THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

# ERNEST H. VOLWILER

Transcript of an Interview
Conducted by

James J. Bohning

in

Lake Forest, Illinois

on

18 August 1986

Dr. Ernest H. Volwiler

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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(Date) Nov 14, 1989

(Revised 20 February 1989)

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

# ERNEST H. VOLWILER

1893 Born in Hamilton, Ohio on 22 August

## Education

1914 B.A., chemistry, Miami University
1916 M.A., chemistry, University of Illinois
1918 Ph.D., chemistry, University of illinois

## Professional Experience

1910-1911 Teacher, Reilly Township Public School, Butler County, Ohio
1913-1914 Teaching Assistant, Chemistry Department, Miami University
University of Illinois
1914-1917 Teaching Assistant, Chemistry1915 Chemist, Organic Chemical Manufacture (summer)
1916-1917 Chemist-in-Charge, Organic Chemical Manufacture (summer)
1917-1918 Fellow in Chemistry
Abbott Laboratories
1918-1920 Research Chemist
1920-1930 Chief Chemist
1930-1933 Director of Research
1933-1946 Vice President, Research and Development
1946-1950 Executive Vice President
1950-1958 President and General Manager
1958 Chairman of the Board
1961- Consultant (retired)
1951-1961 President, Abbott Laboratories Fund
1958-1961 President, The Abbott Foundation
1958-1961 Chairman of the Board, Abbott Laboratories International Co.
1958-1961 Chairman of the Board, Abbott Laboratories Universal, Ltd.

## Honors

| | |
|---|---|
| 1940 | Modern Pioneers Award |
| 1946 | D.Sc. (Honorary), Miami University |
| 1947 | Honor Scroll Award, American Institute of Chemists |
| 1949 | D.Sc. (Honorary), Northwestern University |
| 1951 | Centennial Award, Northwestern University |
| 1953 | LL.D. (Honorary), Coe College |
| 1954 | LL.D. (Honorary), Knox College |
| 1954 | D.Sc. (Honorary), Philadelphia College of Pharmacy and Science |
| 1954 | Chemical Industry Award |
| 1955 | Industrial Research Award |
| 1958 | D.Med.Sc. (Honorary), Southwestern at Memphis |
| 1958 | D.Sc. (Honorary), St. Louis College of Pharmacy and Allied Sciences |
| 1958 | Priestley Medal, American Chemical Society |
| 1958 | Citation of Honor, Indiana Technical College |
| 1959 | Sesquicentennial Award, Miami University |
| 1959 | D.Sc. (Honorary), University of Illinois |
| 1960 | Gold Medal Award, American Institute of Chemists |
| 1977 | D.H.L. (Honorary), Lake Forest College |
| 1986 | Inventors Hall of Fame |

## ABSTRACT

Ernest Volwiler begins this interview by briefly discussing his early years in Ohio, leading to college study at Miami University, where he first became interested in chemistry. He recollects the influence of faculty there and his wish to undertake graduate studies at the University of Illinois. The recounts the circumstances of his decision to proceed to the Ph.D. program after completion of his Master's degree. Research work with roger Adams and the summer organic chemicals production enterprise helped to introduce Volwiler to drug synthesis. His long career with Abbott laboratories started in organic synthesis, including some plant production responsibilities. Volwiler recalls Dr. Abbott as well as some of his fellow workers in the laboratories in the 1920s and 1930s. World War II saw the production of penicillin and company expansion. Volwiler was a member of the pharmaceutics investigating team sent to Germany immediately after the end of the war and he describes his experiences there. Post-war advancement led Volwiler to the presidency of Abbott Laboratories and he tells how he trimmed the production line and initiated development into new areas. His ACS activities culminated in his election as Society President in 1950. Volwiler ends his interview with Bohning by reflecting on the nature of the modern pharmaceutical industry.


## INTERVIEWER

James J. Bohning holds the B.S., M.S., and Ph.D. degrees in chemistry, and has been a member of the chemistry faculty at Wilkes College since 1959. He was chair of the Chemistry Department for sixteen years, and was appointed chair of the Department of Earth and Environmental Sciences in 1988. He has been associated with the development and management of the oral history program at the Beckman Center since 1985, and was elected Chair of the Division of the History of Chemistry of the American Chemical Society for 1987.

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INTERVIEWER: James J. Bohning

LOCATION: Lake Forest, Illinois

DATE: 18 August 1986

BOHNING: Dr. Volwiler, you were born on August 22, 1893 in Hamilton, Ohio. Can you tell me something about your father and your mother? What were their names and occupations?

VOLWILER: Both of them emigrated from Germany in the 1880s. My father was a stone mason by trade. My mother came from a family that did cooperage in Germany. They were skilled craftsmen and controlled a small company over there. They decided that they wanted to come to this country where there were more opportunities and they had relatives here. They came in the 1880s and my father first worked for a foundry in Cincinnati. Later he moved to Hamilton, and then in 1892 he had a small farm. I suspect they had some experience in youth in farm life, but I'm not sure. At any rate, they started farming the year before I was born. I grew up on the farm. I feel these were very happy circumstances in my early life because it gave me a better background of life and contentment and ambition than city life would have. I lived on the farm until I went to college.

BOHNING: Did you have any siblings?

VOLWILER: Yes. I had an older sister and an older brother.

BOHNING: Did you have all your schooling in Hamilton?

VOLWILER: Well, it was in the country, not in Hamilton.

BOHNING: I see. Was it a one-room school house?

VOLWILER: Yes, I attended a one-room school house. Then I went to high school in that area. It was a very, very small high school, almost ludicrous because it was so small. I taught in one of those one-room country schools for a year and then I went to college.

BOHNING: Did you have any exposure to science in that small school?

VOLWILER: No.

BOHNING: As you were finishing high school, had you given any thought as to what you might want to do? Why did you go to college?

VOLWILER: Well, my brother, who was four years older than I, graduated ahead of me from Miami University. My parents were education oriented and very much concerned particularly since they had not gone to college or had much schooling at all. My brother went to college and I just naturally went also. Although my parents were poor, they did not hesitate at all about putting what was needed into our training.

BOHNING: You spent one year teaching. What was the name of the school that you taught at?

VOLWILER: Reilly Township School.

BOHNING: Did you teach all grades? Was it another one-room school?

VOLWILER: Yes, that's right.

BOHNING: Had you thought about teaching permanently as a result of that?

VOLWILER: Yes, it was a possibility. I had planned to go ahead, but that was something I definitely had in mind.

BOHNING: You went to Miami in 1911?

VOLWILER: That's right. I went to two summer school sessions. My parents came from Germany so I had a German background. I spoke German from the time I was a child. So when I went to college I went to the head of the German department and said, "I think I know enough German to pass an examination. Will you give me one and give me credit for it?" He agreed. So I got twenty hours of college credit because of the examinations that I took under him. That, together with two summer school sessions, enabled me to finish school in three years instead of four. I was very, very busy.

BOHNING: What courses did you first take when you enrolled at Miami?

VOLWILER: I went to the summer school and I took a course in history and I took a course in chemistry. The teacher of that course was Professor [John C.] Hessler, who was an exchange professor for the summer from Millikin University in Decatur, Illinois. He was the head of the chemistry department there, and in the summer he taught at Miami. I took his course, and when I finished he said to me, "You seem to like chemistry a lot." And I said, "Yes. I do." He said, "Well, maybe you'd like to follow it up." So that was the origin of my interest in chemistry.

BOHNING: Do you remember anything about that course from Hessler? Did you do much laboratory work?

VOLWILER: No, there was very little laboratory work. Most of it was lectures. There was some laboratory work, and Hessler was a very good teacher. In fact, he later became president of Millikin University. At any rate, I enjoyed the chemistry course more than the history course.

BOHNING: Was that the summer prior to your full-time enrollment?

VOLWILER: Yes, it was. That must have been the summer of 1910.

BOHNING: What other chemistry did you take at Miami?

VOLWILER: I took general chemistry, of course. I took a course in general chemistry and a course in physical chemistry. That's all there was.

BOHNING: Do you recall who the instructors were, especially in the general chemistry course?

VOLWILER: Dr. Harvey Brill, Dr. James Egan, Dr. [William H.] Whitcomb, and Dr. [George G.] Oberfell. Now, Egan was a University of Illinois Ph.D. That is what influenced me in a considerable part to apply to Illinois. Part of that time Brill was on a leave of absence in the Philippines on a federal government assignment of some sort doing chemical work. Beyond that I don't know. Oberfell was an instructor in chemistry and later went into industry and became vice president for research and development of the Phillips Petroleum Company.

BOHNING: Who was the organic chemist?

VOLWILER: Egan and Brill.

BOHNING: Is that where your interest in organic chemistry really developed?

VOLWILER: First with Egan, then with Brill.

BOHNING: Did you have any laboratory experience in these courses?

VOLWILER: Yes. We had some laboratory work in general chemistry and in organic chemistry.

BOHNING: Did you do any research at all?

VOLWILER: No.

BOHNING: Was your organic laboratory mostly synthesis?

VOLWILER: Yes. It was simple and straightforward synthesis.

BOHNING: As you were finishing up your education at Miami, did you have any thoughts about a career at that point?

VOLWILER: Yes, it started growing at that time. These men that I mentioned were stimulating people who could interest students in their field.

BOHNING: Do you recall any other students? Were there many chemistry majors at the time?

VOLWILER: No. No, there were not. There were one or two when we started out who showed some interest but later dropped and went into another field. There were very few who made chemistry a career. I don't know why it was, because I think the teachers were good. They were stimulating.

BOHNING: Did you take math or physics?

VOLWILER: I didn't follow up with math. The farthest I went in math was calculus.

BOHNING: Did you have a physics course at Miami?

VOLWILER: I had a physics course which was a good one and that was taught by Dr. Culler. He was head of the department and a very good teacher. I think he did some research but I'm not sure in what area.

BOHNING: You said that Egan was the one who influenced you toward Illinois. Had you thought about any other place besides Illinois?

VOLWILER: Yes, I did. I applied to Harvard and to Princeton. I think I applied to Michigan, but I don't remember. At any rate, I got an offer of a fellowship of $100 a year from Harvard and an offer of $300 a year from Illinois. Illinois was closer and the money was better, and I decided to go to Illinois. (I was inclined toward Illinois anyway.)

BOHNING: I also wanted to ask you about a summer at Miami when you worked in the iron mines in Minnesota as an analytical chemist.

VOLWILER: That's right.

BOHNING: How did you arrange for that position?

VOLWILER: My brother was teaching high school in Hibbing, Minnesota. Before the end of the school, I was looking around and he said, "I think you might be interested in a job at the Oliver Iron Mining Company, which was a U.S. Steel Company subsidiary. They gave me a chance to do chemical work to make a living as a chemist. It wasn't very complicated.

BOHNING: What kind of work did you do?

VOLWILER: It was straight analytical work.

BOHNING: Was it a gravimetric iron kind of analysis?

VOLWILER: Yes, I think it was gravimetric iron.

BOHNING: Was this done on the ore before it was shipped out?

VOLWILER: Yes.

BOHNING: I understand that you had planned to save that money to finance your last year at Miami, but when you returned to Ohio, you had a very small amount of funds left.

VOLWILER: [laughter] That's right. That's exactly right. When I got back to Miami to start my senior year, I was really broke. [laughter] I got a menial job off the campus. I don't know whether I picked apples or what I did. Then I borrowed more money from my parents.

BOHNING: Didn't you teach somewhere along at the end of the year at Miami?

VOLWILER: No, I was just a laboratory assistant. I was not an instructor in the formal sense. I was actually a lab assistant.

BOHNING: When you first got to Illinois, you worked for Professor [Clarence] Derick. How did you select Derick? Did you do research with Derick for a thesis?

VOLWILER: Yes, I started in and Derick gave me a small research problem to work on. He was head of the organic division so that's where I ended up. Derick was a very enthusiastic guy, and very loyal to his students. He was very much interested in what you were doing. He was really a "pepper upper."

BOHNING: Were there many students in that organic division when you were there?

VOLWILER: No. There were about five in the organic division.

BOHNING: Do you recall who they were?

VOLWILER: Yes, there was [Ralph E.] Rindfusz, who went into the paper industry and had a premature death. He died after about five years in industry. There was Lloyd Howell, who later headed the chemistry department at Wabash College. And there was Potterf, who came down from the University of Chicago and died a few years later of typhoid. Those are the ones I remember.

BOHNING: What kind of work did you do on your thesis? Wasn't it a master's thesis that you were doing?

VOLWILER: Yes, that's right. I couldn't even tell you the exact description of it now. It was a very simple organic chemistry synthesis. It involved trimethylene oxide. [laughter] That's one thing I remember. I can't tell you any further than that because I've forgotten the details, but it was a simple problem in organic synthesis.

BOHNING: What courses did you take when you started?

VOLWILER: I took a course in organic chemistry.

BOHNING: Was that with Derick? Was he teaching it?

VOLWILER: Right. Then I took physical chemistry with Washburn. That was to be my minor. I don't remember whether I took any other chemistry courses that year.

BOHNING: You were on a teaching assistantship so you were instructing in the laboratory.

VOLWILER: That's right. That went on for two years. The first year it was $300, and the second year it was $400. The third year it was $500. My last year required no teaching. It was a straight fellowship, and paid me $400 or $500. With the organic preps that I'll come to later I was able to pay all my expenses during those four years from the income from the fellowships or the teaching assistantships, and the summer work.

BOHNING: As I understand it, you were thinking of leaving after getting your master's degree. Is that correct?

VOLWILER: Oh, not quite. I didn't know whether I could finance the last year, but I wanted to go after a Ph.D.

BOHNING: I understand that Paul Anders, who was a glass blower, told you you should stay. Is that correct?

VOLWILER: I don't know where you got all these little details, but yes, if you want that story, I'll tell it to you.

BOHNING: Okay.

VOLWILER: I went there in the fall of 1914. In 1916 things were in a turmoil because Derick had decided to take a job as research director of the National Aniline Company in Buffalo which later became the Allied Chemical Company. Derick was an impatient man [laughter], somewhat flighty, and they offered him the directorship of organic research at National Aniline. A man named Don Bissel also had received his M.S. after two years under Derick at Illinois. Derrick offered Bissel a job, and he offered me a job as a research chemist. Bissel accepted, but I did not. But, in the following fall, we were back as graduate students working toward the Ph.D. There was a man named A. B. Davis, who was with Eli Lilly at the time. For some reason, he was leaving Lilly to become Vice President and Research Director of the Ault and Wiborg Company which was an ink manufacturing company in Cincinnati. It was fairly good size and they thought they ought to have some research. I didn't know how Davis got my name. He offered me a job, and that job was to be in research. I don't know what research in ink could lead to. It was to pay $1800 a year. Now, at that time, chemists who were in the universities were making less than that. After several discussions I wired Davis and said I would accept.

[END OF TAPE, SIDE 1]

VOLWILER: I talked to Dr. [Roger] Adams in the meantime. He was new there. He had just come. I had switched over to him, and he had accepted me as his graduate student. When this offer came along, I discussed it with Dr. Adams. He didn't discourage me. He just gave the pros and cons, but didn't try to talk me out of it. He was a new young unknown professor there. People were wondering what was going to happen to the department, so he didn't feel like taking a strong position to talk me out of it. After a little turmoil I met Anders in the hall. Anders was a German glassblower and he stopped me and said, "Volwiler, I understand that you're going to quit your graduate studies, and that you're going to take a job. Volwiler, you're a damn fool." [laughter] He said, "You mark my word. In Germany, if you don't have your doctorate, you are not likely to become head of a laboratory facility. It will get that way here. You mark my words. It will get that way in this country." So I thought about it and the more I thought about it, the more I thought, Paul Anders makes sense. So I went in and talked to Dr. Adams, and I said, "Dr. Adams, I've changed my mind. I'd like to continue with you. Will you take me?" He said he would, and that was it.

BOHNING: I would like go to that summer organic chemicals manufacturing. I think it was in the summer of 1914 that Derick was employing people to do those preps that were needed in the

fall courses, and perhaps for some research. In 1915, when the war had started, things had changed. Did you start doing that in the summer of 1915?

VOLWILER: That's right.

BOHNING: So Derick was still doing that that first summer?

VOLWILER: That's right. Wait a minute. Maybe I'm a year off. In the summer of 1914 I wasn't even on the campus. In the summer of 1915, Derick had hired me at twenty five cents an hour to make preps. It was during the summer of 1916 that he accepted the job at National Aniline.

BOHNING: Okay. So Adams wasn't directing those preps in the summer of 1916 yet.

VOLWILER: No, not in the summer of 1916. He was in charge the summer of 1917. He saw what Derick had in mind, how he was doing it, embraced the idea and followed it up very effectively. He introduced cost controls, and developed the program so that it was important by the summer of 1917.

BOHNING: What kind of preps did you do those first two summers?

VOLWILER: One prep I remember was chloropicrin. We weren't in the war yet, but we were getting ready. They needed a kilo of chloropicrin. That was my very first job. Of course, we didn't know much about it but we knew it was a toxic substance. We didn't have very good facilities for working with toxic substances, but we were careful and we got by. That's one. I don't remember anything else.

BOHNING: So they were war related materials. What kind of facilities did you have? What were the labs like at that time?

VOLWILER: My most vivid recollection of the labs was the inadequacies of venting fumes. For example, I had some product to make in which I had to use phosphorus pentachloride, a chlorinating agent. And the hoods just wouldn't carry it away at all. So, I'd take these Kjeldahl flasks and put the ingredients in there and start walking, and I'd do it at night, so I wouldn't be poisoned. I'd go down the halls shaking it to mix it and go around the square building, you know, and I'd walk to keep ahead of the fumes, and by the time I got all the way around, well, they were pretty well dissipated.

BOHNING: How many other students were employed in the pre-Adams summer period?

VOLWILER: Probably four or five.

BOHNING: Were they the same people who were in that group of organic graduate students?

VOLWILER: Yes. One of them was [Carl] Marvel. I don't remember the others.

BOHNING: When you first started your work with Adams, how did you select the problem for your Ph.D.? Did Adams assign that to you?

VOLWILER: He suggested several problems and I picked one of them.

BOHNING: In the summer of 1917, when Adams started directing the prep work, was that related to your Ph.D. work?

VOLWILER: No.

BOHNING: That was separate. Was Adams synthesizing Barbital and Procaine at that point?

VOLWILER: Procaine was a little later. It was a year or so later when Adams and Dr. [Oliver] Kamm working together were interested in drugs, anesthetics and hypnotics.

BOHNING: Adams came to Illinois with that interest, didn't he?

VOLWILER: Yes, I think so.

BOHNING: Did it develop at Illinois?

VOLWILER: Yes. He didn't bring a lot of that interest with him. You see, the pressure of the war had developed somewhat more by the time Adams had come and spent say a year there. So Adams was trying to see some possible problems of interest there for his own work.

BOHNING: But that developed before the Abbott connection was made.

VOLWILER: That's right.

BOHNING: Okay. Wasn't it Mallinckrodt that he tried to interest first?

VOLWILER: Yes.

BOHNING: So, you had some experience in the synthesis of these two materials. When would that have been?

VOLWILER: Somewhat later. That came during the first half of 1917, I suppose.

BOHNING: Was the prep material work for the war effort becoming more and more of a project?

VOLWILER: No, we didn't get a lot of that.

BOHNING: Did you make these in large quantities?

VOLWILER: Oh, if we made a kilo, that was the largest amount.

BOHNING: You completed your Ph.D. work in 1918. Again, let me ask you, had you thought of career plans as that was coming to a close? The war was ending.

VOLWILER: Yes. Of course, industrial chemistry was still a rather new field, but it got into our consciousness. We didn't necessarily want to go into teaching, or plan to go into teaching. But these industrial jobs would look interesting, too. So, it was kind of a toss-up affair, as to whether we were interested in academic or industrial work. I think it was generally true that the attitude of all of us was pretty similar. If there was a good teaching job, that certainly was of interest to us. If there was a good industrial job, that was more unusual, because we were just getting into a stage where industry was hiring chemists.

BOHNING: Let me ask you how the Abbott position came about. I've read different accounts of how you made that first connection with Abbott.

VOLWILER: Yes. By that time, the spring of 1918, Adams was already doing consulting for Dr. [Wallace C.] Abbott. They had accepted a contract from the Navy for several drugs: Barbital, Procaine, cincophen. They had a few chemists, but not many, and it hadn't come through yet. They had a Navy contract for a considerable quantity of those drugs, but had run into some production problems. Dr. Abbott talked to Dr. Adams about it and they agreed that they ought to have some more man power on these problems. So Dr. Abbott got in touch with me and told me to come up on a Saturday, which I did. I remember that. He then hired me. The job looked interesting to me so I agreed to come up a couple of months later and start work. By that time I had my Ph.D. Dr. Abbott was a very dynamic man and he could interest and inspire people, whether he was personally acquainted with the field or not. Well, Dr. Abbott lived for a only a few more years, and was succeeded by Dr. [Alfred S.] Burdick.

BOHNING: Did Abbott explain what you would be doing if you joined Abbott Labs?

VOLWILER: Yes.

BOHNING: Did he have a plan in mind?

VOLWILER: Yes. Dr. Abbott, although he was a physician, had a research sense. He was interested in research. And Dr. Burdick who succeeded him was even more adept in that field. So they wanted to start a research program. They wanted to develop new drugs or old ones if they could be done properly. So that was what they hired me for and it was a nice job to have because it was new. I didn't have to ask permission for every step I took. They encouraged you to do that, and it worked out very satisfactorily.

BOHNING: When you joined in the late spring of 1918, were there other chemists, or were you one of the first, let's say synthetic organic chemists?

VOLWILER: There were just a few synthetic chemists. None was hired in a short period, say three months or six months. I was the only organic chemist who was hired within that quarter of a year.

BOHNING: Wasn't this a transition period between extracting natural products and moving into the synthetic area?

VOLWILER: Yes.

BOHNING: Was Abbott looking specifically at that?

VOLWILER: Yes, definitely.

BOHNING: Did you have any interactions with Dr. Abbott in those two years that he lived after you joined? Did he come through the lab when you were working?

VOLWILER: Yes, he did. He'd come to the lab every once in a while. He was a go-getter. Sometimes he'd be "go-getting" the wrong way. But he had the courage to do things. His batting average was pretty good. A man whom he knew, Dr. [S. Lewis] Summers, came to him with a new process for making phenacetin. That had kited in price terribly. It was hardly available anymore from Germany. Dr. Abbott thought that since phenacetin was not available in Germany anymore, and there was a considerable demand for it in this country, he might buy this process. Well, this man had a small set-up to make phenacetin in Pennsylvania, and Dr. Abbott talked to me about it. I said, "Dr. Abbott, I hope you won't invest in this because it can't work. It has the groups in the wrong position on the ring." They were ortho and they should have been para. Well, he listened, but he wasn't convinced. This man was a good salesman, so Dr. Abbott bought the process and came up to the laboratory to tell me that he wanted me to get involved. And I said, "Dr. Abbott, it can't work. It's like building a brick house out of lumber." He didn't listen.

To make a long story short, he later decided to go ahead anyway. He came up to see me, but I was gone. I was getting married. [laughter] So, he sent a man working with me, Elmer Vliet. He spent a month there. He'd write me a note (I was back by that time from the honeymoon) and say, "Well, we put the intermediates into a pot and turned on the heat and the thing went puff." That happened several times. I tried to get Vliet back. Dr. Abbott finally gave up after a month or so. But then we started developing drugs in a limited way as fast as we could. We tried to do some research alongside the development of drugs that were already known, that were mostly ex-German drugs, and no longer available in this country. That was the saga of our synthetic program.

BOHNING: I believe that when you first arrived you were assigned to work on the production of Procaine and Barbital. You were assigned to that and I think it was Adams who said you were a technical laborer? Was that a term that he applied?

VOLWILER: I never heard it.

BOHNING: I think it was Adams who had said somewhere that you were really a technical laborer in those early days in the production area. Could you tell me something about the production of those materials?

VOLWILER: Well, it was feasible, all right. The only thing was that the equipment was scarce and not readily available. For example, a pressure vessel was practically unavailable. And the kind that we needed was just generally not available, so we had to make due with what was possible, and wait for the availability of the equipment we really wanted. So, to that extent, that term is right. Well, what does technical labor mean? No matter who it was, he had to do the same work. That work had to be done.

BOHNING: What was your involvement in the production? Were you out in the plant?

VOLWILER: Yes. I was in the plant. The first year I spent all my time in the plant. Then we had new laboratories and they started doing some research on new products, new compounds.

BOHNING: The idea of working on anesthetics, was that Adams' influence, was that Abbott's suggestion, or was that your suggestion?

VOLWILER: The first interest came from Adams at Illinois in 1917 or 1918. Adams and Kamm were working on local anesthetics starting with Procaine. Adams turned that work over to me at Abbott later. That developed the anesthetics we call butin and butesin and those were marketed in the early 1920s. We did manufacture Barbital in that period and from that developed two compounds in the barbituric acid series.

BOHNING: I understand that you called butyl alcohol a building stone. When the war was over, some of these new alcohols became commercially available and that allowed you to synthesize more materials than would have been possible before. Is that correct?

VOLWILER: Yes. Well, you see, at that time, quite a number of new alcohols became available that had never been available before.

[END OF TAPE, SIDE 2]

BOHNING: During the 1920s, that ten-year period, your title was "Chief Chemist." Would you describe what that meant? How many chemists were there at Abbott in 1921?

VOLWILER: Oh, I don't think there were more than half a dozen.

BOHNING: And during that ten-year period a number of them were brought in. Were you responsible for locating these people and hiring chemists?

VOLWILER: Yes, I did that.

BOHNING: How did you find out about them? How did you recruit new synthetic people?

VOLWILER: Oh, generally I'd ask the heads of departments, like [Moses] Gomberg at Michigan, and of course I had a direct line to Adams. I'd go around about twice a year and see how the students were coming along.

BOHNING: Did you have any relationship with the University of Chicago?

VOLWILER: Yes, I did. Quite a lot. I had a close relationship with [Julius] Stieglitz. He acted as a consultant on certain problems. As we would go along, we'd talk to him every once in a while. He was quite cooperative.

BOHNING: Did you use Gomberg as a consultant?

VOLWILER: No, not as a consultant. We certainly used him as a consultant to hire personnel.

BOHNING: But not on technical problems.

VOLWILER: No, not on technical problems.

BOHNING: Were there any others besides Adams and Stieglitz that you called on?

VOLWILER: I don't think there were any others.

BOHNING: I think you started by modifying existing products. You said you were doing that as well as working on old ones. In 1923 you had synthesized Neonal.

VOLWILER: We marketed that for a while, but then it was superseded by some others.

BOHNING: Did you make a large number of compounds? Did you just try to make as many derivatives as possible of the original Barbital or barbituric acid?

VOLWILER: Well, we made all those that we could think of from available intermediates. Availability was really the important thing.

BOHNING: Can you tell me something about [Donalee] Tabern? He came to Abbott around 1926, and you were responsible for bringing him there.

VOLWILER: That's right.

BOHNING: What kind of a person was he?

VOLWILER: He was a very intense person. He was a man of boundless energy. He had drive and enthusiasm, and great ability. He was the most capable and energetic person of all the scientists with whom I've been associated. He would encourage people if they were his assistants. He would constantly hammer at them to get the work done. I never knew a more capable person doing synthetic work. It was not only synthetic work, because he did a lot of other things. He is the one who started the radioisotope work.

BOHNING: What was his first assignment? You were already in the middle of something, I believe.

VOLWILER: Yes, it was Nembutal. I had made the ester of Nembutal and wasn't able to finish the job because I had other things to do, so I gave it to him and he finished it in good short order. We put the product on the market quite successfully.

BOHNING: In 1923, you commented in an article that there was a tendency to press on for new therapeutics before old ones were completely studied, and part of that rested in the pharmacological testing of these materials (1). What was the situation like at Abbott in the 1920s?

VOLWILER: Well, pharmacologists were hard to find. There weren't many. Pharmacologists themselves, the established people in the universities, looked down their noses at any pharmacologists or any other person who would go into industry. That is an awful thing to do. [laughter] They had no respect for pharmacologists who went into industry. It took a long time to overcome that.

BOHNING: What kind of testing was going on at Abbott then?

VOLWILER: We had several pharmacologists, but that wasn't nearly enough.

BOHNING: And at this point you were turning out a number of new compounds.

VOLWILER: We were turning out a number of compounds that we wanted to have tested.

BOHNING: I think you used people like [Arthur L.] Tatum at the University of Wisconsin and [John S.] Lundy at the Mayo Clinic?

VOLWILER: Lundy was an anesthesiologist at the Mayo Clinic.

BOHNING: How had you developed that relationship with them?

VOLWILER: Well, before that, members of our medical department (and others too) would go up there and see what was going on, what was new, find some direction and worthwhile projects. So, we became acquainted with these people, and had a good relationship with them and it worked out fine.

BOHNING: How long did you retain that relationship with them?

VOLWILER: Well, I think we still have it. [laughter]

BOHNING: I was interested to note that this year was the 50th Anniversary of Pentothal. It was 1936 when it was introduced in the market. Could you tell me something about how that originated?

VOLWILER: At the start we were making sleep-producing compounds. Lundy at Mayo and [Ralph M.] Waters at the University of Wisconsin Medical School were looking with interest at these compounds we submitted that were sleep-producing drugs. They looked at the side effects and different uses for pre-operative sleep, or anesthesia. That started out with Nembutal, but Nembutal didn't quite have the anesthesia aspect they wanted. When Pentothal came along, it was just one one of many compounds that were made. Tabern and I carried on this work jointly for several years. Tabern did most of that because he could spend full time on it and he was very, very good. He turned out scores of compounds.

BOHNING: How did the original suggestion to put the sulfur in place of the oxygen come about?

VOLWILER: Well, it's just a logical extension. We had Nembutal for example, which had sleep-producing properties, and so we wanted to find out whether by modifying that molecule, we could get improved results. Well, what we wanted to do was to put different groups in that molecule in various places, any place we could. One of the places was the oxygen. When we put sulfur in, that was the real breakthrough because we made scores of compound, and after that many of them contained sulfur with other groups. Pentothal was the best one.

BOHNING: Were there previous examples of sulfur replacing oxygen in any of the other series?

VOLWILER: No. There weren't too many places where we could put sulfur into the molecule, but we put sulfur not only in this molecule, but in a number of similar ones where the structural changes were made in non-urea parts of the molecule.

BOHNING: This period through the 1920s was a very intense period for you and Tabern and others synthesizing an enormous number of molecules. As part of this development there are three things I wanted to ask you about, and let me put them together. What were

you reading? Who were you talking to? And were you attending ACS meetings or other meetings?

VOLWILER: I'll answer the last part of your question first. The company was always quite generous in permitting the research people to attend meetings in their field.

BOHNING: Were these primarily ACS meetings that you were attending?

VOLWILER: Yes. Pharmacologists would also have important meetings in various parts of the country, as well as the bacteriologists. There were quite a few disciplines that we followed up at these meetings that we attended.

BOHNING: What were you reading at that time?

VOLWILER: Well, we were reading what research was going on elsewhere, and that was in the Journal of the American Chemical Society, the pharmacology journals, and the journal that the biologists used. We were interested because we needed to find out what compounds were being developed that were of potential interest and had physiological action.

BOHNING: Were you getting input from the medical community, from the physicians in the field at all, or were they just on the receiving end?

VOLWILER: Yes, so far as the activity that they wanted, there was definite input. As far as specific compounds were concerned, of course, they were not involved.

BOHNING: Were you in touch with your counterparts at the other pharmaceutical companies?

VOLWILER: To some extent, as long as I didn't disclose anything. [laughter]

BOHNING: I was just wondering whether you got together with the other research directors or synthetic people.

VOLWILER: The most prominent example of that was the development of penicillin. There the wraps were off, and the Department of Justice couldn't prosecute us for collusion.

BOHNING: That's one of the things I wanted to talk about. What was the size of the research staff from the late 1920s into the early 1930s?

VOLWILER: The entire research staff? I wish I had that photograph here. I ran across an old photograph recently from the early 1930s and I would say there must have been fifty or sixty people at that time.

BOHNING: If you have that old photograph, would it be possible for us to make a copy?

VOLWILER: I could look.

BOHNING: And we could identify the people. It would be interesting to have a photograph of that period with the identification of the people.

VOLWILER: I'll see what I can do.

BOHNING: In 1930, you were Director of Research, but I understand there was already a research committee that existed and you were chairman of that committee. When did that research committee start and who was on that committee?

VOLWILER: I can't tell you when it started.

BOHNING: Was it sort of an informal arrangement initially?

VOLWILER: No. It was while Dr. Burdick was still alive, and Dr. Burdick died in 1933, so it was in the late 1920s. It was set up on a more or less formal basis. We had regular meetings, and kept minutes.

BOHNING: Who else served on that committee? Was Burdick on it?

VOLWILER: No, he was sort of ex-officio. He wasn't on it. I was chairman. Dr. [Joseph F.] Biehn, the medical director, was on it. [Carl] Nielsen, head of pharmacology, was on it. Dr. [Hobart W.] Cromwell, the bacteriologist, was on it. It took off from there.

BOHNING: I wanted to ask you something else about Burdick. You had already indicated that he was quite involved in support of research. How did that translate into dollars and facilities and equipment?

VOLWILER: You have to have support for your activities if you want to market a product. We got very good support from the top.

BOHNING: Was Burdick involved the way Dr. Abbott was? Did he come through the labs and talk to you?

VOLWILER: Occasionally. Yes.

BOHNING: Did he give directions or did he allow you to develop on your own?

VOLWILER: He would give directions broadly, and say, "Look, here's a new product that was developed at the University of Wisconsin. Can you do anything to speed it up or put more manpower on it?"

BOHNING: In many respects you were free to develop what you wanted?

VOLWILER: Yes, they left that mostly to the research people.

BOHNING: I've forgotten who took over from Burdick when Burdick died in 1933.

VOLWILER: [S. Dewitt] Clough.

BOHNING: Did that transition have any effect on research? What was Clough's attitude toward research?

VOLWILER: Clough was very supportive. He had imagination and drive. He did very well with the research people.

BOHNING: Was it also in the 1930s when Abbott moved into the vitamin field?

VOLWILER: No, it was the 1920s. That gave Abbott income. There was a lot of activity that represented a good deal of growth in that period.

BOHNING: Did you have any involvement with that vitamin development?

VOLWILER: Not too much. That was mainly left to Carl Nielsen.

BOHNING: I have another statement from Roger Adams who said that "your greatest service to the medical profession was the ability to forecast trends and needs and to sense the potentialities of embryonic developments" (2). How did you sense the areas that needed to be followed?

VOLWILER: [laughter] Oh, I don't know how I sensed them, but if someone is really interested and gets deeply involved, he is going to follow the literature, he is going to follow the state of the art. There was nothing unusual about that. It is just an activity you have to engage in to know what is going on. If you know what's going on, you are supposed to put things together and come up with something.

BOHNING: During that period prior to World War II, you published a number of papers, primarily with Tabern, on the barbiturates and other hypnotics. You also published with [Marlin T.] Leffler and with [Marjorie B.] Moore, but primarily with Tabern (3). I noticed a number of those were presented at ACS meetings as well. How did you work the patent rights while you were publishing the synthesis?

VOLWILER: Well, by the time they got published the patent was supposedly underway. We ran into a number of so-called interferences in the patent office where other scientists would get involved in the field we were in, but it got worked out. The mechanism in the patent office took care of that. Potentially the most troublesome one could have involved Pentothal, but it didn't. There were at least three firms who worked on that and got involved in the patent question. The respective rights of individual units could be identified, so it worked out all right.

BOHNING: Who were the other companies that were working on that?

VOLWILER: Parke, Davis and Merck.

BOHNING: Did those patent problems tie up the marketability?

VOLWILER: No.

[END OF TAPE, SIDE 3]

BOHNING: During World War II, Abbott was heavily involved in the production of a number of things, and of course your role was crucial in terms of being research director. The first one and perhaps the major one that I want to talk about was penicillin, and you've already mentioned that. Could you tell me something about Abbott's involvement in the penicillin production?

VOLWILER: Well, it was the major project at that time, not only by itself but in the potential discovery of other compounds involving the same kind of activity. When the information first became available regarding penicillin and its amazing properties, everybody got involved, including Abbott. And I think we did a pretty good job on that because we developed the process, and carried on some research on things related to penicillin but not penicillin itself. That really set the thing going. I don't know to what extent we could have done a better job. I think we did pretty well, not only with penicillin, but in the development of streptomycin. Various pharmaceutical firms developed quite a number of antibiotics, not all of which, by any means, survived. Some of them got as far as the market and then failed.

BOHNING: In terms of that open cooperation with your previous competitors such as Merck and Squibb, did you meet with your counterparts at the other companies?

VOLWILER: Only in the early days. After that competition reduced cooperation appreciably. In some cases, the products under question were marketed by several firms that dropped out, but they weren't particularly important.

BOHNING: In terms of the penicillin, Adams was on the OSRD committee. Did you meet with him in the early days of the penicillin production? Did you have any contact with him at the time?

VOLWILER: Well, we kept in touch with him, but really the involvement with Adams in the production of penicillin was not really great.

BOHNING: The company also was involved with blood plasma.

VOLWILER: That was mainly a matter of equipment. How do you evaporate the water from the blood plasma most effectively? There were several methods developed during that period which got into actual production. That is where freeze-drying got its real start.

BOHNING: What about sulfa drugs? Abbott had been making those before the war, hadn't they?

VOLWILER: That's right. Sulfanilamide, sulfadiazine, sulfathiazole.

BOHNING: Was that just a matter of increasing production?

VOLWILER: We spent quite a bit of time and effort on new sulfa drugs. While there were a number that were of interest, like sulfadiazine and sulfathiazole, we were not originally responsible for important new sulfa drugs. The patent situation with a number of the sulfa drugs was not very clear, so in many cases with a number of products, no one company really controlled the whole field.

BOHNING: I'm sure your involvement as research director during World War II was quite crucial. Can you tell me something about how the company geared up to produce large quantities and the extra effort that was made during that time?

VOLWILER: The extra effort involved more personnel, more equipment, and more buildings to take care of that equipment. You couldn't say, "We're going to put a lot more equipment on this project," unless you had a "barn" to put it in. Otherwise, it wasn't adequate. So, the building program was one thing that was undertaken pretty early and very actively. Then new equipment, of course, can follow. If you have the demand, equipment can be bought and installed as quickly as possible. So, that went pretty well for those products.

BOHNING: Was there any effect when the war had ended? The demand for many of these things must have changed.

VOLWILER: Well, not as much as you might think because the products were still in great demand. The company tried to anticipate that by directing more money and activity into research on products like the antibiotics. That followed the War right along. It didn't stop.

BOHNING: At the end of the war, you made a trip to Europe for the military. I'm interested in that experience. There were a number of scientific people who went to Europe as the troops moved into Germany. Could you tell me something about those experiences and how you were involved in that?

VOLWILER: This was a very interesting development because we didn't have a good idea about the kinds of activities the Germans were involved in and how intensive they were. So it gave us a good idea, through Baeyer, Hoechst, Badische, and others. Our impression was that they knew what they were doing. They had not developed on a scale we thought they had. They used more small vat sizes than we did in this country on comparable products. They had good relations with the universities. Their big committees in the big firms were quite active in the businesses. They didn't mind getting involved deeply in the production of medicinals. When we went over there we didn't know what to expect or how resistant they might be to disclosing information. Of course, they dragged their feet some, but they were quite cooperative in general. I don't know what aspect you had in mind?

BOHNING: Did you follow the troops in?

VOLWILER: Oh, they were scared to death and they were very cooperative. In general they were frightened. Of course they tried to hide some things if they could get by with them, but in general it worked out very well.

BOHNING: Were there any troops present when you went in to talk to the Germans?

VOLWILER: No, not in the room in which you talked to the Germans.

BOHNING: But the plant had already been seized?

VOLWILER: Yes. We got into the Ruhr, for example, Leverkusen where Baeyer was located, within two or three days of when the troops went in. So, we got them fresh. A by-product, something we were not assigned, was the war gases. That was an outgrowth of Baeyer's work on agricultural pesticides, and a man named Dr. Schroeder worked in that field with his assistants. When the Wehrmacht found out about it they took it away from Baeyer and took it over beyond the Ural mountains, where they stored three thousand tons of this stuff. I haven't heard anything, but for all I know, it's still over there because it is very difficult to destroy or inactivate a large quantity of war gases. We're

finding that out now, in the U.S., trying to get rid of what we made.

There were about eight of us in my group. When we went into the Ruhr we asked them, "You must know what you've been working on in the fields." Then they mentioned agricultural chemicals, and out of that came the statement that their work in agricultural chemicals included phosphorus compounds which were very toxic. Then the matter of war gases came up. So we said to Dr. Schroeder, "We want you to tell us everything you can about the war gases." So we left him with a big pad of yellow paper to write out everything that he knew about the development of the compounds. We left him there all night. The next morning he came to us and we looked it over. We quickly dispatched a plane to Paris and another plane to London to the American headquarters there who sent the information to the United States. That was the first information that we had in this country of what the German war gas compounds were. That was an interesting circumstance. It grew out of their agricultural research and I think it's true that they were not primarily interested in war gases, but they did develop war gases.

BOHNING: Do you think they were under orders to do that?

VOLWILER: I don't know. I think they probably were.

BOHNING: You said there were eight in the team. Do you remember the names of some of the other people who were with you?

VOLWILER: Mayor F. Fogler, Lester M. White, John W. Haught, [William S.] Calcott, and [Joseph E.] Smadel. The others were Francis J. Curtis, Gaston F. Dubois, and Percy J. Leaper.

BOHNING: What were their backgrounds?

VOLWILER: Well, Smadel was a physician. He was a colonel in the Army. Calcott was a vice president at Du Pont. Fogler was with Allied Chemical. Curtis was vice president of Monsanto, as was Dubois also. I should remember some of the other connections but I can't.

BOHNING: Did any of the information you uncovered on that trip prove useful to Abbott?

VOLWILER: Somewhat in general, yes, but not in the matter of specific compounds. They were of interest to us but we never developed them.

BOHNING: Was this information that you acquired made available to all of the companies or was it strictly government information?

VOLWILER: At the start when the war ended, we were not supposed to divulge information. That was never really spelled out, though, and some of that information immediately spread pretty broadly. Later it was generally available, but as far as specific products are concerned it was not used extensively.

BOHNING: Were the plants in the areas that you visited damaged heavily?

VOLWILER: Oh, yes. Some of them were terribly destroyed.

BOHNING: You were in Düsseldorf and Frankfort and Munich?

VOLWILER: They were all pretty severely hit.

BOHNING: In the period after the war, you were then vice president and Abbott developed a number of new products. I'm interested in your role in their development. I'm thinking of antihistamines, anti-epileptics, and of course Sucaryl, which came around 1950. I think the early work was much earlier than that, and 1950 was the date Abbott started marketing Sucaryl. Can you tell me something about your involvement with that post-war development?

VOLWILER: At that time I was in pretty close touch with the products that might be of medicinal interest. We would have contacts fairly regularly as to these products, and Sucaryl was one of them. Du Pont had the rights to it, but they thought the market was just a little too small to interest them so they gave us the license. I was pretty much responsible for that. Then later, Du Pont decided that Abbott was doing very well with it and maybe they ought to be in it anyway. They did market cyclamate for awhile but were disappointed in their results. So later, they dropped out again.

BOHNING: How did you find out about this information that was originally at Du Pont? Was it just in a casual conversation?

VOLWILER: No, I think it probably came through the people at the University of Illinois. Eli Lilly also had some rights from Du Pont, but I don't know what they were. They never marketed it.

BOHNING: You were quite active in ACS affairs and were president in 1950. What was the state of the Society like in 1950 when you were president?

VOLWILER: I think the Society was growing pretty nicely during that period. There were about 65,000 members altogether. Now how many are there?

BOHNING: Over 130,000 now.

VOLWILER: Yes, I know it's over 130,000 now. Physically the Society Headquarters in Washington and Columbus did very well. I think we built at the right time. When you see the agenda for the council meeting and see all the reports and groups that are active, it gives you pause because it's so extensive now, and such a big business. I've forgotten what the budget of the Society is, but it must be many millions.

[END OF TAPE, SIDE 4]

BOHNING: There was something in C & E News about Chemical Abstracts. I think you wrote the title as "Milestone or Millstone" at that time (4).

VOLWILER: Yes, that was a worrisome period and the Society was growing so big. Chemical Abstracts, for example, grew when it took on the foreign abstracts too. That was a marvelous job of organization that Dale Baker did. He did a very good job.

BOHNING: That post-war period in 1950 saw a lot of material being published that had not been published during the war, so that there was an unusual increase in volume, I suppose, around that time.

VOLWILER: Yes. And there were all the short cuts and mechanisms that were developed in that period. As far as I'm concerned, I don't understand them. I don't understand the abstract. [laughter]

BOHNING: When you became president in 1950, you dropped three hundred products in the Abbott lab. Why did you start trimming at that point?

VOLWILER: Oh, they never should have been kept on as long as they were because it costs more to promote them. In theory, well, Dr. Brown over here swears by this product. If you drop that, he's not going to be interested in some of the other products. That's a false argument. We started trimming more effectively at that time. That's gone on now. It's much harder for a product, unless it is an orphan product, to stay on the list. Do you know what I mean by orphan product?

BOHNING: I think so.

VOLWILER: The Food and Drug Administration will accept a product with some favorable aspect if it has a hard time staying alive. We had a product under test which was, without question, active. It was a product that had maybe a few thousand patients. That's all there were. Products like that, to some degree (there are probably a hundred altogether now) are given the O.K. for marketing by the FDA. They don't make any money for the company, but they are lifesavers for the patients. So, you don't want to leave that one high and dry. Abbott has one. Other companies have small numbers like that, too. We did get rid of a lot of compounds at that time.

BOHNING: You were the first president since Burdick who was a scientist. Your predecessors and the people in between were not scientists. Do you think that had any effect on the company? Was your approach different from their approach, coming as you did through the laboratory?

VOLWILER: I don't think so.

BOHNING: During that period, what did you consider to be your accomplishments as president?

VOLWILER: Well, I think our standing as a scientifically based company improved. We developed a number of new products which are still very good, like the erythromycins. The most marked change was the development of diagnostics, which was started during my presidency. The first was radioisotopes. That was expanded, until now one-third of our business is in the diagnostics division. Diagnostics direction was a marked development and very successful. And that spreads out over many fields.

BOHNING: Was it Tabern who first developed radioisotopes? Was he the one responsible?

VOLWILER: Yes.

BOHNING: Let me go back and look at what Dr. Abbott did with Summers in Pennsylvania and how he kept that phenacetin project going for some time before he realized there would not be any development out of that. How much were you willing to support development and keep it going before you would say, "This isn't going to work," and then stop.

VOLWILER: I don't know. It depends on the field and the product as to whether or not it is a dead horse. It depends. I don't know how to answer that.

BOHNING: As a person who came through the laboratory, what was your approach to research, and its fundamental relationship to the development of the company?

VOLWILER: I don't think that it was particularly different for the men who didn't come up through the scientific ladder.

BOHNING: You also continued what your predecessors did in terms of employee relations.

VOLWILER: Employee relations have been very good right along.

BOHNING: I understand Dr. Abbott was very good, even in the early days, of taking care of employees and their problems.

VOLWILER: Yes, he knew every employee. He knew something about them personally. Abbott now has the Clara Abbott Foundation, which is an organization to give help when needed to employees and their families. Abbott also gives scholarships to children of Abbott employees.

BOHNING: In a time when many companies find themselves cutting back on research or having new products developed by acquisition of smaller firms rather than internal research, what is the situation in the pharmaceutical industry? Is that happening there also?

VOLWILER: Not to the extent that it is happening in many other fields. I think the pharmaceutical industry has been relatively modest in the amount of absorption of other firms. That can change anytime. Dow has done this, but not as much as they'd like. Du Pont has also, but not as much as they would like.

Monsanto has to a greater extent. But I think it's operating pretty well.

BOHNING: What comments do you have about developments today versus when you started in the drug industry? What are the current trends as opposed to what you've seen in your career?

VOLWILER: The current trends are on a vastly larger base. The whole picture has changed a great deal. I don't know how to make a worthwhile comparison.

BOHNING: Well, let me ask you this. In your days, certain areas were targeted, such as sedatives, hypnotics, and anesthetics. Are those specific areas still being targeted and the developments following that, or is it the other way around? Are people looking at new compounds and then seeing what they might be useful for?

VOLWILER: Well, the latter is not as important in my mind as it used to be, where you target the field. I think that the synthesis of products for use as new drugs is just as important as a generation ago.

BOHNING: What would you say would be the future for a person starting out today as you did in 1918 with Abbott? What does a new Ph.D. from Illinois have for him when he comes to Abbott?

VOLWILER: Well, there's quite a few fields that he can consider there. His possibilities are very good, but he's going to have to convince his employer that he has a feel for the direction that can lead to good new products. It used to be that I could go into a company and say, "Look I'm an organic chemist, hire me." If he comes in that way, he could work in quite a number of lines in the synthesis of organic compounds. Now, he's more likely to find a place in a specific field, a very highly specialized field.

BOHNING: In today's business climate, is there still a possibility for a Ph.D. scientist starting at Abbott to become president thirty years from now?

VOLWILER: Oh, I don't see why not. It takes some sense of business understanding. I don't know why he cannot use his basic knowledge, his basic ability with some business training, possibly an M.B.A., which is available now. I think that he should have about the same chances that a man has who comes up through the business field and not through the science field.

BOHNING: I don't have any more specific questions.

VOLWILER: I'm afraid that I didn't always answer your questions very well.

BOHNING: I understand that you're not feeling well, and I appreciate the time that you did take to spend with me. It's been very fascinating and I've enjoyed it very much. Thank you.

[END OF TAPE, SIDE 5]

## NOTES

1. Ernest H. Volwiler, "Recent Contributions of Chemistry to Medicine," Industrial and Engineering Chemistry, 15 (1923): 906-910.

2. "SCI Honors Volwiler," Chemical and Engineering News, 32 (1954): 4302.
3. See, for example, E. H. Volwiler and D. L. Tabern, "5,5-Substituted Barbituric Acids," Journal of the American Chemical Society, 52 (1930): 1676-1679. For a complete list of the publications of E. H. Volwiler see the research folder in The Beckman Center Oral History Program file #0050.
4. Ernest H. Volwiler and Arthur C. Cope, "Chemical Abstracts: Millstone or Milestone?" Chemical and Engineering News, 33 (1955): 2636-2639

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