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ALFRED R. BADER

Transcript of an Interview
Conducted by

Arnold Thackray

in

Milwaukee, Wisconsin

on

31 July 1987

Alfred R. Bader JH 3/15/96

CENTER FOR HISTORY OF CHEMISTRY

Oral History Program

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

ALFRED R. BADER

1924 Born in Vienna, Austria on 28 April

Education

1939-1940	Brighton Technical College
1945	B.Sc., engineering chemistry, Queen's University
1946	B.A., history, Queen's University
1947	M.S., chemistry, Queen's University
1949	Ph.D., organic chemistry, Harvard University

Professional Experience

1943-1947	Chemist, Murphy Paint Company
1950-1954	Research Chemist and Organic Group Leader, Pittsburgh Plate Glass Company
1951	Co-founder, Aldrich Chemical Company
1954-1955	Chief Chemist, Aldrich Chemical Company
1955-1975	President, Aldrich Chemical Company
1975-1980	President, Sigma-Aldrich Corporation
1980-	Chairman, Sigma-Aldrich Corporation

Honors

1980	Honorary Sc.D. degree, University of Wisconsin-Milwaukee
1983	Engineer-of-the-Year Award, Engineers and Scientists of Milwaukee, Inc.
1984	Honorary Sc.D. degree, University of Wisconsin-Madison
1984	Honorary Sc.D. degree, Purdue University
1986	Honorary LL.D. degree, Queen's University

ABSTRACT

Alfred Bader begins this interview with a discussion of his early years in Vienna, including his family background and Gymnasium education. This is followed by additional education in England, living in Canada as a refugee, and his undergraduate education at Queen's University. Bader then describes his graduate education with Louis F. Fieser at Harvard, and discusses the faculty and friends during his tenure there. The interview continues with Bader's move to Milwaukee, his research with PPG, and the origin and growth of the Aldrich Chemical Company, including the merger with Sigma Chemical Company and the decision to go public. The interview concludes with Bader's comments on his art collection and family matters.

INTERVIEWER

Arnold Thackray majored in the physical sciences before turning to the history of science, receiving a Ph.D. from Cambridge University in 1966. He has held appointments at Oxford, Cambridge, Harvard, the Institute for Advanced Study, the Center for Advanced Study in the Behavioral Sciences, and the Hebrew University of Jerusalem. In 1983 he received the Dexter Award from the American Chemical Society for outstanding contributions to the history of chemistry. He is Director of the Beckman Center for the History of Chemistry.

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INTERVIEWER: Arnold Thackray

LOCATION: Milwaukee, Wisconsin

DATE: 31 July 1987

THACKRAY: Dr. Bader, could you start by telling us about your family background?

BADER: I was born in Vienna on April 28, 1924. My father died when I was two weeks old. My mother came from a Hungarian family. Her father had been a Hungarian count. My father had an only sister who was a childless widow, and she adopted me when my father died. She had been very fond of my father and so it was she who brought me up. I hardly knew my real mother. I met her a few times but I didn't know her well. My mother by adoption, whom I really think of as my mother, was Gisela Reich. Her husband, who had died in 1922, had been a very well-to-do man. He had built a company with the now incongruous name of "Russian-American Oil Company" which was the main supplier of petroleum products to the Austrian government during the First World War. He unfortunately died in 1922 at the very beginning of this terrible inflation in Austria, and my mother Gisela knew nothing whatsoever of his business. He died a multimillionaire and in the years between his death in 1922 and 1938, she was reduced to absolute poverty. She had no one to rely on, watching her capital gradually disappear during the inflation, and then disappearing altogether.

My father and my "mother" Gisela were Jews. Her father had been a very able civil engineer, who had gone from his home town, a small town in Moravia in Czechoslovakia, to study civil engineering in Paris at the Ecole des Ponts et Chaussées, the best engineering school in Europe at the time. He had become friends with Ferdinand de Lesseps (the man who built the Suez Canal) and he went with de Lesseps to the Suez Canal. My grandfather was in charge of the Ismailia section of the canal. He was knighted by Franz Josef at the time of the opening of the Suez Canal in 1870. My mother Gisela was exceedingly proud of her father's achievements. Moritz Ritter von Bader meant a great deal to her for she truly was an Austrian patriot. When the Nazis marched into Austria, she had no thought whatsoever of leaving Austria. She was Austrian and she was going to stay there. The Nazis sent her to Theresienstadt [concentration camp] where she died in November of 1942.

I was brought up by my mother Gisela. It was a home entirely of women. We had a woman who was hired as a nurse for me. She was the cook and bottle washer, becoming a good friend of the family and staying with us long after Gisela was able to pay her. Also with us was the sister of my grandmother Bader. She was very much older. She also died in Theresienstadt. Thus, I was brought up by three ladies. In retrospect, none of them knew anything about bringing up children, but they loved me and I had a very kind, warm home. When it became clear to me from the time that I was nine or ten, I was bothered by the fact that the financial affairs of my mother went down terribly. Little money was left because in the early 1930s she had invested in mortgages and on coffeehouses where the interest rates were high. But the coffeehouses went bankrupt. By the time I was fourteen, I was trying to help her by buying and selling postage stamps, and using the money I earned to buy food.

My real mother occasionally would invite me to come and visit her and she would give me 35 Austrian groschen to pay for the tram ticket. I would walk the five miles to where she lived to save the 35 groschen, not really thinking how much the shoe leather cost! In a way it was both a happy time and an unhappy time. I went to a school--a gymnasium--from age ten to fourteen. (It was the same gymnasium that Carl Djerassi went to but he was one class ahead of me.) In that class I had three very good friends. We were together most every afternoon. The school ran from eight in the morning till one in the afternoon. After school, we went to the park to play soccer. The four of us did almost everything together, until we were split up, of course, in 1938.

THACKRAY: Do you have brothers and sisters?

BADER: I have one sister whom I hardly know. She was brought up a Catholic by my [real] mother. She survived the war in Vienna, as did my mother. After the war, she fell in love with an English sergeant who was in the army of occupation. She followed him to England and they were married in 1949. She was exceedingly happily married, living in Burton-upon-Trent in Staffordshire. Her husband died about three years ago (he was some twenty years older than my sister), and my sister still lives in Burton-upon-Trent, speaking a very funny mixture of Midlands accent and Viennese.

THACKRAY: To go back to your school days in Vienna, can you talk about the curriculum there?

BADER: It was a Realgymnasium, which means it was a high school with some emphasis on science. We had a course in physics. I remember only a very little about chemistry. We had four years of Latin, of which I remember nothing. We had two years of English, so that when I left Vienna, I knew a little bit of English. We had a marvelous German teacher, Professor Sedlmayer, who really instilled in us the importance of speaking and writing correctly. I've recently read a book describing the life in Vienna and the book refers specifically to that Professor Sedlmayer (1). If you passed his courses, you could pass at the university; he was that good a teacher. It has been very, very helpful to me because throughout my life I've written a great many letters in German and I'm certain that my written German is absolutely perfect because of Professor Sedlmayer's teaching. The man was an inspiring teacher. This book that I referred to, mentions that he and most of the professors at the school were avowed Nazis. Somebody asked him after the war about this, and he said, "Yes, it just didn't work out the way we'd hoped it would--Austria becoming part of Germany." What struck me as very significant when I left Vienna and came to England, was the totally different attitude of the students towards authority in the two countries.

I entered a school in Hove, in Sussex, the East Hove Senior School for Boys, which was the lowest kind of school that any student could go to. The kids left school at fourteen and went out to work. If they were lucky, they got 25 shillings a week. The boys were just as wild or just as tame as the boys in Vienna, except that they had a totally different attitude about honesty. In Vienna if you played soccer and you broke somebody's window, or did something that wasn't right, nobody would own up to it. A boy would be considered crazy to own up to anything. Cheating in the examinations was well-organized. Everybody cheated. Only one student did his homework. We took turns doing the work and then everybody else copied it. In the examinations everyone cheated. I came to Hove and the kids didn't cheat. If they broke a window, they knew they would be caned, yet they owned up to it. The morality in Vienna was strange--cheating was all right, stealing was completely out, hurting people was out. You didn't steal. But the government was the enemy, so you didn't pay taxes. You'd certainly play soccer where it wasn't allowed, and then run away from the policeman. There was organized cheating in class, something which was unthought of in the East Hove School for Boys.

THACKRAY: In Vienna, when you were twelve or thirteen, did you have any thought as to what you would do when you grew up?

BADER: I had no idea, none whatever. I had to leave school because I was a Jew. I remember that on July 26, 1938 we celebrated the end of school by what we called a "kneeball" match among the ablest "kneeballers" in class. From July until

December, I did nothing other than run around and buy and sell postage stamps. The entire Jewish community was trying to learn a trade or skill or something to earn a living. There were classes on how to make caps and classes on how to make ties, and classes on how to do anything to earn a living in whatever country you might be able to go to. I remember around April 1938 my real mother coming to Gisela saying, "Look, let's cut out all this nonsense. After all, Bobby (I was called Bobby until I was fourteen, and Alfred after that) is my son. What is all this nonsense about being a Jew? Why don't you let me take care of him and he will be safe here." Gisela absolutely refused. But she heard some time that summer that there might be transports to England of children between the ages of ten and sixteen with no family outside of Austria. She enrolled me in that scheme and on December 10, 1938, I left Vienna with many hundreds of other kids. We were taken via Germany and Holland to Harwich, England. We landed there on December 12 and were taken to a holiday camp near Lowestoft, and then shuttled from place to place until homes were found for us in England.

I had no interest in science, no thought of science. I knew I wasn't at all mechanical. I knew an enormous amount about stamps. I had started stamp-collecting when I was six, and knew enough to make a few hundred shillings during those months in 1938 buying and selling stamps.

THACKRAY: You'd been studying English at school?

BADER: There was a class in English. None of us studied or did any work. We didn't work hard. I could bring you my certificate of leaving which shows I was very average, even below average.

THACKRAY: When you came to England, when and how did you get to the Hove school?

BADER: My mother Gisela had remembered that we had a very distant cousin, a Mrs. Emanuel, living in Hove, England. She wrote to Mrs. Emanuel saying, "My son is coming to England in this children's transport. Is there something you can do to help?" Mrs. Emanuel was a very kind and very able lady who helped hundreds of refugees who had come into England. She went to a very aged and well-to-do lady, Sarah Wolff, who was living on Grand Avenue in Hove, and asked her whether she would support me financially. Mrs. Wolff agreed to pay one guinea a week for my upkeep. I went to live with a family in Hove where the husband had a tiny shop selling printing. His income was about 50 shillings a week. He supplemented that income by taking in boarders. Mrs. Wolff paid 52 guineas a year to have me live there with the Scharffs. Mr. and Mrs. Scharff had two children themselves (two girls older than I) and they took in three

refugee kids, two from Germany and myself. They sent us then to the East Hove Senior School for Boys.

THACKRAY: Was it possible to keep in touch with anyone who came from Vienna at the same time you did? Did you know any of those kids?

BADER: No.

THACKRAY: So you were really on your own?

BADER: I really knew none of the kids, and we were moved so much. I got the measles and was taken to the Lowestoft Isolation Hospital for a week. Before that we spent a week at the St. Felixstowe School in Suffolk. Then we spent two or three weeks at a boarding house in Westcliff near Southend-on-Sea. One day a number of us were told they had found families to look after us. That morning we were taken to a house in London. I remember the day because it was a very frightening experience. Thirty or forty of us were brought to a very large room in Bloomsbury Square. Bloomsbury House was an organization for refugees. Every ten or twenty minutes somebody came to pick up one of us, and none of us knew who would pick up whom. Of course, we were all anxious, wondering what kind of life was ahead of us. I was the very last boy to be picked up. From nine that morning until 5:30, one by one the kids would be called. At 5:30 a frightening-looking man with a bristling mustache came in and said, "Come here boy!" I remember that I had a miserable cold and he took me on a bus. We were on top of the bus and he took me to Victoria Station to take the Brighton train to Hove. Mrs. Scharff, who was an immensely kind lady, fed me tea and cookies.

I learned something about English life that has stayed with me ever since. My mother had often said to me, "Whenever somebody offers you something, the first time you must say, `no'. No matter what happens, no matter how much you like it, you must say, `no'". Mrs. Scharff asked me, "Do you take sugar in your tea?" By reflex I said, "No." Now, I loved sugar in my tea, but for the next year and a half I was never given sugar because it had been determined that I drank tea without sugar. Occasionally I would pinch some sugar and sneak it into my tea as it was so much better with sugar.

Mrs. Scharff was a very kind lady and I had my own room in their house. It was a tiny room. Lying in bed I could touch all four walls, but it was my own room. I walked to school every morning. School was easy. It was really a lead-pipe cinch to do well in that school. The kids didn't try hard, even in French. We had a French teacher who must have been in her forties or fifties. She seemed ancient to me. Now I had taken French lessons from a teacher when I was four or five years old. (My mother still had a good deal of money). In those days French was considered the language of the world. Her father had been to Paris, and French was an important language. This lady had taught me French for only a few months but it was sufficient for me to do very well in French at the East Hove Senior School for Boys. [laughter] The accent of the kids was horrible. I could do well in everything else. I gave you my learning certificate The headmaster wrote: "This boy would benefit from any kind of secondary education."

THACKRAY: Yes, I have a copy here [see following page]. You make it sound very easy but it could not have been easy studying history, geography, and mathematics in a foreign language.

BADER: Well, I learned English quickly. All of us spoke English constantly--we had to. At first, the kids looked at me strangely but they treated me nicely. Within a couple of months I got a badge saying "prefect" which meant a lot to me.

I was buying and selling stamps. What bothered me most was that I had no pocket money. That weekly guinea didn't include pocket money. Things changed when I went to the Brighton Technical College. It was fairly far away, (maybe three miles). Mrs. Scharff thought that I shouldn't come home, or that I couldn't come home for lunch, so she gave me nine pence for my lunch money. She didn't know that Lyons sold beans with ketchup on toast for 5 pence, so I saved 4 pence a day, which, of course, was a lot to me. I still have my post-office savings book showing the shilling or two that I deposited every week. I tried to earn some pocket money by selling stamps to the kids. It got me into trouble because a stamp dealer complained to the authorities that I was competing. He said I was a fourteen- or fifteen-year-old stamp dealer and wasn't allowed to take any employment because I was a refugee. I was supposed to come up before a magistrate but luckily, or unluckily, I was put into an internment camp just before that day. I took counsel with Mrs. Emanuel about this. She said, "This doesn't sound terribly serious to me. On what kind of scale were you selling stamps?" I said, "Well, you know, I would buy a job lot of stamps and split them up. My profit may be a shilling. Here's my post-office savings book. You can see." By that time my savings had gone up to five pounds.

The other two boys from the house were supported by very well-to-do executives from Lyons, who gave them the unbelievable sum of two shillings and six pence a week. Every time they got it, they showed it to me to rub it in. Anyway, I never came before a magistrate.

[END OF TAPE, SIDE 1]

THACKRAY: How did you get to Canada?

BADER: I left the Hove school in the summer of 1939, just before the War. I think it must have been Mrs. Emanuel who kept an eye on me, who decided it would be good if I could go to Brighton Technical College. I forget just how the financing was done, but I got a grant to go to there. I took the bus everyday. There was some science, some engineering, and some machine shop work. I was happy studying. It was a different class of kids, much more advanced than the East Hove Senior School for Boys. I also went to a Sunday school which I enjoyed very much. Everything was going well.

THACKRAY: What degree were you studying for?

BADER: This was for the general engineering degree. It was the first year of a three or four year course. Then, on Sunday May 12, 1940, Churchill decided that there was danger from the refugees. (There were stories of refugees in Holland and Belgium turning out to be spies, and there were thousands of Jewish refugees in Britain.) He [Churchill] decided to intern everybody between the ages of sixteen and sixty or sixty-five. I remember that Sunday morning distinctly because it was the first time I had asked a girl for a date. Sometime during the break, at 11:00 or at 11:10, some plainclothes detectives came to this Sunday school in the Middle Street Synagogue in Brighton and picked me up. We were taken to the Brighton race course where we spent the day and night. Then we were taken to an internment camp near Liverpool, in a place called Huyton. From there we were taken to an internment camp in Ramsey on the Isle of Man. Then early in July, on July 9, we were transferred back to the mainland where there were a number of boats. The British decided to ship everybody who had no family in England, overseas. There were five boats. Two were sent to Canada, and two were sent to Australia. One of the boats, the Arandora Star, was sunk by a Nazi U-boat [sometime after] July 2 because the Nazis knew we weren't Nazis. In the internment camp in Huyton there had been some Nazis, but not many. The Jews very much wanted to get out of the country because they were scared of an invasion. The real Nazis very much wanted to stay in England because that's where the action would be.

Anyway, I was put on a boat, the S.S. Sobieski, and we were taken to Canada. We didn't know where we would be going. Nobody told us. When we entered the St. Lawrence River, we realized that here was an enormous river, and we had geographic geniuses with us who finally figured out that this must be Canada. We landed in Quebec City on July 15 of that summer. We were terribly disappointed because the British government had completely failed to tell the Canadians that we were Jews and in all probability harmless. I knew nobody on the boat. The journey was slow and scary because halfway through our engine failed. The convoy left us behind, so we were a sitting duck. Fortunately, we were able to fix whatever needed fixing. When we landed in Canada, there were Canadians jeering at us and throwing Coke bottles, thinking that we were Nazis.

On the boat I was befriended by some in a very interesting group of 272 people, all of whom had been in a refugee camp called the Kitchener Camp in Kent. They were young, and almost all German Jews. Many of them had been living in farming areas in central Germany, and some had been taken out of Dachau, the concentration camp. They had all lived together and had been taken together from the Kitchener Camp to the internment camp. I made friends with this group of people, but I was by far the youngest. They were all between twenty and forty. They said to me, "Look, nobody's going to know where you came from, why don't you go with us? We'll have a good time together. We're all very practical fellows, come with us."

It so happened that 273 was a good number to fit into one particular internment camp in Canada. It was Fort Lennox, on Ile-au-noix in the Richelieu River near the American border. This fort had been built by the British for use against the Americans before the War of 1812. It seemed to us that nobody had been in that fort since 1812 because when we got there, the dust must have been several inches high and we disturbed many bats that were just flying around. The preparations had been very elementary. For instance, there were no toilets, just twenty-gallon buckets. That night we found out that one of them leaked. We were heavily guarded with barbed wire and machine guns, and we were questioned the next week. The commandant, Major Kippen, questioned me particularly closely because I was the youngest. He said, "Gee, it's amazing that a boy of sixteen would parachute down over England!" I said, "I didn't parachute down over England. I'm a Jew, I'm a refugee!" And he said, "Well, I don't believe you for a minute, but we don't like Jews, either!"

Gradually, over the next few weeks, the camp commandant began to have doubts about whether we were Jews or not. After I was released from the internment camp, I remember seeing a letter which he sent to the Jewish community in Montreal saying, "We have here a group of young men who allege to be Jews. We don't believe them. Could you please send one of your clergy to this camp to prove or disprove these allegations?" And the Jewish community sent a rabbi, an ancient gentleman by the name of Rabbi Hirsch Cohen, and it took him minutes to realize that

of course we were Jews. There was no pretense here. He sat down in the compound terribly shaken by the fact that here were 273 Jews that the community knew nothing about.

THACKRAY: Did that lead to an amelioration in status for you?

BADER: It took a long time. The Canadian immigration people, particularly one man, Blair, who's become infamous for what he did, were absolutely adamant in not wanting any Jews coming into Canada. It took a great deal of effort. Nobody was released from the camp for the first year.

I was one of the first to be released, but only by a fluke. We had no newspapers but the guards, of course, were reading papers. Occasionally papers would be thrown away or the wind would blow them in or whatever. We'd gone into the camp terribly worried that the Nazis might win and if the Nazis won, they would know that we were not Nazis. We guarded these papers and took turns reading them. One day I came across the obituary in the Montreal Gazette of a woman in Montreal that said, "Mrs. Wolff is survived by her husband and six daughters and a mother-in-law in Hove." And here was a reference to this ancient Mrs. Wolff, who had provided the guinea. I remember that when I visited her, she would tell me about her granddaughters in Canada. While I was in Hove, I couldn't have cared less about her granddaughters in Canada. I only remembered that there were a slew of them. But here clearly was her son, the survivor, the husband of Mrs. Wolff's daughter-in-law. I wrote to him but the Canadian censors didn't let any letters go through. Thus, the first letter didn't go through. Then, the Jewish community sent in social workers to talk to us. One social worker, a Mrs. Robinson, came in and when she talked to me she asked, "Do you know anybody in Canada who might help you and provide sponsorship?" I said, "Well, these are the facts--a lady who took care of me in Hove has a son who lives in Montreal." She asked, "Who is the son?" I replied, "Martin Wolff." Then she said, "Oh, Martin Wolff is my neighbor!" So Mrs. Robinson told Martin Wolff about me and in time he got me out of the internment camp. That was in November of 1941. I was in the camp for about eighteen months.

THACKRAY: How did you and the group manage to spend that time in camp ?

BADER: There were two ways. Many of the fellows in the camp knew a great deal about farming. They had lived in farming communities. Many of them, oddly, had been dealers in animals in the area of upper and lower Franconia. The Canadian government provided work--farming, woodcutting, making camouflage nets--and paid us twenty cents a day if we worked eight hours a day. Among these 273 people were a number of very able professionals and they set up a camp school. The principal of the camp school was an engineer. He had been the chief

engineer of Siemens in Berlin. There were people teaching physics, chemistry, and so on. The International Student Service, the ISS, provided textbooks. And McGill University agreed to allow us first to take their junior matriculation, and then the senior matriculation. We studied very hard with those books. The teachers were very good. There was nothing to distract us. (Not a girl in sight!) So, we worked very hard. I took the McGill junior matriculation in June and the senior matriculation in September. Normally, I would not have taken it for another year if I had been outside the camp.

All of us did very well indeed. I remember seeing an article in the Montreal Gazette saying there were enormously large numbers, more than thirty students, who got over 800 in the matriculation, and these numbers were so high because over half of them happen to be in this internment camp. The only subject in which we didn't do well was German because the ISS hadn't sent us books. None of us got good grades in German because many of the questions were phrased, "What did Uncle Fritz do?" None of us had read the book, so we didn't know. We could all speculate and say, "He might have done this" or "He might have done that." We wrote in our best German, but whoever was marking it was a pedant. While we all passed, none of us got top marks in German.

We had a very good chemistry teacher and I did very well in chemistry, both in the junior and the senior matriculation.

THACKRAY: That was your first real exposure to chemistry?

BADER: The man who taught me later became a professor of biochemistry at Johns Hopkins. All of us had such an incentive to work. This was a combination of really good teachers (far better than anything I had known--except for Professor Sedlmayer in Vienna) and very intense work. I did very well indeed. When I was released in November, I applied to McGill. I was living with Mr. Wolff in Westmount, a suburb of Montreal. McGill would not accept me because they said November was too late in the term, and I would have no hope of passing. In actual fact, they had a set quota for Jews and this was already full. People have good reasons and real reasons and these are not always one and the same. Mr. Wolff's daughter, Rosetta, had gone to Queen's, and so it was suggested that I apply to Queen's. I was accepted at Queen's and started on November 15. I remember the scary first day. I had gone to see the registrar and paid my tuition of $250, went over to chemistry to get a locker and so on, and the storekeeper said to me, "My God! You really think you can start here in the middle of November and pass? Have you paid your tuition yet?" I said, "Yes." He said, "Go back! They're robbers! They just want that money! Go back and get your tuition back and go home! You don't have any chance of passing."

Well, I did pass. In fact, that first year I got a scholarship in engineering. It was the first of many scholarships, and amounted to $30. The course that I took was in engineering chemistry.

THACKRAY: Why did you decide on that field of study?

BADER: There were three things. My grandfather had been an engineer. My mother had always drilled into me the importance of engineering. I am totally unmechanical, but there was a family background of engineering. Mr. Martin Wolff was a very able civil engineer. I felt that my whole background wanted me to be an engineer. I liked chemistry by far the best. I had done very well in the two matriculations and wanted to choose chemistry. Here was a course in chemistry and in engineering. To the engineers I could say, "I am in engineering chemistry." To myself I could say, "I am in chemistry."

THACKRAY: The chemistry you had in the camp was presumably entirely theoretical.

BADER: Absolutely. We had no lab. I didn't know what a beaker or test tube looked like. I knew nothing practical. I'd never handled a beaker. I didn't know whether sodium carbonate did this or that. There was no lab work whatsoever, but it had been all theoretical and very much to my liking.

THACKRAY: When you started out at Queen's, where did the tuition come from?

BADER: Mr. Wolff gave me enough money to last for my first year. I paid $3.00 a week for room and breakfast and the other meals were very inexpensive. I skipped lunch on Friday because a very nice Jewish family invited me regularly every Friday evening for sabbath dinner. Expenses weren't very high. Mr. Wolff was not a well-to-do man. His mother had been very well-to-do but her money was in property around St. Paul's. This was all bombed out during the war and it wasn't until well after the war that Mr. Wolff got his inheritance. Unfortunately, he died a few months later. His inheritance was over $100,000, which made an enormous difference, as he earned a very low salary as a civil engineer for the city of Westmount, and he supported five daughters.

THACKRAY: I want to make sure I've got the chronology right. What year did you start at Queen's?

BADER: I started school on November 15, 1941. (I had been in the internment camp in Canada from July, 1940 until November,1941.) In the summer of 1942 I got a job in Montreal with RCA Victor checking and soldering radios. That paid 45 cents an hour. I saved that money because I didn't have to pay room and board. That, combined with the scholarship, allowed me to pay for the second year. I got a number of scholarships that year. In the second summer I had the very good fortune of finding a wonderful summer job with the Murphy Paint Company in Montreal. They paid me the princely sum of $130 a month. It was no longer hourly wages. I loved the work! It was the formulation of paints and lacquers and they invited me to spend the next summer there at $160 a month. When I graduated they offered me a full-time job at $250 a month. I remember the employment counselor at Queen's calling me in and saying, "Alfred, what is going on here? All the other engineers have starting salaries of $160 or $170 a month. You have the highest starting salary of anyone--$250 a month. How come?" I said, "Well, I was so happy, and I worked so hard at the Murphy Paint Company that they wanted me back. That's why they offered me that salary." A year and a half later the president of that company gave me the $1800 that I told you about.

THACKRAY: Let's stay a minute with your undergraduate career. You graduated in 1944?

BADER: 1945.

THACKRAY: And this was in engineering chemistry?

BADER: It was a Bachelor of Science in engineering chemistry. In the engineering department there were a number of sections, such as chemistry, chemical engineering, electrical engineering, mechanical engineering, and engineering physics. The first year all the courses were the same, you had no choice. The second year it was split into two sections. Engineering chemistry, chemical engineering, mineralogy, and geology were in one half. Mechanical engineering, electrical engineering, and physics were in the other half. In the third and fourth year, you concentrated and did a lot of chemistry. We had two years of organic chemistry, two years of physical chemistry, a year of colloid chemistry, and a year of electrochemisty. There was a lot of chemistry and the teachers were good teachers. I got the medal in chemistry in May 1945 and then went to work. I was also very interested in other subjects and I decided, after looking at what is needed for the Bachelor of Arts degree, that I'd taken so many courses that I could get credit in art if I took a few history courses. So I took those by correspondence and in the summer of 1946 got my B.A. Then I had that $1800 from the Murphy Paint Company and decided to come back to

Queen's to strengthen my chemistry, get a master's degree, and then see where I could go for my Ph.D.

THACKRAY: I take it from everything you're saying, that as an undergraduate at Queen's you discovered that the practical side of chemistry was also interesting?

BADER: Not really. As an undergraduate I was not particularly keen on the lab work. I was not particularly good in the lab.

THACKRAY: But you made a great hit at the Murphy Paint Company.

BADER: I was so interested. It was a very interesting situation because there were a number of salesmen who were all on a very small salary and a commission basis. They would take me to customers who described what they needed, and I'd go back to the lab and formulate it. Then we'd have a sale. I would do it quickly. I enjoyed the entrepreneurial aspects of this. As a result, I got very good. I think I could still formulate nitrocellulose lacquers and all sorts of varnishes. But it's a very practical kind of "cookbook" chemistry. What I liked was the commercial part. Sales really went up at the company right after the war because of starting materials. I graduated in May 1945 when the war in Europe ended. Suddenly, starting materials became available. Vinyl finishes became available, and I was involved in formulating them and making the sales. Mr. Thorpe completely (and wrongly) thought that if I got my Ph.D. I would be an even better paint chemist than if I just had my bachelor's. That of course was a great mistake. [laughter]

I didn't really begin to enjoy lab work until I worked with Professor [Arthur F.] McKay. In fact, McKay had talked to the head of the department at Queen's about me. McKay was a very garrulous fellow, who repeated to me what Professor Alexander McRae, the chairman of the department, had said to him. "You know these Jews, they're very good at bookwork, but they're not good experimentalists. Be careful about taking Alfred Bader. He was very good in all of his theoretical work. He won the medal in chemistry, but he wasn't much good in the lab." It wasn't until I worked with Art McKay, about whom, as you know, I have very mixed feelings, that I began to enjoy lab work. He was a superb experimentalist and I worked next to him. We had the same lab and he was very interested in what I was doing. He showed me a great deal. It was that year (it took me one year to get my master's) which really made me enjoy lab work. When I then went to Harvard, I knew far more practical chemistry because of that intense year with Art McKay than I learned during my bachelor's. I don't know anybody who worked completely side by side with his professor. McKay was a very good experimentalist. He had just gotten his Ph.D. from the University of Toronto from a very strange man, George F. Wright. Wright was a miserable character who was tremendously demanding. He had been in the merchant navy where he learned his language

and then had gotten his Ph.D. from Henry Gilman. He published many papers with Gilman.

[END OF TAPE, SIDE 2]

BADER: Years later I remember McKay introducing Wright at McGill saying, "You will hear all sorts of stories about Professor Wright. You will hear it said that people are chained to their desks in his labs. That's not true! They are welded to their desks! You'll hear stories that there is a bed in every lab so that people can lie down when they keel over. It isn't true! They aren't beds. They are cots!" And so on. McKay's hair had turned white while under professor Wright, and he was only a man of about thirty. McKay lived chemistry. He was married to a lady who was a very devout Christian Scientist, and when he would come home drunk she would leave him for a few days. She would leave him with the kids. He got drunk once or twice a month and I would then guide him home. The next day, I would go to his home to wash the dishes and make sure that the kids got to school.

I told you the story of how I almost lost my position. I'd gone home to Montreal to be with the Wolffs for a couple of days at Christmas. When I came back, Art McKay was standing in the door to our joint labs, saying, "Goddamned bastard! You are out of here!" That was exactly his language. "You are a fraud! You are a crook! You will never be able to do research anywhere in the world. You've had it." And I said, "Well my God, what have I done?" And he said, "Well, I've had occasion to repeat one of your experiments and I've gotten a totally different compound." And I said, "Well, look, you had all the compounds analyzed." "Oh, you must have scrounged around and found some isomeric compounds." I said, "Well, gee. That's a lot harder work than making the compound." The reactions where very straight forward. He said, "Well, I don't believe you. I got totally different compounds." And I said, "Well, please let me repeat the work." Finally I persuaded him. And, while he was watching me very carefully, I repeated the work. It was a condensation of nitroguanidine with 1,2-propanediamine. The reaction worked. I got exactly the same compound that I'd gotten the first time. Then, on checking back, it developed that Art McKay had used another bottle labeled 1,2-propanediamine. We made derivatives of that liquid and found that he had 1,3-propanediamine, mislabeled by Kodak. He got pyrimidines when I got imidazolidines. So we had two series of compounds. All's well that ends well. I stayed with Art McKay. I worked on the oxidation of linoleic acid, making all the eight isomeric tetrahydroxystearic acids. It was very difficult experimental chemistry because the compounds are part liquid and part waxy solids. It was a very good experience. I got a paper in which, unfortunately, Art McKay got the stereochemistry all mixed up in writing it (2). Daniel Swern, the famous Philadelphia chemist, then corrected our paper (3). But the

experimental work was correct. As a result of that Dan Swern and I became lifelong friends.

THACKRAY: Was this your first publication?

BADER: That was my first publication. In my undergraduate work, each student in chemistry had a research project. My project was the determination of ultraviolet spectra of some natural products. This was with Norman Jones, a very able professor at Queen's who had gotten his Ph.D. from Manchester. He was a postdoc with Louis Fieser before coming to Queen's as professor. Shortly after I got my Bachelor's, Norman Jones went to the National Research Council in Ottawa as their chief spectroscopist and stayed there until recently. We remain good friends. I visit him and his wife, Magda, whenever I can. Yes. You're holding my first paper on the chemistry of stearic acid. The stereochemistry is wrong, but the experimental chemistry is correct.

THACKRAY: Now, your master's degree there took one year.

BADER: One year.

THACKRAY: And then you went back again to the paint company?

BADER: Well, just for the summer. I had applied to two schools. I looked around to see which were the best in the world. It seemed to me there was either Harvard, where I'd heard a good deal about Louis Fieser from Norman Jones, or at the ETH [Eidgenössische Technische Hochschule] in Zürich where I also realized that there were some tremendously able chemists such as [Leopold] Ruzicka and [Vladimir] Prelog. I applied to both the ETH and Harvard, and I was accepted by both.

THACKRAY: You really had decided that synthetic organic chemistry was the thing to do?

BADER: Oh, absolutely. That's right. Particularly, that Master's degree had shown me the joy of making compounds. My first two or three months of work was on nitroguanidine condensations, and then McKay put me on this linoleic acid problem. The nitroguanidine was related to the RDX explosive work which was a continuation of McKay's work with George Wright. There I got to make the compound which Aldrich would later sell as its first compound, 1-methyl-3-nitro-1-nitrosoguanidine. I decided then that organic chemistry was for me. The question was, should I go to Zürich or should I go to Cambridge?

THACKRAY: What about Queen's?

BADER: No. Queen's had no Ph.D. program; that came much later. So there was no question of going to Queen's. A master's was the highest you could get at Queen's. I remember going to see the chairman of the department, Professor Alexander McRae, who said, "Well, I consider one much better than the other, but I won't tell you which!" This wasn't very helpful. The decision was made financially. Fieser wrote to me saying that I'd been accepted and that if I worked with him, I could have the Abbott fellowship which was $1,000 a year. Further, he said I could have a quarter-time teaching fellowship which paid $100 a month. That was $1000 plus $1200 plus the $1800 which I still had from the Murphy Paint Company. At Queen's, McKay had gotten me $100 a month for a research fellowship, so I hadn't touched that $1800. I had princely sums at my disposal! Living certainly wasn't expensive. ETH said I had to provide my own funds. While I did have the $1800, that extra $2200 made a lot of difference. So I came to Cambridge in September of 1947, after I got my master's in June of 1947.

THACKRAY: Did any of the people going into the Ph.D. program with you have master's degrees?

BADER: Some did and some didn't. With or without a Master's, it could then take anywhere from two years to four or five years to get your Ph.D. There was one fellow at Harvard, by the name of Vlasios Georgian, who is now at the Tufts Medical School. He became [Robert B.] Woodward's good personal friend and liked his work with Woodward so much that he didn't want his Ph.D. at all! He could have gotten it any time he wanted to as all the work was done. In his ninth year, he met a very pretty Mexican girl. She was intent on getting her Ph.D., and she insisted that he get his Ph.D! It was very flexible. The academic requirements at Harvard then, and I think now, were very light. You had to pass four entrance exams. Some people passed them immediately and some people had to take them over and over again. I passed all four. I think they were in organic and physical and inorganic and analytical chemistry. And that was that. There were some course requirements which were very simple--a course in mechanisms from Paul Bartlett, a course in atomic chemistry from Professor [George S.] Forbes. There were cumulative examinations which were given every month. You had to pass eight. There were eight every year. Then you had to do your research thesis. In my case that was the problem given to me by Louis Fieser.

THACKRAY: Were you explicitly admitted to study with Fieser or did that choice seem to develop a little later?

BADER: Well, that was certainly implied. I have only known of one person able to switch. A fellow by the name of [Mayer Bear] Goren, who is now in Denver, started with Fieser but didn't get along with him and decided to change to Woodward. That was very, very unusual.

THACKRAY: Did you choose Fieser because he was the known name in synthetic organic chemistry?

BADER: Well, there may have been an incidental fact that I should mention. I was active in a number of university organizations at Queen's and became, among others, the president of the Hillel Foundation, which is a Jewish student organization. The rabbi who was in charge of the Hillel house mentioned to me that he had heard there was a professor at Harvard, Louis Fieser, who was a very good friend of Chaim Weizmann. Weizmann had been a lecturer at Manchester and became the first president of Israel. This rabbi said he would write to Professor Fieser, highly recommending me to him. Fieser then wrote to me and said, "If you will come and be my graduate student you can have the Abbott fellowship which is under my control." I didn't know of Woodward. I didn't know of Gilbert Stork. I didn't know of Paul Bartlett. If I could live my life over again I would work with Gilbert Stork.

THACKRAY: Can you contrast the atmosphere at Harvard with that at Queen's? What was the life of a graduate student like during that period?

BADER: There were enormous differences. First of all, I saw nothing of Louis Fieser. Art McKay was beside me from eight o'clock in the morning until the evening. We did everything together. We went out and had a bite to eat. He worked and I worked. We worked five feet apart and he showed me how to do a great many things. Louis Fieser invited me to come to his office one morning, and he said, "Now I've made an interesting observation. This particular quinone, 2-hydroxy-3-cyclohexyl-1,4-napthoquinone, when dissolved in alkali turns deep red like every hydroxyquinone. Leave that alkali solution overnight and it turns yellow. So clearly there's a rearrangement going on. Figure out what it is." So my first job was to make a goodly quantity of that quinone, put it into alkali, and then get it out of alkali again and see what happens. I worked on that, but Fieser paid very little attention to me. The first few weeks he visited me and said, "How are you doing making the quinone?" It was a five- or six-step preparation but very straightforward. All of the compounds were beautifully crystalline colored materials. It was day and night from the difficulties of making hydroxystearic acids which are waxy and difficult to purify. These were relatively high melting, easily purified materials. I made a goodly quantity, maybe a kilo of that quinone to study

it. Then I figured out what was involved. It bore on a very interesting reaction.

Many years before a man by the name of Samuel C. Hooker had worked on a quinone from a very out-of-the-way Brazilian wood called Bethabarra wood. He isolated a naphthoquinone called lapachol and had studied its reactions (4). He then entered industry and became president or chairman of the American Sugar Refining Company. When he retired in the 1930s, he had two hobbies--chemistry, which he hadn't practiced for over thirty years, and magic. He was a very tall man, over six feet. He built a magic theater in New York. He had also become very wealthy in the sugar company, so he built his own lab. He went back to the chemistry of lapachol and found that between the 1890s and the 1930s, nobody had worked on lapachol. Lapachol and all related hydroxyquinones undergo a very curious rearrangement. If you take, say, 2-hydroxy-3-octadecyl-1,4-naphthoquinone and you treat it with permanganate, you get another quinone with one CH2 less. That is, it goes from C18 to C17. Then if you do that once again with permanganate, you get to C16. That way you can bring it down to methyl. That's known as the Hooker Oxidation (5). So what is the mechanism of the Hooker Oxidation? Well, it turned out that what you got in this rearrangement that Fieser had given me as a problem was an intermediate in the Hooker Oxidation. You have ring opening and ring closing via an indanone or benzocyclopentanone. So I developed the mechanism of the Hooker Oxidation and got all the intermediates. My paper with Louis Fieser was on the mechanism of the Hooker Oxidation (5). Incidentally, I also figured out a very interesting way of making ortho-naphthoquinones which is described in that paper.

After about a year and a half, in the spring of 1949, Fieser came into my lab and said, "Alfred, incidentally, how are you making out on that problem." I said, "Well, gee, I think I've solved it. It is such and such." He said, "Well, give a seminar." After the seminar, Fieser said, "This is very nice indeed. Write it up so that we can have a paper." But of course this is totally different from the work style of... [McKay]. I didn't really miss Fieser. There were so many able people that one could talk to. Also I had so many things going on in the lab. For instance, I taught everyone how to make diazomethane from the 1-methyl-3-nitro-1-nitrosoguanidine [MNNG], which later became the first compound of Aldrich. We had set up a permanent diazomethane still in the lab, and we had a big bottle of MNNG. I told Fieser about it and he said, "Good. Have every student in Chemistry 20, the elementary organic chemistry course, make a batch of MNNG. Then combine all the batches and crystallize it once from methanol to get out the junk." We had an annual supply of MNNG for the entire chemistry department. Everybody knew if you had to use diazomethane, that you went to Alfred's lab in Converse 205. There was the still, a bottle of KOH, a bottle of MNNG, everything that was needed to make diazomethane.

I was a great believer in seed crystals. That was something McKay had taught me. I had what we called Bader's black box. I'd simply gone into the subbasement of Converse and gotten samples of many dozens of different solids. I brought them up, had them at the bottom of this box, and I invited anyone with syrups that wouldn't crystallize to set them in, with their name on the beaker. Whether it worked because of the seed crystals or not, how can you tell? But many things crystallized in that box. I've always loved lab work. One of the highest compliments that Fieser ever paid me, was when he said, "Alfred you could crystallize a hamburger!" [laughter] I loved crystallizing things. This came from my work with the tetrahydroxystearic acids made by the oxidation of linoleic acids. These compounds are not easy to crystallize.

There were very good chemists around. I discussed my problem on the Hooker Oxidation much more with Bob Woodward than I did with Fieser. Fieser was traveling. He was consulting here and there. He was running around the world. I hardly saw him. And I had heard horror stories about Mary Fieser being very hard on graduate students. I kept away from her as much as I could. I hardly saw her. I was scared that she would find fault with the English of my thesis, but there wasn't a peep out of her. Everything was fine. So my contact with the Fiesers was very slight. In a way, I was grateful because as I told you last night, I was very unhappy about Fieser not allowing me to go home and see my mother who was terminally ill. There were so many other good people one could talk to. My two best friends were Leon Mandell, who is now dean in Florida and Eugene van Tamelen, who went on to Stanford and became a very brilliant chemist. Both of them were Stork students. We would quit most evenings at ten or eleven o'clock and go to Albiani's which was the cafeteria at Harvard Square. We'd have a glass of milk and a bun and talk chemistry. They had interesting problems and I had interesting problems, so we shared them. There was a great deal of give and take.

I failed my first eight cumulative examinations during the first year because I didn't pay enough attention. I had a female lab partner and that was very distracting. Her parents took her out of school at the end of that year. They were worried that she might be getting serious about me or vice versa. The next year I had very nice married fellow, John Tinker. He and his wife Mary sort of adopted me and invited me to their home. Suddenly I passed all of the next eight cumulative exams. Unfortunately, because of the lady sitting here, [Bader's wife Isabel] I decided I wanted to get my Ph.D. as quickly as possible. We met that summer. I went to Europe in 1949 and I got my Ph.D. formally in March of 1950, but my work was finished the autumn of 1949. I went to Pittsburgh Plate Glass [PPG] which had bought the Murphy Paint Company.

THACKRAY: You went back there in the autumn of 1950?

BADER: In the winter of 1949-1950. I finished my work at Harvard in December of 1949 and took Christmas off to go back to Montreal. Then we spent a couple of days with Isabel's parents in Kirkland Lake. I started at Pittsburgh Plate Glass in January of 1950. Now, I'd asked Mr. Thorpe, the president of the Murphy Paint Company, whether I could take a job other than a job at Pittsburgh Plate Glass. He said, "No. You know, I gave you that $1800 on the condition that you would come back to us." (Now the Murphy Paint Company had since been bought by PPG.) "They tell me that they will not do any research whatever in Montreal. Our paint research laboratories are in Milwaukee and therefore you should go to Milwaukee."

THACKRAY: Those years when you were at Harvard were the years in which Woodward was very much coming into his own. Can you talk a little bit about his style and his work?

BADER: Well, we saw a great deal more of Woodward than we saw of Fieser. Fieser gave no courses for graduate students. He gave the elementary organic course to undergraduates and was an immensely popular lecturer. Fieser also worked in the lab and he was a very fine experimentalist. He loved to have races on how quickly one could make a given dye. He would have a competition with students in the undergraduate chemistry course. I was a teaching fellow in this chemistry course and so I realized how very much the undergraduates appreciated Louis Fieser. I had no course from him at all. Bob Woodward gave a course in natural product chemistry, dealing largely with quinine and related compounds. He and Bill Doering had just synthesized quinine. Woodward was a meticulous lecturer. I mean, he loved to begin at the upper left-hand corner of a blackboard, and by the time the blackboard was filled, it was perfect. I still have the notes from that course. He was a wonderfully clear lecturer. Gilbert Stork had just started as assistant professor. He didn't get tenured. (When I think of the idiocy of not giving tenure to Gilbert Stork....) Stork went on to Columbia and built up the Department of Chemistry at Columbia to the point, where, today, I think Columbia is the best department of chemistry, certainly organic chemistry, in the country. Bartlett gave Chemistry 5, which was reaction mechanisms. It was an eye opener to me. I knew very little about reaction mechanisms from my Queen's days. He was a very clear lecturer. Bartlett had a number of students on the third floor of Converse, and they very much kept to themselves. They were very theoretical and interested in reaction mechanisms. I was much more practical. I liked the kind of chemistry that Fieser did. It was very practical, very down to earth. Woodward, of course, was very much into syntheses of natural products. If I had stayed in academic life I think I would have been much more interested in the structure determination and isolation of natural products.

But, there was such an easy give and take. There were so many people willing to help. Some of these were the people on my own floor, the second floor, where I had a lab just across the hall from Louis Fieser's office. Next to me was Bernhard Witkop, who had come over from Germany and then became a very big wheel at the National Institutes of Health. He was a very able chemist. There was Jacob Szmuszkovicz and we became very good friends. Jake went to Columbia. At Harvard he was a postdoc with Louis Fieser. He had gotten his Ph.D. in Israel and treated me very, very kindly. His lab partner was Ed [Edward J.] Modest who then did very fine work in cancer chemotherapy in Boston.

[END OF TAPE, SIDE 3]

BADER: In the lab next to me was Morris Kupchan, who was an assistant to Louis Fieser. We didn't think much of Morris Kupchan, but he turned out to be a very brilliant isolator of natural products that were of great interest in cancer chemotherapy. There was Martin Ettlinger, a member of the Society of Fellows, who had gotten his Ph.D. from Louis Fieser on quinone chemistry. Martin was a very curious bird who helped me enormously and incidentally, was accidentally instrumental in my becoming for a time the sole owner of Aldrich. Martin Ettlinger was one of the most brilliant men I've ever known. Some years later I bet Ettlinger's father (who was professor of mathematics in Austin, and whose mother was professor of biochemistry) a hundred dollars that within twenty years Martin would have the Nobel Prize. I lost the bet. Martin is brilliant, but I didn't realize that he suffered terribly from depression. When he got a professorship at the Rice Institute in Houston, he didn't get tenure. While he is wonderfully able, he just hasn't produced a great deal of work. But Martin was there at Harvard and knew all about quinone chemistry. He was very helpful. I must have often asked him what he considered really stupid questions, because, rather than answer, he would just walk away!

Towards the end of my stay at Harvard, we decided to tackle a problem together. When Mary Fieser was a student of Louis Fieser at Bryn Mawr, she worked on the reaction of 1,4-naphthoquinone with diphenyldiazomethane and had come out with a compound to which she had assigned a totally impossible structure (7). In reading over that paper, I realized that it was impossible and asked Martin whether he would care to join me in figuring out what really happened. We determined that if you did the reaction you got the compound exactly as described by Mary Peters Fieser. Then if you chromatographed that compound you saw that it was really two compounds. We determined the structure of the two compounds. I remember, just a couple of years ago, I was invited to give a lecture at the Technion in Haifa, and one of the professors introduced me there by saying, "Alfred Bader was the most undiplomatic of all people I've ever known." After the lecture I asked him what he meant. He said, "Well, I remember your giving a seminar at Harvard just before you got your Ph.D. You talked about Mary Peters' work and there

were Mary and Louis in the audience, gnashing their teeth because you corrected her master's thesis. This is undiplomatic." It also led to the very best paper, chemically, that I ever published, with Martin Ettlinger (8). I remember now writing up the manuscript and sending it to Martin. (I finished the work while at PPG.) I started out saying, "In the elegant work of Fieser and Peters, they describe such and such". Martin sent the manuscript back to me saying, "Oh hell, to be elegant at least it should be correct!" And we changed it from "elegant" to "interesting". [laughter] Anyway, there were a great many people helping, many of whom, like Gene van Tamelen, went on to brilliant academic careers.

THACKRAY: You might very well have headed that direction if you'd have been a free agent.

BADER: Well, I would never have been brilliant, especially when I compared myself to chemists on my own student level. For instance, there is no question that Gene van Tamelen or Martin Ettlinger were abler chemists than I. I was a good experimentalist, better than most. But I couldn't compare with the ablest of the students. I wondered whether I shouldn't try and do something in the fine chemical business. The thought occurred to me even then.

I had to make a number of naphthoquinones to check the generality of this rearrangement that I referred to. I had to make 2-isopropyl-3-hydroxynaphthoquinone and needed ortho-isopropylphenol. It was listed in the Kodak catalog and I ordered it. I ordered 500 grams for a few dollars, and it didn't come. (This was in February or March of 1949.) It was the last compound I needed for my Ph.D. thesis. I went down to the stockroom manager, Warren Stockwood (who is now the lab manager at Harvard), and said, "Warren, what can I do?" He gave me a piece of chemistry department stationery and said, "Well, why don't you write to Kodak and see what happens." I wrote to them saying, "Look, I need only this compound to finish my Ph.D. thesis. It's number so and so." I got a form postcard, which I wish I'd kept. I would frame it. It was a form postcard saying, "We have your order number so and so, please do not inquire again. You are just adding to our paperwork. When we have it, we will supply it."

Much later I found out that Kodak didn't make it. They bought it from Dow, who made it once a year. Then, when Kodak had it, they shipped it. I said to myself, if that is the way the fine chemical business is run in the United States, maybe I have a place in it. I then ended up making it myself from isopropanol and phenol. From this you get a mixture of ortho and para isomers which can be separated because they boil fifteen degrees apart. So once I knew that it would be delayed, I just made it myself and that was it.

THACKRAY: Going back to the work context, you were morally obligated with PPG and went back there at the start of 1950.

BADER: Well, I'd been offered an instructorship at the University of New Brunswick that intrigued me a good deal. I would have liked to accept it, so I wrote to Mr. Thorpe, the former president of the Murphy Paint Company, explaining this. He said, "No. We want you." The director of research at PPG, a man by the name of Dr. William Lycan, interviewed me. He actually asked me to do some work for PPG while at Harvard. When I finished my Ph.D. work, I was just marking time, so I worked on this Fieser-Peters problem, and I worked on the oxidation of sitosterol to the C17 ketone for PPG. Lycan offered me the job, but before I joined PPG, he had accepted the position of Director of Research at Johnson and Johnson. So I never got to see him at PPG. He was succeeded at PPG by Dr. Howard Gerhart, an able chemist who I believe had gotten his Ph.D. from Northwestern under Charles Hurd.

I was very disappointed when I joined PPG. Nothing was said about my salary when I was offered the job. When I got my first salary check it turned out to be $400 a month, which was very much less than what any Ph.D. from Harvard was earning. When I chatted about this with Dr. Gerhart, he said, "Well, that's what we pay and we can get all sorts of Ph.D.s around here for $400 a month." (They happened not to be Harvard Ph.D.s.) My boss and his boss had each gotten their Ph.D. in oil chemistry from the University of Wisconsin, but it certainly wasn't sophisticated, up to date chemistry. It had all to do with iodine numbers and saponification of all sorts of fats and oils. They were very far from any kind of chemistry. These people at PPG really had no idea what to do with me. I was foisted on them. In fact, one of the men working in my lab confided in me. He said, "You know, it's amazing that you are here. We have eight hundred people here in Milwaukee working for PPG and it's an absolute rule: we don't hire blacks and we don't hire Jews. I don't know how we got you, but here you are." I'm sure this was true, even though Milwaukee was a large metropolitan city with a large black community.

The labs were the crummiest labs imaginable. We were in a temporary building put up in 1917 during the First World War. Everything was wooden construction. There were no safety measures of any kind. We were on the second floor and there was a big box with a rope. In case of fire you would throw the rope out of the window and find your way down. I had very little equipment. On the other hand, they left me alone. They told me of a very odd observation. They'd been interested in countercurrent extraction. They'd tried to extract such steroids as sitosterols, and tocopherols from soybean oil by countercurrent extraction. They found that when they used methyl acetoacetate they got odd compounds which they couldn't identify. They said, "Why not look at this." I discovered that you could take any beta-keto ester, such as methyl acetoacetate, ethyl acetoacetate, or cyanoacetates, or malonates, and transesterify them without any catalysts. This had been

unknown. It's a very interesting reaction. Many years later, Gilbert Stork said to me, "Probably chemically the most significant thing that you found was that noncatalytic transesterification." Well, that was the first couple of papers (9). I put my bosses' names on the papers so there would be no problem with permission to publish.

[END OF TAPE, SIDE 4]

BADER: Well, it seemed to me that what the paint industry needed were new monomers that could be easily and cheaply made. I thought this through very deliberately. The people at PPG worked extensively with dicyclopentadiene which came from natural gas and cost eleven cents a pound. It is easily cracked to cyclopentadiene. They were doing a fair amount of work with phenols. I wondered whether it wouldn't be possible to take phenol and cyclopentadiene, or butadiene or isoprene--any of the cheap dienes--and akylate phenol to give alkenylphenols. From them you could then make phenolic resins which would have a double bond in the side chain for further polymerization. When I looked into the literature, I found that it had never been done. It always gave tars.

I thought it might be worthwhile to do a systematic study of the reaction. So I started with cyclopentadiene rather than butadiene because it's a liquid. I found that if you picked your catalysts carefully, it could be done. You could get a good yield of ortho- and para-cyclopentenylphenols. If you reactedphenol with butadiene you could get a good yield of ortho- and para-butenylphenols. The same is true with isoprene. It's a general reaction. Furthermore, I discovered that practically any Friedel-Crafts catalyst works, if correctly diluted. If it is slightly too strong, you get tars. If it is slightly too weak you get nothing. It had not been reported before. Furthermore, if you used the Hammett indicators, any catalyst of exactly the same strength would work. Then having these unsaturated phenols, it also seemed interesting to isomerize them to the conjugated unsaturated phenols. Out of that came a series of papers on unsaturated phenols (10). It was very practical work. PPG made a number of phenolic resins that were used in highly resistant can coatings. A lot of patents came out of that work (11).

I then went on to other practical work. One day, the salesman from Quaker Oats stopped by in Milwaukee. The day before he had been at Johnson Wax in Racine and the following day would be at 3M in Minneapolis. He told each group, in Racine, Milwaukee, and Minneapolis, that levulinic acid would become commercially available from furfural. I said, "Well, levulinic acid is a nice keto acid, why not make the bisphenol?" The next day I ordered some levulinic acid. The day that I got the levulinic acid, I made the bisphenol. I still have the

dollar check for that patent (12) [see following page]. I published a note in the Journal of the American Chemical Society [JACS] on what became known as diphenolic acid, the reaction product of phenol and levulinic acid (13). One day, some months later, Howard Gerhart called me to his office and said, "You know, the people at Johnson Wax would like to buy our patent on diphenolic acid. What do you think I should charge them?" I said, "Well, Howard, it was all of two-days' work. I've already gotten a note in JACS out of it. If we got $10,000, we'd be overpaid. But look, they want it badly, and they have a lot of money. Why don't you ask them one million dollars and see what happens?" And they entered into a contract to pay PPG just that over a period of years. They built a plant to make six million pounds a year of this diphenolic acid and had full-color, two-page spreads in Chemical & Engineering News on this compound. It struck me as crazy because Johnson Wax was basic neither in levulinic acid nor in phenol. Eventually they gave up the production completely and sold it to Emery Industries.

3M fought the patent like tigers, as if it were a Scotch tape patent. They lost because the fact was I had good notes and had made it two weeks before the man in Racine and four weeks before the man at 3M. It was a "motherhood thing" but commercially so interesting. From then on, life at Milwaukee became quite different. A whole team of patent attorneys descended to look at my notebooks to make sure that nothing had been overlooked that might be patentable. Those four years at PPG were very productive. There were a lot of papers, and a lot of interesting chemistry.

I became very good friends with Michael Carroll, a chemist in England. He discovered that diketene reacts with acetone, but he didn't know the structure of the product. He gave me the bottle and I took it home and determined the structure. I knew Marshall Gates, the editor of JACS, very well, since he was also a Fieser student. I sent him a "Communication to the Editor." He wrote back to me and said, "Alfred, this seems very unlikely. After all, Union Carbide has been making diketene from acetone for many years. If this were correct, they would know about it." And I said, "Well, here it is. Here are the facts (14)." In fact, it became product number two of Aldrich. We sold all of 100 grams for $10. Nobody else bought it and we discontinued it. Many years later, when diketene became too dangerous to ship, it became the substitute of choice. Today, Kodak and Wacker and Lonza make many tons. It is a case of an idea whose time hadn't come.

THACKRAY: From the sound of it, if PPG had stayed in Milwaukee, you might have stayed with PPG.

BADER: That's right. I was happy in my work. The work with Aldrich was really very much of a side line. It was sort of a game. I enjoyed it, but there wasn't very much in it.

THACKRAY: PPG must have been reassessing you, since they didn't quite know what to do with this person with the Harvard Ph.D.

BADER: Well, I became a very good friend of Charles Hurd, a professor at Northwestern. We still meet once or twice a year, and just recently he told me that when PPG decided in 1954 to move to Pittsburgh Howard Gerhart told him [Hurd] about it. Charles Hurd said, "Well, aren't you going to lose most of your chemists?" And Gerhart said, "Well, I don't really care if we lose all of them as long as Alfred Bader comes with us."

However, I'd gotten to like Milwaukee very much. I liked the community. I had a job teaching Sunday School. I just liked the city. I'd been to Pittsburgh a number of times. In those days it wasn't what it is today. It was really quite a miserable city, and I just didn't want to move. This meant I had a choice to make. By that time Aldrich was selling $15,000 a year with a few hours a week of my time. If I joined it full time, what could one do with that? Neither Jack Eisendrath nor I had any money. But we both knew that Bill Kesselman, the father of one of my students in Sunday school, was quite a wealthy guy. We talked to him and he agreed to put in, over a period of twenty months, $25,000 for one-third of the company. He was to put in $5,000 immediately. In those four years at PPG I had gotten very good raises and my salary had doubled. Now my salary was to drop from $800 a month to $500, and I had to guarantee between the summer and winter of that year not to cash the payroll checks to conserve funds. We rented a laboratory on the east side and hired George Skeff, who is still at Aldrich. (You might like to meet him. He is a very good experimentalist and just a handy fellow.) We paid him $250 a month, which was an increase over what he was getting at PPG. He didn't want to go to Pittsburgh. We also hired a full time secretary.

Bill Kesselman was to put in this $5,000 up front and then $1,000 a month for the next twenty months. We wrote into an agreement that was hammered out in many, many hours of discussion between my partner Jack Eisendrath, myself, Bill Kesselman, and Bill Kesselman's lawyer, his brother-in-law from Chicago. There was a clause in there, that if at any time Bill was unhappy for any reason, he could give us notice, return his third of the stock and get whatever money he had put in back over the next two years. After seven months, Bill came to me and said, "Alfred, I am unhappy. I would like clause so and so to take effect." I said to him, "Bill, why are you unhappy?" He said, "Well look, there are two reasons. One is, you've been growing nicely. Every month sales do go up. You've seen the sales chart. But this company is never going to be worth $75,000 to justify the $25,000 investment in a third.

Furthermore, I didn't like what you did with Martin Ettlinger." What happened with Martin Ettlinger? Martin had helped us a great deal. He'd helped me with publications and suggestions and all sorts of things. One day Martin had visited us and without asking the board of directors, I paid him a consulting fee of $100. Bill felt I shouldn't have. The last board of directors meeting before the breakup had only one action, and that was to allow me to buy a second hand desk for a price not to exceed $35. Control was that close. I didn't mind. I was careful anyway. But Martin Ettlinger had helped us so much that it seemed "motherhood" to me.

Bill pulled out, and at that point, I owned half and Jack Eisendrath had half. Jack had a law firm in town. I was working fourteen hours a day on this. So the fifty-fifty arrangement didn't strike me as right. I went Harry Kovenock, an attorney friend of ours. He was really a lawyer's lawyer, a very scholarly man. And I said to Harry, "Look, this doesn't strike me as fair. Jack and I each put in $250. I gave up my job and am working full time. Jack worked very hard the first year. He did all the paperwork, all the office work and so on. I don't want to hurt him, but it doesn't strike me as right. Can you think of some alternatives?"

Harry came up with three alternatives. "One, you sell your half to Jack for $3,000 and then take the $3,000 and start the Bader Chemical Company. Or, offer Jack $10,000 for his half and then you have all. Or, offer to buy from Jack twenty percent so you have seventy percent and he has thirty percent, and pay him $6,000 for the twenty percent." Jack was absolutely furious. He said, "None of these are any good. I didn't do all this work to get a few thousand dollars. This is going to be a great company. I don't like any of these."

THACKRAY: He had a lot of faith in you.

BADER: Yes. Anyway, I said, "All right. I will walk out. Do what you like. We each own half. I'll walk out." After some negotiations, Jack wouldn't talk to me. He hasn't talked to me since. He accepted $15,000 for his half, to be paid over a period of a few years at three percent interest. At that point I owed Jack 15,000, and I owed Bill Kesselman $12,000 (five and seven).

Just at that point, something very lucky happened. A month or two before, we had gotten a request from Du Pont to quote on 500 pounds of suberic acid. Well, suberic acid is easily made. 1,6-Hexanediol was available from Union Carbide at fifty cents a pound. You brominate it to make the dibromo, react it with sodium cyanide to make the dicyano, and hydrolyze it to suberic acid. I had no idea how to cost it. I felt it would be easily made and I pulled a sum out of a hat. Thirty-eight dollars a pound for the 500 pounds. Within weeks after Bill walked out,

we had the order for 500 pounds of suberic acid to supply by December. The price was $19,000. We made some, and I asked my friend Max Gergel at Columbia Organic to make some. It was elementary. The chemistry was so straightforward, and the cost was so little. We filled the order and we had the money to repay Bill Kesselman.

Actually, I paid Jack Eisendrath the last installment way ahead of time. He was so angry. Marvin Klitsner delivered the check. (Marvin Klitsner became my best friend, lawyer, and Secretary of the company.) He said that Jack said, "Well, I wonder what's in Alfred's mind that he's repaying? There must be something going on here." I just hated owing money to anybody, and that was all.

THACKRAY: That Du Pont request had reached you because Aldrich was becoming known.

BADER: Sure. We had our first advertisements. People asked us to quote. Of course, I looked at those quotations very carefully.

THACKRAY: To go back for a minute to 1951, you had plenty going on at PPG. Why did you do this thing on the side?

BADER: Well, I was interested in making chemicals. I thought there was a market for fine chemicals. We come into a very touchy area--permission from PPG. One day I asked Howard Gerhart, "Would you let me do this within PPG and come up with a price list?" He said, "No. Your business is making monomers. You're doing a fine job. Keep doing what you're doing." I said very lightly, without any detail, "Well, what would you say if I made some chemicals on the side by myself?" I don't think he considered it carefully. He said, "Well, you know, what you do on your own time is your business." I then was very careful not to tell the people at PPG what I was doing, for I think they would have stopped me. A principle in my life has been, never ask a question if you can't take "no" for an answer. I was careful not to tell the people at PPG, but I simply took the loose answer of Howard's as a blanket permission. In fact, I turned out a great deal of work for PPG. They were very satisfied.

A number of the chemists said, "What are you trying to do here? Show us up? Why do you work so hard?" I loved the work. It was also very easy to plug in a few things that one could buy here or there and put them into Aldrich. I mean, the beginning of Aldrich, except for MNNG, was largely bought materials,

compounds that I would purchase here or there and put into the Aldrich catalog. We rented a garage on Farwell, (on the east side here) for $25 a month, and used that as a store place. Jack Eisendrath's office had a tiny side room, not as big as a quarter of this, with a sink. We put up a bridge table which filled the room and it was there I bottled the chemicals that we had bought. Gradually this got to be too crowded. The first two investments we made were, $3 for this slide rule, and $10 for a white box, a storage cupboard, which we moved to the garage. Then we could store more at the garage. A girl would type the labels. I would put the labels onto the bottles, pack them up, and take them to the post office. Everything was mailed.

THACKRAY: The MNNG came out of your work with McKay.

BADER: That's correct. McKay was the first to make it, and he almost died doing it! He thought that the gas coming off was nitrogen, and it was diazomethane. He ended up in the hospital. It developed into an very elegant reagent for diazomethane because it's the only reagent that gives diazomethane with aqueous alkali. All the other compounds require alcoholic alkali, and when you distill over the ethereal solution of diazomethane you distill some alcohol with it. This had no alcohol. It's a great advantage. Many years later it was found to be the most carcinogenic compound there is. Today it is used as a carcinogenic standard. I wonder what the mortality tables of the students in Chemistry 20 are that made these batches in 1948 and 1949.

THACKRAY: Was that one compound an important ingredient in the sales in the early years?

BADER: Well, it was the only compound in the first year. We sold $1,705 worth. Then I added a few other compounds.

THACKRAY: That was all you had in the first year.

BADER: That's all we had. That was flier number one. Then came flier number two, which we called catalog number two. There were twelve items, and sales went to $5,400 a year.

THACKRAY: What were the other eleven items?

BADER: They were compounds that I knew how to make. For instance, there was the diketene acetone adduct. I got it from Michael Carroll in England and put into the catalog, but it didn't sell.

THACKRAY: Were these other eleven compounds basically coming out of Europe?

BADER: Well, for instance, Pittsburgh Plate Glass in Barberton, Ohio, was selling butadiene epoxide at fifty cents a pound. It is a very interesting compound. We got a gallon and put it into the catalog. We bought it from Barberton.

THACKRAY: So this was really a combination of your own knowledge about what were interesting compounds and what might be available?

BADER: Sure. The butadiene epoxide is an exceedingly interesting compound. Today it costs hundreds of dollars a kilo. PPG gave up making it. One might be a millionaire if one could have bought a few tons from PPG in Barberton. Then, they offered it to anyone who wanted it at fifty cents a pound. I thought it was a damn good compound, but few people knew about it. I knew about it. They advertised it once in a little ad. So we bought a gallon. I wish I had bought more than a gallon.

THACKRAY: How did you distribute the flier and catalog two?

BADER: Well, we sat at home and compiled a mailing list of about two thousand chemists who were the senior authors in the Journal of Organic Chemistry, and then the organic papers in JACS. We mailed the flier and then got orders. The first order came from Parke-Davis. We got many dozens of orders for MNNG. We contracted with a small company in Milwaukee to make it. I contracted with Art McKay to make it. As a thank you to Art McKay, we sold him a share of Aldrich stock at $5 a share. You know what happened to that.

Gradually, we added more compounds. I got to know Michael Carroll in England pretty well. He was the chief chemist of A. Boake Roberts, Ltd., a company specializing in perfumery chemicals. They sold us a number of compounds. In 1952, I visited Fluka, and they sold us a number of compounds. It got to be a buying and selling operation with my doing the bottling and the labeling. It was very much a part time thing.

THACKRAY: What were your sales in 1954?

BADER: They were $15,000. And in the next year they went to $39,000. That was full time. In no year did we lose any money. The first year we made $20. Each year was profitable, but of course, not wildly profitable. However, I had another job. At the beginning of my full time employment at Aldrich, I had a job as principal of an afternoon school. That paid me, I think, $300 a month. So I had that, and I had $500 from Aldrich.

THACKRAY: This was a religious afternoon school.

BADER: Yes. It was a Hebrew school.

THACKRAY: How long did you continue that?

BADER: Two years. Then I felt that I needed the time at Aldrich much more. It would be much more productive if I didn't have to quit at three o'clock, and could work from three until the evening. The school was very close to Aldrich. I didn't know how to drive in those days. I could walk.

THACKRAY: You've covered an extraordinary amount of territory here, both literally, geographically, intellectually, and organizationally.

BADER: Well, you don't look at it that way in the midst of it. You know, this is hindsight. I enjoyed what I was doing. It was good work. I've always enjoyed working with people, working with customers. The people who called Aldrich to get chemicals realized that I wanted to do a good job. It grew very nicely. We then added more people. By 1957, that lab which we had rented in 1954 was much too small. At first, it was one thousand square feet and then the landlord gave us another thousand square feet. We outgrew that. We then bought an old shoe company on the near north side of Milwaukee that had twenty-seven thousand square feet. Within a couple of years, we had outgrown that and bought a very much larger building one block away. It was a large building--130,000 square feet of the Badger Meter Company. Eventually, both buildings were taken by the expressway and we bought the building in which we are now. We've now bought a good many buildings around us.

THACKRAY: What were the Aldrich sales in 1957?

BADER: Several hundred thousand dollars.

THACKRAY: During that period, from leaving PPG in 1954 to going full time with Aldrich in 1956, what kind of competition were you facing?

BADER: Well, as far as I know, I had no competition in that little niche. There was Eastman Kodak. If you look at the Eastman Kodak catalogs of the late 1950s or early 1960s, they didn't change. I don't think they added or deleted many chemicals. They didn't much care. I never added chemicals in the Kodak catalog. I would go to Europe every summer looking for chemicals to buy. I always had the Kodak catalog with me. I didn't need ours. I knew what was in our catalog. If it was in the Kodak catalog, I didn't buy it. If it wasn't in the Kodak catalog I would buy fifty or one hundred dollars worth, one kilo or five kilos, and put it into the Aldrich catalog to see what would happen.

THACKRAY: So you didn't get any real response from Kodak or J. T. Baker because you were in a different niche.

BADER: That's right. In the early 1960s Baker came to us and said, "We've looked very carefully at your business. We know what you're doing. We would like to buy Aldrich. You have two choices. You can sell us Aldrich and we'll pay you. You can take it either in money or in stock." I think it was $1.5 million, a princely sum! "Or, if you say no, we know what you're doing. We will do what you're doing, and we have such a distribution system, you'll simply go out of business." It was polite but very blunt. We were close to accepting it, but after some thinking about it we said, "No." Baker then proceeded to buy thousands of compounds from Fluka, who gave Baker just a modest discount.

[END OF TAPE, SIDE 5]

BADER: Now, at Aldrich we produce thirty-five percent of our entire line. In those days, we made just a few compounds, and Fluka made only a few compounds. Each of us marked up the compounds. If we bought something for a dollar, we would sell it for three or four dollars. Fluka did the same. We had the same sources. Baker necessarily had to be more expensive than Aldrich, and it was a total failure for them. Two or three years later they came to us and said, "We've decided to discontinue this line. It isn't going anywhere. We've got $40,000 in inventory. Would you like to buy it?" And we said, "No, thank you." We didn't want to buy it. So nothing came of that. We kept away from Kodak very religiously until one day we

discovered that we could compete. You know the story of the dicyclohexylcarbodiimide suggested by John Sheehan.

THACKRAY: And so from that date on you were in competition?

BADER: From that date on we said, "Well, we don't have to worry about Kodak." We then deliberately began adding a good many compounds that Kodak had. Kodak today is very, very expensive. In those early days when I visited universities, purchasing agents would say to me, "Ah, you're from Aldrich, the cheap fellows." We were cheaper in every case than Kodak. We don't hear this anymore. For one thing, we are hardly cheap anymore. But Kodak is very, very much more expensive, so they've lost the business.

THACKRAY: What was linked with the decision to go public in 1965?

BADER: Well, there were maybe two hundred chemists and purchasing agents around the country who said, "Look, you're doing such a good job, if you ever go public, we would like to buy stock." Being a very frugal fellow, I went to a small brokerage house in Milwaukee, the Marshall Company, where Mr. Ernst Tag had been the salesman who had sold me stocks. I said to them, "Look, I'd like you to sell stock on a best efforts basis with the lowest commission permissible by law." Under the SEC rules, a $10 stock could have as low a commission as seventeen cents. The usual commission was $1. We knew a number of people would buy the stock, best effort up to 100,000 shares. We had a hard time with the Wisconsin security people because they thought that asking $10 a share for a stock earning twenty-three cents a share was outlandishly high. We pointed out that there would be no sales pressure. It would be purely best effort. So they permitted it.

What we didn't realize was that when people in town called the Marshall Company and talked to any of their salesman saying, "We understand that you are recommending Aldrich", the salesman would say, "Hell no. If you want to buy speculative stock at $10 a share, we have something much better," meaning something with $1 a share commission. So we added all of seven stock holders in Milwaukee, and two hundred from around the country. We had quite a few Eli Lilly people in Indianapolis. The late Jack Mills really promoted our stock and told everybody at Lilly, "You should buy that stock." There were others in Kalamazoo. In Philadelphia, a number of chemists at Smith Kline & French became stockholders.

Then the Marshall Company did something very odd. When the stock sale was closed, the market generally went down very sharply, and they figured this was a disaster. The stock had

sold at $10 a share, so they began selling it short, not realizing that there was no stock. And before you knew it, they had sold 4,000 shares short and they couldn't deliver. Of the first stock sold in that first best effort underwriting, the first year, only one stockholder sold forty-five shares. That was an investment club of Parke-Davis chemists at Ann Arbor which folded. Because it folded, they had to sell the stock. And here these dummies were selling stock they didn't have. They put up the price to ten and a half, eleven, eleven and a half, twelve, thirteen, fourteen. The price appeared only in the Milwaukee Sentinel. Nobody else knew and nobody else was selling. They came to me after two or three months, rather frantic, and said, "Look, we are in deep trouble. We cannot deliver. You are permitted to sell up to one percent of the total stock issued, which was 600,000 shares. So you could sell 6,000 shares every six months without registration. We did everything we could to help you. Will you please now help us and sell us 4,000 share at $14 a share?" I did this, and they got out of this business, but the stock kept going up.

The following year I met a very able stockbroker at Robert W. Baird, which is a much classier house in town. They arranged for a second underwriting of either 100 or 120 thousand shares at over $20 a share. That was a guaranteed effort with the commission being something like a dollar a share. I can get you the prospectus. That sold out and the stock has been going up ever since. But it was that fortuitous beginning which set the stage. Anybody who bought shares at $10 a share, today with all the splits, would have twelve shares for every one and the stock is selling in the forties. So it was not a bad investment.

THACKRAY: Part of the reason for those public offerings was the growth of the company.

BADER: Well, no, it wasn't the company that was selling stock. The company has never sold stock. It was myself. I wanted to diversify. I wanted to give opportunities to our own people. We had stock options for our own executives, and for all the people who had helped us. For instance, I gave Martin Ettlinger a stock option to to buy stock at $10 a share. It worked exceedingly well.

THACKRAY: We'd been talking about the reasons why Aldrich went public, and perhaps we should go on to the establishment of Aldrich-Boranes.

BADER: It's all clearly described in this talk, of which you will get a copy (15).

THACKRAY: Could we talk about the causes and consequences of the merger with Sigma?

BADER: Well, it had seemed to me for many years that Sigma was the best biochemical company around. And it became obvious toward the end of the 1960s that there was far more money for research in biochemistry and biomedical work than there was in pure organic chemistry. So around 1967 I approached Sigma and said, "Would it not make sense to merge?" At that point Sigma was still a family company, two or three families, but not public. They were totally disinterested. Then in 1972 Sigma went public. It became obvious that their figures were very much like our figures. The stock that Sigma sold publicly wasn't sold by Sigma (just as ours hadn't been) but was sold by the families. I approached them again. This time they listened. The merger negotiations were relatively simple. I can give you a document showing how Goldman, Sachs & Co. figured out what the evaluation should be. We valued B-Line, which was a subsidiary of Sigma's, at exactly zero, even though then and particularly now it has become a very valuable company. Sigma was worth twice what Aldrich was worth. We merged on that basis. Dan Broida, who had been one of the founders, and the guiding spirit of Sigma became the chairman. I became the president.

I had great worries that he and I might not get along because we're very different. Yet we did get along quite well--surprisingly well. Unfortunately, Dan died in October 1981 of cancer. He is very, very much missed.

The merger has been very good for our stockholders. It's been very good for our employees. We have learned a good deal from each other. Aldrich learned a great deal from Sigma with regard to service. If Aldrich shipped chemicals the second or third day, we still considered that pretty good. At Sigma, if something was in stock it had to be shipped the same day. Nobody went home until it was shipped.

We learned a good deal in analyses. Biochemicals need careful analysis, much more than organic building blocks. With me, when I was buying a chemical, if I had the choice of buying a given building block at 98% which was yellow rather than white, and could be sold for $6 per 100 grams, or 99.9%, which was white and would sell for $20 for 100 grams, I would always opt for the less expensive. At Sigma, if a compound can be white, it must be white. Many compounds purchased by Sigma are rejected because they are cloudy in solution, or they have hairs floating, or filter paper. The analytical requirements at Sigma are more stringent than at Aldrich, so we learned a good deal.

Sigma learned a good deal from Aldrich. We had, and still have, a very good rapport with hundreds of our suppliers. Many of our suppliers, whom I first visited in the 1950s have become my good personal friends. To Dan Broida, a supplier was a necessary evil. If the compound sold well, Sigma would make it itself and drop the supplier. Sigma was very careful always to tell the customer whether Sigma made it or Sigma purchased it. If PFS (prepared for Sigma) appeared in the Sigma catalog, it was purchased. Sigma makes much more than Aldrich, but they very often had poor rapport, or no rapport at all with their suppliers. Aldrich had a very good rapport. When we merged, that good rapport was an eye-opener to Sigma. Sigma was now able to buy from the original manufacturers (whom Aldrich knew) many compounds which they had been buying from dealers.

We've continued growing at a very satisfactory rate. We've helped each other a great deal. When the compound sells well in the Sigma catalog and is an organic, it automatically enters the Aldrich catalog. If it's a biochemical in the Aldrich catalog, it automatically enters the Sigma catalog. We've had about the same rate of growth, strengthening each other a good deal.

THACKRAY: The making of such a merger is a remarkable story.

BADER: Well, the problems have been on a human level. I have not gotten along particularly well with some of the directors at Sigma. That's been very difficult. Two years ago the three founding families of Sigma decided to sell two-thirds of their stock. Before they did that, they had very much wanted to sell the company. They went to Goldman, Sachs & Co. asking Goldman, Sachs to find a buyer for Sigma-Aldrich. I was very much against it. Management of the company was very much against it. Goldman, Sachs was not successful, but the stock offering by Goldman, Sachs was very successful. It all sold and the stock has kept going up. And so the founding families no longer have quite the same influence that they had before. They are still large stockholders. If you have a look at the proxy statement you'll see how large they are.

THACKRAY: As you look backward and forward to the first beginnings of Aldrich and where you are today, what do you see of things that endure and things that have changed?

BADER: Well, many things have changed. Analyses have changed completely. In the early 1950s a great deal of junk was sold and neither the supplier nor the customer knew it. GC [gas chromatography] has made honest chemists out of all of us. I'm sure if you took any liquid sold by Kodak or Aldrich from the middle 1950s and you took a GC, it would look like a Christmas tree.

The quantities that people work with have changed. When I started the company, if something was commercially available, and it was a few dollars a pound, I would put it into the catalog at 500 grams or one kilo for a few dollars. In the last few years, a great many people have complained and said, "Look, we don't want one kilo. We want 10 grams or 25 grams. We don't know where to store it, we don't know how to dispose of it." I kept asking the same fool question, "Tell me, what would you say when in the catalog you saw 25 grams for $6, and one kilo for $8." And the answer was, "We'll take the 25 grams." Well, I'd feel like a pig on two legs doing that, and so we did a "fiddle". We took all of these common chemicals and we distilled them once or crystallized them once again and put them into the catalog, and called them "Gold Label". People buy 25 grams for $6 from us. We lose money because it costs more than $6 to handle the order. But we satisfy that demand for small quantities. So the quantity has changed. We have a great many chemicals in the catalog now at one gram for $10. I would have been ashamed in the 1950s to list one gram at $10, but people want small quantities. They work on a tiny scale. When new chemists join us, we ask them what they know how to make. They tell us and we say, "Good. Here's a list. Make one kilo of each." And they have a cultural shock. They'd never made one kilo of anything! They've made 100 milligrams. But it's a different situation. So that has changed.

The attitude towards chemistry, of course, has changed completely. Chemistry has become a dirty word. You know the story.

THACKRAY: I know you've spoken very eloquently on that. One of the trademarks of Aldrich has been the paintings on the catalog covers. Your interest in art is very well known. Can you say something about where that began and how it's developed over the years?

BADER: Well, it's an interesting story. I've been collecting paintings since I was a boy. In the mid 1960s, some of our directors suggested that we put an old master painting on the catalog cover to set us apart. I was against it. What place did an old master painting have on a chemical catalog? In those days our board of directors really argued things like that. We had an interesting discussion and the vote was three to two for a painting. We put the Quill Cutter on the cover. It turned out to be a very good thing. Today we no longer say Aldrich on the outside front cover. If it's a good old master painting, people know it's Aldrich. People have identified with it. I get a great many comments. For instance, on the last catalog, we had a painting of a very beautiful, pensive-looking lady in black. I didn't know who painted it, and a number of people wrote to me saying, "We can tell you who painted that." That's an example of art history through chemistry.

THACKRAY: That interest went right back into your childhood?

BADER: Well, I remember buying my first drawing when I was ten, and I bought a number of drawings in my boyhood. I bought my first painting when a student at Harvard. I even spent one dollar in internment camp to have a painter paint me--it was a painting I wanted to send to my mother. Unfortunately, she never received it. Isabel pointed out to me the other day that this was the first painting I really ever bought. One dollar meant five days work at twenty cents a day. [laughter]

THACKRAY: In recent years you have arranged a number of exhibits and catalogs of paintings.

BADER: Sure. For instance, Anna Harrison, the past president of the American Chemical Society, worked with me on some committees of the ACS. When she retired I suggested that Mount Holyoke have an exhibition in her honor, and sent the paintings there. When Herbert Brown got the Nobel Prize, we had an exhibition of Rembrandt school paintings in his honor. Then again, last April, for his seventy-fifth birthday, we had another exhibition of Italian paintings in his honor.

THACKRAY: I'm very much aware of your interest in Dutch paintings.

BADER: Yes. I like old master paintings of all kinds, and I've bought quite a few that are Italian.

THACKRAY: And that is still a very active pursuit.

BADER: Oh, yes. I haven't bought anything since Friday. That one was very nice indeed. I bought it in London and it may well become a catalog cover. It is a very beautiful portrait of a boy age fourteen.

THACKRAY: At the start of the interview you talked about your family origin. Can you say something about your own family?

BADER: Well, you've met my wife Isabel. I met her in 1949. She felt that coming from a religious, Protestant background, she couldn't make a good wife to a man who was a convinced Jew. So we didn't get married. I married a girl I met here in

Milwaukee a couple of years later. She had a very similar background to Isabel's. We were happily married and had two boys. Years later, in the 1970s, Danny, my first wife, realized what Isabel still meant to me. She said, "Look, you know, why hurt each other so very much. Why don't we get a divorce?" It was, I think from a legal point of view, the strangest, easiest divorce two people ever had. Our mutual best friend, Marvin Klitsner, was a lawyer, and he acted as lawyer for each of us. We had no financial arguments, whatsoever. I had always shared everything with Danny. She owned practically as much stock in Sigma-Aldrich as I did. The cost of the divorce was enormous emotionally, but only $150 cash, out of pocket disbursements for Marvin Klitsner, our lawyer. I see Danny often. We talk to each other a good deal. It was a very unhappy situation. It still is. Of course, Isabel and I feel terrible about hurting someone so very much, but those are the facts. The boys of course are very fond of their mother. They've gotten to know and truly like Isabel very well.

THACKRAY: What are your sons doing?

BADER: David, the older one, is an architect in Philadelphia with a firm of architects. He just finished working on a building for Eastman Kodak.

THACKRAY: In Great Valley?

BADER: Yes. My younger son is just finishing a course of business administration at the Rochester Institute of Technology. He's seriously considering accepting a job as the assistant manager of a radio station in upstate New York which I think he would enjoy doing. He is driven by wanting to do things.

THACKRAY: That's a very different line of territory. Your own life, I know, has contained within it enormous amounts of travel in relation to Aldrich and being engaged with the chemical community throughout the Western world.

BADER: Well, we spend a great deal of time walking from lab to lab, talking to chemists and asking what they need and what we can do better. The last two weeks we were in England, where we spent two days in Cambridge, a day in Oxford, a day at King's College, a day at University College, two days at the Imperial College, a day in Sheffield, and a couple of days in Manchester and Salford. We simply walked from lab to lab. I spent maybe two hours with Professor Steven Ley at Imperial College, just talking about what compounds he might need that are not in the catalog, and considering new reagents.

You've seen our advertisements and catalogs, which have much of what professors have suggested. It's a great deal of fun. The first visit is usually strained and doesn't lead to much. On the second visit, people remember you. By the third visit, you're their friends. You get into your routine. I know when I come to Cambridge, Professor Ralph Raphael and his wife, and Isabel and I will go to the same Chinese restaurant and spend the evening talking chemistry and music. At noon I always have lunch with the stockroom manager, who has become a very good friend. He tells me all the latest about our service and about our competitors' service. You get to know people and they open up. You try and figure out what is needed and then you do it. If you want to, you can look at the letters I write home. They are enormous affairs, suggesting that this or that should be done.

THACKRAY: Do you have a file of correspondence that goes back to the origins of Aldrich?

BADER: Oh, absolutely.

THACKRAY: One day that will be a fascinating file in relation to the history of the company.

BADER: Of course, it involves correspondence with some of the greatest chemists of our days. There you see the picture of Barry Sharpless. He's bound to win the Nobel Prize for his work on epoxidation. It's the great invention of this decade.

THACKRAY: You must have seen first hand the movement of the spirit of research between institutions and countries. I wonder if you would be prepared to venture any sort of opinion.

BADER: You mean how it differs from country to country?

THACKRAY: Yes, and how it's gone over the years.

BADER: Well, the most puzzling situation is in England where you have brilliant chemists working in very poor surroundings, often with very little funds, but turning out superb work. You go to the Continent and you'll see marvelous buildings, but the chemists, by and large, are not as competent as the chemists in England. In this country, whatever one may say, there's a great deal of funding and the buildings are fine and equipment is fine. I'm trying to think how one can best help British chemistry to improve. Of course, Isabel lived in Britain for twenty-five years and her heart is there.

THACKRAY: You are back and forth quite a bit.

BADER: That's right. Well, we have a retirement home in Sussex and spend a good deal of time there.

[END OF TAPE, SIDE 6]

NOTES

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11. For example, see Alfred R. Bader and Lowell O. Cummings,"Preparation of Novel Dialkylol Derivatives of Alkenylphenols," U.S. Patent 2,802,882, issued 13 August 1957 (application filed 4 December 1953); and Bader, "Preparation of Terpene Diphenolic Compounds," U.S. Patent 2,811,564, issued 29 October 1957 (application filed 21 September 1955).

12. Alfred R. Bader, "Addition Products of Phenols and Keto Acids and Derivatives of the Same," U.S. Patent 2,933,520, issued 19 April 1960 (application filed 27 August 1953).

13. Alfred R. Bader and Anthony D. Kontowicz,"Gamma,gamma-Bis-(p-hydroxyphenyl)-valeric Acid," Journal of the American Chemical Society, 76 (1954): 4465-4466.

13. Michael F. Carroll and Alfred R. Bader, "The Reaction of Diketene with Ketones," Journal of the American Chemical Society, 74 (1952): 6305.

15. A copy may be found in the Beckman Center Oral History Program File #0074.

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