot of guys said, "You're just nuts for taking this." And I probably was. But I didn't keep it very long. So when I was general secretary and they had the executive committee meetings (they usually had them at the international congresses), I invited all the council to come to the executive committee meetings. There was practically a revolution. The president was really upset. That was [Hugo] Theorell, the Swedish Nobel Prize winner. He was a nice guy, but he was very upset by this procedure. But at the end of the meeting he said, "This is the best meeting I've been to in a long time. There was a lot of good discussion here." So, he accepted it.

Well, I was general secretary two or three years, and I was looking for a way to get out. There was a person named [William J.] Whelan who was a professor of biochemistry in Miami, and he loves this kind of work and he's good at it. He had an offer of a job in England and somehow or other that fell through. When he took that job, he was secretary of the Pan American Association of Biochemical Societies and resigned. He had done a good job there. I heard he was going to stay in the States, so I quickly got on the phone and called him and said, "Bill," (but I didn't know him very well then), "what about you being general secretary of the IUB?" He said, "I'll think about it." And he accepted, so I got out. I was lucky to get out. When he took over we made a lot of changes. They made me president, so Bill and I were secretary and president. They had a council, an assembly, and there was a tier structure. We finally cut it down to where the council was really the executive committee and it was ten to fifteen people, each assigned a job. We made a rule you could only be on for three years and then you had to be reelected. We started up a bunch of publications that were run by IUB which brought in money. In general we made the administration a little more democratic and a little more down-to-earth. In a way, and it was just as much Bill as it was me, more Bill probably, we did make a lot of changes.

China and Taiwan were at loggerheads. Taiwan had been admitted to the union, but mainland China had resigned and would not come in again. That was true with all of the international societies. We got to work on these guys. It was a tough job, but we finally got Taiwan and mainland China signed after agreeing on a little change in the titles of their respective societies. That went right through the rest of the societies. To get that straightened out was a major accomplishment. I don't have anything to do with the IUB anymore.

BOHNING: You were on the President's Scientific Advisory Committee under both Johnson and Nixon. What kind of experiences did you have during that time?

WOOD: Well, when Kennedy was President, he apparently listened to the science committee a lot. Johnson was a bit suspicious of scientists. We had an input, and the input, if it fit with what they wanted to do, was fine. If it didn't fit with what they wanted to do, then it wasn't so good. But, at least we met and we discussed. Johnson didn't come himself, but he did send representatives. They made recommendations about the Viet Nam War. When Nixon came, of course, he didn't really like the whole idea at all, so he canceled it. It was an interesting experience. There were a lot of very high-powered guys there. When Nixon was there, there was a lot of discussion about terminating the graduate student fellowship program. We spent I don't know how much time in lots of meetings of the committee. We wrote a whole big book on the advantages and why we should train young people and the returns from such training. They figured they had to have Nixon's signature in order for it to have any input. God, he'd keep postponing and postponing and postponing and the thing never did get put out after doing all the work. They just kept saying, "Yes, it's fine, it's fine," but it never really went through, which was a disappointment. If it had gone through, perhaps the fellowship program would have stayed stronger than it is. There was a lot of discussion on armaments which I didn't have much to do with.

BOHNING: We haven't talked at all about your sabbatical leaves. You've written about them extensively. I was personally intrigued about your reason for selecting the first one in New Zealand because it's about as far away as you could get. I know that's only partially the reason.

WOOD: Well, I'd been on this curriculum business and that had taken up a lot of time. I was so closely involved that even if I wasn't chairman, they were always coming to see me. I thought, "I'll fix this." Well, I was intrigued with New Zealand because there was a guy who came through here named A. T. Johns from Wellington. He was a biochemist or a microbiologist. He gave us a lecture on the beauty of New Zealand. I thought, "Well, it would be an interesting place to go with my family." It was. It was a fascinating place. Actually, we did some good research while I was there. But the hunting and fishing were fun, and the scenery was beautiful, and the people so friendly. At least they were then. You see, that was right after the war, and they were very grateful for what the Americans had done for them. You couldn't open your mouth and they'd recognize right away you were an American, and they'd all come around and ask you, "How do you like New Zealand?" Dunedin was a beautiful place, but it was kind of cold and primitive in a way. There was no heat in the bedrooms at all, and no central heating. They had a charcoal burning stove in the living room. When it really got cold we were all in there. As I say, our family got acquainted better there than anywhere else.

BOHNING: I had one other question I wanted to close with. I'm pretty much through my set of notes, and I wonder if there's anything that we haven't touched on that you wanted to cover.

WOOD: I don't know. I guess not.

BOHNING: We've not covered a lot of the scientific details because you've described them fairly carefully in writing.

WOOD: Yes, I think that's pretty well taken care of.

BOHNING: That is why I've been moving around to these other areas. I guess what I wanted to close with, then, was that I know it's not easy for us to summarize the enormous changes you've seen in your career for the past fifty years. Where do you think it's going in the future?

WOOD: That's a terribly hard question to answer. You know, the changes have been so tremendous in the fifty years or thereabouts that I've been associated with science. It's unbelievable, that's what it amounts to. You know, when I look at this FAX machine and somebody writes a note congratulating me and it comes out of that FAX machine an hour later in his own handwriting, it's just beyond conceiving what changes have occurred and will occur.

I can remember my Dad, who was interested in baseball. The neighbor had one of these crystal radio sets and it was brand new and the World Series was on. He said, "Why don't you come down and listen to the World Series with me?" And Dad said, "That's the craziest thing I ever heard in the world." But he went down. "Well," he said when he came back, "you could hear Babe Ruth hit a home run, you could hear the crack of the bat. I don't know whether they put that on or what went on." But then he read the paper and he said, "Sure enough. In the third inning he hit a home run. I was listening to it." Well, now you look at what's happened since then. And it's the same in science.

Science has just grown more and more. I can't quite visualize it. I know that molecular biology is going to have a big impact on what one can do. There's no doubt about that. When I think in terms of finding out what causes cancer and things like that, there is not much doubt in my mind that the answers to some of those problems are going to come. Whether the cure is going to come, that's another question. But if they begin to find out what the real causes are, they've got a lot better chance of bringing about the cure. And the computer business has changed the whole picture too. I see these guys out here asking this computer all kinds of questions so that if you want to know the structure of a six-hundred-long protein compared to another one, they put it in that computer and it comes out and tells them what the structure is and it picks out areas that you wouldn't recognize otherwise. You can begin to point your finger and say, "Well, here's where we better look. These things are possible." And, of course, sequencing. They sequence DNA and tell you what the amino acid sequence is. They do that in a relatively short time. I don't know, when they get the sequence of all the genetic material in a human, what it's going to do. It's hard to tell. But there's not much reason to think the revolution isn't going to be just as big in the next fifty years as it was in the past. Maybe it can't be as much, but it's going to be a lot. You can be sure of that. So, it's a fascinating time to live.

BOHNING: Would it be fair to say that this next revolution in science will be one more closely tied to affecting human life than any of the other previous developments?

WOOD: Probably. Looking at the function of the brain and why people have depressions. Well, I've seen the depression a little bit in one of my own children. They really don't know a lot about this. Why are some people ambitious and why are some not? All kinds of things. Well, I think they'll begin to sort this out. They're going to have the tools to do some of it, and they're already doing it to a certain extent. When I first came here they had psychoanalysis, and it was all how you were raised as a kid and how your parents treated you. But now they have chemicals which change it. It's chemistry. The psychologists are beginning to work on these kinds of things too. It's a difficult area but I think they'll be able to probe it. And the whole thing about differentiation and how organs come from one cell and become different cells. They're beginning to get their mitts on this kind of stuff. For us to sequence a protein was a lifetime work, and now they do it in months or weeks. They make mistakes too. I know that. Some of the old sequencing procedures are still needed to confirm. We've had that experience here. We've been working on a biotin enzyme for years and it's got three sub-units. They're all cloned now, and we can study the protein-protein interactions and the active sites. I can just go to them and say, "Let's change this." And they can change it. Right in the middle of the doggone protein. It isn't all easy. They change something and properties of the protein change. You can't purify it the way you usually did. They're having a lot of trouble in industry with this. That's why they want protein chemists. They want them more than they want molecular biologists now, because they run into all kinds of problems. So that stuff has got to be sorted out, a lot of it. I think the next fifty years are going to be exciting. And I've been lucky. They've let me stay here and work. Some places they root you right out.

BOHNING: You've got your grants, and you've got students. They'd be foolish not to let you stay.

WOOD: Well, I've got a brother who's at the Mayo Clinic, and he had a lot of grants and everything, and they kicked him right out. I think they were very silly because he's younger than I am and he's perfectly capable of doing good work. He's the one that worked on G-suits for pilots of airplanes. In fact, they did this during World War II and he's now got a grant writing about those experiments because they were all secret. Of course, nowadays the planes are so much faster, and the problem is bigger. He's trying to convince them that they've got to put these pilots so they're lying down with some sort of a prop for their heads so that they can run the plane. When they go through a sharp turn, the heart will not have to pump blood up to the brain against all the centrifugal force, it just pumps it laterally so they won't lose consciousness. He says there are a lot of pilots today killed in practice flights. But, to get the military to go along with these ideas is very difficult. And the pilots are not very crazy about it either, because they don't want to lie prone. But he feels that that's the only way that you can really take the kind of Gs that they get nowadays. He thinks they've got to do something like that. He doesn't have a lab, but he's got a grant to write about his work and also try to convince some of the military to take this on. There was a fellow named Dave Clark who ran a fabric shop in Boston. Dave Clark was in our deer hunting outfit. He and Earl worked together. Dave designed practically all the suits that the astronauts used for landing on the moon as a result of the equipment that they made for the pilots. But Earl couldn't stay at Mayo's.

BOHNING: The fact that you stayed, is that departmental? Does the university have any policy?

WOOD: Yes. Hanson has to go every year. I didn't know this, but he told me a while back. He has to go every year to get permission to let me stay on, which is all right. Some people just don't keep producing. That's all there is to it. Some people ought to quit before they're sixty-five. I don't mind being evaluated on an annual basis. I know if I can't get grants I'm through, that's for sure. And I've known that for a long time. I always figured, "When I don't get a grant it's time for me to hang up my shingle and get out of here." And of course a lot of that depends on whether you attract good collaborators and motivate them. Motivation is the biggest thing. If you can motivate your crew and you've got a few guys that are helping motivate, things keep going. They do things I don't dream up, that's for sure. That's the advantage of working with younger people because you've got younger brains. If you can sort of give them the freedom that they ought to have and give them the enthusiasm and of course help direct them because you're trying to get certain things done, why that's fine.

BOHNING: I think on that note we'll close and I thank you very much. I've enjoyed it.

[END OF TAPE, SIDE 7]

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