CHEMICAL HERITAGE FOUNDATION

O. THEODOR BENFEY

Transcript of an Interview
Conducted by

James J. Bohning

at the

Chemical Heritage Foundation

on

24 May 1991 and 5 June 1991

With Subsequent Additions and Corrections

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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OTTO THEODOR BENFEY

1925 Born in Berlin, Germany on 31 October

Education

1945 B.Sc., chemistry, University College, London
1947 Ph.D., chemistry, Univeristy College, London

Professional Experience

1947-1948 London University Post-doctoral Traveling Fellow, Columbia University
Haverford College,
1948 Instructor of Chemistry
1948-1955 Assistant professor of Chemistry
1955-1956 Associate professor of Chemistry
1955-1956 Harvard University, Research Fellow
Earlham College
1956-1973 Associate professor of Chemistry and History of Science; Professor of Chemistry and History of Science
1971-1972 Chairman of Chemistry Department
Guilford College
1973-1988 Dana Professor of Chemistry and History of Science
periodically Chairperson of Chemistry Department
1977-1979 Clerk of Faculty
1985-1986 International Christian University, Mitaka, Tokyo, Japan, Visiting Professor of Chemistry and Research Fellow
Chemical Heritage Foundation
1989- Editor, Beckman Center News; Othmer Library News; Chemical Heritage
University of Pennsylvania
1990- Adjunct Professor, Department of History and Sociology of Science

Honors

1961 Doan Distinguished Teacher Travel Award, Earlham College
1967 Manufacturing Chemists Association Chemistry Teacher Award
1967-1968 Danforth Foundation E. Harris Harbison Award for Distinguished Teaching
1970-1971 Fulbright-Hays Faculty Research and Study Award, Kwansei Gakuin University, Nishinomiya, Japan

ABSTRACT

O. Theodor Benfey begins the interview with a description of his childhood in Germany during the rise of the Third Reich. He tells of his experiences in England, where he was a student during the war, and then his move to the United States for a postdoctoral fellowship at Columbia University. He describes the development of his interest in physical organic chemistry and structure, and the history of chemistry, and recounts his career as a professor of chemistry and history of science at Haverford, Earlham, and Guilford Colleges. Benfey also tells of his parallel career as a writer, translator and editor and gives details of the various translations he has published, and recalls his term as editor of Chemistry magazine. He concludes with his memories of his studies in Japan and China and his current interests.

INTERVIEWER

James J. Bohning, Assistant Director for Oral History at the Chemical Heritage Foundation holds the B.S., M.S., and Ph.D. degrees in chemistry. He was a member of the chemistry faculty at Wilkes University from 1959 until 1990, where he served as chair of the Chemistry Department for sixteen years, and chair of the Earth and Environmental Sciences Department for three years. He was Chair of the Division of the History of Chemistry of the American Chemical Society in 1986, and has been associated with the development and management of the Foundation's oral history program since 1985.

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INTERVIEWER: James J. Bohning

LOCATION: Beckman Center for the History of Chemistry
Philadelphia, Pennsylvania

DATE: 24 May 1991

BOHNING: Ted, I know that you were born on October 31, 1925, in Berlin. Could you tell me something about your father and mother, and your family background?

BENFEY: By the time I was born my father had become the Senatspräsident or chief justice of one of the two supreme courts of the Weimar Republic in Germany. There were two such courts, one for civil cases and one for commercial business cases. He was the head of the second, the supreme court of economic arbitration [Reichswirtschaftsgericht].

My mother [Lotte Maria Fleischmann Benfey] was his second wife. He was about twenty-four years older than my mother. My mother was trained as a children's nurse. She was twenty-two when she married in 1923 and gave up her career. She never got into medical activity again until she was sixty-five and became a part-time ward secretary at Massachusetts General Hospital.

My father had two daughters by his first marriage, Janne (Marianne) [Pentman] and Gerda [Meyer], who were almost my mother's age and became her close friends. Gerda was an interpreter (in French and German) and married the second violinist of the Budapest String Quartet and later conductor Alexander Schneider. She was stranded in Australia during the war and later married an Australian physician. My younger half-sister Janne escaped to Switzerland where she married a Russian chemist, Ilya Pentman, who became owner of a major Swiss and German laboratory supply company, Dr. Bender & Dr. Hobein. The company is now run by their son, Mikhele Pentman.

Of course, that was an awful period in Germany. It was during the height of the inflation. On payday my father would call my mother to bring a baby carriage, because they weren't printing large enough denomination Deutsche mark notes and his salary came in huge wads. They immediately had to buy things, because by noon that day the inflation would have gone up, and it wouldn't be worth anything.

My father's family came from Göttingen. He'd gone to Göttingen University and obtained his doctor of law degree there. He was proud of the Greek he had to know. He would always carry

Homer's Odyssey in his back pocket, even when he was an old man. Whenever he had a minute to spare, he would read it. He could recite the first twenty lines of Homer's Odyssey in Greek and was very proud of it.

BOHNING: What was his name?

BENFEY: Eduard. His parents were active in the Göttingen synagogue. Thanks to two cousins of mine the family has now been traced in great detail. There's one person with my name, Theodor Benfey, in the Encyclopaedia Britannica. He was a Sanskrit scholar who was a key person in pointing out the relations between the folk tales of Germany, Persia, and India. He developed comparative folklore studies. But he went too far, suggesting that all folk tales came from ancient Sanskrit sources, but North American Indian tales and many others do not. He is one of the people the family is proud of.

My grandfather, my father's father, was a banker in Germany. That family had this bank before 1870, so they were active during the time when Göttingen was still part of independent Hanover before it had become part of the German state. Göttingen had a very close relationship with England. My father's mother came from Manchester, England. I've been interested over the last few years in the close connection between the state of Hanover and England because many of my own family are English or migrated to England. In fact, until the accession of Queen Victoria, the Georgian kings were the kings of Great Britain and of the state of Hanover. But the state of Hanover would not take a woman as the chief sovereign.

My mother's family was also of Jewish background, but both my father and mother became members of the German Lutheran state church, and I was baptized as an infant as a Lutheran. My mother's grandfather [Leopold Ullstein] was the founder and head of the Ullstein publishing house. It produced the leading Berlin newspapers and paperback books and clothing patterns, and became well known. My mother's father had a furniture factory. During the Hitler era, when people of Jewish background were being rounded up, my father and grandfather would sleep at the furniture factory. When the Gestapo turned up at my home and knocked at the door and asked where they were, my mother truthfully could say they were not in. Not so truthfully she also told them that she didn't know where they were.

BOHNING: I'm curious about the membership in the Lutheran church, with a Jewish background. Why were your parents members of the Lutheran church?

BENFEY: They were converted. Even the Sanskrit scholar converted to Christianity to become a professor at Göttingen. This was a very active movement in Germany, of Jews becoming assimilated into the German culture, and many of them trying to forget their Jewish background. Intermarriage between Jews and non-Jews became very common. There was a feeling among these people that in another twenty years or so, there would be no Jewish problem in Germany. We'll never know if that would have been the case if it hadn't been for the economic conditions and the chaos that led to Hitler's rise to power.

Chaim Weizmann, whom we may go back to later, had connections with members of my family. He was aghast at the assimilationists, because, having had the eastern European and Russian experience, he saw their hopes were doomed. He was very upset with his reception in Berlin, because there the whole mood was towards forgetting the Jewish background, becoming Christian, and being accepted in high level circles, like Fritz Haber and many others.

BOHNING: As you said, that was a very chaotic period, and I guess you were around ten or so when you went to England. I'd like to explore what you remember of those ten years in Germany. Did you stay and live in Berlin that whole time?

BENFEY: Yes. For me it was an idyllic period. I was too young to realize what was going on. In the summer we would spend several weeks somewhere in the mountains, either in the southern tip of Germany on that side of the Alps, or in Austria. We also had close friends in Switzerland, and I remember visiting there once. We did not go rope climbing, but we made long hikes up the mountains to the snow-covered areas, and threw snowballs in August. The beauty of those resort areas is that there are always huts after a three or four hours hike. You could stop there and drink naturally cool apple cider, without ice, just straight. It was delicious after the strenuous climb. My mother and her father did more significant mountain climbing, multi-day, staying overnight at some of the huts very high up.

In Germany, most of my family and friends sent their children to private schools. My mother chose to put us into the German public schools. I had a very good teacher, Fräulein Giese. The same teacher stayed with you from age six through a four-year period, and I was very happy there. After the fourth grade, I switched over to what we called the high school, but it was really junior high. I spent only a month or two there before going to England.

At the end of my elementary school period I was beginning to notice the antagonism against Jews. At one point it became known that I was planning to leave for England and that I was Jewish. I was followed home by a gang of kids my age, jeering at me, calling out I was a Jew. I remember that one non-Jewish friend in my class went with me and came up to my apartment, and his parents then came to take him home.

I also remember watching the massive parades and watching Hitler, and joining in the excitement. My parents didn't want to tell us too much, so I didn't know what was going on. I remember this iron discipline that was being imposed. There was some event when I was in high school. A line of older boys were standing at attention on the platform. One of them, still standing straight, just keeled over, because he couldn't step out. He couldn't say he wasn't feeling well.

Another frightening thing, which may have had something to do with the mood, was when some boys pushed one boy of their age into a wire trash basket and rolled him down a hill, jeering him. I don't believe that boy was Jewish.

Somewhere around this time, maybe it was after I left, laws had been promulgated that children of Jewish background had to go to separate schools beginning and ending at separate times, so that the public would recognize them as Jews. When the Aryan kids were at school, if you saw boys and girls still out on the street, you'd know they were of Jewish background.

Friends of ours, the Mendl family, had emigrated to England maybe two years earlier, to establish a London branch of a German firm that the father, Gerald Mendl, was associated with. They had a son my age, Wolfgang, who was going to Watford Grammar School. They suggested to my family that I should go to school there and go home during vacations. So I went in 1936, and my mother came with me to deposit me. The plan was that I would come home each vacation, which I did through the summer of 1938. I sailed on those transatlantic luxury liners, the Bremen and the Europa, which stopped at Southampton and then went on to either Bremen or Hamburg.

Again, it was idyllic. I was really going off to a lovely place and coming home on vacations. On one of the boats, Max Schmeling was coming home after being defeated by Joe Louis. I saw him at a distance in a lounge area, and there was great excitement.

My mother would hide things in my suitcase. She denied it later, but this definitely was told to me by the Mendls. She would hide jewelry, for instance, in my suitcases to smuggle it out of Germany, and sent notes to the Mendls to look for the items when we opened them.

BOHNING: Do you have brothers and sisters?

BENFEY: Yes, I have a younger brother and older sister who left Germany in 1938 after the Kristallnacht, the smashing of Jewish shops and the rounding up of Jews and taking them to concentration camps. My brother and sister came to England and were also looked after by the Mendls. Rudolf, my younger brother, lived with us, and Renate, my sister, stayed with the local minister of the Church of England. All along my father and many others believed that Hitler was a passing phenomenon who wouldn't be a serious problem. In fact, because Hitler had said that people who had served the country well would not be harmed, and since my father had served in a cavalry regiment in the First World War and won the Iron Cross, First Class, he thought he was safe and didn't apply for a visa to America when my mother did. After the Kristallnacht he realized the situation. He had told my mother earlier, "You go with the children. I'm safe here." But my mother refused to go without him.

My mother applied for an American visa. Her sister, Anni Albers, who is the well known weaver and printmaker, and her husband, the painter Josef Albers, were active at the Bauhaus in Germany, and then were invited to teach at Black Mountain College in North Carolina, a liberal arts college with the arts as its center. Because they were there, and also by then my mother's brother was in America, they could vouch for her financially and supply affidavits, because that was needed for an American visa. When my father realized he had to get a visa, fortunately the American consulate put him on my mother's number rather than putting him at the end of the line. If they had, my father would never have gotten out.

Two of his brothers were in concentration camps. One got out before the war, and I saw him in England just after he'd gotten out. His hair was totally shaved off. His wife, who was not Jewish, actually visited the commandant of the concentration camp. She was a woman of unbelievable courage who managed to get his release. She was related to a rather famous general, and got him out. The other brother was in the Theresienstadt concentration camp during the latter part of the war, but survived, because that was a concentration camp with many older people, where the regime wasn't quite as vicious.

My brother and sister left Germany in late fall 1938, and my parents left in May 1939. They only had a six-month visa in England, just a visitor's visa on the way to the U.S. There were discussions with the Mendl family about whether I should go with them, but the Mendls suggested to my parents they should leave me for a while longer until my parents were settled, and then I

would join them. I didn't know too much about what was going on, and I was very happy. I was doing well at school by then. So that was the decision that was made.

Many years later, we were at a conference on child development at Vassar, and I was interviewed by a psychiatrist, who suggested that some of my quirks and problems were due to this separation from my parents. But it was the most idyllic separation, living with friends whom I'd known in Berlin already.

BOHNING: Your brother and sister didn't stay in England, though. They came here.

BENFEY: They stayed just those few months and came here. So my family, other than myself, experienced the proper way for immigrants to enter the U.S., starting with nothing, in an awful hole in New York, and slowly working their way out of it. Whereas I sailed in with a Ph.D. and a traveling fellowship to Columbia, and never had the feeling of what it was like to be an immigrant. I'm very grateful to America and I'm accused of seeing it with rose-colored spectacles.

BOHNING: Until the war started, didn't you attend the Lutheran church in England?

BENFEY: Yes. We didn't attend often, because we were in Watford in the northwest suburbs, and the only Lutheran church was in the center of London. Once the war started, and the bombing, which began pretty soon, it was impossible to get there, so we went to the Church of England. Wolf and I were actually confirmed in the Church of England. But the church became terribly nationalistic, and prayed for God's help in destroying the Huns, the devils, etc. We knew enough about Germany, and Wolf Mendl's mother, particularly, who had great sympathy for the German people, felt the suffering of the Germans under the Nazis, and just couldn't stand that. She rather quickly discovered the Quakers as a religious community and joined them, and Wolf did also.

I, on the other hand, went through the whole gamut of nonconformist churches before joining the Friends. The only religious group that I knew nothing about was the Jews, until I came to this country and started being interested in my roots. My wife, Rachel, who comes from totally non-Jewish environs has many Jewish friends, one of them from the Vassar College experience. Since coming to this country I have been discovering my Jewish roots. This fall, in October and November, we will be in Israel to pursue that further, something we've wanted to do.

I'll be doing research at the Edelstein Center for the History and Philosophy of Science, Technology, and Medicine, part of the Hebrew University in Jerusalem.

BOHNING: Let's go back to your education in England. When did you develop an interest in science? Did you have any early exposure to science? There doesn't seem to be any science in your family background.

BENFEY: At one point, I thought I was the only chemist in the family. Since then, I've discovered there were quite a number. Charles Dreyfus, who founded the Clayton Aniline Company in Manchester and hired Chaim Weizmann, wasn't related, but he married a Benfey. Hans Benfey, a Göttingen chemist, was brought to Manchester in the late 1800s to work there and became a close friend of Weizmann. I have a cousin in Montreal, Bruno Benfey, who's a biochemist. I've been discovering several others. But in my immediate family, there were none.

Maybe I should first say something about my early educational experience.

[END OF TAPE, SIDE 1]

BENFEY: In Germany, we had had an English governess for a brief period. My parents could speak English. My father's mother was English. This is part of the Göttingen-England connection, and particularly the Göttingen-Manchester connection, because the Manchester area was the area where scientists could find jobs. Many Jewish people were trained superbly in German universities in chemistry, because universities were rather open to Jewish students, less so to accepting Jews as teachers. Even so the scientists did better at letting at least converted Jews teach than the humanities did. Alan Rocke, historian of science, is documenting this now.

There were a number of Germans from the Göttingen area and from elsewhere, who came to England because that's where industry needed trained workers to help. They would go back and forth. Many of them went back to Germany permanently when Germany had developed its own industry, largely after Bismarck, but even before. But the Jews stayed on because there was less discrimination against them in England.

As I said, my father's mother was English, so my father spoke English. In the Göttingen-Hanover area the second language was English, whereas in Prussia and the rest of Germany it was French. My mother had had an English governess, and she spoke good English.

But I had very minimal English when I first was put into Watford Grammar School, which was an excellent public school, public in the American sense. I couldn't understand anything. They had dictations, and I would just write down what they said without any idea of the meaning. When they said "comma" or "fullstop," I would write out c-o-m-m-a and so on. But I very quickly learned the language, and particularly liked math. When science came around I liked that, probably in part because it required less facility in the language.

I was always fascinated by math. In Germany, in the brief period I had in high school before I left for England, they had oral arithmetic, just quick mental arithmetic. Again there was this German discipline, a public humiliation game that seemed to be so prevalent at the time. They lined us up, and the teacher would pepper us with questions. If the first in line couldn't answer, then the one behind him moved to the front of him. So the people who were the dumbest ended up at the end. I could do all right. I remember one of the questions that stumped me was, "What is three to the zero power?" The memory of my failure stayed with me for a long time as well as the puzzle of a zeroth power.

I enjoyed math, and even considered it as a possible field to specialize in. The English education system specializes rather early. First it was science versus non-science, still within the academic track. There was also a track that wasn't university-directed. I chose the sciences, but I have no memory of whether I seriously looked at anything else. I was strongly urged to go into language teaching, because of my German background. I thought that was an insult, because it was not something that I had mastered, but something that I just knew.

But between math and science, it was a much more conscious, deliberate decision. I loved math but felt that I wanted to bring it down to earth, to do something useful and practical with it. I liked the connection of the mental and physical. The science teaching was of the kind that's now being discussed in America. Rather than a year of chemistry in the junior year or something of that kind, we started having chemistry one or two one-hour classes a week, from age eleven on. So we had a little bit of chemistry every year, and all the other subjects would go forward similarly. We didn't have this massive switching of one's thinking cap, turning chemistry off and forgetting it and trying to recall it much later.

We had a rather prosaic, standard exposure to substances, properties, and syntheses. We did a lot of the syntheses ourselves, and we just wrote down what we observed, "chlorine is a green gas." I didn't have the problem of Americans à la Derek Davenport (1). I was exposed to most of the elements through those years, but I didn't get terribly excited about that. What excited me was the sixth form, that is, past the O level, the normal school leaving age. The next year, our sixth form, was for people who were thinking of going on to university, and here we were introduced to systematic organic chemistry.

We never learned the periodic table enough to sense its predictive power. We were exposed to it, but it was just there. But with organic chemistry, you could see the sequence of boiling points along a homologous series, you could predict on paper the number of isomers you'd find in nature. We even got introduced to stereochemistry and the van't Hoff-LeBel asymmetric carbon atom. I found this tremendously exciting. I always loved geometry. We had earlier gone through many of Euclid's proofs. That was my favorite.

In this connection, in Germany we had had art classes where we had to do freehand drawings of Easter eggs. Later we even had to draw the swastika flag, and I had a terrible time making it look right. In England, it was very exciting to have one period of several months, where we did church architecture with compasses and rulers, and drew the Gothic and the Romanesque and so on. So it was, again, the geometric and the visual, the tying in of math and visual art. They've been very much with me. I've always pursued the geometric aspects of chemistry, and emphasized the importance of three-dimensional structure and so on.

BOHNING: Given your later interest in the history and philosophy of science, were you pursuing any of that in this early period, or did that come later?

BENFEY: No, in fact history classes ended when I chose science, so my awareness of history is British history through Cromwell and the Civil War, before the Hanoverian kings around 1701. It was as limited as my exposure to geography, which did not look to any great extent at America but confined itself to the British empire. Even in history, for those who went on, once America dropped out of the English orbit, it was dropped. [laughter] History and philosophy of science became an interest at University College in London.

BOHNING: How did you make the connection with University College? You started there in 1942, and the war was already under way.

BENFEY: Right. I was under-age for being drafted. The British got so scared of a fifth column, of the possibility that the Nazis had infiltrated Nazi agents in the wave of refugees, that many refugees of Jewish background were rounded up. Wolf Mendl's father, from then on, had a packed suitcase ready to go. Many of these people were shipped off to Australia, and at least one of my relatives ended up there. Wolf's father was an engineer designing oil refinery equipment. Before the war started, his branch of the German firm had become an independent company in England. When the war started, I think he worked for a British firm who kept on supplying him documents saying he was doing work of national importance.

But for Wolf's mother, it must have been frightening. She was quite frail, and she kept on taking on other waifs and strays like myself. At times there were at least three other people living there pretty permanently, and others who would come for briefer periods. For a while, she also had her sister helping, doing the cooking. That produced strains, but we all managed. During the bombing, we all slept downstairs, so we had this gang of six or seven boys and girls of all ages, sleeping in sleeping bags or the equivalent, all in the living room. That too was enjoyable rather than frightening, because again, we didn't know the details of what was happening. I was too young to visualize the panic or what might happen to us if the Nazis invaded.

The way I connected with University College was that I chose chemistry during the year in the sixth form at school. My father in Germany had been able to give some of his money to a Jewish fund that could still ship money out, whereas by the time he left he could take nothing. Right at the end, whatever the Nazis found you still had in the bank, they demanded from you as a leaving charge. There was an organization in London, Woburn House, which dispensed money for people like me who were separated from their parents, and maybe because my father had put this money into these funds in Germany, I received money from them. That money kept on coming regularly, this "Wundergeld," this wonder money, that helped with my expenses.

There must also have been funds made available for universities to offer places for students. One day I got a message saying that University College had a place for me and would I like to go? And so I went.

I should mention that we did have a chemistry teacher in Watford Grammar School who was not a very exciting teacher. He was rather stiff and formal as I remember him, but good and solid. He certainly didn't turn me off from chemistry, and helped me, presumably, writing letters of recommendation. We called him Inky Knight [R. W. Knight]. Wolf still lives in

Watford and still keeps contact with Watford Grammar School. In fact, one chemistry teacher at Watford Grammar School and a former teacher there are working with Tony Travis, a former British high school teacher who is now running the Edelstein Center where I'm going to, in developing history of chemistry materials for the British high school curriculum. Strange convolutions.

BOHNING: Did you still live with the Mendls while you were at University College?

BENFEY: Yes. That was my base. University College is located in Gower Street in London, and during the war it was evacuated. I think the arts went to Oxford. Most of chemistry went to Aberystwyth, but some students went to Bangor. Engineering, I think, went to Cardiff and Swansea. So I began my London training at a seaside resort in Aberystwyth. I wasn't physically aware of the war, except during vacations, when I would come back into London and see the bunks, five layers high, in the tube, the subway stations, where people spent their nights. The only time that the war got close to us was during a physics lecture, when the professor was talking about something having an impact, and at that moment a mine exploded at the end of the pier. That was good timing.

At Aberystwyth we lived in a cooperative student hostel.

BOHNING: How many students were there?

BENFEY: It was just chemistry. I doubt if there were more than thirty or so in chemistry. Physically, we were part of the University of Wales. There were large numbers of Welsh students, and so some of the students at this hostel where I lived were Welsh, and very interesting. One of the things that impressed me there was that Wales, which had a certain autonomy, gave scholarships to young people who would be willing to teach for four years in their school system. So Wales had a lot of superb people going into the school system, and a lot of people received an excellent education all the way through college who would have had difficulty economically in England, even with free tuition, because they needed to earn money. In Wales, they were paid. Interesting. This is being picked up here in the U.S., state by state.

We had a Nigerian chemistry student at the hostel who had tried to find housing. He was black, about as dark an African as I had ever met. It wasn't so much that the Aberystwyth people were anti-black, it's just they'd never seen such a person, and

there was no chance for him to find housing in a family. So he and I became very close friends. His name was Stephen Awokoya. Later on he headed science teacher training in Nigeria, became education minister for western Nigeria, and then became head of science education at UNESCO in Paris, where I met him again. He had a heart attack but survived, and lived on, but he's dead now. Many of the black, foreign taxi drivers I've encountered over the years have come from Nigeria, and they have all heard of Awokoya.

BOHNING: Really?

BENFEY: Yes. He was a fascinating person. Big, heavy fellow. I saw him again and met his wife in Paris at UNESCO.

The other thing that's very memorable about the Welsh people is their singing ability. They have these eisteddfods, competitions among the villages, and all students know how to sing in four-part harmony. Whenever they weren't busy, if they were in a group together, they would start singing. For example, if there was a crowd of students waiting for the curtain to go up for some performance, they would start singing, "Why are we waiting?" One person would start slowly. They would all enter, just repeating, "Why are we waiting," to the tune of the hymn that is sung at coal mine disasters when relatives are waiting to see who was still alive. They also sang wonderfully the Elijah, Handel's Messiah, with the massed church choirs singing from the balconies around the church, with us sitting below.

BOHNING: There is a large Welsh community in Wilkes-Barre, and there was one station that played this magnificent Welsh music on St. David's Day, and I always appreciated it.

What kind of chemistry instruction did you have in this unusual setting?

BENFEY: There was a professor in Aberystwyth in the Welsh university, Charles R. Bury, who is known for proposing the detailed arrangement of atoms in the periodic table, nonspectroscopically but coming out with the same conclusion reached also independently by Niels Bohr. People asked him, "How do you pronounce your name? Is it pronounced Bewry?" He said, "When people die, they don't bewry them, they berry them." He had such a rough time having his paper published in England that his famous paper appeared in the Journal of the American Chemical Society (2).

So the Welsh students' chemistry was going on, and I think some of the classes we had jointly, and some separately.

BOHNING: Did the UCL instructors come out there, too?

BENFEY: Yes. We had [Christopher K.] Ingold and Edward [D.] Hughes. I think Hughes was commuting to Bangor, because the "general students" were there, the students who were aiming more towards teaching careers who wouldn't specialize in chemistry or physics, but take about an equal amount of each. At some point, Hughes became head of the Bangor chemistry department, which has a distinguished history that Bill [William H.] Brock's been pursuing. [K. J. P.] Orton was there, and he emphasized kinetics and taught it to Hughes, and it seems Hughes brought the kinetics emphasis to Ingold. When I started my Ph.D. work, my research was also in kinetics, but I didn't realize it came from Hughes and the Welsh contribution to physical organic chemistry.

We had lectures from nine until ten in the morning and four until five in the afternoon, every day, and labs in between, solid.

[END OF TAPE, SIDE 2]

BENFEY: At UCL we didn't have a quantized and fixed number of classes. People would come in and give us a certain number of lectures until their subject ran out, and then somebody else came in. For example, we had a handful of lectures on spectroscopy, and a handful of lectures by [Samuel] Sugden on the parachor. Henry Poole taught physical chemistry. J. N. E. Day taught organic chemistry, rather prosaically. D. J. [James] Millen, a young man, already on the faculty, taught spectroscopy superbly. C. [Clifford] A. Bunton, now in California, was supervising the organic lab for a while, and I remember him when I was doing organic preparations there. We used Julius B. Cohen's little book, Practical Organic Chemistry (3). Six or seven years ago I discovered that Julius B. Cohen was the first cousin of my father. Nobody in my family had ever told me about that connection. So after I'd given one of my Julius B. Cohen books to the Beckman Center, I asked if I could have it back.

That was the group. Oh, [R. J. W.] Lefèvre turned up briefly, and talked about dipole moments. Christopher Wilson gave us a few lectures at some point. He later was invited by Charlie [Charles C.] Price to join him at Notre Dame. He often served as an expert witness in court cases involving science. For a while he was helping some company in Winston-Salem, North Carolina. He later retired there, taught at High Point College, and set up a small company, Unipoint Industries, for recycling plastic residues discarded by the furniture manufacturers into

cushions, mattresses and so on. That's where I met him again, at a local ACS meeting. He saw me coming down the steps, and shouted out, "Benfey!" Because in London we were always known only by our last names.

There was also somebody called [E. S.] Halberstadt, who was one of the junior lab supervisors, and I didn't care much for him. There were a number of older graduate students, such as Tony James, and Rusty [R. I.] Reed. Of the fellow students who later were doing graduate work at the same time as I, there was Ron [Ronald J.] Gillespie, who is now at McMaster University in Canada. Francesca Leake, now Francesca Leake Garforth, taught for a while at the University of Leeds and has become a leading figure in chemistry education in England. I've renewed contact with her and I'm learning more about what she was doing. Somebody I developed a friendship with was Daniel Goddard, but I haven't kept up with him.

In the lab, we did syntheses, including making many of the derivatives as sort of subsidiary syntheses. I got a tremendous esthetic delight in preparing colored, beautiful crystals. It was really fascinating that in addition to completing something, we were given this reward of something to delight the senses. It's something I've always tried to teach my students in the lab, to enjoy the crystals as they formed.

BOHNING: Didn't that also hold a fascination for [Robert B.] Woodward?

BENFEY: Yes. He was also fascinated with geometry and geometric forms. He became interested in Archibald Scott Couper, whom I helped bring back to chemists' attention. I wrote the biographies of Couper in Eduard Farber's volume, Great Chemists (4) and in the Dictionary of Scientific Biography (5). I emphasized Couper in the Kekulé-Couper centennial celebrations of the structural theory that the ACS held at a history-organic joint symposium (6). It's not clear whether Woodward learned about Couper from my having worked with [Frank H.] Westheimer and pursuing Couper in 1955-56, reprinting his papers in Classics in the Theory of Chemical Combination (7), or whether he had come across him independently.

BOHNING: Did you use any other textbooks?

BENFEY: Yes. Interestingly enough, the ones that I remember were not written by British authors. There was the mammoth physical chemistry text by [Samuel] Glasstone (8). The big texts were intended to be used as reference works. The inorganic was

by the German [Fritz] Ephraim, in translation (9). The organic was by Julius Schmidt (10). The quantitative analysis was [I. M.] Kolthoff and [E. B.] Sandell's (11). For qualitative analysis we used William A. Noyes (12). I've learned since that it's generally accepted that Americans produce the best textbooks. They're very fine. We're beginning to see a change. The Englishman [Peter] Atkins has a physical chemistry text that is now widely used in this country (13). Oh, there was one British text, Julius B. Cohen's organic lab text (3) which I have already mentioned.

BOHNING: What about research as an undergraduate?

BENFEY: None. Our exam system was very interesting. We had exams the first day on returning from each vacation. The feeling was, and I think this wasn't just London, that you go home for vacation to study and review, and so the first day back at school you knew as much as you were likely to know. The teachers could move on from there. Very clever. Instead of giving time for reading periods and the like, while you're still using university facilities.

But the only exam that counts is the bachelor degree final, which fortunately for me was after two years, so I only had to remember two years' worth. Almost immediately after the war the degree course was expanded to three years. I understand now it is split into two parts, so that you only are responsible for the second part at the end. But we did have a long reading period before our bachelor degree exam, and some people commit suicide or have nervous breakdowns, because they just can't take it. It's an endurance test, to be able to know everything throughout this period.

The earlier exams were used to screen people out, to tell people they might as well forget it and go home. Of course, with the war going on, that meant no longer getting deferment. After [Henry G. J.] Moseley's tragic death, the British realized they shouldn't draft everybody indiscriminately, because soon after putting forward the X-ray evidence for atomic numbers Moseley was drafted and died in Gallipolis [in World War I].

BOHNING: What kind of attrition rate was there in your group during that period?

BENFEY: We were in the special degree program, going on to specialize in chemistry. I wasn't aware of anyone dropping out. I don't know if they all went through to the bachelor's degree, but if they didn't I would have learned. The exams were on a

reasonably set body of material. I do remember that in Ephraim there was a chapter in the beginning about the production of the elements from chemicals you can find in nature, which I found quite fascinating. For example, elements you can get by simple heating, like mercury by heating mercury sulfide. Those you can get by a reduction of oxides with charcoal, all the way to the electrochemical methods. Sure enough, there was an essay question on this in the exam. I guess the exam was all essay. No multiple choice. No short answer. You were meant to develop whole arguments.

There was a special section of the degree exams going along with the special degree, that was set by our faculty. Most of the chemistry tests were created by London University for all its colleges. Hughes expected you to write down his lectures verbatim. I remember one lecture, when I decided not to put something down and just listen while he was saying something. He happened to meet my eyes. He had what I've always felt was a pained expression. I was losing those hallowed words by not preserving them for posterity.

Derek Davenport still has his notes, and we really should try and get them here at some point. Unfortunately, I threw mine out. Largely inadvertently, I think. Anyway, they're gone. I have a few notes, but I'm not sure they're from those classes.

I remember one touch of humor, or at least I thought it was funny, when E. D. Hughes, when he got to dyes, put on the blackboard, "Dyes and the art of dyeing." He did spell it correctly with the e.

The final examination was itself an endurance test. We had three days of three-hour tests, morning and afternoon. It was six tests, one after the other, Monday, Tuesday, Wednesday. There were four in chemistry and then there were two physics tests; that was our minor. Then there were six-hour lab tests. We were given unknowns for qualitative analysis. There was some quantitative analysis requirement. There were some organic preparations, and a segment of organic qual. You were given nothing, no hint in the area that you had to solve. Frightening.

BOHNING: Compared to what we do today in examining students, there's no comparison.

BENFEY: That's right. And the students complain that we're too hard on them.

BOHNING: Did you have much interaction with Ingold during this undergraduate period?

BENFEY: During the first two years, he was sort of a benign presence. We'd heard rumors that he spent three hours preparing each lecture. He would lecture just on the areas that he had worked on himself, except for one strange series of six or eight short lectures called history of chemistry, which turned me off almost permanently to ever conceiving of teaching a course concentrating only on chemical history. I'm pretty sure that he went through that early period of stoichiometric development that the British author Ida Freund had already published in 1904, called The Study of Chemical Composition (14). It was a superb book that later on Dover republished with my biographical note and Laurence [E.] Strong's preface (15).

Those Ingold history lectures were deadly dull. Only very recently, during the last year or two, have I realized that my interest in presenting topics historically, in fact my sense that you can't understand anything in the sciences unless you see it in its intellectual and historical context, stems directly from Ingold's organic lectures. Ingold would develop aromatic substitution, for instance, always terminating with his solving the whole problem, but the whole area of aromatic substitution and how the nitronium ion had developed, and aliphatic substitution, and rearrangement, were all done historically. You can see this in his Structure and Mechanism in Organic Chemistry, (16) his magnum opus, where the lectures are in a more polished form, somewhat transformed. Not everything was intellectual history. The early sections were just developing basic information and presented didactically.

On the other hand, in thermodynamics, Harry Poole's lectures were not of this kind. But Ingold's were. When I started teaching at Haverford in 1948, and I wasn't twenty-three then, that's the way I taught. I assumed that's the way one would teach. I wasn't told to teach any differently. There was a real problem correlating my approach with the text, because I realized very quickly that in America, students study from texts and expect you to pattern your lectures accordingly. But I insisted on providing the historical context, and made it pretty clear to the students that I expected some knowledge of this, and not just knowledge of the text.

You asked earlier where I really became consciously interested in the history and philosophy of science. That probably came right after the undergraduate period. For one thing, the department had the tea break, which I think is typically British. There we interacted extensively with other people in the department.

UCL at the time had quite a group of very left-wing graduate students, and there were members of the Communist Party also on the faculty. They were very active in the Association for Scientific Workers, or something like that, which was very left-wing and very close to communism. They were convinced that, after the war, communism would take over, science would stop being this freewheeling activity, which in fact meant that it was just the tool of capitalism. Instead, it would be planned and organized and run to serve the people.

During my graduate years I was head of the Student Christian Movement of University College, London. My religious convictions convinced me the extreme left-wing position was wrong, but there was no way of arguing against its proponents because they were so well informed that in any argument they could abolish you instantly. I started reading Arthur Eddington's Nature of the Physical World (17), and some of his other books. I read quite a number later. He had been the Astronomer Royal, and I think Einstein said he was the best interpreter of relativity theory. He also was a Quaker, which made me feel it was safe to be influenced by him. He had actually written a lovely little book, based on a Quaker lecture, called Science and the Unseen World (18). I also read James Jeans. Those two men were quoted by students frequently. They were my introduction to the history and philosophy of science.

During that time, Michael Polanyi, whose name I'd come across as a chemist, at some point during the war stopped being a chemist at Manchester and had taken a chair in social studies there instead. He slowly moved into philosophy. He had started with an M.D. in Hungary, and went through these enormous phase changes.

During that same period, he was vigorously fighting the claims for planned science and communist totalitarianism, because he had sensed it and experienced it on the Continent, or family members had experienced it. He was one of the key people in that battle, but I was playing as an amateur, not knowing if many others were playing, sensing that many of the critics' analyses and criticisms were correct, but their solution was doomed.

BOHNING: What was the origin of this kind of movement within England? What did that grow out of?

BENFEY: The Marxist view of science in the twentieth century, although earlier persuasively espoused and presented by Carl Schorlemmer (1834-1892) of Manchester, grew largely out of a history of science conference held in London in 1931, whose proceedings were published in a book called Science at the Crossroads (19). A Soviet scientist, Boris Hessen, spoke about

the social and economic roots of Newton's Principia, and suggested it was connected with the upcoming bourgeois mercantile class that needed that kind of physics. This view upset the British establishment greatly. A very different view was presented by G. N. Clark in his Science and Social Welfare in the Age of Newton (20).

The Hessen paper influenced a large number of people who were, or later became, very distinguished scientists, who certainly became powerful thinkers in the relation of science and society, and made history of science popular as something to look at that could contribute to general education and public understanding. Thus there was J. D. Bernal, the famous crystallographer who was enormously influential. He was one of the scientists who welcomed women into his lab, which is probably one reason for the large number of women crystallographers, and not the deprecatory suggestion that women like tedious things and that's why they went into crystallography. They could get in. Rosalind Franklin worked with him for a while. I should have said earlier, when I was talking about Woburn House, that Rosalind Franklin was doing a lot of volunteer work at Woburn House, so maybe she processed some of the money that went to the Mendl family.

[END OF TAPE, SIDE 3]

BENFEY: There was Benjamin Farrington, who later wrote the two Penguin books on Greek science (21). Joseph Needham had become a fellow of the Royal Society through his biochemical cell-biology work and later developed his enormous interest in China. I've gotten to know him. He considers himself a High Church Marxist. He accepts the Marxist analysis of the great importance of trying to understand the social context of scientific developments, but is a profoundly religious person who has even preached on radio. His father, who had been in the Oxford Movement, was also very religious. Needham has been deeply and widely misunderstood, because he has been open to what the Communists have tried to do, although he never joined them as a party member. He's much too independent. But he loves to hero-worship. For years, Soviet Russia could do no wrong, and then he switched and felt Maoist China could do no wrong. If you look at the leading historians of science, and there are three or four others whose names I don't recall at the moment, most of them in that period were influenced by that conference. Many of them were Marxists, and some were Communist Party members.

BOHNING: Was this still a very small group within English society, or was there a larger movement?

BENFEY: It's my impression that the Association of Scientific Workers wielded a tremendous influence, and that it was dominated and run by party members who were recruiting people actively. There was great disillusionment with the laissez-faire, lackadaisical way that science was conducted. The recognition of its power, even before the atom bomb, and Hitler's and others' misuse of technology, convinced large numbers of people that science had to be planned and organized. Then you'd better get the right people on top, so that it's not surreptitiously organized by the people who were in power, whom you don't want there.

This we sensed as a powerful movement. In fact, we were quite worried. I think there was a general foreboding about what might happen after the war ended.

BOHNING: You mentioned Derek Davenport before, and I know there's a connection, but I'm not sure what it is. Was he there at the same time?

BENFEY: No, he came a few years later. We only met up in this country.

BOHNING: During the time the war was going on, had you given any thought about what you were going to do when you finished your degree? Were you intent on continuing for a Ph.D., or had you ever thought of other things?

BENFEY: I was attending Quaker Meeting by then. I was also a pacifist, as large numbers of British Quakers are. I decided that I couldn't do war work. Most of my life I didn't wonder where I would be going; it just sort of happened. I was asked if I wanted to stay on. I would be drafted otherwise, and I'd have to go through a conscientious objector request, and I'd probably be accepted, as a Quaker. I'd gotten a first class degree (there is the first, the upper second, the lower second, and then the pass), so when I was asked if I wanted to continue with the Ph.D., I said yes.

That was in 1944, and I was eighteen by then and of draft age. I was aware that the people who were doing research were working on problems that were of national importance. But I told Ingold that I could only do research if I could work on a problem that was not related to weapons, destruction, and so on. Most of the group were working on aromatic nitration, which had a direct relevance to TNT production, pentaerythritol tetranitrate and similar explosives.

Ingold managed to get me to work on aliphatic substitution, the evidence for the carbocation, the carbonium ion as we called it then. I was blissfully confident that my topic could have no destructive consequences, and later on discovered that mustard gas action is all tied in with the carbocation mechanism, or rather its analog, the sulfonium ion mechanism. I later on discovered that the nitrogen mustards also have a medically useful application by hooking across DNA strands. So I became very much aware that the purer the science, the more it is capable of positive and negative, constructive and destructive applications.

At some point I came across Aldous Huxley's comment that modern society is organized in such a way that the chances of the misuse of science are much greater than the chances of the right use of science. Also emphasizing, from his point of view, the need for sensitivity and not to think "pure" science is by definition pure.

BOHNING: In selecting a research problem, how would you ascertain in advance what its end use might be?

BENFEY: We weren't given a choice. We were just told what our problem was. If it was obvious that the only problems available were tied to aromatic nitration and bomb production, I would have said I couldn't do it. But it never came up. I was just given this problem which had to do with a paper by Louis [P.] Hammett (22). The effects of salts on the rates of aliphatic substitution reaction, the alkyl halide hydrolysis reactions, had been used by Ingold as bolstering evidence for the carbocation. Hammett proposed that there could be specific solvent effects of the salts affecting the dielectric constant of the medium. So I was given the task of checking through the salt effects, and see if I couldn't demonstrate that they really were only explainable by the carbocation mechanism.

I should say that my undergraduate work finished in 1944, and D-Day, the landing in Normandy, occurred that summer. The decision was made that the war would have to end very soon, so the college returned to London. We came back to London that fall, and I did my Ph.D. research in London, living in Watford, and commuting into the bomb craters and the messes. At University College the main entrance faces the center structure with its two enormous wings. One of them had had the library in it. Bombs had destroyed both of those wings. On the left hand side in front of the library was a side building, the Slade School of Art. Behind the Slade was the chemistry department. These two buildings were old but practically untouched. Now, there's a new Ingold Chemistry Department, but for many years that was the chemistry department, which now houses history of

science down in the basement, just about where I did my Ph.D. work. Before the Ingold building was built, the older building became known as the Kathleen Lonsdale building. But it's not being used by chemistry any more. Now the history of science department is there.

We were working down in the basement, in a big, long lab. The graduate students were lined up, one in each bay. Ron Gillespie was separated from my area by a wooden partition. There was the usual two sided lab bench, one person working on each side. He was working on preparing nitronium perchlorate, to demonstrate the existence of the NO_2^+ ion. He had triplex glass between his experiments and himself. I only had a wooden partition, and it wasn't hardwood. [laughter] I take it that it was meant to deflect some of the explosion if one occurred.

There was another person, Dorothy Usher who was using cyanide for some reactions. I think she too was working in the aliphatic area. I guess the women didn't have to do war work. I became very worried--I don't know whether it was physically or only mentally--about the cyanide smell. At some point during my two years, I took off for several weeks, and stayed in bed much of the time, because the doctor said that was the best thing for me. I think it was sort of emotional exhaustion, but I tied it to either the fear or the effects of cyanide. It was a very formative period for me, because during that time I did a lot of religious reading, Albert Schweitzer's autobiography (23), the French Catholic writers and mystics in English, and so on.

We thought the war was going to be over that fall, but of course it wasn't. Patton's armies got caught in the Ardennes Forest, and the war dragged on into the following year. Then the V-2's started coming down. There was a difference between the V-2's and the V-1's. The V-1's were the drones, the pilotless bombs. With a V-1, you'd listen carefully, and if the droning stopped, you knew it was about to descend, and you ran for cover. With the V-2, you couldn't hear it coming, because it would come straight down and moved faster than sound. It's a rocket. If you heard it, you were safe, because it had landed elsewhere, and you heard a massive explosion. These landed about a fifteen-minute walk from us all around University College, but never got closer. I remember one vividly, because it hit the headquarters of the Presbyterian Church. Some of us from the Student Christian Movement helped clean up there afterwards, not coping with the casualties, but just with salvaging some of the stuff in there.

The Hammett argument which was the basis for my research, and which later led to my spending my postdoc year with Hammett, was that if the various salts had their effects on the solvent, then a given sequence of salts should always affect rates of reactions in the same order. If salt A was more effective than

salt B in one situation, with one alkyl halide, it should be with another also. What I managed to do was to show that two salts could invert their effect, depending on the leaving group of the alkyl halide. That is, with an alkyl chloride, chloride ion would slow down the rate, but bromide ion would speed it up. Whereas with the corresponding alkyl bromide, it would be the bromide that would slow it down, and chloride that would speed it up. But it was more complicated, because the chloride would get in there, joining with the alkyl carbonium ion, and forming the alkyl chloride, and the chlorides react much more slowly than the bromides. So you get very complex kinetics.

Then I learned how to determine chloride in the presence of bromide, electrochemically. I was very proud of that, because I found the method by my own literature search. Hammett accepted our new evidence. He wasn't fighting the carbocation the way [William] Taylor had done earlier, which led to Ingold's public dressing down of the poor man (24). Hammett just said his was an alternative explanation, and that it needed to be looked at, which I did. Hammett was very gracious.

Derek Davenport claims that he didn't see Ingold during his research more than a handful of times a year. I saw Ingold and Hughes every day. It was war time, there was little traveling, no grant committees. They were there. They came around every day to find out what was happening and they were very encouraging. I became quite discouraged during the first year, because I couldn't obtain repeatable data. We were expected in those days, certainly during the war, to finish our Ph.D. in two years. At one point Ingold said, "You'll do one-eighth of your work during the first year, and seven-eighths the second." It was very encouraging.

Bill Brock, who is the Edelstein Fellow here at the moment, tells me that even in later years, whenever Ingold was around, he would walk through the undergraduate labs every day. Bill Brock didn't go on to do graduate work in chemistry. I've read in a paper by Ronald Nyholm (25) that Ingold was always interested in chemical education, and in fact initiated some of the conferences for high school teachers, to tell them the latest developments in chemistry, something that Nyholm then continued as did D. J. Millen also.

BOHNING: What kind of a person was Ingold? I've heard stories on both sides of the fence, I guess partially depending on what environment he was in, as to how he reacted.

BENFEY: The general summary, I think, is that in personal relations he is the most warm and gentlemanly individual, with great charm. But when you put a pen in his hand, he can be

absolutely vicious. People were most surprised when they met him in this country, to find what a warm person he was. Herbert C. Brown, who comes from Liverpool himself, has challenged Ingold in public. I was at one of the physical organic gatherings, when Brown publicly quizzed Ingold. Brown has been described as a bull in a china shop. Brown gave the keynote address at the ACS Kekulé-Couper centennial in 1958, and he is a very warm and wonderful person, also very interested in history of science. It seems he and his wife wrote a history of chemistry, but it was never published. People had predicted that he would just talk about the work of H. C. Brown. He didn't mention the work of H. C. Brown. He just talked about the history of the structural theory. It was very impressive. The Journal of Chemical Education published it (26).

BOHNING: When you were doing your graduate work there, were you aware of the larger picture of physical organic chemistry, and the debates that were going on?

BENFEY: No, and I think this was really shocking on the part of the University College people at the time. They didn't encourage us to read the literature. In fact, when I read it myself, I felt as if I was doing something that was perhaps not quite approved of, as if I was checking on them. We were taught chemistry as if it was the word of God, the answer. Even though we were given a historical background in Ingold's lectures, it was not to see that science keeps on changing. It was to show how science was being completed by University College London.

When I came to this country and worked with Hammett, I was visited by [C.] Gardner Swain, Ted [Edward S.] Lewis, and George [S.] Hammond, because I think I was the first Ingold person to turn up in this country after the war. They were graduate students at the time, I believe. This is one of the most embarrassing moments in my life. They quizzed me. I'd never heard of them or their work. I'd read Hammett's book (27) and a few of his papers because I was coming here, but that didn't lead me to read more widely. They quizzed me about what Ingold meant by something or other. So I put on my exam-taking head, and paraphrased Ingold's answer. They said, "We know that. He published that, but what is meant there?" I was dumbfounded. I didn't even know how to cover up my ignorance. I must have looked extremely stupid.

I'd never heard of them. I had no idea that Frank Westheimer was working on the nitration mechanism. I knew nothing about what was going on. It's really awful to think that one could have a significant book like Hammett's and just use it

for your own purposes, for your Ph.D. work, and to get ready to work with him, but no sense that you might be expected to know the wider context.

[END OF TAPE, SIDE 4]

BENFEY: So my real education in chemistry started in this country, if one can separate education in chemistry from chemical education in the sense of training in one segment of chemistry.

BOHNING: I understand that at the time of Hiroshima, the impact that it had was sufficient to give you serious thought about continuing as a scientist.

BENFEY: Yes, I was stunned. One reads now that many people felt stunned in various ways, and it led to their rethinking their life's aims. I was stunned because I still basically felt that science was good, and that its uses for good would outweigh the misuses in warfare and so on. We had been given a hint about nuclear energy during our physics lectures, probably on atomic physics. Of course, Fermi's work had been published, but it was all hush-hush. I remember in one lecture at the time we were told that it was just an engineering problem to be able to release the atomic energy. The teacher may even have hinted about the bomb possibilities, but that was it. It was just that there was some massive energy there that could be used.

When the bomb was announced by Truman, and the announcement came in the form of destroying a whole city, it was very frightening. I walked in a daze through the streets, wondering what had happened, whether humans had gone totally mad. We'd gone through the Churchill era--I was well aware of this through Mrs. Mendl's sensitivity--the increasing lack of concern about killing humans, the blanket bombing of the German cities, as against pinpointing targets, etc. I seriously questioned what place there was for someone with any kind of sensitivity and concern for doing constructive work to work in science, if this is what can happen to scientific knowledge. One became very much aware of the validity of the left-wing claims that this kind of development had to be controlled.

That was in 1945, and I probably toyed with the idea then of switching to medicine, something that I really seriously thought of doing in 1946 at Columbia. I attended some biology lectures, but very quickly decided it didn't make any sense, in terms of the many decisions and commitments I had already made, so I gave that up. I pretty much compromised, talking myself into feeling it was all right to stay in chemistry as long as I could work as

far as possible towards directing it in humanitarian directions. If it turned out that wasn't possible, I would get out later. It was the typical way one justifies one's continuing in what one's doing.

But the experience stayed with me, because that shock was so violent. There was Albert Schweitzer's example. He devoted himself to pure scholarship until the age of thirty and then went into medicine, because he felt that the joys of just pursuing pure studies were terribly self-centered and selfish. I pretty well had committed myself to switching if necessary, and probably at age thirty, to work more directly for humanitarian causes, and then later on rationalized my continuing in science by being in science teaching and hoping I'd be of some use there.

BOHNING: You spent essentially two years on your graduate work, from 1944 to 1946.

BENFEY: Right. That was the Ph.D. work. I received my degree in 1947, at the end of the academic year.

BOHNING: I don't know that much about the British system. Was there any formal course work, or was it just based on research work?

BENFEY: Research only.

BOHNING: Was there an exam of any kind?

BENFEY: There was a purely formal oral exam, which I took the week before my ship departed for the States. (It was a partially converted troop ship taking transatlantic travelers. Six bunks on top of each other. A hundred people sleeping in one of these dormitory rooms.) The oral was also embarrassing, because right at the start they told me the abstract was much worse than the thesis. I guess they had read the abstract and said, "Oh, dear, what's coming?" Then they were fascinated by the thesis. They asked me some very fundamental questions about rate constant. I couldn't give a straightforward answer.

BOHNING: Who composed the examining board?

BENFEY: I remember that Hughes was there, as the internal examiner. I have no idea who else was there, and I have no record. But as I say, it was a pure formality.

BOHNING: So the key is getting entrance into the program.

BENFEY: And completing it to the satisfaction of your supervisor. But if your supervisor in those days said--and I think this is still very widely true--if he decided that your work deserved a Ph.D., you'd get it.

BOHNING: How many students did Ingold have working for him at that time, towards the end of the war?

BENFEY: There were people who didn't continue to their Ph.D., but stopped at the master's program. Stephen Awokoya was one of them. My guess is there were somewhere between twenty and thirty. The Beckman Center has photographs of the department with staff and students both graduate and some undergraduate. There are close to forty people on those photos, of whom maybe ten are faculty or people who'd just finished their Ph.D. and were in junior-instructor type positions. It was a very small number. I believe there were only eight of us working for the Ph.D. in my class. There was a larger group of master's and beginning graduate students.

BOHNING: The papers that came out of that work, though, weren't published until much later (28).

BENFEY: Oh, I know. In 1952. In England, publications at the time weren't the important thing. I guess it was the old boys' network. I was given the University of London postdoctoral traveling fellowship to go to Columbia for a year, and the assumption was that I would come back, and in this network they would find a position that was right for me. So I wasn't worried. I knew I had a teaching job. I should say that I took that fellowship only because I was totally convinced I was coming back. I had been brainwashed into believing--it was a quite general sentiment--that no one in his right mind would immigrate to America unless they had to, as a refugee or something. Culture was in Europe. Americans are far too materialistic.

BOHNING: But your parents were here in the States.

BENFEY: Exactly.

BOHNING: You must have had some contact with them at least, in writing.

BENFEY: Yes. Writing was slow, because the mail was carried on boats in convoys to protect them from U-boats. My mother, particularly, had an uncanny way of sensing my development and staying in tune with it as I changed from a fourteen-year-old when they left England to a twenty-one-year-old when I joined them. My letters were not impressive. She also carried on a lively correspondence with the Mendl family.

When I came to the U.S. in 1946, our relationship resumed as if there had been no separation at all. By then my father, unfortunately, was quite old, and after those ten years of not being with him, I never really became close to him, which is very unfortunate. He was very proud of me, although he found it very hard to accept my being a CO [conscientious objector], especially in America, with all that America was doing for me and accepting me. I was told that in his younger days he was an idealistic militarist. He loved being a reserve lieutenant. During the First World War, he was awarded the Iron Cross First Class for some courageous action with his motorized cavalry unit. He had a sense of military discipline. I met a relative of his first wife recently who told me of visiting my father when he was a young man. At dinner, my father said to him, "If you have this kind of table manners, you'll never be accepted in the Army." But there was little of this when I was growing up. He was the gentlest of persons, in my memory. Except one sensed he could get very upset if you behaved in certain ways.

You were asking about how I could conceive of not staying in America. I thought that after the war I'd be visiting my parents from time to time. But since England trained me, I would be working there. Who knows, maybe my family would come back to Europe. Again, there was a total lack of awareness of the real world.

BOHNING: And you still had the Mendl family there.

BENFEY: Right. They became very much a second family, which is one of the great boons of this kind of arrangement. One has two families instead of one. I feel very close to the Mendls. Wolf's grandmother was born in Manchester, and she and her family, her siblings and mother, I believe, were invited at a certain point to come to Göttingen, to live near my grandparents in Göttingen because my Manchester-born grandmother knew them

well. So that relationship is already three or four generations old. Then I stayed with the Mendls, and Wolf Mendl's daughter stayed with us and with friends of ours at Guilford College and worked in Rachel's preschool. It was just a few months, but it continued this very close family connection.

BOHNING: With the postdoctoral fellowship from University College, could you select Columbia, or was it predetermined?

BENFEY: To get it, I had to apply with a specific plan. I proposed going to spend a year with Hammett, and on that basis I got the fellowship. My guess is there were very few people at that point who were competing for this, but I really have no idea. It was very generous, and paid for my life there. I stayed at International House. Again, it is not the way an immigrant should enter America, living in an academic international atmosphere.

BOHNING: Were your parents still in New York when you arrived?

BENFEY: No, they were in Massachusetts by then.

I quickly changed my ideas about America. First about its technical proficiency. The taxi bringing me from the boat had difficulty going up the hill to International House because the road was iced over. This was about 120th Street and Riverside Drive. I arrived during the last days of 1946. On the other hand, the skyscrapers didn't scrape the skies. When I saw Central Park, what was so exciting was to see those enormous boulders. When I took the train from New York to Boston at twilight, with the sun setting, seeing the New England landscape, the coastline and the ocean, I was just overwhelmed with the beauty of America.

BOHNING: Did you have any impression at all, before you arrived, as to what you would find?

BENFEY: My mother wrote some fascinating letters about her first impressions, which I still have, about New York and its technical wonders. How one person in a bus drives, opens doors, hands out tickets, gives change, and gives directions. Also about those self-service devices which were much more efficient than what we had in England. The elevators didn't have those metal grates that you open and close, the garbage went down chutes, all these

things. She just loved them and always loved America. Deeply grateful. She never went back to Europe, but just wanted to see America.

BOHNING: What was it like at Columbia when you arrived? Of course, there was Hammett, but there were other people there. [Harold] Urey was there.

BENFEY: I didn't have any contact with Urey. [William von Eggers] Doering was there and I met him several times. There wasn't much contact between the groups. There was a Dawson there. It seems there were two Dawsons, and I've mixed them up recently. There was [Raymond] Dawson who worked on poison ivy extract. It was a lab that people pointed to and said, "That's where they're concentrating and extracting poison ivy." We had these great natural products people at Columbia, including Robert C. Elderfield. Then there was [Ray] Dawson of the biology department whose reminiscences of Percy Julian we recently published in the Beckman Center News (29). Julian worked on steroid plants in Mexico and with the Glidden Company. He was the first black to be elected to the National Academy of Sciences. We now have some of his letters. It seems the Dawsons got their mail missent; I wasn't the first to confuse them.

Again, I was living in my own little world. I guess my interior mental world was so intense that I didn't do much exploring. We did have some interesting seminars, but I had just the 1946 calendar year at Columbia, and I had to complete a research problem. I delved into the literature, because I was all on my own except for my maybe once-a-week conferences with Hammett. I had to produce. Mine was a strange problem about mercury-catalyzed solvolysis and olefin formation, which I sort of solved, but I've never felt very confident that I did something significant.

BOHNING: I was struck by the fact that Hammett's name doesn't appear on the paper (30).

BENFEY: Right. I guess he wanted to give me stature, that I deserved to have it listed as having been done by myself. I did all the experimental work. To get grants nowadays in this country, it's important for the top people to have their names everywhere. I hope I gave credit to him.

BOHNING: Oh, yes.

BENFEY: But Ingold has his name on every paper. Of course, the British system is so hierarchical. Until he appointed Kathleen Lonsdale, I think every research problem at University College was related to his interests. The diversification occurred much later. I remember Kathleen Lonsdale. She gave a lecture in late 1945 about her synthetic diamond crystallography studies and other work she was doing on kidney stones. She was a Quaker also, and I knew this, and people probably knew of my interest in her visit. At London, in England generally, you don't plan much of an introduction praising a speaker, but you ask someone to give a flowery vote of thanks at the end, and I gave that vote of thanks for Kathleen Lonsdale. It turned out that it was her lecture to introduce the Ingold community to her as a possible professor there. She was appointed very soon thereafter.

She was in prison for a while during the war, because she refused to be drafted for fire watching, even though she was doing that in her own building. But as a Quaker she didn't want to be drafted to fire watch in some war production establishment. She was sent to Holloway prison. As soon as she was released she wrote a book about prison conditions for women (31), a good Quaker tradition going back to Elizabeth Fry. She was very active in the right use of nuclear energy after the war, and learned quickly that you don't achieve anything by scaring people. You have to give positive reasons to support your proposed course of action.

BOHNING: But you were still quite young. You were only twenty-one when you arrived here.

BENFEY: Yes. I had an immigrant visa, just in case I decided I wanted to stay in this country. Francis Bitter of MIT was one of my sponsors. I had to register for the draft here, and I registered as a CO, because the draft laws were still in force.

BOHNING: But you were coming in at twenty-one, and the American undergraduate students at Columbia would have been your age.

BENFEY: That's right. Even when I started teaching at Haverford, I was twenty-two and there were people coming back from war service who were older than I was. It was all very strange.

BOHNING: Did you spend much time exploring New York City?

BENFEY: Yes, New York City I explored, because my grandfather was still living in New York. He was over eighty and lived on Manhattan's East Side in the German quarter. Even today there are still some central European stores, like Schaller and Weber and Paprika Weiss. He would show me the glories of New York. He loved New York. He taught me that I was welcome to go into hotel lobbies to see how other people lived. He took me on the nickel ferry to the Statue of Liberty. I got a wonderful introduction to New York from someone who loved it.

BOHNING: During that year, were there any other impressions of Columbia?

BENFEY: I think what impressed me about Columbia was the vigor, the scientific turmoil that was going on. I probably didn't even know how significant the quinine synthesis was, although it was said that Doering had just finished working on it. I sensed that, but the greatness of Woodward I only realized later.

I also had these religious interests. I might just mention that between International House and Columbia is Union Theological Seminary. There, at a quarter of nine in the morning, they had fifteen-minute worship services with mini-sermons by the faculty and visiting faculty. So you had [Reinhold] Niebuhr, [John] Bennett, and [Henry Pitney] Van Dusen, these great Americans emphasizing socially concerned theology, and giving their immediate thoughts. Occasionally there was [Paul] Tillich. Sometimes I heard them later give a polished sermon on the same subject in a church, but the original ones were so much more exciting. Again, that was an idyllic period, this morning transition to my involvement in my own work. [Arnold] Toynbee came through, and I remember his talking to a group one afternoon.

[END OF TAPE, SIDE 5]

BENFEY: During that year, as I mentioned, I seriously thought of dropping out of chemistry and going into medicine, but I gave up that idea. I had joined the Quakers about a month before I left England, and I'd heard about Haverford College and about Rufus Jones, who was a leading theological figure, who taught there. I also heard about Douglas Steere. I had made the comment that, if I did get stuck in America, that's the place where I wanted to teach chemistry. One day, at Columbia, out of the blue, came a letter from the head of the chemistry department at Haverford saying, "Dr. Henry Cadbury of Harvard University Divinity School informs us that you're here and you might be interested in a teaching position."

I toyed with the idea of starting that fall rather than staying at Columbia the full twelve months, but decided it wasn't fair to the British, but I did explore it. By then I'd come to the conclusion that I really wanted to stay in America, and thatI had to make my peace with the British. I decided I should complete the year of research and possibly might finish something significant. It was worked out that I would move to Haverford in January. The reason why Henry Cadbury of Harvard knew about me was that he had a house in Haverford, and had taught at Haverford before he went to Harvard. My sister was in touch with him in Cambridge with the American Friends Service Committee, and she told him that her brother was a chemist and was coming to this country. That was quite amazing.

BOHNING: You then informed UCL that you weren't coming back? Did you return at all to England?

BENFEY: I did return in the summer of 1948 and talked to the University people personally, but I had written to them before and explained my decision to them. Unlike most systems where you have to pay back money if you don't come back after a paid leave, there was no requirement. It was just an assumption that you would return. I sort of got the hint that they were quite aware that this might happen. They probably didn't mind so much having one of their representatives talking about what was going on in England in this country. I never had any sense that they were upset. I also visited Ingold of course.

BOHNING: Then you returned back here. How long did you stay? Was it a short period of time?

BENFEY: I stayed in England for a summer work camp of some peace group (it wasn't the American Friends Service Committee), and that summer I actually met someone who still remembers me from that time. I had gone to a Young Friends conference in Holland, and this woman, who's now a fellow resident of the Bryn Gweled community of Southampton I'm living in, remembers meeting me there. And I visited with the Mendls. I spent probably five weeks overseas at least, and then returned to Haverford.

BOHNING: How would you contrast the intensity of activity within chemistry at Columbia that you mentioned, with your UCL Ph.D. experience and the Hughes-Ingold group?

BENFEY: I had no sense of pressure in London. Looking back, it was as if I was just living in a daze. I would just go in day after day. I had no sense as to whether I was meant to finish at any particular time because other people's lives were operating the same way. Somebody would just move on to some position somewhere. You just waited until the word came from above telling you where you were going. It must be very much like a Catholic order, where you are not in charge of your own life, you just proceed.

Suddenly at Columbia, to have this vitality and activity--well, I haven't talked about Hammett yet. Hammett, very early, and I haven't been able to document this, but I mean to, told me how influenced he was by Percy W. Bridgman, and Bridgman's emphasis on operational definitions. Many of Hammett's emphases in physical organic chemistry come from that philosophy, that you have to define every term you use in terms of measurable parameters, some measurable procedure.

That's what made him ask, "What do you mean by pH?" You can't just say it's the hydrogen ion concentration expressed in a particular formula. You have to say how you measure the hydrogen ion concentration, and that only works in a limited range of conditions. It gets complicated and gives strange results when you get to highly acid solutions, when there's very little water, and so you don't have much H_3O^+ ion yet the solution is highly acidic. That's how he developed the H_0 scale. He told me this influence guided him in many of his studies.

Hammett only got his undergraduate degree at Harvard. Bridgman was doing high-pressure work at Harvard at the time. What I haven't found out, and I've looked at a Hammett memorial lecture and there's no hint in there, is how he was influenced by Bridgman, whether he attended Bridgman's lectures, or whether he did some undergraduate research with him, or what.

There's another thing that interests me about Hammett. He spent his last years at a Quaker retirement community in Medford, New Jersey, so it must be Medford Lees. That is something I want to explore, if he developed any interest in Quakers, because I had no awareness of that.

Thomas Y. Crowell was there as a graduate student and Vernon Haskell and Phyllis Dunbar. I got to know them slightly. But I never became too involved in what they were doing. There wasn't much research interchange, at least not with me. Maybe they felt that I was just there temporarily. I also remember a Vietnamese student at International House, Tony Vangly--a lovely person--I lost touch with him and wonder if he survived the Vietnam war.

BOHNING: I've had people tell me both sides of the coin about Hammett as a department chair, and I think he was chair at that time.

BENFEY: I was told, generally, about Columbia's chemistry department that it was a collection of individual professorial groups. That's not the place to get a chemical education, because there was no coherence. But you had all these superb people there. I did go to the thermodynamics people and to the electrochemistry people and so on, to get advice. Richard Noyes was very helpful. They were very gracious and helpful.

[break]

BOHNING: Before our break, we were finishing up the Columbia period, and looking at starting the Haverford period, which was 1948.

BENFEY: It was January, 1948, the second semester.

BOHNING: You started the middle of the year. I don't have any dates here. Just for the record, you had three different ranks when you were at Haverford--instructor, assistant, and associate professor. Do you remember the years for those different ranks?

BENFEY: No, except that I was promoted to associate professor on gaining tenure--when I resigned.

BOHNING: Okay. What was Haverford like when you got there?

BENFEY: It's a beautiful campus laid out in the 1830s or so by an English landscape architect. Around the campus was a lovely nature walk. In 1948 it was still quite small. I think it had only 550 students, all men. It had just gotten a wonderful new president, Gilbert F. White, who was an internationally renowned geographer, known for his expertise in water resources. He's still active, although he must be in his seventies, and he's a major consultant to all sorts of projects. He was active in the Mekong river development project in Vietnam, Cambodia and Laos, and went through all the warring zones. They all let the river development continue because they all were counting on it. He's worked in Africa. He'd also been very active in Quaker circles, relief work in France, and was Board chair for a while of the American Friends Service Committee. At Haverford, there was a

program for training people to serve all over the world in development projects. A number of people were trained who moved high up and were really superbly gifted and competent.

It was a time of great curricular innovation, and I was involved in some of them. At some point I chaired the faculty group in charge of the non-academic program. Haverford is a Quaker school, and at all the three schools I've been to, the faculty do not vote. It's all done by consensus. At Haverford, or maybe it was at Earlham, the presiding person was no longer the president, but was chosen by the faculty. The story is that a president before Gilbert White still ran the faculty meeting dictatorially. He would draft the minutes, the summary of the faculty meeting findings before the meeting was even held, and after the presentation and a few minutes for questions he would say, "Is there any discussion?" [laughter] Then he would read his minutes. But during the time I was there, faculty meetings were wide open and very productive.

The non-academic program was designed to give students an alternative to phys ed. Instead of having required phys ed every one of the eight semesters, they could drop two or three of them, and replace them with either an art activity--sculpture, pottery, fine art, and so on--or community service such as work camps. Since the Barnes Foundation is currently in the news, it might be of interest that Gilbert White arranged for the Barnes Foundation to teach a non-academic art appreciation course at the Foundation in Merion for our students who wanted to go.

One day after this had been going on successfully for some time, every member of the faculty and of the board of trustees received a letter from Albert Barnes, blasting Haverford College, Gilbert White, the board, the faculty, everybody. That was typical of Albert Barnes. He antagonized everybody, and he once said he enjoyed this as much as anything else in his life, to antagonize people. Anyway, he blasted us because he had discovered that Haverford students could gain academic credit for art courses taken at Bryn Mawr, and he felt we were downgrading his teachers because their course was a non-credit course. He stopped it immediately.

BOHNING: What was the chemistry department like? You were still only twenty-three at the time.

BENFEY: I started at age twenty-two.

William Buell Meldrum was head of the department. He wrote the book, Introduction to Theoretical Chemistry (32), sort of a basic freshman text, but the early chapters were very historically and philosophically organized, to show how all the

bits and pieces of chemical evidence came together to produce what we now know as atomic theory and the periodic table. Again, it was historically structured teaching, so that I never felt the need to ask where my history emphasis came from. I thought that was the way to teach. I discovered later that it was very unusual. He has an interesting tabulation in that book about all the strands that fed into the final working out of the stoichiometry concepts of atomic weights and so on, the Cannizzaro synthesis.

With him was William Cadbury, who was particularly interested in the premed students. He became a national figure in premedical education and later in programs for placing black students in medical schools. [He died in 1992.] He also was Dean for a number of years. The third person was Thomas O. Jones, who had worked with [Glenn T.] Seaborg in the Manhattan Project. It was very exciting for the students, but very worrying for me.

BOHNING: I was going to ask if you would have a problem with that.

BENFEY: Seaborg came through Haverford. That's when I first met him, and I had a delightful meeting with him. He still greets me warmly. He later allowed us to use several of his speeches in Chemistry magazine.

T. O. Jones had been there some years before I came. Maybe he was there already before the war, but I doubt it. Anyway, he clearly felt that Gilbert White had put me into the department to reorganize the department from its classical patterns. He felt very suspicious of me, and in fact raised questions about my overall competence.

The reason this was so crucial was that Meldrum was retiring, and T. O. Jones was going to take over. So with a great flourish, I announced my resignation in a letter to all the faculty. A week or two later, Gilbert White visited me and said he was leaving too, because he had been offered a chair at the University of Chicago in geography. I did get tenure, and I was also going on sabbatical that year. White said I could have it and I deserved it based on the previous work, even though I was not coming back. That was very different from present arrangements, where it's a condition for getting a sabbatical that you return at least for a year. So I got my half pay. One of the wonderful professors there, Albert Wilson, gave us a financial gift to help with the other half. That's when I worked with Westheimer and Leonard [K.] Nash at Harvard.

I was all ready when I came to Haverford to introduce physical organic chemistry and mechanisms into the first year organic course. But T. O. Jones and Meldrum were not at all in favor, and one day Meldrum told me that he had written to Roger Adams and [Henry] Gilman, and a whole group of eminent organic chemists, to see how they felt. They all agreed with him. He obviously chose his people well. I never taught the first year of organic. I taught analytical and physical and all sorts of things, but I never taught the first year of organic. Meldrum and Jones taught that. Then I could present the rest of the stuff as theory, highfalutin stuff.

BOHNING: Did you teach any introductory chemistry?

BENFEY: Yes, although I don't think I ever taught the year course. I taught a course for non-chemists, which was fun. I had the students write about some famous chemist, something that I've done ever since, even in the majors' course, to get them to realize that chemists were human beings. I had them look at the non-chemical aspects, their other interests, their education, their hobbies, things of this kind.

BOHNING: What text did you use in freshman chemistry? Do you remember?

BENFEY: I only taught the non-majors and I don't recall the text. But Meldrum used his own book. I taught the premed physical chemistry course sometimes. There we used Frank Gucker and Meldrum's book on physical chemistry (33).

BOHNING: You also did some research there, because you had some papers with some students (34) which you later wrote about in an editorial in Chemistry (35) which I enjoyed. That was interesting.

BENFEY: People talk about that kind of gap-filling research as the worst kind of chemistry. But I enjoyed qualitative organic analysis very much. I had come across these molecular addition compounds between picrates and naphthalene, naphthylamine, and so on. Thereby you can get two derivatives from alcohols, for instance. First you make the dinitrobenzoate, then the naphthylamine addition compound. The second derivative comes out very easily, just mixing the solutions of the two compounds to get beautiful crystals. If you can't distinguish two alcohols from the first derivative, it almost never happens that the second derivatives also have melting points about the same. One

set may be close, the other not. [Tadeus] Reichstein, the Swiss Nobel Prize winner, had done the whole set of double derivatives for all the isomeric hexanols except for one or two.

And it seemed to me this was the most obvious thing to do with students to get them started with research. Then I tried to see how I could make such pairs of derivatives not just from alcohols, but from esters (and there are procedures for doing that) and from alkyl halides, and I could show that I could use this pair of derivatives to characterize quite a number of different substances. In the process, we discovered that these crystals produce whiskers. We never published this, but I had students do quite a bit of research on them. We discovered if you put two beakers, one with the dinitrobenzoate ester of ethyl alcohol, needlelike, glistening, yellowish, the other with pale violet alpha-naphthylamine crystals, under a bell jar or large beaker, then on the dinitrobenzoate crystals you would see whiskers forming of the addition compound. So you sort of wonder, "How do they form up here? Do both of them vaporize and stack up one molecule after the other, forming and lengthening the chain?" We found that in the crystals of 3,5-dinitrobenzoate, near the base of the whiskers, you see pits, so that probably the dinitrobenzoate molecules would slither up the needles and meet the naphthylamine coming in from the air.

So then I became interested in whisker formation, which is a major technical problem, because it seems you sometimes have whiskers produced in telephone lines, short-circuiting lines, and problems of this kind. That's how outgrowths of native copper and silver are formed. That was fun.

[END OF TAPE, SIDE 6]

BENFEY: One of the students doing research with me was John Mikhail from Ramallah near Jerusalem, now part of the West Bank. There's a Friends school there. He went to Princeton for graduate work in chemistry, but switched to Arabic studies and later became an adviser to [Yasser] Arafat. John died in mysterious circumstances, and I never found out what really happened to him. I believe he drowned in a boat accident, but nobody knows quite why and how.

Barton Milligan now teaches at the University of Mississippi, and has been active in the AAUP there. He was very active in interracial concerns during the civil rights era. I've lost touch with the others.

BOHNING: How many chemistry majors were there during that time period?

BENFEY: Something like four to eight graduating each year. There are two other students who are of interest here. One is Ed [Edward B.] Patterson, whose father was president of Arthur H. Thomas Company at the time. Ed is now president of the company. Its present name is Thomas Scientific. When Ed graduated from Haverford and was starting to work for the company, he was repairing Beckman pH meters. He recently told us at the Beckman Center that he remembers [Arnold] Beckman visiting the family since his father and Beckman were close friends. Ed's father was a spectroscopist also. Ed remembers Beckman saying at one visit that he planned to become a millionaire. It seems that when Beckman wanted to introduce the DU in 1941 at a meeting at MIT, he wasn't able to come East from California, so he asked Ed [Edward B.] Patterson, Sr., to make the presentation for him.

The other chemistry major, with whom I've just made contact last week again after all these years, is Edwin P. Tuttle. During his Haverford years he spent a summer working with the Solvay Process Company in Solvay, near Syracuse, New York. He decided that he didn't want to be behind a lab bench all his life, and chose to go into business. He tells me he was a little worried about telling Meldrum that, wondering how Meldrum might react. He went on to Harvard for his M.B.A., was offered a training program position with Pennsalt Company, and eventually became CEO of Pennsalt, which later became Pennwalt, and then Atochem of the Elf Aquitaine group. I had a wonderful reunion with him last week.

BOHNING: It looks like on these two papers alone, you've got six different students. There are three on each one. You had six students alone in that time period.

BENFEY: But this is different. I published them together, but that covers several years of work. My guess is there were probably about ten junior plus senior majors on an average, including quite a number of premeds.

BOHNING: Was it more common for the premeds to go through chemistry than biology, or were they split?

BENFEY: Yes. The feeling all along has been, at least I've heard this over the years, that even though medical schools say it doesn't matter where you major, if you've got a pretty decent degree in chemistry, that does make people feel that you're competent.

BOHNING: The reason I was asking about the students is that there were only four of you in the department. You made the fourth person when you joined.

BENFEY: Right.

BOHNING: Did it stay at four all during that period?

BENFEY: Yes.

BOHNING: Were the others doing research of any kind?

BENFEY: Meldrum was not. My research was done on a Research Corporation grant for summer research. I don't think the others had such funds. I may in fact have been the only one. Cadbury certainly didn't. He was becoming more and more involved in administration.

BOHNING: I'm going back to my own experience, as being the fourth person in a department, and the only one doing research. How did they react to your getting a grant and doing laboratory research? What was the reaction of the others?

BENFEY: I think they were delighted. Did you sense some envy on their part?

BOHNING: It wasn't quite that.

BENFEY: I never sensed any tension there.

BOHNING: What were the laboratory facilities like at Haverford at that time?

BENFEY: Nothing out of the ordinary. I was able to purchase a polarizing microscope. You'd get these marvelous color effects using perfectly straightforward yellowish-white crystals. They'd appear in all different colors. That was my major acquisition. Spectroscopic equipment didn't exist. We probably had colorimeters. In the organic and organic-qual labs, the equipment was simply test tubes and flasks. We didn't have ground glass equipment.

BOHNING: Was the size and number of laboratories sufficient?

BENFEY: It was quite adequate. Haverford was a select school, with four chemistry faculty for a total student body of 550. At Earlham we also had four with twice the number of students. At Guilford we only had two [laughter] with a much larger student body.

BOHNING: During this time period, there were a number of other things that happened. One was your paper on Prout's hypothesis in J. Chem. Ed. in 1952 (36), which I think was presented at an ACS History of Chemistry Division meeting somewhere, which means that you already were beginning to write in the history of chemistry.

BENFEY: Yes. In 1948 I went to England. I went to Harvard in the summer of 1949. I know that's true, because I got engaged in 1949 to Rachel Elizabeth Thomas and we were married in the fall of 1949. That summer she went to the Vassar Family Institute that I've already mentioned. I worked in Conant's summer school on case histories in experimental science.

I don't know how I got onto Prout's hypothesis. It probably came up in Leonard Nash's lectures, and I was just interested, because it involved a numbers pattern. So I started reading about him. I thought I was terribly original, and everything I found that was new to me I assumed was new to everybody. I knew nothing about literature searches, so that when I either submitted a draft or showed it to someone, they said, "Well, most of this isn't new." I slowly learned what was new and original, at least what was somewhat novel, and so I reworked it. I was helped here by Claude Deischer at Penn. Maybe it was he who set me straight a bit; he certainly helped me with that manuscript.

BOHNING: How did you make the contact with Deischer?

BENFEY: I was at Haverford. I think Meldrum probably told me. Claude was a chemist and historian, and he sort of ran the Edgar Fahs Smith collection. I was also active in the Philadelphia Organic Chemists Club, and was treasurer of it for some time. That's when I first met Glenn Ullyot. I think [Herman] Skolnik was around then and Glenn Ullyot and some of the Rohm and Haas people I am meeting again now after forty years.

BOHNING: Was there a pretty closely knit group of chemists within the Philadelphia area?

BENFEY: I think the organic chemists were such a group. There were local ACS meetings, and Philadelphia Organic Chemists Club meetings. The organic chemists felt much closer, coherent. They had large meetings. I doubt if I went to many ACS meetings, but I attended the Organic Chemists Club meetings regularly. I understand there was also an active Catalysis Club.

BOHNING: When did that group start?

BENFEY: I have no idea. I think that story is probably worth researching--when it started and when it ended, whether it became an interest group within the ACS or just folded. It was a nice mix of industry and academic people.

[Newman Bortnick of Rohm and Haas informs me that the Organic Chemists Club was founded in the late 1930s and that Newman and Ernst Berliner of Bryn Mawr College ran it in the 1950s and 1960s. Ralph Connor was one of the founders. There was also a Wilmington Organic Chemists Club, and the two clubs hold joint symposia biennially. The Philadelphia club is still very active, 400-500 members, with 150-200 often attending in the large chemistry lecture room of the University of Pennsylvania.]

BOHNING: During the Haverford period, you were involved with the Society for Social Responsibility in Science.

BENFEY: That was founded in 1949 at Haverford. It was a group of individuals, most of whom, I think, became energized by the Hiroshima bomb. Many of them were Quakers or like-minded people or CO's who felt the need for scientists with that kind of orientation to get together, to see if there would be some way of influencing society to move, as far as possible, to the prevention of science being used for greater and greater destructiveness. They also wanted to see if they could help with problems where science could be used for good purposes, such as in technical and agricultural aid to underdeveloped countries, countries in the process of development as they are now called, or Third World countries. As I said, it was founded at Haverford, and I attended the founding meeting. It was the brainchild of Victor Pashkis, a Viennese immigrant and engineer at Columbia [who died in June 1991].

There was much controversy whether scientists should be concerned about how their knowledge would be used. In a democratic society, it was claimed, it was for the voters and elected representatives to decide. Norbert Wiener and Louis Ridenour debated the question in Atlantic Monthly [January 1947] after Wiener declared he would no longer publish what might be useful to the military. But after SSRS was founded, the idea spread rapidly; German and British groups sprang up and similar organizations appeared among educators, psychologists and many other disciplines. The most famous probably is "Physicians for Social Responsibility" which even obtained the Nobel peace prize for its work. SSRS counted Einstein, Linus Pauling, and Max Born among its members.

So I threw myself into that organization, and after two years of Victor's presidency, I became the second president of the SSRS. Possibly the majority of the heads of committees of the SSRS came from Bryn Gweled, the community group of homes that had been created in Southampton, Pennsylvania, where I now live. Bryn Gweled is a very democratic group. No ideological requirements, it is designed for interracial housing, away from zoning laws. The land is communally owned by the group. There are about seventy families there now, and each family has about two acres. They do own their houses. They develop their two acres. It's a very supporting community. The land started as a disused, worn-out cornfield farm. We remember it barren in the early fifties, and now it's largely wooded [laughter], with lots of deer. Some of those people from the early SSRS days are still living there. One of them is Edward Ramberg, who at RCA developed much of the theory of the electron microscope. So Bryn Gweled wasn't new to me or Rachel, and wherever Rachel and I lived, we looked for similar communities, but never found them. So we're delighted to be part of it now.

BOHNING: Where did you meet your wife?

BENFEY: At Haverford. She was teaching at the Haverford Friends School. It is an elementary school attached to the Haverford Friends Meeting. Rachel is a North Carolinian, a Guilford graduate, and had been in the Philadelphia area before. She had become a Quaker and had been engaged to a young Quaker, Sergei Thomas, who soon afterwards was drowned in a canoe accident on the Delaware.

Rachel had been in the Philadelphia area one summer before, in a camp that was run for city children. (The head of the camp was Ted Peters. We recently found out he too was living at Bryn Gweled.) Because of the death of her fiancé, she was rather lonely, and the physics professor at Haverford, [Richard] Sutton,

decided she and I would make a good match. So he had us for dinner one day, and we both became active in the Philadelphia Young Friends movement. In August 1949, we got married.

At Guilford, Rachel wanted to go into biology, but the president's wife there decided the only people who go into biology were premed students, while Rachel wanted to pursue botany and other areas of biology. Therefore Rachel was told she ought to go into religious education, which she was not that interested in. But the President's wife was heading a program for Guilford graduates to teach religious education in North Carolina's public high schools. Rachel was interested in art, and has taken art courses with some impressive people in Vienna, when I was taking a student group from Earlham there, as well as at the three Quaker colleges. She's worked with various media, particularly textile dyeing. Maybe we'll come back to some of that later on, when we talk about Japan. She taught art for elementary school teachers at Earlham.

BOHNING: Let me go back for a moment to that first visit to Harvard, when you spent the summer in Conant's program. Could you tell me a little bit more about those case studies, how they developed, and what that summer was like?

BENFEY: That's a long time ago. By then my parents were living in Cambridge. My mother began working in the States by being sort of a housekeeper, maid, cook, etc., for a family outside Boston. There was a guest house in Groton, Massachusetts, near the Groton School that a group of people in the area had decided should be made available as a way to employ immigrant people. So my parents were asked to head up this Welcome House in Groton together with a former leading German coffee importer, Albert Zuntz. My mother's mother also worked there, and my sister did during vacations. My sister was at Black Mountain College with our uncle and aunt the Albers who were on the faculty there in North Carolina.

That worked as long as America was not at war, but as soon as war started, nobody could spend time on vacation. The House was also visited by the Groton students, just to have tea, but it was mainly a guest house. During that time, also, Harvard and MIT faculty wives organized what became known as the Window Shop, a restaurant now on Brattle Street, in Cambridge, as a nonprofit organization to employ new Americans. My mother was asked to be the personnel manager there, which she then became, before I came to this country. There she met Frank and Jean Westheimer and many others.

So I lived with my parents that summer. Conant's program also involved Leonard Nash and Thomas Kuhn. Most of those people whose case studies are in the two-volume collection (37) actually lectured and trained us in presenting their case studies. Of course, these were the case studies that Harvard was using for its non-science courses. Edwin C. Kemble was another one.

I found this tremendously exciting, getting at the original writings of Boyle, Dalton, and others and seeing their struggles within the total context of their work. I just loved teaching the Boyle case, when I got to Boyle's Law. Instead of just thinking Boyle sat down and said, "What's the relationship between pressure and volume?" and then proceeded to do some experiments to find out, I learned of the Aristotelians and the medieval hatred of a vacuum, and Galileo and [Evangelista] Torricelli. It was fascinating. And I think it gave me the real impetus to explore history of chemistry. The fact that my Prout paper got published was a great additional stimulus.

The other event in that general area was that Henry Margenau from Yale came to Haverford and met me. He was involved in at least one of the books that Yale was putting out by Ernst Cassirer. Yale was publishing a whole series of Cassirer books in English, and two people had worked on the translation of Cassirer's Determinismus und Indeterminismus in der modernen Physik (38). But the translation was not acceptable to the Yale Press, and so Margenau decided that I was the person to make something of it. That was my first book publication (39). I found I enjoyed working over other people's translations and turning them into something useful, which is something I've been doing off and on.

BOHNING: How did Margenau get your name as the person to do this?

BENFEY: Margenau came to Haverford as a Philips visitor lecturing about physics, and philosophy of science. A Mr. [William Pyle] Philips had left many millions to Haverford for periodicals, rare books and distinguished visitors. Since the library was of limited size, and rare books were of limited educational value at a college, most of the money was used for visitors. Niels Bohr came and Seaborg, Martin Buber, C. F. [Carl Friedrich] von Weizsäcker and Eleanor Roosevelt. Students became quite blasé about breakfasting with politicians and world leaders, scientists and philosophers.

I must have been intensely interested in the history of science ever since my graduate work. I asked to spend the summer of 1949 with Conant's group, and after my second full year at Haverford, 1949-1950, I spent the summer just reading the classic

works in history and philosophy of science. I taught a course at Haverford, and have been teaching one course almost every year on some aspect of history and philosophy of science ever since. I called that Haverford course History and Philosophy of Science. Once I got to Earlham, I just called it History of Science, one reason being that a philosopher, Grimsley Hobbs, also from Guilford, was teaching a philosophy of science course, and later he and I collaborated on that.

My course at Haverford was well accepted. With the Haverford students you could be very demanding. I went through the whole of Kant's Critique of Pure Reason (40). Never after Haverford could I have even dared do it. I plowed through it, and I'm glad I did. I'm not sure I understood very much of it!

BOHNING: So you found that you enjoyed the translation aspect. I hadn't realized it had started this early, although I should have from your publication list.

BENFEY: The nicest part of it was that Yale University Press had a stylist who went over my work and turned it into beautiful, flowing, idiomatic English, so that I got compliments in reviews about this wonderful English, which wasn't even all mine. My manuscript came back to me with lots of K's on it. The K's stood for awkward. That was something the Press was going to smooth out. But today most editors do little more than just follow a certain number of rules. I am very fortunate that the Beckman Center has the old kind of stylist for its publications in Frances Kohler.

BOHNING: From my own experience of translating just scientific papers, translating an entire book would seem to be an enormous undertaking.

BENFEY: That's why it's so much easier if somebody's done the basic work, so that you already have the official dictionary translations of practically everything. What you do then is make sense of it, and you can easily tell where something doesn't make sense. You also turn it into much better English.

BOHNING: When you announced your resignation at Haverford, you knew you had a sabbatical year you could still take, but had you given any thought beyond that?

BENFEY: It was scary, because our third child, Christopher, had been born in October 1954, and he was seven months old. The others were two and four. We just went on that half pay plus Albert Wilson's gift.

[END OF TAPE, SIDE 7]

BENFEY: Rachel gave me the courage to leave. She convinced me that I shouldn't work under someone who didn't want me, who was suspicious of me. So I had an interview at Boston University and with somebody from Claremont Men's College. I was scared and nervous. Then a letter came from Larry [Laurence E.] Strong at Earlham College, asking if I wasn't interested in joining his department, and we went out there. At Haverford, people had said, "Whatever you do, don't go to Earlham." It's just like in England when I was told "Don't stay in America if you can help it."

One of the events that had occurred some years earlier was that Wayne Booth had been at Haverford, and later became very well known as a University of Chicago English professor. Wayne C. Booth's book The Rhetoric of Fiction (41) inaugurated a whole new direction in literary criticism. He was stolen by Earlham from Haverford, whereas Haverford had always felt it was way above Earlham. In earlier years they had taken people with Earlham degrees and gave them an extra year to prepare them for graduate school. Here were people moving in the opposite direction. That was quite a shock.

BOHNING: Before we say something about Earlham, what about that year with Westheimer? Did you purposely select Westheimer? You had the sabbatical year available.

BENFEY: Yes.

BOHNING: Did you look at different places, or was that specific in your mind?

BENFEY: I'm pretty sure I chose Cambridge to be near my mother and grandfather, and Harvard because of its prestige, and picked Westheimer because he was in physical organic chemistry. If you look at my little book, Introduction to Organic Reaction Mechanisms (42), it's dedicated to Ingold, Hammett, and Westheimer. They represented three very different approaches to

physical organic chemistry. I didn't know much about them when I went to work with them. Between them they gave me a marvelously broad education.

I had known about Westheimer because one chemical problem that I got fascinated with at University College was the racemization of optically active biphenyls whose optical activity was due to steric hindrance. I tried to figure out, from Ingold's teachings, what factors might have led to the observed order of racemization rates--size of substituent groups, inductive, resonance effects. That was the time I really did some literature searching and developed some ideas. I showed them to Ingold, and he rather liked them. Nothing came of that, but then I discovered that Westheimer had done calculations on the biphenyl racemizations. That intrigued me about him. I also had discovered he had worked on the nitration mechanism. So he seemed the right person.

At the same time, Leonard Nash was one of the people in the case study approach. I planned to work part time with him, which I did, because I had the idea that I could turn the organic structural theory story into a case study. Perhaps Leonard even suggested it. I never went to Conant to talk to him about it because by then he was president of Harvard. I never saw him that year. I just went ahead and drafted the whole thing. I sent it in to Harvard University Press, and they showed it to Conant. It turned out that Conant had attempted to do the same thing, and decided it couldn't be done, so he wasn't about to admit that I had done it. [laughter] Luckily, that was the era when all the publishers were looking for supplementary texts to enrich the freshman curriculum. Harold Hart was editor of the series published by Houghton Mifflin. So my book became number one in the series (43), because the manuscript was all there, and it was exactly the kind of thing they wanted, quoting from the original papers and then commenting on them. That was exciting.

From Vital Force to Structural Formulas has been very well received. It's a book that Arnold Thackray still recommends in his classes, and Sy [Seymour H.] Mauskopf used it at Duke until it went out of print again. When Houghton Mifflin let it go out of print, it was picked up by the ACS, but ACS has now let it go out of print also. My guess is it will come back into print again sometime. [Beckman Center is reprinting it in 1992.]

Sometime earlier, I met Gerald Holton because my sister was married to a physicist who knew Holton when he was getting his Ph.D. at Harvard, and they got us together. Holton and I became friends and he asked me to prepare the volume on Classics in the Theory of Chemical Combination (7) for an English language series somewhat like the Ostwald Klassiker der exakten Wissenschaften. The book published the major papers discussed in my From Vital Force to Structural Formulas. The two came out pretty close

together, in fact. It became volume one in Dover's Classics in Science series that Holton put out. It was reviewed in the Journal of Chemical Education (44). I. Bernard Cohen of Harvard liked the Classics. He called it "both classical and modern," which is a nice phrase.

BOHNING: One thing that chemists haven't done, for the most part, that physicists have done, is that physicists seem to have pursued this kind of thing in their writing a great deal.

BENFEY: What kind of thing?

BOHNING: Physicists tend to have a more historical approach and more historical interest in their work, than chemists tend to do. And physicists have written, in the historical sense, and in the popular press, and everything else, for a long time.

BENFEY: They turned philosophical with the quantum and relativity impact. The biologists later had the DNA challenge, which made them face all its ethical and other implications. Chemists have always had much too much to do without a major conceptual crisis. There were always so many open-ended possibilities. And still there is nothing to challenge chemistry's basic theoretical structure, to make chemists rethink the whole meaning of what they are doing. Physics had to start all over again, looking at its foundations. Perhaps now the environmental movement is shaking chemistry's foundations.

BOHNING: In this same time frame, and I'm pretty sure we're still in the Haverford period, you had this article in the Bulletin of Atomic Scientists (45).

BENFEY: That came out of my SSRS (Society for Social Responsibility in Science) activity. I gave that as a lecture at one of our annual meetings, and somebody suggested it be published. So out of these three experiences, the Cassirer translation, history of chemistry, and this Bulletin article, I really gained the feeling that I could write publishable stuff. It gave me considerable encouragement.

BOHNING: By this time your work in the laboratory, as it were, has diminished, given the other activities.

BENFEY: Well, I don't know if it diminished. It was very little throughout the Haverford period. Of course, with Westheimer I went back into research of a significant kind. He gave me a problem. It was something left over from his earlier studies, and of course it's connected to the biphenyl problem. It was the bipyridyl problem. That's when I first became acquainted with the Beckman DU. You set the wavelength, determine the absorption, and plot the graph yourself. That was an esthetically very pleasant technique. Compare that with what D. J. Millen had to do in London in the forties, when they got the photographic spectrum plates for the benzene spectrum, and had to match each line for intensity against master graynesses. Unbelievable. During my Ph.D. days, we were still using hand calculators to calculate rate constants, for instance, turning the crank to speed up the repetitive calculations.

I guess I should mention one other thing that goes back to the British days, and that is my one experience in chemical industry. One summer I worked in the lab of a yeast factory, analyzing the amount of dry matter in the yeast. I think somebody else was doing the alcohol content. It was a company making baking yeast and brewer's yeast. My work was a very repetitive process, because we were always using one of two crucibles, determining its weight empty, with the material before drying, and then after drying. It was obvious to me that you could speed the calculations enormously by preparing tables using the crucible weight and a range of initial sample weights to obtain our answers. I was tremendously proud of having worked out these tables for each crucible. When I showed it to my fellow workers, I was amazed to find that they didn't like it at all, because I was destroying their livelihood by finding a quicker way of doing things. [laughter]

BOHNING: I was struck by your comment about doing the spectra in the way you did, because I also did a number of things like that. To me there was always something magical about isosbestic points.

BENFEY: And for me. Theoretically it's perfectly obvious, but the fact that it actually happens is amazing. [laughter] Westheimer was really delighted with this work, because it completed something that he had published a number of papers on. It separates out the hydrogen bonding of an added proton from steric repulsion, and the resonance tendency to planarity as against nonplanarity. It was closely related to what I'd been fascinated with in the biphenyl problem.

I did continue this work at Earlham and had a student, James [W.] Mills, [now professor at Fort Lewis College, Colorado] work on the next, more complicated case, where the two pyridyls are tied together, to try and keep them flat. In the

o-phenanthroline complex, when you put a methyl on one nitrogen it really blocks planarity and the molecule has to be distorted. Protonation then is extremely hard. Even with o-phenanthroline itself, with one proton you get strong hydrogen bonding, but there isn't room for a second proton without distortion, so you can't get the second hydrogen on until you're way up in concentrated sulfuric. That paper was accepted by the Journal (46). That was my last experimental paper. By then I'd become heavily involved in chemical education and the history of chemistry.

BENFEY: Maybe we should stop at this point.

BOHNING: We can continue next time with the Earlham phase.

BENFEY: With Earlham we get into CBA and Brazil. And the Advisory Council on College Chemistry, where I met up again with Charlie Price. Also the Japan links, and Chemistry magazine sending me again to South America.

[END OF TAPE, SIDE 8]

INTERVIEWEE: O. Theodor Benfey

INTERVIEWER: James J. Bohning

LOCATION: The Beckman Center for the History of Chemistry

DATE: 5 June 1991

BOHNING: Ted, the last time we finished by discussing your move to Earlham College, but we haven't said anything about those seventeen years that you were there. What was Earlham like when you got there in 1956?

BENFEY: Earlham is in Richmond, Indiana, a small town which, I think, can be described as having been rather reactionary in politics in those days. Its local newspaper, The Palladium Item was violently anti-United Nations. If it mentioned it at all it spelled it in small letters, and it was also violently anti-British. It had a crusade against drunken driving so that it would put on the front page anyone even arrested for drunk driving. If the person was later acquitted, that would be a small item on the back page. Just to show you how reactionary they could become, during the Berlin Wall era, they actually built a little replica of the Berlin Wall across Main Street to make you vividly realize what was happening, in the confrontation with Communist Russia. At times the town and gown tension became quite serious. I was told that at one point the president paid his faculty in dollar coins, so that the faculty who would be spending the money in town would make the town people realize just how important their local college was to the community.

The faculty felt something like a missionary spirit. They really were a very educated, intelligent group in a community that had very little awareness of them. They were active in what cultural activities there were. We had a branch of the University of Indiana on our campus for night school students which has now expanded and has its own campus in Richmond. The faculty also felt somewhat inferior relative to the colleges on the east coast and wanted to aspire to or rise to them. I think I've already mentioned that in earlier days students who had completed their work at Earlham often went for one more year to another Quaker school like Haverford to prepare for graduate school. But during the years I was there, this changed rather significantly. As I mentioned, Wayne Booth and I left Haverford for Earlham.

I don't think I described the tremendous vitality of Haverford when its young president, Gilbert White was there. He was very young. Just to emphasize his youth, one year when commencement time came he couldn't attend because he had mumps.

[laughter] Similar transformations occurred at Earlham through the new president who took over about a year after I arrived. Landrum Bolling was a former newspaper reporter and political scientist who had tremendous energy and vitality. He was a constant traveler. The college rose very rapidly in public esteem, so that it has become one of the leading colleges.

Many of the older faculty, on the one hand, couldn't believe the change in the college's stature, and on the other felt an increasing pressure to excel, to do more research. They felt judged by the newer faculty. So there developed to some extent a separation between the older faculty, who were largely a group of Quakers, and the newer arrivals. Many of the older ones had been in civilian public service camps as conscientious objectors. They worked in the forest service and in mental hospitals and had endless discussions in their camps. With very similar ideals they were a tightly-knit idealistic group, and there was this tension between them and the world-class faculty who were being recruited who had less interest in Quakers and less interest in the community, often more interested in just developing their career and the college's name through their achievements.

The students were a delight because for many of them the intellectual atmosphere of the college was very exciting. It was markedly different from their own environment. Haverford was a men's college at the time, and half of the students were the sons of professionals. Earlham was coed, and many Earlham students came from rural backgrounds or small towns. One could see them blossoming and thriving under the stimulus of the new ideas. There were fields that were opened before them that they didn't know existed as possible for their careers. There's the famous Knapp-Goodrich study about the origins of scientists which showed that a remarkable number not only came from small midwestern colleges, but came from farm or rural backgrounds and found in science the first area most accessible, an exciting area in which to make their mark in the world. Earlham was one of the colleges that was listed as a significant producer of scientists.

BOHNING: What was the chemistry department like when you arrived?

BENFEY: The chemistry department was headed by Laurence Strong, who had come from Kalamazoo, Michigan, where he had taught. He had worked earlier in the Harvard Medical School on blood fractionation. One of his papers, with Edwin C. Cohn, was listed recently by the Institute for Scientific Information as one of the highly cited papers (47). Larry Strong recently took early retirement, and has gone back to significant research with grants from the Petroleum Research Fund and others. He is publishing papers in physical chemistry, thermodynamic properties of

substituted aromatic acids, determining entropy and enthalpy data from conductivity studies. It is very significant and interesting. He was a mature scientist who, at the same time, was very eager to do something new and interesting in chemical education. We very soon started discussing what was wrong with the general chemistry course, and decided the problem was that there were too many subjects, going all over the map. We began to think that we ought to develop a curriculum where each course would deal with a much more concentrated group of subjects. So we developed a new curriculum which began with a course called "Particles of Chemistry," and then "The Covalent Bond" which was a freshman exposure to aliphatic organic chemistry and other non-ionic compounds in the context of bonding. That was followed by a course on ions which included an introduction to inorganic qual and quant. There was a course on chemical energy, and then back to organic in the junior year with a course called "Resonance and Aromaticity." Then there were the advanced courses. Advanced organic became "Kinetics and Mechanism."

The third faculty member at that time was Wilmer [J.] Stratton, who is still active at Earlham. He was treasurer of the ACS Division of Chemical Education for many years. He has recently developed an analytical laboratory on campus doing analyses of environmental interest for the local community, and using students for it. Larry Strong uses students for his research also.

BOHNING: So there were just three of you then?

BENFEY: Initially, yes. Then we picked up a fourth person, Reino Hakkala. He was from Finland and later taught in upstate Michigan where Finish communities concentrate. He was also a rather competent artist. The more permanent person we hired who is still at Earlham who was for several years dean of the faculty with interest in faculty development was Gerald Bakker, another organic chemist, from Hope College. Joe Rogers [Joseph E. Rogers, Jr.] joined us for some years also. He later moved to ACS headquarters, now heading the Petroleum Research Fund.

It was through our developing the college curriculum, focusing on concepts rather than the classic divisions inherited from the nineteenth century, that Larry Strong was invited to a conference on precollege chemistry at Reed College in Portland, Oregon, in 1957, organized by Harry Lewis of the Institute of Paper Chemistry, where a group of disgruntled people were worrying about high school chemistry. There Larry proposed that the high school chemistry course, instead of just being a series of topics that the colleges insisted be covered, should have coherence and structure. This is related to Jerome Bruner's concept that the only way one can really understand a subject is

by tying data (facts) and details into a broader conceptual structure. Larry was encouraged to see if he could get a grant to develop such a high school course. Out of that came the Chemical Bond Approach [CBA] Project (48). The CHEM Study [Chemical Education Materials Study] Program soon followed. The ACS helped organize that in 1959. The suggestion then came to the American Chemical Society they needed a journal to stimulate students who had become excited about chemistry through these new high school experiences. Chemistry magazine was bought from Science Service initially just in order to prevent another organization having a journal with the name "Chemistry". They then decided to launch it as a journal for high school students and teachers and they asked me to be the editor.

BOHNING: There are a number of other things I want to cover, but maybe we should pursue that link in more detail. When you started the curriculum, you talked about the second course being "The Covalent Bond". Was the only available class text the one written by [Leallyn] Clapp (49)?

BENFEY: The book hadn't come out at that time, but we knew that Brown University under Clapp had been developing an organic course at the freshman level. Of course, the difference is that Brown could assume significant high school chemistry. We could assume no high school chemistry whatsoever. We were much aware of that, and then Clapp became one of the directors for the CBA program together with M. Kent Wilson, who later became head of chemistry at the National Science Foundation, Arthur Livermore, who became a key person in the AAAS, and Larry Strong.

BOHNING: What was the time frame? You went to Earlham in 1956, and you said that almost immediately you and Strong started revising the curriculum. When did you first introduce your new curriculum?

BENFEY: The National Science Foundation got interested in all this because of Sputnik. Physics had started before Sputnik in trying to dream up a new program and they developed the PSSC [Physical Science Study Committee] Project. They received large grants from NSF. Chemistry was the next major group, and because of the concern to lift American chemistry to keep ahead of any Soviet challenge, there was also set up a group called the Advisory Council on College Chemistry, which Charlie Price for some years headed. That's where he and I met again and I discovered that he was a Quaker. They had a conference and published a report on the new curricula. We published a paper on our program in that report in 1958 (50).

BOHNING: So within two years you already had it published.

BENFEY: We were not "accredited," that is we were not on the American Chemical Society list of approved chemistry departments. We asked to be approved when our fourth person came on board. The ACS Committee on Professional Training [CPT] came and examined us, and we totally satisfied them on every count, except that we were involved in this massive transformation, and they were skeptical whether we should do that. This was all right for a university, to experiment with one section because that doesn't endanger the whole department. On the other hand, in their publicity CPT always says it does not look at curriculum, it doesn't judge curriculum, it only looks at such factors as facilities and qualifications. But still they said they could not put us on the approved list until at least the first group of graduates under the new curriculum had been produced. So at the end of the fourth year, we produced our first graduating seniors on the new plan, and three of them won National Science Foundation Fellowships. ACS examined us again, and this time we were put on the approved list. The committee almost immediately started using us as proof that they believed in curricular experimentation. We've had our battles with the [ACS] Committee on Professional Training.

BOHNING: You're not alone there. What did you do for textbooks when you revised the curriculum?

BENFEY: We now realize we should have created our own. What we did was to use the most amenable one, and I tried to teach the way we were taught in England--that is, not based on textbooks--but I found that extremely hard. Even though we had the new names and new concepts, we tended always to slide back into being textbook courses. But we did feel freedom to move far and wide, and to insist that the students move in those directions.

BOHNING: You didn't write any specific text?

BENFEY: We did produce the first-year lab manual which I have almost forgotten about (51). We developed new labs, and I had also done that sort of thing at Haverford with T. O. Jones. That's very common.

BOHNING: Beyond the first year, were the laboratory experiments more your own or were they more standard? Let's say, in organic and qual and quant.

BENFEY: The qual and quant experiments in the Ions course were often of course modified from standard experiments except that in that course the students worked on synthesizing a compound and then analyzing it, rather than just doing syntheses and analyses as totally unrelated experiments. The course emphasized coordination chemistry because you could build on the chemical bond studies from the previous course. For the energy course, Larry Strong and Wilmer Stratton did develop one of the little paperbacks called Chemical Energy (52). That was in the series put out by Prentice Hall.

BOHNING: Yes, I remember the series.

BENFEY: The series provided a lot of supplementary and overview material to go along with the text. We did use that.

BOHNING: Putting the CBA just off to the side for a moment, once you were on the ACS approved list and what you were doing at Earlham was publicized, what kind of reaction did you start getting from other institutions?

BENFEY: We were always very warmly received, and people recognized us as not being on the fringe but as very sound people. We were active in the Division of Chemical Education. Then in 1963, I took over as editor of Chemistry. Our contact with the big university departments was always more tenuous because they didn't participate very much in the Division of Chemical Education except for some key individuals who were very wonderful, impressive people who supported us. Then you had this phenomenon of people being stolen from the college campuses to head the freshman programs of universities. Universities were recognizing that their freshmen were really suffering from the research-oriented faculty. These new university faculty members would not be judged by the usual publishing or research standards but by the quality of their educational work. Gil [Gilbert P.] Haight went to Illinois from Swarthmore at that time, and there have been a number of others who've played that role. I was invited to move to the University of New Hampshire and I did consider doing that, but it didn't appeal to me. I'm not good at mass manipulation of students, multiple sections, and so on.

BOHNING: Were there many imitators?

[END OF TAPE, SIDE 1]

BENFEY: That's a good question. There were no imitators in the sense that others took on our pattern. In fact, we don't know of anyone who has switched to a conceptual approach in their program. The general feeling was that you just can't move that far from the textbook and the accepted practices. There was also the problem of student transfers. We always had to write long letters explaining that the work they had done up to the point when they were transferring was equivalent to a given number of conventional courses, and we sent them the exams.

BOHNING: I know there are a number of other places that did as Brown did in the sense of using organic as the first course. Bucknell started doing that very early. At Wilkes we talked about changing things around in the curriculum, but there's a tremendous activation energy needed for doing that.

BENFEY: At Guilford we found it extremely hard to move far from the accepted patterns although we did do some, because we had a marvelous program in Greensboro, North Carolina, an agreement between the area's colleges, as well as the local branch of the University of North Carolina, and the originally largely black North Carolina Agricultural and Technical University, that students from any of them could enroll in any of the others. So courses had to be essentially equivalent to make that possible. But overall, between CBA and what we were doing on the college level, I'm quite sure that we had a significant impact on rethinking, stimulating, developing new ways of teaching and presenting chemistry.

BOHNING: To continue our time frame, what year did you say was the conference that Strong went to in Oregon where the seeds of CBA were sown, so to speak?

BENFEY: That was in 1957. NSF then gave an initial grant, and another conference--of high school and college teachers--was held in Portland, Oregon, in 1959 to begin the CBA writing process.

BOHNING: You had published the new college curriculum by 1958.

BENFEY: Right. In the summer of 1963 I was teaching CBA to teachers at Reed College in an NSF financed summer institute to train CBA teachers. In January 1963 I had already gone to Brazil teaching CBA to Brazil's teachers.

The Reed College conference that led to CBA was on high school chemistry. I'm just noticing that we got a grant from SmithKline and French to support our college laboratory manual. I'm pretty sure that the curriculum experimentation report came from an ACS meeting symposium. The committee on curriculum and advanced courses of the Advisory Council on College Chemistry invited reports on new curricula from the University of Illinois, MIT, Brown, Beloit (a combined physics and chemistry course (53)), Earlham, and Harvard, where they were doing something called the Harvard experiment. This was for majors, with E. B. Wilson, E. J. Cory, Westheimer, and others putting students through the basic course program in two years so they were ready for research by the time they were juniors.

BOHNING: I'm really amazed at the time frame. Very few years elapsed here, because in 1961 you were already talking about it in Ireland, and you were lecturing about chemical education and the CBA approach starting in 1961.

BENFEY: Before that we were training teachers. It was an absolute conviction that you can't just hand out materials. What we wanted the students and teachers to do was to think about the concepts and how they related to data rather than memorize. Both students and teachers always tried to find the easy ways of coping with material. So we had trial schools all over the country. We had regional conferences. There were six-week long summer institutes. There was one at Reed College and one at Kenyon that I participated in. At Earlham we had more than one, one of which I directed. These would try out the material and we'd get feedback. There was an enormous interest all over the world, and we reported then to the European high school teachers in Dublin, who were organized by the Organization for European Economic Cooperation, which has now become OECD, the Organization for Economic Cooperation and Development. Then the Brazilians started getting preprints. By 1963 CBA had been published, and they were working from page proofs to translate that. Brazil was doing it under Ernesto Giesbricht, and Japan was translating CBA under Bunichi Tamamushi. And the Spanish were translating. I've had close contact ever since with the Brazilians and the Japanese, who treated us superbly. I spent two weeks in Brazil in January 1963, which was their summer, teaching the teachers at the aeronautical college at San José dos Campos. Even though the teachers had stated that they could understand English, a number of them could only follow written English and found spoken English very difficult. There were teachers from the Santa Katarina area in southern Brazil. That's a large German community, so large that it tended to be pro-German even when Germany went Nazi. Its schools had been largely in German. The Brazilians had to stop all this instruction in German, and insist on Portuguese instruction to get them integrated into the rest of

Brazil. I discovered that if I would use German words after I used English key words their faces would light up, they would catch on to whatever I was talking about.

BOHNING: When did Chem Study start? Where was most of your support coming from for the CBA?

BENFEY: CBA was mainly supported by the National Science Foundation. But there were a number of smaller grants. I should say that the laboratory component was developed through the genius of Tony [H. Anthony] Neidig at Lebanon Valley College. That's the college from which GE's famous plastics engineer, Daniel Fox, graduated whose papers we [Chemical Heritage Foundation] now have. One of their key emphases was the continuous variation method. We used a series of test tubes and varied the proportions of two reagents which usually produced precipitates. You could measure the precipitate's height in the test tube, and could get from that stoichiometric equivalents, since you could see what proportions would produce the maximum precipitate. That way you could quickly get a feeling for what stoichiometry means and what the law of constant composition means. Furthermore, it has tremendous pedagogical advantage in that with many of these experiments, a group of students would divide up the measurements. There were conductivity studies and all sorts of other approaches. We also developed these in the ions course. Students would divide up the work, and each student would then mark observed data on a group graph that was posted. Students would quickly see if their own results were way off. Instead of just waiting for a bad grade, they would tend to redo their measurements. They clearly became aware of curve fitting and seeing patterns emerging and checking their work for this group activity. This broke away from the awful sense of isolation that students so often have that they're just being judged by a teacher who only looks at the final result. They were getting instant feedback.

BOHNING: Yes, I used similar experiments at Wilkes.

Let me come back to the question I asked about CHEM Study. CBA was first.

BENFEY: CHEM Study must have been organized beginning with discussions in the ACS very soon thereafter. For one thing, there was all this federal money around. CBA had hoped to have ACS support, but I guess ACS felt we were too radical or we were already launched and were too set in our ways before they had a chance to give us input. So they organized an impressive national advisory group headed by Glenn Seaborg and George

Pimentel. J. Arthur Campbell from Harvey Mudd College became the head of it. It's interesting that Art Campbell became a Quaker late in his life. [CHEM Study was launched by the ACS in 1959.]

BOHNING: Really? That is interesting.

BENFEY: They, as everyone I think agrees, were much less innovative in repackaging chemical material. But it had a lot of prestige and a lot of money, and was much closer to accepted practice and more easily accepted and used by people trained in standard university and teacher-college training programs. So that received by far the greater adoption and was translated and used in other parts of the world far more than CBA. It has continued to be published in several new editions through today. Herbert Bassow of Germantown Friends School who has helped us at the Center here is one of the latest authors of the new revisions.

BOHNING: What happened to CBA?

BENFEY: CBA was published by McGraw-Hill, and Leallyn Clapp was in charge of the second edition. There was a second edition in xeroxed form which was used by a number of schools, but McGraw-Hill didn't want to take it up. So only one edition is in print. Larry Strong has hinted that the second edition would probably be deposited here in our archives.

BOHNING: What was the response of the Chem Ed division? Did they fall more in line with the party line of ACS and CHEM Study?

BENFEY: I don't think so because the Division of Chemical Education, more so than now, tended to be at war with the ACS. There was considerable distancing, and that was before the ACS had a significant education office which also was developed as a response to the emphasis on education, the need for it, and funds that were available. So that slowly changed. The Chemical Education Division incorporated itself and was publishing its own journal, independent of the ACS journals. There was rivalry between the people who actually were in CBA and in CHEM Study. The CBA people felt that there was a certain disdain--that CHEM Study people were looking down on us. We felt it particularly with Art Campbell. He had this strange way of making you feel that he was clearly a successful person. But he mellowed with the years. Chemistry magazine used people from both groups and others. My advisory board helped me in creating a magazine that informed and stimulated teacher and student. I never sensed any

tension in the group that focused on rivalry between CBA and CHEM Study. It was very exciting working with leaders on both sides. Glenn Seaborg, who was head of the Atomic Energy Commission, let us use many of his public lectures as head of AEC in modified form, and constantly cooperated with us.

BOHNING: Let's explore the Chemistry magazine aspect. You said that is was purchased by ACS from Science Service so it would have its name. Who made that decision? What was the impetus behind that?

BENFEY: Richard Kenyon was the head of ACS publications. It is my understanding that it was a board decision, just to acquire it. Under Science Service, it was small, Reader's Digest size. It had earlier been a weekly called the Chemistry Leaflet. This originated the same year as the Journal of Chemical Education in 1927. It was put out by Pauline Beery Mack at Penn State. We acquired a complete run of Chemistry's forerunners and I hope ACS still has it. They should give it to us. Initially, it was geared carefully to the high school curriculum which was pretty much nationally set. You studied the halogens one week and the alkaline earth metals another. The Leaflet would come out speaking to those topics and having interesting information about each. ACS had no intention of continuing anything of the style of the Science Service product. It just wanted the name.

Then it set up an advisory group to determine what to do with it, and the group decided to move in the direction of serving the top forty percent of the high school students taking chemistry. That was my mission, and that was the magazine's downfall later because of the growing attack on elitism. Anything being produced had to serve everybody. To continue the magazine at that time without major changes was costing what ACS felt were excessive amounts of money. That was the era of accountability, that each unit had to be self-sufficient. Because we were under the Publications Division and not under member-supported services (dues-supported services) we were meant to break even, which included our paying for our proportion of the overhead of Richard Kenyon, the building, and so on, in part in proportion to the number of our subscribers. We had far more (close to 30,000) than many of the ACS journals, and many of the journals were edited in offices all over the country, whereas I had my main editorial group at ACS.

My first year as editor, I spent on sabbatical at ACS in Washington, making that a condition of my taking over. When I returned, ACS agreed to pay one-third of my salary so that I could have a reduced teaching load. I also insisted on this, which no other editor had. But my situation was very different. We didn't just publish articles submitted by others. We were

doing a lot of creating of copy. I had a managing editor at the ACS together with an editorial assistant and a secretary. We also used the ACS publication's supporting services. The artist, Joe Jacobs, was wonderful to work with. He designed the covers and initially also created the whole design of the magazine. Joe Jacobs is the person who deserves the major credit for the layout. One of our innovations was to use handwritten doodles in the wide margins, which livened up the magazine.

[END OF TAPE, SIDE 2]

BOHNING: Why were you selected as editor? Was that because of your CBA activities?

BENFEY: The invitation came completely out of the blue. I had no idea the ACS discussions were going on. There must have been a combination of factors. CBA of course was focused to serve the high school level. So there was CBA, Haverford, my British education, and the number of papers I had already published. In 1958 I had organized the centennial symposium on the structural theory of organic chemistry, jointly sponsored by the ACS organic and history divisions. (The benzene symposium came in 1965.) I had published enough for people to see I could write. They probably recognized the range of my interests.

BOHNING: What were the dates that you were in that position?

BENFEY: 1963 to 1978, fifteen years.

BOHNING: Fifteen years. I remember it as being an excellent journal. Somewhere I have a number of years worth packed away in a box right now.

BENFEY: It really got tremendous acceptance. We inherited about five or six thousand names and it went up to the upper twenty thousands, and occasionally up to about thirty thousand.

BOHNING: That's excellent. Our local ACS section used to give subscriptions to outstanding high school students. I imagine that was probably a common outside activity.

BENFEY: This was one of the criticisms, that it went mainly to teachers and not so much directly to students. We tried to keep the price down by combining the July and August issues when costs were zooming up. Later on, we also combined the January and February issues. We were down to ten issues a year.

After we launched our publication, many other countries followed. The German chemical society produced Chemie in unserer Zeit for its students in 1967 and in its first issue mentioned the appearance also of Canadian Chemical Education and the South and Central American Revista Iberoamericana de Educacion Quimica (published with the support and assistance of the ACS), and stated that the model for all of them was our journal Chemistry. The French Le jeune Scientifique appeared, as well as the South African Spectrum, and the Japanese Gendai Kagaku whose editor, Atsushi Ueki, I count as one of my valued friends.

BOHNING: I've forgotten the date, but I know it no longer exists. Is that correct?

BENFEY: That's right. The change came about because an oversight group was set up by ACS, and it decided there should not be an external editor. Richard Kenyon died, and a new man became head of publications. An evaluation group for all ACS journals evaluated one journal after the other, and they decided to rethink the whole purpose of the magazine. They thought it could be done in-house. Eugenia Keller was a superb managing editor for most of my years with Chemistry, and she could write wonderfully. One of her great articles was on the nylon story (54), but she created many others also. She was replaced by Patricia Morgan. I worked briefly and satisfactorily with Pat. But it was made pretty clear that my presence was no longer wanted. ACS suggested some conditions under which I could continue which I turned down. Chemistry was changed to Sciquest. I was very thankful that the magazine Chemistry under ACS was always my magazine, and that it didn't go on with a different editor and purpose to confuse readers. Sciquest didn't last long. Then another committee shifted the magazine over to the ACS education office and made it a dues-supported activity. They poured money into it, and decided it really ought to become a mass magazine for students who bought it in class lots. It had significant success then, up to the fifty thousands. Given the huge number of students taking chemistry, that's still very small. It's now called ChemMatters, and ACS also puts out a quarterly teacher journal called ChemUnity, playing on the word community.

BOHNING: Do you think that those changes also reflect a larger

change in approach toward education during the same time period? I don't want to call Chemistry an intellectual magazine, but it certainly delved into philosophical matters and historical matters, and many of your editorials really made people think about things in depth and in a broader sense. I've looked at these new descendants of Chemistry, and I wonder if they're more pragmatic.

BENFEY: There are no editorials. The magazine is much more designed to get immediate attention, to be more like a news magazine. They have good people, very imaginative people. The editor is a high school teacher. I don't know him. There is a good supporting staff. They do well what they do. They take interest in topics that are currently in the news--as I did too, of course. But the magazine is not making a significant effort to make the student go in depth into the implications, and particularly into the structure of chemistry as an intellectual discipline. They do emphasize the science and society connection, but I did that, too, to a very great extent. My feeling was that, since the courses dealt with the internal material of chemistry, Chemistry magazine could fascinate the students by showing all the interconnections. So I devoted all of my concern to finding unusual interconnections and emphasizing them. The magazine was not only read by high school teachers. I had letters from professors in graduate schools and medical schools, and all sorts of people, because they found Chemistry was a useful way of keeping up with the whole of chemistry whereas technical journals were beyond them or of no interest. Chemical & Engineering News had a strong academic intellectual opposition who disdained the industrial orientation of C&EN. Dick Kenyon and I agreed that what the chemical community really needed was something like Scientific American but focused on chemistry. Psychology Today was doing that for psychology, something that you could pick up on the newsstand. Over the last few years the ACS has come up with a magazine that is moving in that direction, Today's Chemist, edited by Pat [Patrick P.] McCurdy. That really is the closest to what Chemistry magazine was doing in intent and content.

BOHNING: I only wish Today's Chemist would be in a more standard size. That big square size is very unwieldy.

BENFEY: Why don't you tell Pat McCurdy that when he's here on July 18 when Fred Aftalion is speaking here and his book is coming out?

BOHNING: I don't know why they ever went to that kind of thing.

BENFEY: To stick out.

BOHNING: Yes, but it can also be annoying. You can't put it in a file. You can't put it in a box.

I want to talk about some of the other things you were doing while you were still at Earlham, but maybe this is the point to talk about your activities in HIST, because you were chair of the division in 1966 while you were still actively involved in CBA and editor of Chemistry. You had already run some symposia prior to that.

BENFEY: Yes, the one on structural theory (1958) and the benzene centennial in 1965 (55). The structural theory symposium was interesting particularly because I had met Herbert C. Brown at Purdue sometime earlier. I remember Larry Strong and I driving from Richmond to West Lafayette to the Purdue campus, getting so involved in discussing curricula and the implications of all our exciting ideas, that we missed the turn and it took us a long time to reach Purdue. I met Herb Brown there and was charmed by him. Somehow I discovered his interest in history, and with very little consultation of other people, I asked him to give the keynote speech for the symposium on the Centennial of the Structural Theory of Organic Chemistry. It was a remarkable lecture about the significance and the legacy of Kekulé and Couper's work (26).

A few years earlier I was rediscovering Archibald Scott Couper who independently of Kekulé developed the structural theory. In fact, he was the first in organic chemistry to draw organic structural formulas the way we do with bond lines between connecting atoms. Interestingly, he became one of Robert B. Woodward's heroes, and R. B. W. mentions in his Cope lecture how Couper was instrumental in developing these formulas and the structural theory. I haven't discovered if R. B. W. knew about my work because I was working on Couper during my year at Harvard or whether his own research led him to Couper. I wrote the Couper biography for Eduard Farber's Great Chemists (4), and then was asked to write the Lothar Meyer piece for the Dictionary of Scientific Biography (56). I suggested to the editors that they should include Couper, so they asked me to do that also (5). I also wrote shorter DSB biographies of [F. K.] Johannes Thiele and Edward Wight Washburn (57).

So I was involved, and I wrote. Couper was really rediscovered by Richard Anschütz, Kekulé's biographer. It's quite amazing how important for Anschütz that discovery was. He did the same for Josef Loschmidt's ideas and formulas. He published an Ostwald Klassiker reprint of Loschmidt's paper (58) and Leonard Dobbin put out an Alembic Club reprint of Couper's

papers. This Scotsman also wrote a long paper called "The Couper Quest" (59). Couper had a nervous breakdown soon after his publication came out in France. [Charles Adolphe] Wurtz had procrastinated before submitting the paper, so Kekulé got in first. Couper lived another thirty years but never again worked in chemistry. He just lived in his home, with his mother. It was quite tragic. It's interesting that I wrote about two underdogs, Couper and Lothar Meyer. Couper was overshadowed by Kekulé and Lothar Meyer by [Dmitri Ivanovich] Mendeleev.

The invitation to be chair of the HIST division came out of the blue. I felt honored. I had not been active in HIST governance, so I really had no sense of what could be done, what I ought to be doing, and I did very little. Sidney [M.] Edelstein was still the HIST secretary at that time. For years I had the feeling that he was not impressed with my performance while chairing the division, and I told him about that recently when we were talking about my hopes to study at the Edelstein Center in Jerusalem. He said, "Oh, Ted, don't worry, you're an intellectual. I'm an entrepreneur and a mover and shaker." No, I didn't play an active role in the division, but I spoke at enough of the division's meetings and published papers in history.

I was also interested in the creative process in the sciences. I published the translation of van't Hoff's inaugural speech (60) after he launched the tetrahedral carbon theory. Hermann Kolbe, a very influential German chemist, accused him, since he was teaching at a veterinary school, of mounting Pegasus, the mythical horse, and going up to Mt. Parnassus to view the chemical world from there. Kolbe was concerned that in chemistry we must stick to observable, justifiable data and derive our ideas strictly from them. He accused van't Hoff of reverting to Naturphilosophie and medieval mysticism, just dreaming, speculating. When van't Hoff was appointed to a chair at the University of Amsterdam, he chose as his inaugural speech, "The Role of the Imagination in Science." He listed one great scientist after the other and their creative interests, their activities as musicians, as poets, as dramatists, expressions of their creative abilities. I published one or two other papers on similar topics, including the Kekulé dreams (61).

The other history paper that comes to mind is a paper on Alexander William Williamson. For the Williamson ether synthesis he published one article using personal pronouns and one in the now standard impersonal passive style. I compared the two versions (62), and suggested that the papers probably represented the period of transition to the modern way of reporting research, and then speculated why scientists made the transition, which no doubt has to do with the feeling that science is universally true. Once data are discovered they have nothing to do with the particular context of the discovery or the scientists involved,

and the publication should reflect that. I guess I did enough in the history area that when the ACS became serious about looking at the feasibility and desirability of a Center for the History of Chemistry, I was asked to be on the committee looking into that feasibility.

BOHNING: Had you been a member of HIST before you were asked to be chair?

BENFEY: Oh, yes. I think it cost $2.00.

BOHNING: Considering how Sidney Edelstein managed the division, it almost seems the invitation must have come from him.

BENFEY: Yes, very likely. Although it never crossed my mind.

BOHNING: Because as I understand it, he operated that out of his office, he organized the meetings, and he literally ran the division.

You've been discussing many of the aspects of your papers in the history of chemistry, but one thing you didn't mention is your lifelong interest in geometry. You've written a number of papers involving geometry and chemistry. I'm quite fascinated by that because at one point in my life I went back to Plato's solids. While I never pursued it beyond that, I also had a similar early fascination with it.

BENFEY: It seems to be quite common because Woodward did, too. Buckminster Fuller loved playing with regular solids and made geodesic domes based on variations of them. I once briefly corresponded with him.

BOHNING: How did that interest in geometry develop?

BENFEY: Clearly it developed in high school, but maybe even earlier because I remember an occasion when I was sitting on my grandfather's knee. It must have been before I was ten, and he was explaining Pythagoras' theorem to me, so maybe it started there. I know that in England at the Watford Grammar School, we went through Euclid's theorems, one by one. I just loved the logic of it and the geometric intellectual play there. After the rather dreary high school chemical preparation and properties of one element after another, in the sixth form I discovered

structural theory, that you could write out structures and predict isomers and then go on to three dimensions and stereochemistry. I mentioned earlier that the one area of art instruction that I really enjoyed was church architecture, where we used compasses and rulers. I could create these drawings and they looked pleasing, whereas my freehand efforts looked pathetic.

Structural theory, to my mind, seemed almost synonymous with chemical geometry. If you love structural theory you love geometry, and you can explore it further and further. At one point I bought a set of plastic sticks and flexible plastic connectors which can connect up to six or even eight sticks. These "D-stix" were for my children, and they are still for sale for children as construction sets. Being a chemist, I first made a tetrahedron, and then I made an octahedron. Then I realized the tetrahedron had three triangles meeting at a point. The octahedron had four, so I asked what happens when five meet a point, and I created an icosahedron. I got very excited about that. I wondered if any chemicals used that geometry, and within a few weeks Robert Parry lectured at Earlham about boron, B_{12}, and the boron hydrides, which are based on icosahedral geometry. Of course, with six equilateral triangles you get a flat plane as in graphite.

Then I discovered that Euclid's last book goes through exactly that argument of triangles meeting at a point. You need three to create a solid angle. With three squares at each vertex you get the cube, and with four squares you already have the checkerboard plane. With five pentagons all you can have is the dodecahedron, and with hexagons all you can get is the flat plane. So there are only five regular polyhedra, whereas there are an infinite number of regular polygons. I got fascinated with this discovery of Euclid's thought patterns. Some people believe that Euclid built the whole of his Elements of Geometry to culminate in the regular solids in the last book. Ever since, I've been pursuing the intellectual history of the regular solids. I discovered Plato's Timaeus who builds the universe from the regular solids. At first there were the four Aristotelian elements, earth, air, fire, and water, which got linked then to the four regular solids because the dodecahedron had not yet been discovered. When the fifth one was discovered it didn't fit, but then they realized the heavens (according to the Greeks) were made of different stuff. There were twelve pentagons and that fits with the twelve signs of the zodiac. So pentagonal dodecahedra became the material of their heavens.

Then I found out about the rediscovery of the complete Timeaus in the late middle ages, and the excitement which that produced, causing the emergence of neo-Platonism because only the first part, the non-geometric part, of the Timaeus was known up to that time. The Scot [William] Davidson, teaching in Paris,

uses the regular solids in his book as templates, demonstrating that everything in nature such as crystals and plants is made up of geometric patterns (63). I've been pursuing this. When I became interested in the Orient, I noticed Joseph Needham saying that one finds no interest in the regular solids in China or even much interest in geometry. I discovered that was just plain wrong. What Needham should have said was that there was no interest in ancient China in deductive Euclidean-style geometry. But endlessly in Japan and China one is aware of their love of geometric pattern. Chinese window lattices and Japanese wrapping papers attest to that. I think many of Euclid's conclusions just seemed too obvious to the Chinese to require elaborate demonstration.

[END OF TAPE, SIDE 3]

BENFEY: In fact, there is a bronze spherical incised incense burner at the Imperial Treasure House, the Shosoin, in Nara, Japan, on which one can see twelve pentagonal units. It seems I was the first to point out the regular solid geometry in that piece or of any of their treasures for that matter. I am now quoted in a mammoth new book on the patterns of the treasures of the Imperial Treasure House (64), all of which come from the eighth century. I've published my thoughts about how the Chinese might have come across the dodecahedron because the usual assumption is that Euclid didn't get to China until approximately 1600 with Matteo Ricci (65). It's pretty clear to me that the way the dodecahedron was discovered was by using vines in basketry. When you use vines in basketry to make hexagons you produce flat areas such as chair seats and backs. Whenever a basket is meant to turn a corner, then you just leave out one strand, so you put a pentagon at the corner. Somebody must have asked what happens when you only use pentagons, and they produced the wicker ball, which is six intertwining equators used in ball games still all over southeast Asia. You find it in many museums. You find students using wickerballs in the Orient and they bring them here and teach their games to American students. I've learned how to make these balls.

Pottery is believed to derive from basketry, early pots being produced by making a basket and lining it with clay and then burning off the basketry. Probably the early patterns on bronzes were made by braiding a basket exterior around them that was used for marking and then incising the metal. So I've been pursuing this and looking for ways in which one can teach solid geometry to students. Since the introduction of the new math and logic into math courses, high school curricula have largely neglected any training in solid geometry, so that college

teachers in the sciences can't assume that when a student sees two dimensional representations of forms they can visualize the third dimension. I used the plastic sticks and connectors, the D-stix, numerous times to demonstrate this to teachers and education groups. When I was in Japan I discovered a physical chemist, a quantum mechanics man, H. Hosoya, and others who use origami techniques to train their students in the construction of the regular solids. A number of people have published articles on using paper construction to make regular solids in the Journal of Chemical Education and elsewhere.

In the 1960's joint U.S.-Japan chemical education seminars were developed, and Charlie Price was involved with them. They had a conference where about a dozen college and university teachers from each country got together. The first was held in Japan in 1964, then one in Berkeley (1968) and one in Minnesota in 1981. I attended the last two. At the one in Minnesota, the Japanese teachers taught us how to make star octahedra out of origami. I've been teaching that to my students in freshman chemistry and history of science, when I discuss regular solids. I learned how to make all of the regular solids, together with the older of my Japanese granddaughters. (My oldest son, Stephen, is married to a Japanese.) One day a little box came airmail to me from Japan--postage was seven dollars! It was very light, beautifully wrapped in simple geometric wrapping paper, typical of the Japanese. Inside were four cubes each made out of six different colored origami papers with a little note in Japanese. It said, "Grandpa, you taught me how to do this." Touching. I think one of those is hanging in the Beckman Center passageway now. So the regular solids have been a continual fascination. In the London Museum there is a metal dodecahedron with different size holes. It is believed to have been used as a surveying instrument. You hold it a certain distance from you until looking through the small hole in front and then the bigger hole, an object such as a tree just fits the opening.

BOHNING: Have you read Edwin [A.] Abbott's Flatland (66)?

BENFEY: Oh, yes.

BOHNING: I've recommended that to a number of my students over the years.

BENFEY: That ties in with Arthur Eddington's marvelous ways of explaining relativity by talking about flat fish on a spherical globe.

BOHNING: It's a fascinating interconnection.

Another thing I want to ask you about, and this covers a broader period of time, is the number of translations you've done. You mentioned van't Hoff earlier, and that goes way back.

BENFEY: The first one that dealt with the place of creativity was my translation of the complete Kekulé speech at the Benzolfest in 1890, twenty-five years after the benzene ring formula had been put forward by him (61). Then I went on to van't Hoff and collected other items related to scientific creativity. The first book translation I did, the one of Cassirer, came through Henry Margenau. I believe I mentioned his visiting Haverford and asking me to do the Cassirer book. I've always enjoyed translating because I'm sure it has to do with reawakening my childhood language. I found when I took a student group back to Germany, certain words that I hadn't used since I was five or six would just come back and appear almost like a long lost friend. It's an amazing experience. Similar to the rediscovery of smells, like those of the little villages in the Alps that I'd been to as a child and visited again with my family.

So I did the complete translation of Kekulé's speech. I emphasize <u>complete</u>; [F. R.] Japp had done almost half, and he used the word "dream", although the context, to me at least, meant reverie, daydream. I foolishly repeated his words because I used his translations whenever available and just added the rest. So perhaps I helped to perpetuate the mistaken concept that Kekulé was actually asleep when he had his chemical insights.

Three major translations have come much more recently, the first originating during my year in Japan, 1985 to 1986. I was interested in the history of both Japanese and Chinese science, and it's very difficult finding any good accounts of the development of Japanese science. [David L.] Swain, together with a Japanese scholar, [Masayoshi] Sugimoto, has recently written a major overview in English of early Japanese science through about 1850 (67). The period since then is very sparsely covered except in Japanese. So I was very excited while I was at International Christian University outside Tokyo as a visiting professor, to have a German visiting professor bring me a German translation of Masao Watanabe's book on the Japanese and western science (68). I was absolutely enthralled with it because it's a paperback and reads very easily. Almost immediately I started feeling that's something I must share. I first translated a section on the environmental impact of science in Japan where he shares some of his own feelings, but the rest of the book is a more didactic development of the social history of Japanese science in the last century.

It turns out that the author, who is a physicist, had helped create the history of science department at the University of Tokyo. Bunichi Tamamushi, the colloid chemist and educator who had translated CBA, was also very much involved in the development of that department. At the major universities, the retirement age for professors is very young. It was fifty-five at the leading universities, maybe it's edged up recently, and retirement pay is not enough to live on. So the assumption is you move elsewhere. It's a brilliant system by which the big universities can get new blood, new energy on the frontier of research and the eminent people with great insight and teaching and research ability move to the second rung universities and the third rung, where the retirement age is higher, and teach there.

The first time I went to Japan (in 1970 to 1971) my host at Kansei Gakuin University between Osaka and Kobe, was an eminent crystallographer, Tokunosuke Watanabe, who had retired from Osaka University and became Dean of Sciences at Kansei Gakuin. He died very recently. Masao Watanabe (no relation--the name is very common) was now teaching part-time at International Christian University, and he lived five minutes from campus, so we became acquainted. We had met briefly at the 14th International Congress of the History of Science held in 1974 in Tokyo and Kyoto. In 1985 to 1986 we talked about translating the book into English, and he said he had explored the question. It seems he had written to [Nathan] Sivin and [Gerald] Holton, but neither of them had any suggestions. So he was delighted when I proposed that I would do it. He speaks good English, and he reads English fluently. He knows if something is inaccurate. So I translated and he checked it. It was a very good relationship. It was similar to, it seems, though much simpler than Matteo Ricci's translating Euclid into Chinese around 1600 using several people who could translate a part of the work and having the translation checked by others.

I asked Holton where he would suggest I publish the translation. He supplied a list of possibilities, including university presses and the Institute for Scientific Information. I wrote to ISI because I had known Eugene Garfield for many years, and he very quickly accepted it for publication. I was constantly reassured that the official contract was on its way but it never came. One day I received a letter from ISI Press saying that the press section of ISI had folded, and they just hoped I'd find another publisher. I had gone quite a ways working with Watanabe, and he was very upset. I knew what would happen if I sent the manuscript to university presses because I created another manuscript many years ago and submitted it to Harvard University Press. The answer I received was, "It's very interesting but it needs a lot of work," and I could see this going on and on.

When I moved here to the Beckman Center and was dealing with the University of Pennsylvania Press, shepherding manuscripts through and negotiating with them, at one point I just sent them my manuscript and asked them if there was any interest. The head of their advisory committee, who was in biology and had worked in Japan and has graduate students from Japan, got very excited about it. So the Press accepted it. That's the book that came out first (69).

In the meantime, I had a call from Jeffrey [I.] Seeman, who's a brilliant physical organic chemist at the research center of Philip Morris in Richmond, Virginia. He said that Ernest Eliel had suggested I might help him in completing a translation of Vladimir Prelog's autobiographical memoirs. Prelog was at the ETH in Zurich. This was for Seeman's ACS series of books Profiles, Pathways, and Dreams. At the time it was just a segment of a large book of twenty of these reminiscences, from twenty living eminent organic chemists from all over the world. Prelog's was about eighty pages, but he kept on adding pages under Jeff Seeman's urging. The original translation was done by David Ginsburg of Israel, who then had one or several heart attacks. David Ginsburg didn't know his German too well and he didn't know English too well, so the result wasn't acceptable.

I took it on because I loved working from other people's translations as I had done for the Cassirer book earlier. It makes it much, much easier because the first translators had already done a lot of the checking of the technical words. Prelog's book (it eventually came out as a separate volume) is called, My Hundred and Thirty-two Semesters of Chemistry Studies (70). All twenty-two authors are now coming out as separate autobiographies in the ACS series. The only way Prelog could continue keeping his office after retirement was to continue as a student. So he continues to register for one course each semester, and he's gotten through a hundred and thirty-two semesters. I visited him in Zurich. Delightful. That book will be published in three to four weeks' time.

The third book which is running neck and neck with it and is to be published on July 18, is Fred Aftalion's book on the history of the international chemical industry (71). Fred Aftalion is a Frenchman, a high executive in French industry who worked for some years at Hercules in this country and South America. He speaks excellent English. His book is really the first major study of all of chemical industry from its origins to very close to our time throughout the world, including coverage of Japanese industry, eastern Europe, the Soviet Union, the Middle East, and South America. There is an extensive index that we added which in typescript came to a hundred and seventy pages of personal and company names. The book will be the first really usable way of pursuing the history of companies through all the changes they made, splitting up and being absorbed by other

companies, and so on. The Aftalion book is the second volume of the University of Pennsylvania Press series on the Chemical Sciences in Society, which is sponsored by the Beckman Center. Arnold Thackray asked me to take on the translation when I first discussed with him the possibility of working at the Beckman Center. A draft translation by a British journalist was available but quite inadequate. I don't know the name of the journalist. He refused to be listed as a co-translator.

BOHNING: That will be very valuable because today it's so hard to follow all these changes. Not all of the original names have been preserved.

BENFEY: Your assistant, Megan Thomas, pursued many of the puzzles left in the French version that we needed to straighten out to make it acceptable for our purposes. She was most helpful.

BOHNING: I'll look forward to seeing that.

BENFEY: This is to be something useful, both as a textbook in history of chemistry and chemical industry courses and for industry people and scholars. It'll come out both in paperback and hard back.

BOHNING: Let's talk some more about Earlham. Once the new curriculum was in effect, you were there yet for a number of years. Were there any further changes in the curriculum? Did it stay pretty static or did you tinker with it a lot?

BENFEY: Oh, we tinkered with it endlessly because you can always dream up a curriculum on paper, but it's in the teaching you discover what works, what doesn't work, what needs to be included, and what problems occur in the transition from one course to the next, given by another faculty member, and so on. So that was part of it. One of the temptations was to move ever closer to whatever text we found suitable. We found the [William L.] Masterton and [Emil] Slowinski introductory text very useful (72).

Richard Ramette at Carleton College has developed a text similar to what we were doing in our ions course and inorganic laboratory, so Wilmer Stratton used it. In the organic course we tended to use one of the big mammoth books so that I could pick and choose from it whatever I needed. The covalent bond course emphasized aliphatics and the resonance and aromaticity course

aromatic compounds. That is considered an old fashioned separation, but I find a great conceptual distinction between them. Recent textbooks mix it all up and try to convey most of the major concepts in the first six or seven chapters. For many years I used Morrison and Boyd and found it very teachable (73). I wrote the review for that in the Journal of Chemical Education (74). I was absolutely enthralled by the style of it. Obviously, that was the reaction of most of my peers. But the students found it hard to be told, "Now we're jumping to chapter six, so study all but the aromatic part. You'll study that in a future course." But we made them stick to it.

BOHNING: How much did the department grow over the time that you were there? You said you were the third person when you arrived.

BENFEY: There were four as a pretty standard permanent crew. Strong, Stratton, Jerry Bakker, and myself. The CBA activity warranted an extra staff person, and sometimes more than one, as well as occasional visiting faculty who helped with the teaching. Then we received a major grant from the Research Corporation which allowed us to add a faculty member to do research and to relieve us of some teaching so that the permanent staff also could do more significant research. Through that we got a Brazilian, Ricardo Ferreira from Recife, who is in quantum mechanics. He is very brilliant and is very active in chemical education. I published some of his pieces in Chemistry magazine on chemists who were also revolutionaries (75). Do you remember that series?

BOHNING: Yes. How many students did you have? What was the average number of chem majors in a graduating class?

BENFEY: These are the sorts of questions my memory doesn't have much information on. I think it varied between six and fifteen.

[END OF TAPE, SIDE 4]

BOHNING: We should probably talk now about the move to Guilford and how that occurred. I think we're at that point. That was 1973.

BENFEY: I think I mentioned that my wife, Rachel, is from North Carolina, and is a graduate of Guilford College. She then came to Haverford to teach at the Haverford Friends School. At Haverford a few years later was Grimsley Hobbs, also a Guilford

graduate and the grandson of a former president of Guilford. Even as a student he had talked about being a future president there. He was at Haverford as a graduate student in philosophical studies, and I met him there. He was already married at Guilford. He had served in the Coast Guard during the war and had come back to study. He was an older student, and Rachel had baby-sat his first child at Guilford. I sat in on some philosophy classes by Martin Foss, a German immigrant at Haverford and I met Grimsley there. He then moved to Earlham to teach in the philosophy department. When I came to Earlham, he and I jointly taught a philosophy of science course. It was a very unusual course where we invited other faculty to come in because we wanted to educate them about the fascination and the importance of the philosophy of science. A lot of the time in class would be spent in discussions among the faculty members about topics in various fields, to the great fascination and enjoyment of the students.

Grimsley did finally get the offer from Guilford to become president, and from then on he tried to lure me to come and head the chemistry department there. Rachel and I were quite annoyed with Guilford for many years because it was so slow in integrating. The University of North Carolina in Chapel Hill had integrated long before them, thanks to Frank Graham's leadership. But Guilford did slowly change, particularly because they wanted the next Friends World Conference to be held there. It refused to come unless Guilford was integrated because there is a big Quaker community in Kenya. Some African chiefs had become Quakers, so all their regions became Quaker too. So by the time of the World Conference in 1967 Guilford had changed.

When Grimsley first asked us to come, we were not ready to leave Earlham. Right after our year in Japan, 1970 to 1971, he invited us again. We felt we owed it to Earlham to stay two full years after our return from Japan, but we also saw in the invitation our chance to move back to the more familiar East coast. The opportunity to create and head a department was appealing. My situation at Earlham had become pretty set and defined. We also felt in Richmond, Indiana, the lack of cultural diversity and the distance from the major eastern seaboard centers. North Carolina with its climate and cultural offerings were very tempting. So we said, "Yes, but we will not come next year, but the year after." I think Earlham felt that during that year they could change our minds, but we were pretty set and we went.

The head of the department there, Harvey Ljung, almost <u>was</u> the department, with other people helping. At first, he said he would stay on and overlap with me one year. But as things got closer to my taking over, he announced he was not staying on. So I had a totally blank department, but I was promised it would be a two-person department. I discovered a former Earlham student

of mine, David MacInnes [David F. MacInnes, Jr.], with a Ph.D. from Princeton. During the last years of completing his research, he had taken on a job at Westtown School, near Philadelphia, a Friends School, as a chemistry teacher, and I met him at a Quaker conference. He was hoping to move to a college. He became my colleague and remained throughout the time I was there.

The year I took over at Guilford, Ciba-Geigy decided to shift the headquarters of its dye and chemicals division and its agricultural division from Ardsley, New York. Two of the five divisions from then on had their headquarters in Greensboro, North Carolina. There was quite an influx. They were taking over the research facilities of Burlington Industries, which had decided some years earlier during the big layoff period to lay down their central research facility and just do research at their separate smaller centers. Burlington and Ciba-Geigy had some overlapping board members, so Ciba-Geigy learned about the research center's availability and moved in. They immediately discovered that whereas in the New York City area there were endless opportunities for their lower level staff to continue their education, there was no such opportunity in Greensboro. There was a group of people concerned about the problem in the local section of the ACS from different schools, including AT&T, the University of North Carolina at Greensboro, Guilford, and some others. Christopher Wilson was at High Point College by then. There was also Greensboro College, as well as Bennett College, a private black women's college. We were all asked to cooperate in creating evening programs, and Guilford ended up being the only one that did anything significant about it. We created an evening program that rotated over a three year period. All Ciba's technicians had freshman chemistry. We rotated organic, analytical-inorganic, and physical, one each year into the evening, so that over a three year period a technician could pick up those remaining courses for a bachelor's degree with a major in chemistry.

All our regular chemistry students from then on would take at least some of their courses at night. Because of the requirements of the night school administration based on experience with how many evenings people who are full-time employees elsewhere are willing to give up, we had to teach our courses concentrated into two evenings each week. That's lab and class, so we either had three classes one evening and lab the other evening, or break it up by having two classes and one lab one night and the opposite the other. It was very wearing to teach three hours of classes one after the other. I did have a marvelous person to help me in the evening labs and also some of the classes. That was Lawrence Gains of Lorillard Industries, who is the counterpart of Jeff Seeman at Philip Morris, two brilliant chemists at tobacco companies. Larry Gains is a synthetic organic chemist who tries to produce new flavor

chemicals to restore cigarette flavor after all the carcinogenic ingredients have been removed--which removes some of the characteristic taste, too. He's in pure synthesis and in the theory of sensory-organ physiological chemistry.

The educational program is still going on. We were almost frightened by what happened to people when we gave them a degree because Ciba-Geigy immediately gave them tremendous responsibility. We've produced some impressive people. One was put in charge of a whole instrumentation section because he had been actively involved and was all ready for it except for not having a degree.

As I mentioned earlier, we had this cross registration possibility with the other Greensboro schools, which allowed us to send our majors to the University of North Carolina at Greensboro, which has a master's degree program, to take advanced courses in chemistry beyond what we could offer. That provides a marvelous stimulus for students preparing for graduate school.

BOHNING: How many chem majors did you average then? Was it smaller than Earlham?

BENFEY: Possibly a slightly smaller number of day students, but offset by an ever larger number of evening students. The two colleges were very similar in structure and experience. Actually, Earlham is a slightly larger school in the day college, but Guilford had an extra five hundred in the evening college. Many of our majors were night majors. If one looks at the list from the [ACS] Committee on Professional Training, these small colleges really do remarkably well compared to many schools that are much larger.

BOHNING: How would you contrast Guilford and Earlham on an overall basis?

BENFEY: The traditional conception was that of the Quaker schools, Haverford, Bryn Mawr, and Swarthmore are the leading intellectual group, Earlham comes next, and Guilford's a little behind Earlham. But all that is very rapidly changing. Guilford and Earlham have always been able to produce some people who gained great eminence and significance. The training of the top people has been good, and the quality of these people has been as good. The number of students going on to graduate school was greater at Haverford than at Earlham or Guilford. In terms of intellectual stimulation on the campus, the delightful thing about Guilford was to be part of its transformation from a very traditional regional campus to a nationally recognized one. So I

was involved again in an exciting transforming period both during the time Grimsley Hobbs was president and then later with William Rogers as president. Bill had been my colleague for some years at Earlham, but then was appointed to a named professorship at Harvard. He gave that up to become president of Guilford in order to work out a vision he had of a supportive community, not just intellectual, but of a total community. It's been very exciting working on all three of these campuses because in each one, three younger presidents were transforming campuses in directions I very much was in sympathy with.

At Guilford, particularly, there had been a lot of active interdepartmental programs for faculty interaction. They had instituted an active freshman program which was originally called "Man in the Twentieth Century." Then the term "Man" was no longer acceptable, so the course became "Being Human in the Twentieth Century." This involved some fifteen faculty from all disciplines, each teaching a student group of sixteen to twenty. The faculty would meet once a week to interact about how to teach a given topic. You would go from reading a novel to learning about genetics to all sorts of intriguing other topics loosely related, as well as teaching students to write. At Earlham the humanities course for freshmen that Wayne Booth developed was kept strictly to the English Department and maybe a few humanities people. I tried to squeeze my way in, and now they do permit scientists in, but not during the time that I was there.

Thanks to Guilford's faculty development program under Carol Stoneburner, there were also faculty study groups that would read different books together, meeting once a week. We read everything from history of science, using books commenting on Thomas Kuhn's "Structure of Scientific Revolutions," to books on androgyny, books by black novelists, plays, and so on. So we got to know other faculty, and were introduced, particularly, to other areas of the intellectual world.

BOHNING: Your position was the Dana Professor of Chemistry and History of Science, so both were combined in one position.

BENFEY: I was already professor of chemistry and history of science at Earlham. At Guilford I became a Dana professor. The Dana Company Foundation supplied the extra money for small increases on top of the standard salary, and there were about four Dana professors. They have some other named professors now, but when I came, they were almost the only ones. That allowed Guilford to match my salary at Earlham.

BOHNING: What approach did you use in teaching the history of science at Guilford? Was it similar to what you had done at Earlham?

BENFEY: Yes. At Earlham it had been enormously successful because it was one of the four courses that would satisfy the science requirement. Two of the courses had to be in a laboratory science, but the other two could be anything, and mine was one of those. At times over fifty students satisfied one of their science requirements with my course, and that way I got to know many of the top humanities students. One of them was Frances Moore Lappé who later wrote "Diet for a Small Planet" (76). She's become very well known. There are many other people that I meet up with who were fascinated by that course. I did not do a chronological presentation. It's a one semester course. I chose three themes, cosmology, atomism and evolution. I started with Ptolemaic and even pre-Ptolemaic cosmology through Copernicus to relativity. To understand the major transformation of relativity coming from Newtonian physics, it is useful to look first at how Newtonian physics came from Ptolemaic cosmology. Then I went back to atomism from Aristotle and Plato's Timaeus to Dalton and the subatomic particles. Usually I was running out of time by then and spent much less time on evolution, but I emphasized the Great Chain of Being, going back to the Greeks, and the "discovery of time" from around 1800 through the nineteenth century. There was a text, Readings in the Literature of Science (77), by William C. Dampier and his daughter, Margaret Dampier, that was organized in that three-part way. It had excerpts from the original writings and brief commentaries. Later there was a book by [A. E. E.] McKenzie published by Simon and Schuster that had the first half giving the historical development, and the second half containing excerpts from the original papers (78). It was organized differently but still usable for my course.

Out of my history of science teaching at Guilford, came David Rhees who is now at the American Philosophical Society [he became director of the Bakken Library and Museum, Minneapolis, in 1992], and William Newman, who is now on the Harvard history of science faculty. Bill was a student at the University of North Carolina at Greensboro who joined the UNCG-Guilford six-week summer course in East and West Germany. He had taken my history of science course and had written a paper on Thomas Vaughan and Agrippa von Nettesheim which was so abstruse and scholarly I couldn't follow it all. I gave him an A for the paper. He later reworked and published it in Ambix (79) and won the Partington prize with it. During our weeks in Munich he became acquainted with the Newton Scholar Karin Figala at the Deutsches Museum and they corresponded afterwards. Dave Rhees took his master's degree with Sy Mauskopf in Chapel Hill, and his Ph.D. with Arnold Thackray here at Penn. He helped the Beckman Center with the

Priestley exhibit. When Sy was at the Beckman Center in 1989, Dave had all his three mentors, bachelor's, master's and doctoral, in one place.

BOHNING: Was it your course that turned him to the history of science?

BENFEY: He was a humanities major. He'd already taken most of his course work. He just took one independent study course with me, in which I vaguely steered him through parts of my course and also had him do an independent project during the summer. That fascinated him enough that he went on to take some graduate work at Chapel Hill which led him to where he is now, a historian of science. Actually, there's one more student now, Robert Kraus, who's been doing graduate work at Chapel Hill with Sy Mauskopf and Mike [Michael] McVaugh.

BOHNING: We haven't discussed any of your Oriental connections as such.

BENFEY: That came about in an intriguing way. By going to Earlham I joined one of the centers of Japan and China studies. Earlham had hired Jackson Bailey in its history department to develop the oriental side of Earlham. The president, Landrum Bolling, was very interested in it. Jack was an Earlham graduate and a Harvard Ph.D. under Edwin Reischauer, who later became ambassador to Japan. Reischauer wrote the preface to my Watanabe translation. He was one of the leading American authorities on Japanese history. Jack Bailey came to Earlham to create non-Western programs with emphasis on Japan.

This was the time when the American government was very worried about the fact that its diplomats and other representatives overseas didn't know and had great difficulty in learning foreign languages. So they were dependent on local interpreters which led to problems. The Soviets would bring a total staff from their country and most of them were quite fluent in the language they were coming to. Earlham became one of the centers supported by the U.S. government for the training of area specialists. We were the Japan center of the Great Lakes Colleges Association. Oberlin was the China center. Other colleges were responsible for other areas, such as the South American regions, India, and so on. Because we had government funds, Earlham could also nominate one faculty member each year for a Fulbright-Hays Research Study Fellowship only available to people from these centers. Almost every year somebody from Earlham was sent to Japan to broaden faculty horizons to include non-Western aspects, to deprovincialize you, as I like to call

it. For example, the literature people, instead of teaching just western literature, would bring in Japanese and Chinese literature. The theater people learned about No drama. Arthur Little of the drama department actually wrote a play about a Western theme, but in No drama format. People from the whole spectrum of disciplines would go over and come back transformed because it is a transforming experience to spend a year in the Orient where everything is different. Some people say everything is done backwards. The Japanese drive on the left-hand side of the road, writing goes down and from right to left, and so on.

[END OF TAPE, SIDE 5]

BENFEY: The beauty of these fellowships is that you don't have any teaching responsibilities. My only responsibility was to learn what I could in the history of Chinese and Japanese science. I didn't even know there was such a thing until Jack Bailey told me about Joseph Needham, who since the 1950s has been publishing large volumes on Chinese history of science. Jack suggested that I needed to be exposed to it. So I contacted Nathan Sivin, who's now in the history of science department here but was then at MIT, and asked him to help me. I did some reading. It was quite fascinating, and he gave me contacts whom I could look up in Japan. Shigeru Oae was a Japanese sulfur chemist who had worked with Charlie Price and with whom I had been in touch since the late forties. He was in Osaka at Osaka City University. He made contact with Kansei Gakuin University, and they said they were open to having me come for an academic year. That's where Tokunosuke Watanabe was our host.

Rachel and I had spent the previous year learning what we could of the Japanese language. It's extremely difficult to learn. Its Chinese ideograms are mixed with syllabic symbols, and the ideograms give you no clue as to how to pronounce them. Similarly, learning how to talk doesn't help you write it. They are two separate problems, and this makes learning extremely slow. Even though I had spent a year or so doing language study with some of the Japanese nationals who were at Earlham in the Japan Center program, I knew very little. That was a good thing because if I'd known Japanese I probably would have spent endless time in libraries. Since I didn't know enough for that, I went looking, for instance, for signs of geometric patterns. That's how I came across the dodecahedral incense burner. We went to antiques shops and museums and local stores and the major craft and pottery centers. Oae is a potter himself. He took us to some of the traditional pottery villages.

Rachel decided to spend her year learning the stencil-dyeing technique with which the Japanese dye their kimonos, obis and paper products. It's a dye-resist technique based on stencils

and rice paste, dyeing the areas that the stencils didn't allow the paste to go to. We visited some key people, the "national treasures", such as the stencil dyers Keisuke Serizawa and Kotaru Shimizu. We also went to one place where the whole village is involved in stencil cutting.

I continued my language study by trying to translate Kyoshi Yabuuchi's little book, Chugoku no Kagaku Bunmei (80), on the history of Chinese science. We had two of our three sons with us, Christopher and Stephen (Stephen's the eldest) and they took classes and learned to speak Japanese. Between us we did quite well because I could decipher some of the signs, and they could talk to people. I slowly learned to communicate on a question and answer basis. I've not been able to carry on a conversation.

In addition to doing what general reading I could on Oriental science in English and German sources, I developed this fascination with the incense burner, and how the Chinese might have learned about the dodecahedron and the other regular solids and the connection of the flower design decoration that's on it with designs that you find on Persian pottery, and how that reached China because this incense burner came to China and then to Japan. In 1983 I was in China, serving as academic expert for Joseph Schmuckler's Temple University study tour on history of Chinese science. In the history museum on Peking's Tiananmen Square I was shown a spherical bronze incense burner similar to the Japanese one but with an octahedral rather than a dodecahedral design.

Joseph Needham is not just an authority on the history of Chinese science. He's working on his fifteenth volume now, and there are still several more to come. His books are really a presentation of interconnections of all kinds, cultural, intellectual, conceptual, and of the flow of materials between cultures throughout the world. His writings were very helpful in allowing me to trace many of these interconnections.

I was in Japan in 1970 and 1971. In 1974, the International Union for History and Philosophy of Science held its first meeting in the Orient in Tokyo and Kyoto and I went back then to Japan to present my findings. The conference was fun. Needham was the honorary chair and scholars from all over the world in oriental Chinese science were there. Willy Hartner from Germany was there, whose book I had looked at (81). Masao Watanabe showed us his magic mirror, whose history is discussed at length in the book that I translated. If you look into the mirror, you have perfect reflection, you see yourself. But if you let the sun shine on it and let it reflect on a wall, you see the pattern that has been engraved on the back of the mirror. The cause is the pressure effects while creating that pattern, that show in just a few molecule thicknesses difference in the surface, which early Western teachers to Japan figured out slowly. There were

all sorts of theories discussed in the book. But it has to do with these minute variations on the surface. That sort of knowledge and technique is in fact one of the methods by which surface imperfections are now studied and interpreted.

[break]

BENFEY: I should mention that of our three sons, the eldest, Stephen, was so fascinated with Japan that he devoted a tremendous amount of time to mastering the language. He became so intrigued with the country that he decided to stay on, becoming a student on the Kyoto campus of Friends World College, which had its headquarters on Long Island and campuses all over the world. Then he returned to Earlham where he had studied earlier, to take some more courses in Japanese studies. He went to Middlebury College for a summer, by the end of which he was told there was nothing more that he could learn there, and that he should just go to Japan. He did that very soon and found some work at very minimal wages. He was doing some video taping for a company for a while. He met his Japanese wife Kikue there, a wonderful person. He then moved to Tokyo where he joined an organization called Network that does English language work for Japanese companies that want to sell their products in foreign countries. He's become an expert on electronic music, so he usually handled those accounts. He has now created his own company, Ventura. He's the president of this company that continues to take on accounts from organizations like Sony, Panasonic, Technic, and also motorcar companies, doing their English language advertising. He used to write a number of user manuals. His company also helps foreign companies entering the Japanese market. While he was at Network, he would translate into English and his English would then be used by the German nationals and French nationals to translate into their languages. Stephen and Kikue have two daughters who are learning English. They're reluctant to speak English until they perfect it, but they hear a lot of it. They understand quite a bit of what their grandparents say, but they don't talk back in English yet.

The youngest of our three sons, Christopher, was with us also. He actually attended a Japanese high school for part of the time. But he did a lot of the work by correspondence with his U.S. school, Putney. The second son, Philip, was in his senior year at Groton School in Massachusetts so he felt he couldn't come with us. He sort of caught up with his brothers' international travels by spending five years going around the world later on, spending several months in Japan and, like Stephen, working with a landscape gardener, learning Japanese gardening skills. On his way home he met his future wife, Elisabeth, a French woman, in Paris. He finished his undergraduate studies in Paris. He'd been to Hampshire College,

but after a year decided that he really didn't want to study more until he knew what he wanted to do. He's now in molecular biology, cell development and he's on the faculty at Rockefeller University. He's just gotten an NIH grant which makes him somewhat independent and free to look around for interesting places to move to. [He moved in fall 1991 to New York University to help create a plant molecular biology lab.]

Going back to Christopher, he is now teaching in the English department at Mount Holyoke College, where my sister, Renate Wilkins, had been Dean of Students before her retirement. He wrote books on Emily Dickinson (82) and is just completing a literary biography of Stephen Crane that is now in press (83).

I should mention also that in 1963 we began looking after a three-year-old Korean orphan who stayed with us for twelve years. Karen Boyd is now married in Winston-Salem, North Carolina, and is a social worker for the police department.

To round out the survey of my family, my brother Rudolf spent most of his life at General Electric, for some years being GE's manager of radar systems engineering.

At Guilford, in addition to my history of science course, I was involved in a number of joint courses with other faculty. I taught one course with William Beidler on oriental science. He was a chemist who had studied under Saul Winstein and had worked on poison gases in the chemical warfare service at Fort Dietrich. He decided that wasn't the thing to do and got into Indian philosophy, spending some years in India. So he was an expert on India, and I knew about China and Japan. Between us, we taught an oriental science course. With Mel [Melvin] Keiser in the religion department, I taught a science and religion course looking at Michael Polanyi and at Loren Eiseley of the University of Pennsylvania. Rex Adelberger in physics and I taught a course in history of technology. That got us involved in exploring a ruined industrial site that turned out to be a quite historic place. It was the building for a steam engine to run massive wheels like stone mill wheels to crush ore to break out gold. North Carolina was the major gold-supplying region before the California gold rush. One of the U.S. mints used to be in Charlotte, North Carolina. One of our students in that course, [J.] Randall Catoe, really discovered the details. He discovered one of the tracks on which mill wheels went in a circle, and told about it at the 1976 history of technology society (SHOT) conference. He got the historical engineering record people to investigate it. The place is now on the national register, and has also been taken over by a genius engineer who has rebuilt it to satisfy the historians. It's become a reception center-restaurant-clubhouse place now known as Castle McCulloch. It's very spectacular. When the southeast regional ACS meeting

held a special symposium in my honor in Winston-Salem in October 1989, we had our speaker banquet at this place. It's worth visiting.

BOHNING: Why did you decide to retire early?

BENFEY: I had already decided on it much earlier. A Quaker friend of mine had told me it was a Quaker tradition, if financially feasible, to retire early from worldly pursuits to devote yourself to the needs of the Quaker community and other social concerns. It was always in my mind to retire early so that I could devote more time to my other interests. I found chemistry teaching and all my endless involvements in a small college community ever more wearing, making it very difficult to pursue many of my not-paid-for activities like writing and historical research. Research, generally, at a small college of any kind, is hard, as you know.

My youngest son, Christopher, married Sheila Rathbun from Virginia in 1983. She was trained in law, and she practiced law for several years in New York. When all my sons were sufficiently independent, it was clear that I could take early retirement. Rachel and I had met at Haverford. We liked Philadelphia. We decided we'd come here for a while and look if something was available, because I did want to do something part-time, but definitely not full-time and not more than half-time. I explored a number of possibilities and contacted a number of people here. Arnold Thackray heard about me through Nathan Sivin, with whom I had been in contact. He asked me to stop by, which I did. Then he asked me if I was interested either in a full-time or part-time position, and I chose the half-time position heading the publications area as editor. We lived for a year in the Society Hill Towers. We had sold our house. We made a total break, sold the car, and thought we could live in a one-bedroom apartment in the city and be footloose and travel. We discovered that wasn't our lifestyle. We explored going out in the suburbs or beyond, and rediscovered the Bryn Gweled community, which we knew about already through my years of involvement in the Society for Social Responsibility in Science in the 1950s. At first we rented a house and during the year decided we wanted to be a permanent part of the community. We went through the membership process of being interviewed by somebody from each of the seventy households. To become a member, the community has to vote eighty percent in your favor. Within a week of that process being completed, we bid for the house in which we now live. It is on two acres of land, quite a showplace, thick with rhododendrons and azaleas, and there's something blooming almost the year around including witch hazel in January.

BOHNING: I don't know much about that. Can you tell me more about this community?

BENFEY: It was founded fifty years ago (they had their fiftieth reunion last year) by a group of Quaker and like-minded people, many of whom were working in town in settlement work but who wanted to bring up their children in a community that was inter-racial and free of land speculation. They were influenced in part by some theories of Henry George regarding land values, income taxes, and the evil of land speculation. They looked around for a piece of land and finally found this two hundred and forty acre plot that had to be bought in total at something like seventy dollars an acre. It was an old corn farm that was worn out, practically without trees. Now, it's thick with trees. So they moved out there. They just shared what resources they had. I think initially they borrowed no money at all. They usually built their houses themselves. They had some expert amateur builders and some professional builders. They had an architect in the group soon thereafter, who had been trained by Frank Lloyd Wright. Some of the architecture is fascinating; some of it was way-out at the time.

They also wanted to do things that might be of help in third world countries. One of the houses is built with the rammed-earth technique, just ramming earth between forms. There's a lot of common land with trails, and the roads belong to the community. All land is communally held, and homeowners get a ninety-nine year lease for a two-acre plot around their house. We pay something like condominium fees for common expenses, though much less than condominiums usually charge because so much of the work is done by our members. We have monthly meetings and work parties. There are many committees, so one gets to know people. Last year they built a new community center, a multi-sided, almost circular, big open structure. It is beautiful. We're doing all the finishing work in there. You learn a lot of skills by working with people who are skilled, and you get to know the people even better. Bryn Gweled has a committee on health and welfare. When someone is sick, other people will do the shopping, visiting, sharing of medical equipment. It's very impressive. You're essentially independent, you're on your own and yet you can get help from people. But only approved applicants can bid for the houses.

BOHNING: Is there anything else we should cover that I haven't covered?

BENFEY: I can't think of anything now. Maybe the rest should be an addendum when we see the final transcript.

BOHNING: Well, thank you very much, Ted. We really appreciate it, and I enjoyed it.

BENFEY: I enjoyed it.

BOHNING: Thank you.

[END OF TAPE, SIDE 6]

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