CHEMICAL HERITAGE FOUNDATION

GEORGE M. WYMAN

Transcript of an Interview
Conducted by

David J. Caruso and Jody A. Roberts

at

Carolina Meadows
Chapel Hill, North Carolina

on

1 and 2 May 2013

(With Subsequent Corrections and Additions)

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GEORGE M. WYMAN

1921 Born in Budapest, Hungary, on 13 October

Education

1941 A.B., Cornell University, Chemistry
1943 M.S., Cornell University, Chemistry
1944 Ph.D., Cornell University, Organic Chemistry

Professional Experience

Allied Chemical & Dye Corporation, General Chemical Division, New York, New York
1944-1945 Research Chemist

General Aniline and Film Corporation, Linden, New Jersey
1945-1949 Research Chemist

National Bureau of Standards, Washington, D.C.
1949-1953 Research Chemist

Quartermaster Research and Development Center, Natick, Massachusetts
1954-1956 Chief of Spectroscopy Section

U.S. Army European Research Office, Frankfurt, Germany
1956-1960 Chief, Chemistry Branch

U.S. Army Research Office, Durham, North Carolina
1960-1974 Director, Chemistry Division
1974-1977 Director, Chemical and Biological Sciences Division

Max Planck Institute for Biophysical Chemistry, Göttingen, Germany
1972-1973 Sabbatical

University of North Carolina at Chapel Hill
1973-1977 Adjunct Professor of Chemistry

U.S. Army European Research Office, London, England
1977-1983 Chief of Chemistry

U.S. Army Research Office, Durham, North Carolina

1983-1985 Director, Chemical & Biological Sciences Division

University of North Carolina at Chapel Hill

1983-1985 Adjunct Professor of Chemistry

Consultant

1985-1995 Advised university departments on acquiring research funding

ABSTRACT

George M. Wyman was born in Budapest, Hungary, in 1921, one of two children; his father sold textiles, and his mother was a housewife. Wyman attended the Lutheran Gimnázium in Budapest until his last semester of high school, when World War II threatened, and his father sent him to the United States, telling him to become a chemist. Wyman's mother died shortly after Wyman left Hungary, but his sister and father eventually made their way to the United States.

Wyman was able to enter Cornell University as a sophomore, despite having no high school diploma, and he finished his undergraduate degree in chemistry in two and a half years. He remained at Cornell for his master's and PhD degrees, working on fluorine chemistry with William T. Miller. Wyman reminisces about faculty changes in Cornell's chemistry department, as well as the new quantitative approach to inorganic chemistry and the mechanistic emphasis in organic chemistry that came at the same time. He also notes that his research interests fell between the fields of physical and organic chemistry and that later in his career he found a disciplinary home in organic photochemistry.

After graduation Wyman began his career in industry working with fluorine compounds at Allied Chemical and Dye Corporation's General Chemical Division, but leaving shortly thereafter to join the Process Development Division of General Aniline and Film Company. In this second position, Wyman developed expertise in dye chemistry. Before long Wallace R. Brode offered Wyman a position in his research group at the National Bureau of Standards (NBS). During his four years at the NBS Wyman conducted various spectrophotometric measurements of indigo and azo dyes, resulting in some twenty publications, and cultivating in Wyman a lifelong interest in the *cis-trans* isomerization of indigo. It was at this point in Wyman's career that he was married.

In 1954 Wyman took a position at the Quartermaster Research and Development Center in Natick, Massachusetts, beginning a decades-long career working for the U.S. Army. At Natick, Wyman taught himself fluorescence techniques and continued his work on isomerization of dyes. Next, Wyman and his family moved to Frankfurt, Germany, where he worked in the chemistry branch of the Army's European Research Office. His responsibilities there were administrative; he identified and established networks of chemists whose work could be useful to the Army. From Germany he moved to the Army Research Office in Durham, North Carolina, where he was also able to conduct research at University of North Carolina; thus he was able to combine two loves and skills. In the sixties Wyman established the International Conference on Photochemistry.

Six years as chief of the chemistry division at the Army's European Research Office in London, followed by another year and a half back in Durham, finished Wyman's stint with the U.S. Army. He retired but spent ten years consulting, essentially still bringing people and universities together with funding.

INTERVIEWERS

Jody A. Roberts is the Director of the Institute for Research at the Chemical Heritage Foundation. He received his PhD and MS in Science and Technology Studies from Virginia Tech and holds a BS in Chemistry from Saint Vincent College. His research focuses on the intersections of regulation, innovation, environmental issues, and emerging technologies within the chemical sciences.

David J. Caruso earned a BA in the history of science, medicine, and technology from Johns Hopkins University in 2001 and a PhD in science and technology studies from Cornell University in 2008. Caruso is the director of the Chemical Heritage Foundation's (CHF) Center for Oral History, president of Oral History in the Mid-Atlantic Region, and the book review editor for the *Oral History Review*. In addition to overseeing all oral history research at CHF, he also holds an annual training institute that focuses on conducting interviews with scientists and engineers, he consults on various oral history projects, like at the San Diego Technology Archives, and is adjunct faculty at the University of Pennsylvania, teaching courses on the history of military medicine and technology and on oral history. His current research interests are the discipline formation of biomedical science in 20th-century America and the organizational structures that have contributed to such formation.

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INTERVIEWER: **David J. Caruso**
Jody A. Roberts

LOCATION: **Carolina Meadows**
Chapel Hill, North Carolina

DATE: **1 May 2013**

CARUSO: All right. Today is the 1st of May 2013. I am David [J.] Caruso, I'm here with Jody [A.] Roberts. We're at Carolina Meadows in Chapel Hill, North Carolina. We're here to interview George [M.] Wyman. This will be the first session of a two-day interview. Thank you, again, for participating in this oral history project. What I'd like to do is—I'd like to hear a little bit more about your childhood. From what I can tell, you were born on the 13th of October in 1921, in Budapest, Hungary. So I was hoping that we could kind of start from there, if I can hear a little bit about your childhood, growing up, your parents.

WYMAN: Yes. Budapest at that time—in the 1920s, 1930s—had a very well-to-do, largely Jewish, middle class. And my family was among these, and my father [Alexander Wyman] had a textile business. He was selling materials by the yard—by the meter, actually—and was doing very well financially.

And I was born in '21, as you said, and when I got to be…. Now the European educational system is different from the American one. There, we have—in Europe—you have four years of elementary school, which is just read and write and talk, that sort of thing, and then eight years of a secondary school, which is quite rigorous. And in Hungary at the time of my childhood, the best school—the generally recognized best school—was a private school run by the Lutheran church [*Fasori Evangélikus Gimnázium*]. They owned it, operated it, whatnot. It was not exclusively for Lutherans, but if you [were] not a Lutheran, you paid an extra fee. Everybody had to pay a fee. It was not a public school. And my family thought that would be a good place for me to go when I was ten years old, and I did.

Among the alumni of the Lutheran school, earlier than my entrance, were Eugene [P.] Wigner—a Nobel Laureate in Physics,[1] Princeton [University]—and John von Neumann, recognized in general as the father of computers. And another economist [John C. Harsanyi],

[1] Eugene Wigner was awarded half the 1963 Nobel Prize in Physics for his contributions to the theory of the atomic nucleus and the elementary particles, particularly through the discovery and application of fundamental symmetry principles.

just a couple years older than I was, also got a Nobel Prize,[2] later […]. So it was an outstanding school. There's no question about it.

The other thing that's—well, I was one of the best students in the school of my vintage. But as [Adolf] Hitler was making more and more inroads on central Europe, my father was a pessimist, looked at my age, and said, "You're going to be military age just at the time when you're going to graduate, and we just can't have you stay here that long." So he insisted I drop out of my school one semester before graduating. So I'm a high school dropout. And [he] arranged with friends of the family and whatnot to have somebody meet me when I arrived in New York, arranged visas and all that sort of thing, and shipped me off to the U.S. [United States of America]. This was in the winter of 1939. I was seventeen years old.

ROBERTS: And did you come by yourself?

WYMAN: By myself, but I had friends [of the] family, […] a long list of people to look up and that sort of thing. And a couple of people, especially, were very helpful in getting me settled.

But the object of the game was for me to continue my education in the U.S. And the question was, well, will a university in the U.S. take a high school dropout **<T: 05 min>** from Hungary? My English—knowledge of the language was very good. I spent two summers in England, so I was fluent in German and fluent in English. I had a smattering of Italian and French. All this is not unusual, because when you grow up in a country like Hungary…. [Use of the] Hungarian language is a hundred miles in each direction and it ends. When you get to the west, it's German, to the east, it's Romanian, or Russian, or Serbo-Croat, or whatnot. The Hungarian language is lost. I mean, nobody speaks Hungarian unless you're born Hungarian. So we—every middle class family in Hungary started their children on languages when they were two or three years old. We had what we called a governess. We had two children in the family. I have a younger sister [Kathrine], and by the time my sister was born, we had a governess who didn't speak Hungarian. She was German or Czech or something, but that was the language of the family, German, because German was an international language, Hungarian was not. And it was silly to look for a German and Hungarian speaking governess because there weren't any.

CARUSO: I have a question. Were your mother, [Maria Neurath], and father from Hungary originally?

WYMAN: [Yes].

[2] In 1994, John C. Harsanyi shared the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel with John F. Nash Jr. and Reinhard Selten for their pioneering analysis of equilibria in the theory of non-cooperative games.

CARUSO: Okay, so going generations back they had been there?

WYMAN: A couple of generations.

CARUSO: A couple of generations. You mention that your father was in textiles. Was your mother working at all or…

WYMAN: No. Mothers would not work in those days. Women were not working in those days.

CARUSO: Okay. So she stayed at home taking care of the household?

WYMAN: Yes. Yes. Actually, my mother was ill much of the time, so she was in sanitariums and she—we saw her about half the time, the other half she was gone in [hospitals and sanitariams].

CARUSO: Was it—so it was a chronic illness she was suffering from?

WYMAN: Yes, a variety of illnesses. We never really knew what they were. And she was quite young, and she died in 1940. She was born in [1901]. […] She was about [thirty-eight] years old when she died. […].

CARUSO: Okay. Can you tell me a little bit more about what life was like for you? I want to know a bit more about what the education was like, even in the early years. But I also want to know a bit more what your home life was like at the time. You mentioned you were in a middle-class neighborhood.

WYMAN: Well, my father believed in investing in real estate, so he bought an apartment house, and we had a large apartment in this apartment house. And then he had somebody else living in the apartment house who took care of the everyday things. If the water system wasn't working, this guy would fix it. If whatever…. He didn't get personally involved. He just owned it.

And it's…things that may be hard for you guys to believe—hard for me to believe: for toilet paper, we used newspapers. I mean, we were well-off, upper-middle-class Hungarians. If you wanted a hot bath, you told the maid about a half hour beforehand. She'd go down to the

basement, to the coal, bring it up to the second floor to fire it, to put fire to the coal, and a half hour later, you'd have hot water. So that's the way it was.

But the most amusing thing from my childhood, that I still remember, is this. As I said, my father was worried about the advance of the German **<T: 10 min>** occupation of Austria in 1938 and thought that I should—since I was just growing up, I was sixteen at the time—I should have a profession. Originally he wanted me to go in the textile business, but as the political situation got worse and worse, he didn't think that was a good plan. He had a small factory, he thought I'd inherit it, but then he sold his textiles out. He, who knew nothing about chemistry, said, "You should take up chemistry when you grow up." Why? He had visions of chemists being somebody who could mix up all kinds of things and get some kind of a shoe polish out of this that he could sell at a good profit. Well, I—after I came to the U.S., I managed to get a pretty good chemistry education at Cornell [University]. I got a doctorate, but I still wouldn't know how to make a shoe polish. I'm afraid my father would be disappointed. But that was his idea of chemistry, he knew nothing about chemistry. He only knew about textiles and he made a very good living with his woolens, and cotton, and whatnot, that he was selling in a store.

ROBERTS: Had you had much technical training in your high school up to this point?

WYMAN: None. The high schools were very, very—well, you can imagine. At age ten, you started pretty serious stuff in high schools. You started a second language at age ten. It was German. And Latin. So you had Hungarian, German, and Latin at age ten. You had—you didn't have physics. You had math right away—yes, age ten—and then more and more topics. By age fourteen, you had another language. In my school, you had a choice. You had either—you could choose English or ancient Greek, because Ancient Greek was the traditional thing for a classical education. In my class of forty, four kids chose ancient Greek and thirty-six went with English. But as I say, my English got especially good because the family could foresee my need for English. And they sent me out for two summers, when I was thirteen and when I was seventeen, to England.

ROBERTS: So what was making them think that they could foresee that you'd have a need for English? Was this just larger political awareness…

WYMAN: Wait a minute, foresee what?

ROBERTS: You said that your family could foresee your need for English in the future.

WYMAN: Well, because Hitler was gobbling up Europe piece by piece, and by 1938, he was at the gates of Hungary, and he'd taken over Austria. And there was—my father could see that nobody would stand up to him.

ROBERTS: But even as early as thirteen, so you would have been—that would have been 1935—1934 or ’35.

WYMAN: Well, in ’35, I think that was purely an academic exercise, that growing up in central Europe, the Hungarian language is useless. If you’re going to do any traveling or something, then you need German and you need English. And yes, the way things were in ’35, if you spoke German and you spoke English, the only place you couldn’t get anywhere would be France. But you can’t have everything.

When I was fourteen, actually, I had a choice between French and Italian, and I took Italian. So I had some private tutoring in Italian. And I didn’t try French until I was about seventeen and I learned a little of that. But…

ROBERTS: I’d love to hear about your experiences in England. What was the—what did you do while you were in England for the summer?

WYMAN: Sunned myself. This was strictly just for the language and there was no—I got to London, [England]. There was a friend of my father’s who was an acquaintance, and he was going to look after me enough to get me settled somewhere along the seacoast. **<T: 15 min>** So in those days, there were a lot of ads in the English newspapers, what they called “paying guests.” Somebody had an extra bedroom and they were looking to rent it out for the summer for a kid or two—brother [and] sister or two brothers or something. And of course *X* number of pounds, and you had it for the summer.

So I spent both of those summers in the Brighton area, which is a south coast of England. And there was—one of them was [an] English schoolmaster, actually. He had a whole bunch of kids staying there. He had a big house. He had a back yard, we could play tennis. I mean, it was—see, he made quite a business out of this. The other one was just a private family, an English doctor with one child. And they decided they had an extra room, and they were a ten minute walk to the beach, and they advertised and we answered, and I spent the summer there.

CARUSO: So for both of these trips, did you travel there on your own?

WYMAN: Yes.

CARUSO: Okay, so at age thirteen…

WYMAN: My father took me on a trip, when I was fourteen or thirteen, I think, on a Mediterranean cruise that touched a lot of places. Wound up in Israel—at that time it was Palestine—for a week. And the school had a trip to Italy. I went on that: Rome, Florence, Venice. I mean, these were the highlights of the Italian trip for kids. And then on my trip to the U.S., the family decided that yes, there's a hurry, but it's not—the hurry is not such that you really have to take the first boat out, but you could actually do a little sightseeing. So I spent a week in Paris, [France], on the way to England, where I got the ship to come to the U.S.

And Paris was very impressive. There, I met a fellow I used to know back in my school, who was on his way to Australia. And he also had about a week in Paris, so the two of us roamed around Paris. This was all [1939]. On the other hand, there was a French family I met when I was in England, and I stopped in and visited them and their—one of the daughters was about three years older than I was. And they said, "What? Your family's worried about war? War, [1939]? No, no. No question. No war." The French didn't take it serious. And this was a very, very wealthy family. They lived near the Arc de Triumph.

Anyhow, this was a long time ago. And now, finding a college was a [top priority]! In the first place, the way I could come out was only on a student's visa.

ROBERTS: This is when you moved to New York?

WYMAN: To get the American visa, the immigration visas…. I would have had to wait about forty years to get an immigration visa because the [Hungarian] quota was used up for forty years. That obviously was ridiculous. So the family, or my father and his friends in the states, came up with the idea, "Come out on a student visa." But to be on a student visa, you had to be enrolled in a school that was on the state department list of approved schools.

And my family friend in the U.S., he didn't know much about me. He didn't know my education or anything like it. So he looked at the lists that were approved by the state department and he found a secretarial school to enroll me in—a secretarial school in New York because he himself lived in New York at the time. And then, he figured, you can always change schools. If you went from one approved school to another approved school, you're fine.

So as soon as I got there, **<T: 20 min>** he says, "Well, you're going to this secretarial school. And of course, we know you don't want to learn how to type, but about once a week, you should put in an appearance. And in the meantime, we'll look for the real thing."

And I arrived early March, and then I started looking and got my feet on the ground. And late in March, I started looking. I had an introduction to, among other people, John von Neumann. And he was a friend of a—he was a cousin of a friend of my father's and that sort of thing. And the word was everywhere, all the colleges in New York area, Princeton, that March was too late for entrance in September, because all the slots were committed. So—at least the New York area. And in the meantime, I felt New York was too big a place for me. I mean, too big a city. I still feel that way.

And so I looked around. I said, "What's there that's not too far from New York," because I had some friends there, "but far enough so it's not like New York?" And somebody mentioned Cornell. I said, "Okay, let's try Cornell." So I wrote to Cornell, sent them a—I had the copies of English translations of my report cards and whatnot, to submit to these schools. And the registrar at Cornell says, "Well, we like your credentials and we'd be happy to admit you, but we don't know at what level to admit you."

So I told them what was true. I said I'd been to Columbia [University], and Columbia said if they had space, they would give me a year's credit for—advanced credit. So Cornell says, "All right, what's good enough for Columbia..." They took my word for it. But actually, it was true. And, "All right, we'll admit you as a—temporarily, with one year's advanced credit." Fine. I heard more about Cornell and everything I heard, it was a good school. It was 200 miles from New York, 220 or something. And I entered Cornell even though I was a high school dropout in Hungary.

And then at Cornell, the question came up, what did they give me credit for? Because, as a chemistry major, I had some requirements in physics. Did I have physics credits or did I not have physics credits? So I went up to the dean, and he didn't know. He sent me over to the physics faculty, and this was sort of interesting. Whoever was in charge of undergraduate education in physics says, "Have you got your notebooks, textbooks, whatever from Hungary?" Physics notebooks.

I figured—I said, "Yes, but they're in Hungarian."

He said, "That's all right, I want to see them." He didn't speak Hungarian any more than you do. But he was looking for—did I have differential equations that I've used in physics? And if I did, they'll give me credit. They didn't tell me that. But I had the stuff, I gave it to him and he gave me another year and a half of credit. I mean—I'm sorry, eighteen credits, that was a semester and a half of credit. So it turns out, with the physics credit and the advanced credit, I finished the undergraduate school at Cornell in two years plus one summer.

CARUSO: One thing that I'm a little curious about, I mean, you mentioned that your family was from Hungary and that's—for a few generations. So how is it that your family had so many connections outside of Hungary? Was it just through your father's textile business? Was he shipping?

WYMAN: No, he was not shipping anything. He was selling **<T: 25 min>** the retailer twenty yards of this and ten yards of that and whatnot. No, this was—because other people—Hungary was a pretty poor country. And as some people tried to make a living, well, in Hungary and didn't do so well, and they thought the United States was—there is a man who became very famous in Philadelphia, [Pennsylvania], who was courting my mother before she married my father. But she decided to marry my father and not him. And he thought that America offered

him more opportunities. You guys ever go to concerts at the Academy of Music? You've heard of Eugene Ormandy?

CARUSO: [speaking to Roberts] Have you? No?

WYMAN: Well, he was the conductor and music director of the Philadelphia Orchestra for, like, forty years. I'm sure he's dead now. So he had a different name in Hungary [Jenő Ormándy-Blau]. He was a violinist. He didn't get anywhere, and he came to the U.S. He played in a movie in an orchestra. In those days, the big movie theaters had their own orchestras in New York. […] He just played the violin. And one day the conductor was sick or something and they had him come up and conduct. And it turned out he was a great conductor. And Eugene Ormandy is a phenomenon in—if you look at records, classical records, [he recorded just about everything!]

So anyhow, he was one of the people who sort of looked after me a little more through some of his sidekicks, people who worked for him. I mean, he was—he was one of the best known conductors in the U.S. for—from 1935 to probably 1985 or 2000 maybe, and lived in Philadelphia, in Rittenhouse Square. I visited him a couple of times.

So anyhow, he was one. And he was an example, he couldn't get anywhere in Hungary and was a big, big success in the U.S. […] My mother had very close friends when she was going to school. And one of her close friends had a brother who was trained as a chemist. And he wasn't getting anywhere in Hungary either, so he packed up and went to the U.S. and started his own chemical company.

CARUSO: So would you say that during this—during the period that you were in Hungary—that the U.S. was seen as the place to go if you couldn't make it where you were living?

WYMAN: It seems that way.

CARUSO: Okay.

WYMAN: In retrospect. I mean, it was not obvious at the time, but I think that a fair number of people who thought they could do better and they were not satisfied with what they are doing in Hungary and what they are accomplishing, they looked around the world. And maybe some went to Australia, and maybe some went somewhere, but a rather large number went to the U.S. Now, of course, a violinist, you can't tell how good he is until he's in big competition. And it turns out that he wasn't all that good as a violinist, but as a conductor, he was probably the fourth or the fifth in the world, when he had a chance to conduct. This is—well, they had orchestras in Hungary, and I was there when he came back to conduct in Hungary in nineteen—that was about

1936, 1937. There was a big to-do about it, that he's the famous conductor of the Philadelphia Orchestra, was coming to play in Hungary. **<T: 30 min>**

So yes, I had some help and I think […] that every family does, especially if you have a child going—they search their minds and see if there's anybody who emigrated who they know about. My closest friend, for seventy years now, was a fellow I met after I arrived in New York, [Thomas Polgar]. He was a year younger than I was, but a friend of my mother's had this friend, and she knew that that lady emigrated a year before that with two sons. And one son was about my age. And it turned out they lived very near where I settled in New York. So I looked him up a couple of weeks after I got there, and this was 1939, and we're still friends. He [lived in Orlando, Florida, with his second wife until he died in March 2014].

And he had a big career with the CIA [Central Intelligence Agency]. Now, he dropped out of high school two years before finishing, and he looked to me to help him get the high school credit. And I was at Cornell—I told you what happened to me at Cornell. And well, he wasn't anywhere near a college, but I went to the same dean who gave me the advanced credit. By then, I was well established. I think I was—by then I was a Phi Beta Kappa. So I said, "Look, I have this friend. He's in the Army, and the Army will send him to this specialized training program if you could somehow say that he had the equivalent of a high school degree," but he didn't. So I got a letter from this dean at Cornell saying that if this fellow had gone to Cornell, he would have been—if, if, a bunch of ifs—but the Army bought it.

But then the whole thing had a really silly ending. This was the beginning of the war, and we weren't quite sure what was what, I think. Anyhow, my friend at that time was sent to Yale [University] for this specialized training program based on what the Cornell dean said about me. And this was an immersion course in Japanese. Everything—Japanese breakfast, Japanese everything. And when the course was over after six months, they shipped him to Europe. And then he—then he wound up with the…what? Army intelligence. And then the OSS [Office of Strategic Services] and the CIA. And he was the last CIA chief in Saigon [Vietnam] as we were pulling out of Vietnam.

CARUSO: I guess he did make it out of Europe at some point, though not, clearly, to Japan.

WYMAN: Yes, but he had a—he had a very stellar career with the CIA, from—oh, I saw him when he was station chief in Vienna, [Austria], in Bonn, [Germany], in Mexico, so he was everywhere. In fact, his first wife left him because he was too damn busy. He always wanted to be out on the firing line and she wanted him to settle down and raise the kids. So he had a very full life. […]

But this business, the equivalency of the educational system—articles have been written on it. But the European—going back fifty years, forty years, the European educational system was, oh, about four or five years ahead of the American system, and at that time it was generally accepted. I think it has changed **<T: 35 min>** some. I don't know of any recent articles about it, but anyhow, I had the benefit of it. My friend had the benefits. A lot of other people I know had

the benefits, so—the president of the National Academy of Sciences wrote an article about that, but that was in 1960, 1955, I remember, because I met him then.

CARUSO: So I've got a couple of questions. I do want to hear a lot more about your transition to the U.S. and your time at Cornell, but before doing so, I just want to sort of finish up with your time while you were still in Hungary. You mentioned having a younger sister. Did your parents have a similar plan for her in terms of coming to the U.S.?

WYMAN: Well, you see, she was not in the—on the firing line. At least as my father saw it, I was reaching military age, a.) because I was older and b.) because I was a boy. So—and of course, he's right. Eighteen was military age in Hungary, so when I reached eighteen, he visualized that I would not get a passport to travel anywhere, so I had to go out before I was eighteen. That's why he yanked me out of school. My sister was fourteen and a girl. She got to be eighteen, her situation didn't change. It wasn't likely to change. So what—the plans were as follows for the family. My mother was very ill and she died within six months after I left. So that left my father and sister. My father was overage for the military. Oh, there's a funny story there.

As I told you my father was quite well off. When Ormandy came for visits, he rented a car, was driving all over. That really impressed my father, and from there on, he wanted to have a car, but he didn't know how to drive, so he bought a car and bought a [driver] to go with it. And he paid the [driver]—I don't know. Whatever the going rate was. And the [driver] took us here or there on excursions.

Well, then this was 1938, and the political situation was getting tighter and tighter. The Hungarians mobilized, and they decided there are not many cars in Hungary. They're going to mobilize all cars, all privately owned cars. They all go into the military. So here's my father. He bought the car. He's paying a [driver]. The car goes in the military, so does the [driver]. No, I'm sorry, the car didn't go. Only the—the [driver] gets drafted and he goes in the military. My father now has a car, no [driver], and he can't really drive. So he says, "I'm going to get them." So he hires a [driver] who has a lame leg. And now the next time the political situations gets even tighter, now the government decides they're going to mobilize all cars and all chauffeurs that go with the cars. Never mind if he's got a lame leg or not. So my father was without a car and he had two chauffeurs in the military and he had to pay them.

ROBERTS: So he had to keep paying them? Even after they'd been drafted?

WYMAN: Yeah, until the emergency was over.

ROBERTS: Wow.

CARUSO: I mean, given that there was a lot of…

WYMAN: Two [drivers], one car.

CARUSO: Given that there was a lot of political awareness at the time, I was partly curious if your father was concerned with the fact, that—I mean, war was going on. Were they—your father was just comfortable staying in Hungary? He didn't want to leave the country and avoid this mounting European conflict?

WYMAN: He didn't want to leave the country unless he had to. He felt that he probably would have to. Now, Hungary's position during the war was a very, very mixed up position. And I don't know if you want me to analyze it. It was a mess. Anyhow, my father decided that he and my sister would leave in 1940, which is a year after the war broke out. And he also had a girlfriend by then, and he was taking her, and he arranged visas, and tickets, and whatnot. And they were coming **<T: 40 min>** through Spain, and the only place you could get a visa for was Buenos Aires, Argentina. By boat, I think it was Spain or Portugal. I'm not sure which. But train to Spain, boat to Buenos Aires, and then hoped to get to the U.S. from there somehow.

Well, [not all plans] work out. The boat took off from wherever it was, Lisbon, [Portugal], or somewhere, and somehow the British got wind of some high-powered German intelligence people being on that ship, so they stopped the ship on the high seas and took it into a British base in the Caribbean. And they found the Germans that—they thought they had them. And then they asked everybody on the boat, "Now, who were these Germans friendly with?" And it turns out that my father's newfound girlfriend was quite friendly with the Germans. So they said, "All right we'll take her off too." This was in Trinidad, is where they stopped. They stopped them and took them to Trinidad. And as they get the girlfriend off the boat, well, it turns out, she can't really get off the boat with her belongings because her belongings and my sister's belongings were put together into a couple of suitcases. Well, the British didn't care, took my sister off.

CARUSO: Wow.

WYMAN: Well, my sister was fourteen so she couldn't go off alone, so who's she with? Her father. Took him off. Everybody got off the boat. So they were stuck in Trinidad for six months because no boat had room to go to South America. Everybody wanted to go to South America. So at the end, they finally got to Buenos Aires and then a couple of years—a year or so later, they came to the U.S.

CARUSO: So they came to the U.S. while you were still in college?

WYMAN: I was at Cornell, yeah.

CARUSO: You were at Cornell? And did they—where did they settle?

WYMAN: New York.

CARUSO: New York City, [New York], or…

WYMAN: New York City.

CARUSO: And that's where they stayed for the rest of…

WYMAN: Yeah. For a long time. My sister is now in California, but she was in the New York area until, oh, fifty years or so.

CARUSO: You mentioned that the *gymnasium* system was quite rigorous, so I'm guessing that from your early age of ten, you were probably heavily invested in your studies. You also mentioned that you had done quite well at school, but I am curious to know two things. One, whether or not you were participating in things outside of school or in addition to school. Like did you play tennis? Did you read books a lot, were you in the backyard mixing chemicals together to make shoe polish?

WYMAN: I was captain of the water polo team. Water polo is a great Hungarian sport. I was captain of the water polo team when I was sixteen, and our school was never very noted for athletic ability. It was mainly brains. So everybody was surprised when we made the finals in the water—in the national water polo championship, but we didn't get very far.

But I was captain of the water polo team, and I did some individual swimming. I had a couple of medals, scholastic medals. What else? I made the second team in ice hockey. Again, our school was never very good at ice hockey. It's—so yes I did participate. And I was fencing. As a matter of fact, my fencing was good enough, so when I got to Cornell, I continued the fencing and I was on the Cornell fencing team while I was an undergraduate there.

CARUSO: What was your weapon of choice?

WYMAN: Saber.

CARUSO: Saber. So the other…

WYMAN: But, again, fencing is a traditional Hungarian—they won a lot of Olympic medals—just like water polo is, and at Cornell, very few people were interested in fencing.

CARUSO: In terms of your education while in Hungary, was—you mentioned that your father said, "Oh you should become a chemist." You mentioned—your father mentioned that he thought you should go **<T: 45 min>** into chemistry. Was there something that you were interested in doing? Did you want to be a historian, for example, or…

WYMAN: Geography was my favorite subject. My father's suggesting chemistry was enough for me when I got to Cornell, I signed up as a chemistry major, but I could have changed. But it so happened that Cornell had a very good chemistry professor for freshmen, the elementary chemistry course was very well taught and made it very interesting, so…

ROBERTS*:* Do you remember who that was?

WYMAN: Yeah. A.W. Laubengayer. You want to write this down? I'll spell it for you. I think he's dead now. L-A-U-B-E-N-G-A-Y-E-R. He was a professor and he was very good. He gave very good lectures. He may not have been completely up to date on the quantitative aspects of inorganic chemistry, but he certainly got the attention of his people, his audience.

ROBERTS: And so that solidified things for you?

WYMAN: Let's see if I can stretch out my leg. I think I'll just go back a little.

Yes, Laubengayer—I mean, if this lecture had been a bad one, I probably would have looked around to see what else I could learn, but he was good. And from there on, everything was—and the next one was very good too. Organic chemistry was the next course and he, [J. R. Johnson], was an excellent lecturer.

CARUSO: Was it at all difficult transitioning from Hungary to the United States? I mean, they're different cultures. The U.S. is—let's see, by the time you were here, it was probably

improving in terms of its economy, though it had been hit pretty hard with the Great Depression. Was it difficult being in the U.S., transitioning to the way of life here?

WYMAN: I had a little bit of money the family gave me. "Money" meaning dollars. And enough to pay for entering Cornell. And the people were all very nice, very friendly. The students were all very nice, very friendly. And I—perhaps thanks to the two summers in England—I don't know—I adapted quite easily. I mean, it's a different culture, yes, but still, it was painless. I always, almost every summer—we spent the summer abroad from Hungary, much of the time in what was then Yugoslavia and there we had international friends. I mean, this was international resorts. They came from all over.

So all that was fairly easy. And yet, I had a few Hungarian friends, friends of the family, like my friend in Orlando. We were friends. Now he's—he has no interest, no knowledge of chemistry, but we had a lot in common and grew up together. And were together when the Germans invaded Russia, and we spent a lot of different times together over, what is it? Seventy years or so.

ROBERTS: Were you able to keep track of what was happening with your family while you were at Cornell?

WYMAN: Yes. We corresponded while I was at Cornell for a while, and then my father packed up and left for Buenos Aires, which took him about a year to get to. But in the meantime, yes, we could correspond when he was in this internment camp. **<T: 50 min>** And in the 1940s, if you were going to be interned anywhere, the Caribbean was a…

CARUSO: Yeah, Trinidad is probably…

WYMAN: A choice place to be. I mean, it was not like being in present Trinidad hotels or something, but it was certainly no great danger of any sort. They were safely stashed away in a British internment camp until they found a ship that would stop and had room to take them. And it took six months, because ships were full and they didn't take people. And then they stayed in Buenos Aires for a couple years or something like this.

CARUSO: When your father got in to New York, did he take up textiles again?

WYMAN: Yes, but at a very low level. He was—he could not become an entrepreneur. He was working for other people. It was very hard for him, and also he didn't speak much English, which was another hardship. So after a couple years, after the war, he tried to—he went back to Hungary and tried to reopen the old business. And that didn't work out. By then, the

communists were knee deep in the business world in Hungary and there was no hope. So then he came back, and lived a few more years, and died.

A lot of Hungarian men who—especially the ones who had a position, reached some sort of a position in their adult life—when they came to the U.S., where they had to start all over, they found it very difficult. They frequently escaped and went back to Hungary. My friend in Orlando, his father went back. He was a bank director. I mean, not all, but frequently. It was bad for the fathers.

CARUSO: So when you got to Cornell, you were—I think based on what you said, it sounds like you were coming in with the equivalent of maybe—as a sophomore, a second year student.

WYMAN: First as a sophomore, but then they upped it to almost a junior.

CARUSO: Close to a junior.

WYMAN: I had to complete two years of undergraduate work plus one summer session and that was it.

CARUSO: Okay so you were sort of a second semester sophomore coming in as an undergraduate.

WYMAN: Right.

CARUSO: You mentioned you had a great entry-level chemistry professor. I'm curious to hear a little bit more about what made him just a great professor. Was it just his lecture style?

WYMAN: His lecture style. It was very good.

CARUSO: Okay. Did you have to—were there laboratory components to your classes?

WYMAN: Sure.

CARUSO: And what were those laboratory components like? Was it just a predefined project that you had to pour A into B, see what happens?

WYMAN: That's right. They told you exactly what to do. It's asking me now to think back to nineteen—I entered Cornell in 1939.

ROBERTS*:* It's only seventy years ago. I'm sure you remember it just like yesterday.

WYMAN: Nineteen thirty-nine.

CARUSO: So pretty much from the outset, you mentioned that you weren't necessarily—I mean, you entered as a chemistry major, but you wouldn't—you made it sound as if you would consider doing something else, possibly.

WYMAN: Yes.

CARUSO: Was there ever a time at Cornell that you thought to yourself, "Actually, I don't want to do chemistry"?

WYMAN: No.

CARUSO: No. Okay.

WYMAN: After my undergraduate degree, after two years, I was fully committed.

CARUSO: Okay.

WYMAN: And I—now I've come down to chemistry. At Cornell, in graduate school, you had to choose—either major in organic chemistry, inorganic chemistry, physical chemistry or what. And I was always a fish out of water. The organic chemists looked upon me as a physical chemist, and the physical chemists looked upon me as an organic chemist. And **<T: 55 min>** then much later, I really became a photochemist. And I guess when I was at Cornell, there was no such thing as a—yes, there was. Very few schools had photochemistry, and it was mainly vapor phase photochemistry, which was not really…. Organic photochemistry really developed in the 1950s.

And here I have something.[3] I don't think I ever gave it to you. Is that the photochemistry one?

ROBERTS: Yes.

WYMAN: This […], I think, is a pretty good description of—sort of an autobiographical description—of how and why I landed in photochemistry accidentally. And, now, there is a tremendous revolution, of course, after the war in chemistry. You guys are too young to know this. And that all involved instrumentation. I mean, today you go into a chemistry laboratory, and you've got instrument after instrument after instrument, and there was no such thing in 1939. And the spectroscopy was the first, and I had the good fortune of—well, maybe I'm jumping ahead of you. Okay. I'll take a chance. I finished Cornell.

Now a couple of things—although there are a couple of things that happened at Cornell. No: one happened at Cornell, one later. I got very much, very concerned about ethics in chemistry. I got stung by an unethical chemist at Cornell. I got stung by an unethical chemist in my second industrial job, and then later on, by a university professor.

CARUSO: So…

ROBERTS*:* Go ahead.

CARUSO: I was just going to—just for clarity, when you say your second industrial job, are you talking about at General Aniline and Film Corporation?

WYMAN: Right. The first two were very similar.

ROBERTS*:* Can you explain what you mean by unethical?

WYMAN: All right. It's really quite simple. I was at Cornell and starting on my master's, and this war was going full blast. Cornell—faculty was in Pittsburgh, [Pennsylvania], and Chicago, [Illinois], wherever. Hardly anybody in Ithaca, [New York]. So the only professor I could work for was a young man named Bill [William T.] Miller.

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[3] George M. Wyman, "Reminiscences of an Accidental Photochemist," *European Photochemistry Association Newsletter* 50 (1994): 9-13.

And Bill Miller was a very fine chemist, and Bill Miller says, "Well, we have you here for a master's. Well, the first thing I'd like you to do—" he says, "I had this fellow working for me last year, and he finished his master's work," and his name—I only remember his first name. His name is Harry somebody. Okay. "But Harry synthesized this compound and it's all in his thesis. So why don't you start by synthesizing Harry's compound?" And now I don't remember what Bill Miller wanted me to do with the compound, but he wanted me to pursue something beyond what Harry had done. But to do it, I had to synthesize the compound. And it was all described in detail in his master's thesis. And I followed it step by step and I don't get his damn compounds. And I do it again, and again, and again, and it still doesn't have the proper—I don't know, it didn't have the melting point. It didn't have—I don't know what. But it obviously—I was not getting what he said he was getting.

So I went back to Bill Miller and I said, **<T: 60 min>** "Look, I can't get what Harry got, what Harry said he got." At this point, Bill Miller doesn't know. Did Harry cheat or am I a dodo? It's one or the other. And I don't know how that was resolved, but he finally gave up on the whole thing, gave me a new project to work on, and everything went on. I met Harry at a meeting later on. I said, "Harry, Bill gave me your—he wanted me to continue working on your compound and I couldn't make it."

And then Harry said—by then, he was in industry. And then he said, "Well, you know, George, it wasn't quite exactly the way I described it in the thesis." Well, that's enough. I mean, that means that his thesis was a fluke. And we gave up on that. And I'd say the guy's probably dead by now. I don't know what this was, but I had to do something completely different for my master's.

And believe it or not, about five years later, the same thing happened at General Aniline. I was a new guy. I had been there about a year, and the big chief was—the chief of the laboratory was a—this was at the end of the war. No, after the war just ended. This guy was an older German chemist, Austrian, they imported from Austria, and he worked for the big German cartel, the BASF [SE], originally, and he came to work in New Jersey. And he put me to work. He says, "There's a guy named [Benjamin] Price Truitt. He left us a year ago and I want you to repeat what he did, and then we want you to do something beyond."

So I got his report—you know, thesis. And I repeat exactly what he did. And this was a dye company, and the ultimate test of the whole dye company is, there's a dye lab, which has a very controlled way of dying fabrics, and then the human eye decides. In my case, this was a bright yellow dye, and the end result should have been a bright yellow dye. And by the time they got through, I made—I did the synthesis, and they dyed the standard batch of cotton. Instead of bright yellow, it was kind of *bleh*. It was sort of a light beige. It obviously was not what it was supposed to be.

Well, in this case, the previous guy who left, he had—he got some bright yellows. How did he get it? Well, he could have gotten it one of two ways: by going through the procedures that he outlined, or by doing through half the procedure, because he'd gotten some imported intermediate. It was a five step synthesis, and after step three, if he took some out of that tin box that came from Germany, and did the last two steps using that intermediate, and not what he

made, then maybe that intermediate will give you the bright yellow. But using the stuff he made, it will give you a *blah*, which is what it was.

Well, the lab director really was mad. "How can this happen?" Same question: am I a dodo or is the other guy a crook? And this time, he's decided he's going to do it himself. I mean, he had all the know-how of thirty years of synthesizing dyes. And he stayed in the lab over the weekend and whatnot, and he made this stuff directly from A to Z, and it came *blah*. So he decided it wasn't me, it was the other guy, **<T: 65 min>** who apparently took the German intermediate, but he couldn't make it all the way from A to Z. He took it at the halfway point, the German intermediate, and then he got nice bright, bright.

Again, I met him at a meeting later on. I said, you know, I said, "This report—you had this dye in your report, and it came out beautiful yellow, and when I tried to repeat it, it didn't come out beautiful."

"Well yes, some of the time, I did use that intermediate, imported it in."

So this—these are the things that made me very, very sensitive to ethics. And then I discovered—then I had one more thing that was quite different. Then I discovered that, unlike most societies, the ACS [American Chemical Society] did not have an ethics committee. The British [Royal Society of Chemistry] have a real serious ethics committee. They have sort of quasi-legal procedures, and if that quasi-legal procedure finds you guilty—like in these two cases they would have—the guy's tossed out of the society. Well I—then I started agitating to have an ethics committee in the ACS. And I used my political influence, in that [I have] friends—Ernest [L.] Eliel was a friend who was a former president of the ACS. Also a former resident here, until he died about seven or eight years ago. And other people I knew who were quite high up in the ACS. And we went through about four or five years of hearings and whatnot, and committee meetings, and finally it came up for a vote in your city [Philadelphia, 2004], when the ACS met there about ten years ago or something, and the vote was to table. They still didn't want to take a chance on a watered down version of an ethics committee. And finally a year later, at another ACS meeting, we had a new president [William F. Carroll, Jr.], he helped push it through. And the ACS now has an ethics committee, but it doesn't have the bite that the British have in theirs.

ROBERTS: So was that an issue you stayed involved with through ACS during that entire period?

WYMAN: Well, I was involved on that one topic. I attended a fair number of ACS meetings after I retired. Now, we've got a big long jump there, but I was a consultant to university chemistry laboratories, chemistry departments, including the University of Pennsylvania, where I was twice. A fellow with an Italian name like yours […], was sort of the assistant chairman of the chemistry department. [George Palladino] […]. He had me there talking to his faculty. The consulting I did for the chemistry departments was [to] advise faculty, especially younger faculty, on how to—how best approach research supporting agencies to get some money. Either by giving them tips for news and supporting agencies that they don't know about, or the ones

they knew about, but how—what sort of things the agency is likely to be looking for. Since I was director of one of these agencies for, I don't know, twenty years or so. But that was part of…

ROBERTS: Maybe—do you want to go all the way back and fill in <**T: 70 min**> the time at Cornell?

CARUSO: Sure. Sure.

ROBERTS: Can we take a quick break?

CARUSO: Yeah, we can take a break. We'll stop this.

[END OF AUDIO, FILE 1.1]

CARUSO: What I'd like to do is just sort of finish up with your time at Cornell. I know that you mentioned that you got your master's and you got your Ph.D. You were working with Bill Miller for your master's, I'm not sure if you were working with him for your Ph.D. as well.

WYMAN: Yes. He was the only one left.

CARUSO: He as the only one at the department. I am curious to know how it is…. Nowadays when you are finishing up your undergraduate degree, you have to apply to other universities for master's and Ph.D. Did you have to do any of that?

WYMAN: No, they sort of encouraged it, but especially after your bachelor's, you should go somewhere else for your graduate work. But I was looking in other universities for a teaching assistantship, which is what goes with graduate work, at least at that time it did. And I couldn't find one. So I was—you might say I was stuck at Cornell. So I said, "All right, I'll stay at Cornell and pay my way." And I paid my way for one semester, and then Bill Miller found me a teaching assistantship. And so thereon, I became a teaching assistant for the rest of my two years or so.

CARUSO: Why is it that you wanted to get advanced degrees in chemistry? I mean, I know there are a number of people…

WYMAN: My father told me to get the top degree that you can so you can make the shoe polish. No—I mean, certainly the idea was in my mind that to be competitive, you want to learn as much as you could.

ROBERTS: So it was pretty clear for you from the beginning that you would stay for a graduate degree?

WYMAN: Probably.

CARUSO: You said that you did look for teaching assistant…

WYMAN: And those days, graduate school tuition was cheaper than undergraduate school. And of course, if you got a teaching assistantship, you had no fees, no tuition, no nothing. It was all—and you got a little pay. I got fifty dollars a month for being a teaching assistant.

CARUSO: You mentioned that you were looking for teaching assistantships elsewhere. Were there specific universities that you were looking to do?

WYMAN: Yes. Penn State [Pennsylvania State College] was one. I had some friends there. I didn't get one, and I can't remember if any others—I'm sure I tried a few.

CARUSO: Okay. You also mentioned that during your graduate career, the organic—I think you said the organic chemists saw you as a physical chemist and the physical chemists saw you as an organic chemist.

WYMAN: That was more after [starting] graduate school.

CARUSO: That was more after—okay. Okay. So after the—you told us a bit about that first project…

WYMAN: Now, Bill Miller was a special kind of organic chemist because he was very much interested in mechanism. […] You see, physical organic chemistry, as such, which became a very important subfield of chemistry, was just beginning to get on its way.

When I got to the [National] Bureau of Standards seven or eight years later, I taught a course in physical organic chemistry because staff members were encouraged—people with a doctorate were encouraged to give courses for the others. And to find the kind of—to find a textbook for a course of physical organic chemistry in 1951 or 1952 was not easy. I finally settled on one on—a textbook by Michael [J. S.] Dewar, whom I didn't know at the time.[4] Later on, we became good friends. But Michael Dewar was sort of—he was an outcast in British chemistry because he questioned some of the…. The big name in British physical organic chemistry was [Sir Christopher] Ingold. [Edward D.] Hughes, and Ingold at the University College [London]. And Dewar questioned some of Ingold's ideas and that put him out in the doghouse, literally. **<T: 05 min>**

So Dewar, when he was due for a professorship, they gave him one on the east side of London, which is about the worst place in any way you want to see London. Then later on—Dewar is a brilliant man, and the Welch Foundation, when they established a professorship [Robert A. Welch research chair], the first one in the U.S., Michael Dewar was their choice. They dragged him, brought him over from England, and made him the first Welch professor at [University of Texas at] Austin. And as I said, Michael and I became good friends for a long time.

CARUSO: So you mentioned that Bill Miller gave you this research project. It didn't—that first one didn't really work out, so he assigned you something else. Can you tell me a little bit about—do you remember what project you were working on? No?

WYMAN: Oh yes, yes, yes. No, that may be my doctoral project.

CARUSO: Okay. What I was going to ask you, in part, was, did you get to choose what you wanted to do for your doctorate or was it something that was assigned to you as well?

WYMAN: No. I didn't know enough.

CARUSO: Okay.

WYMAN: What Bill Miller was interested in was—Bill Miller was a fluorine chemist and there are very few fluorine chemists in the 1940s. And in fact, that's why after a while, Bill Miller took off, over to the Manhattan Project in New York, and he took his whole damn research group with him except me.

[4] Michael J. S. Dewar, *The Electronic Theory of Organic Chemistry* (Oxford: Clarendon Press, 1949).

ROBERTS: Why was that?

WYMAN: I was an enemy alien. So he left me in Ithaca. He left one other guy in Ithaca because that fellow was almost finished. And the rest of the group went to New York with him. And what we were working on at the time was the whole idea of the acidity of negatively substituted amides. In organic analysis, it was well established that if you had sulfonamides, and if here was still a hydrogen on a sulfonamide, that hydrogen would be acidic because of the SO_2 group, what, the inductive effect of the SO_2 group, and would be and could be removed by base. But Bill Miller wanted to have something similar or look for something similar with the trifluoroacetamide because trifluoroacetyl group would also be electronegative, and, again, if there's a hydrogen there, you could use it. It was something like that in my Ph.D., I think that was a Ph.D. thesis.

CARUSO: So I mean, you mentioned that Bill Miller was pretty much the only chemist left at Cornell because of the war work.

WYMAN: Yeah, and then he took off.

CARUSO: And then he leaves, and so my vision, now, is you and one other person who's finishing up sitting in a lab doing your dissertation, your doctoral thesis completely on your own. Is that…

WYMAN: Pretty much.

CARUSO: Pretty much.

WYMAN: Now wait a minute. There was one very important addition then. How important only turned out later on. The only faculty around then [were] the very old—that they didn't really want for the war work—and the very young, who haven't really established themselves. Among the very young—I'm giving you now the history of Cornell during the war. And the—well they also, in the meantime, Cornell also acquired Peter [J. W.] Debye as department chair which was a big surprise to everybody, but I don't want to get into that. I was not involved [with] Debye at all.

ROBERTS: Well, why was that a big surprise to everybody?

WYMAN: Well, Peter Debye had gotten **<T: 10 min>** the Nobel Prize say in 1935.[5] I don't know which year. And he was an institute director of the Kaiser Wilhelm Society [for the Advancement of Science], which was like the National Academies in Russia. They had their own institute, and if you were an institute director at the Kaiser Wilhelm Society, the sky was the limit. And if you wanted your office plated in gold, you get your office plated in gold. So Peter Debye, like every other institute director, had everything he wanted.

In 1939—now, Cornell has always had, and may still have, a famous, well-established visiting professorship. And it was called the George Fisher Baker visiting lecturer. This is a very, very senior professor from Timbuktu or wherever, who comes to Cornell for a semester or a year. They pay him whatever they pay him, and he interacts with the faculty at the faculty benefits, the students benefits, everybody. Anyhow, Debye signed up for the Baker lectureship for 1939. And here it's 1939, and Debye is there. It's one year or one semester, I'm not sure. And then you go back home to the gold-plated institute.

But war breaks out. And all of a sudden—and I was just a beginning graduate student, not even that. All of a sudden, the word gets around that Debye wants to stay. But he doesn't want to stay just to be a professor. In Germany, professors come in grades. So he wants to stay, but he wants to be chairman of the department. Well, there was a chairman, but they can get rid of him. So Debye is announced as the chairman for 1940 of the chemistry department, and he remained chairman for God knows how long. And the present chairman will step down a notch, and Debye just stayed at Cornell until he died.

And then about fifteen years after all this, there's a controversy: was Debye a Nazi? And the guy who started the controversy was Roald Hoffmann, who's Jewish, is a Nobel Laureate and whatnot.[6] And he wasn't there at Cornell at the time. The only Jewish faculty member at Cornell at the time was a fellow named Simon [H.] Bauer, [whom] I knew very well. He was my boss in my teaching career. I mean, as a teaching assistant. And Bauer's a great guy, and a great scientist, and a fine fellow. And the other thing that—when this controversy came up—the controversy came up because the entrance hall to Baker Laboratory was christened for Debye. This was a Debye Reception Hall or whatever it is, and Roald Hoffmann said we've got to change that, that Debye was a Nazi.

And there was quite a bit of controversy about this. And as an alumnus, I took—and I know Roald Hoffmann, and I said, "I don't believe this. In the first place, if Debye was a Nazi, why in hell did he stay at Cornell instead of fighting in the—" I mean, "being present in the great war, or liberation of the Germans, and whatnot?"

And my main evidence that says Debye couldn't have been a Nazi is that one of Debye's first graduate students was a fellow named Ulrich [P.] Strauss, my age, who was a German refugee Jew. Arrived at Cornell, looked for a professor to work for, and picked Debye. And worked for Debye for five, six years, and then he became a faculty member at Rutgers [the State University of New Jersey]. And I told Roald Hoffmann, I said, "You ought to talk to Ulrich

[5] Debye was awarded the 1936 Nobel Prize in Chemistry.
[6] Roald Hoffman shared the 1981 Nobel Prize in Chemistry with Kenichi Fukui.

Strauss." I mean, he wouldn't have put— **<T: 15 min>** if there'd been the slightest indication that Debye was a Nazi, Strauss, by the time the war was over, he had ten others he could have worked for. Even at the beginning, there was a top-notch physical chemist, John [Gamble] Kirkwood, who was at Cornell. Later on moved to Caltech [California Institute of Technology]. But I don't believe that Ulrich Strauss would have worked for Debye if Debye had the slightest tinge of being a Nazi. I just—it's just incompatible. I mean it's like water and oil. They just don't mix.

ROBERTS: So how was that issue resolved at Cornell?

WYMAN: I don't know. I'm sort of curious to know if that entrance hall is now named Debye or if it's not named Debye. I don't know.

CARUSO: I went to Cornell, and I actually don't know the answer. I've been in Baker Labs [Baker Laboratory] a number of times, but I never paid attention to whether or not it was named—the entrance hall was named.

WYMAN: No, and at some stage when you're there, it may say Debye and if you came—you probably wouldn't pay any attention for the entrance hall that's got a name. And if the name is Debye, it wouldn't be a big surprise. And if the name is not Debye, it would also not be a great surprise. But this was a big stink. It was written up in the *Chemical & Engineering News*.[7] Must have been about ten years ago, and I couldn't…. By then, Ulrich Strauss may have been dead. I don't know. I didn't—he was my age, so ten years ago, he would have been eighty and eighty's a respectable age to be dying. So—but anyhow, Debye stayed on, and stayed on at Cornell. They also found a teaching job in physics for his son [Peter P. Debye]. It was all part of the package deal. This was a big thing for Cornell to have Debye at Cornell.

CARUSO: So we had been talking about you doing your Ph.D. thesis. You had mentioned that Miller had left to work on the Manhattan Project.

WYMAN: Oh, I started—I was going to tell you about two new arrivals. Yes. This was now 1944, the last year of the war. Sort of 1944, 1945. Okay, the two new arrivals were a couple of inorganic chemists, both men, of course, and young men from [University of California] Berkeley. One I'm sure you've heard of, Henry Taube, who had a Nobel Prize, I don't know, thirty years later.[8] And the other one was a fellow named Thor [R.] Rubin. You've probably never heard of him.

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[7] William Shulz, "Nobel Laureate Is Accused Of Nazi Collaboration," *Chemical & Engineering News* 84 (2006): 19.
[8] Taube was awarded the 1983 Nobel Prize in Chemistry.

CARUSO: No.

WYMAN: Well, he and I roomed together. We had an apartment together and both Rubin and Taube came all indoctrinated with the new ideas of inorganic chemistry, how inorganic chemistry is no longer just a field of qualitative—what we know about how this atom will act this way, and this ion will act that way and whatnot, just all the very qualitative way. But a couple of top people in California put things in a quantitative basis. The oxidation potential from this state to this state would decide whether this atom is going to react this way or not. I mean, I didn't know much about this.

Anyhow, it kind of put Laubengayer's chemistry into the freezer, and that was quite a controversy among the inorganic chemists at Cornell. Laubengayer had a couple of other people there. [Jacob] Papish and [A. W.] Brown were even older, and these were all very qualitative inorganic chemists. And these two young men were sort of revolutionaries.

All right, now Henry Taube…. In my thesis, if I could ever find it again, but it's too late now, might not have been found. It never—hasn't hindered me in any **<T: 20 min>** way. I acknowledge Henry Taube. I had a lot of very useful discussions with Taube when he was just arrived at Cornell because he was an inorganic kineticist. Laubengayer wouldn't have known—he wouldn't have known what kinetics was all about, probably.

All right and I got Henry Taube to organize a kinetics seminar, which is something that's organic, inorganic, everybody could take advantage of the new methods of kinetics. New meaning nineteen—what was it?—forty-four. Kinetics for everybody, and many of the graduate students participated. And it was really a seminar in that Henry gave the first one, and Thor Rubin gave the second one, and then it was the graduate students who gave seminars. And people like Taube and Rubin were there to criticize, and even learn. That was very good. Even after I got out of school, I went back for a visit one time, and I gave a seminar also.

So we benefited a lot from these two guys. There was a young man in organic chemistry. His last name was Russell, and the last I heard about him is he was put in jail in Colorado for stealing some of the chemistry department hardware, hammers and stuff like that. So that was after he left Cornell and went to Colorado. So he was—he didn't get a Nobel Prize for that [laughter]. All right, but he had the organic—he was about the only organic…. Bruce may have been there. I think it was William [F.] Bruce. He was the third—there were three organic professors. [John R.] Johnson was the senior. Miller and Bruce, and Bruce may still have been—I think Bruce stayed at Cornell. Yes. And then this guy, Russell, they hired him from somewhere, and then after the war and he had the permanent job at Colorado. He was caught with his finger in the till.

All right, the—oh, oh, oh, oh, [Alfred T.] Blomquist. Now, Blomquist is a special case. Blomquist was a student of…. Organic chemistry in 1940 was all derived from the group in Champaign [Illinois], and University of Illinois [Urbana-Champaign]: Roger Adams, Carl [S.] Marvel, [Ralph L.] Shriner until he moved to Iowa, and all that bunch. [Harold R.] Snyder,

[Reynold C.] Fuson, the great names, all great names in organic chemistry. Johnson was one of Adams' students. My own former boss Wallace [R.] Brode also was a student of Adams' but he got into instrumentation. And these guys at Illinois, they were all very much—well, there was a compendium, about six volumes taking up this much in the bookshelf. It was organic chemistry and this was written by one—each volume was written by one of these guys I just mentioned to you. And that was real synthetic organic chemistry and that's why I say that I was never that kind of a chemist.[9]

ROBERTS: What was different about what they were doing compared to maybe what you were doing with Miller and what you were seeing at Cornell or other places?

WYMAN: Well, most people—I mean, no, that's wrong. These guys would start with something, would end with something, they would establish that end product for sure, and they'd be happy. Bill Miller, Michael Dewar especially, others who were more interested in the [mechanism] and would say, "All right, **<T: 25 min>** we started with this. We ended with this. How did we get there?" That's the question that these guys didn't ask. They didn't care. "We went from this to this," and frequently, that's all you need. I mean, if you have a new way of making aspirin, well, you start with—I don't know—a floodwater and you wind up with aspirin, you're very happy. You can sell the floodwater a lot better price than you did before. And that was quite a controversy at the time between the mechanistic organic chemists and the purely synthetic organic chemists. I was never—I don't know. I was never happy with just starting with this, finding out what this is, and quit without knowing how did we get from here to here. And this was the beginning of mechanistic organic chemistry, physical organic chemistry. Call it what you wish. And Bill Miller was interested in that, but then he was taken the war work. When he came back, he did a lot more work on this.

ROBERTS: So this is a period of big transition between these two groups.

WYMAN: Yes. I was going to tell you about Blomquist. Blomquist was a student either of the Illinois group, or perhaps once removed, a student of J. R. Johnson at Cornell. And when he—and he was a very, very methodical synthetic chemist. He was fairly young when the war broke out. He was maybe thirty-five. And Cornell hired him because there was a big gap. There were no organic chemists inside. And Blomquist was—he had a photographic memory. I watched him once giving a lecture where he—where the lecture consisted of page, by page, by page, and page of the textbook. There's no point in paying somebody to read word for word from the textbook. His family had a very fancy menswear store in Chicago [Illinois] right at the main intersection in Chicago at Michigan Avenue and the lakefront, and when he couldn't get a decent job in teaching or chemistry, he went back into the family store. And when the war broke out,

[9] Wyman refers to *Organic Chemistry: An Advanced Treatise*, Vol. I and II (New York: John Wiley & Sons, 1938), the second edition of the first two volumes (Wiley, 1943), and *Organic Chemistry*, Vol. III and IV (Wiley, 1953).

Cornell invited him out from the family haberdashery store and gave him a professorship. And he stayed on for a while. I don't know, I think he may have stayed on till he retired.

But it was a—at Cornell there was a very clear cut difference between Blomquist's kind of organic chemistry and, say, Bill Miller's kind of organic. You see, Bill Miller was a fluorine chemist. And fluorine in an organic molecule has electrostatic effects or other—even other electronic effects. So you choose one of two things. You want to pay attention to these effects and understand the chemistry a lot more, or else you just say you go from here to here and that's the way it is. And I like the other kind. I mean, I like the mechanistic kind. […]

ROBERTS: So you were accidentally at the vanguard of this new mechanistic approach to organic synthesis. Would that be right?

WYMAN: Well, now, Miller had a very large research group, and they all packed up and went to New York. He had about ten people. I mean, it was very large in those days. And I was the one left behind. But I—I mean, New York to Ithaca at that time was eight hours on the train. No other way. Or six hours by car.

And every now and then, Bill Miller would come to Ithaca. He came to give me my exams. I would sometimes visit him in New York. **<T: 30 min>** I had friends in New York and whatnot, but—so we were not completely out of touch. But I had much less direction in my master's and doctorate than most people do in theirs. Although, I don't know, Debye didn't pay any attention to his students. So—I was dating a girl, and she signed up for Debye and never saw the man.

CARUSO: So when you're finishing up your degree at Cornell…

WYMAN: You have to give me time.

CARUSO: Sure.

[END OF AUDIO, FILE 1.2]

WYMAN: When I finished my Ph.D., it was a special situation, in that you can't just look for a job. I mean, […] the draft board was after me all the way.

CARUSO: So I guess one thing that I'd like to know more about—I mean, you came to the U.S., you were not a U.S. citizen when you enrolled at Cornell. Did you…

WYMAN: I was not a U.S. citizen when I graduated from Cornell.

CARUSO: In terms of your Ph.D.?

WYMAN: I didn't become a U.S. citizen until 1949.

CARUSO: Okay.

WYMAN: And even when Wallace Brode offered me a job at the [National Bureau of Standards in 1948], he says, "Let's wait six months until you go through and become a citizen, then we can hire you as a citizen rather than get an exemption for a foreigner." So I was not a citizen all this time, but that didn't stop the draft board.

CARUSO: Even though you were technically from a country that we were at war with?

WYMAN: Yeah.

CARUSO: Okay.

WYMAN: I mean, like my friend in Orlando, he tried to enlist in the Marines. Now, they wouldn't take him when the war broke out. So then he went to the Army, they did take him, and then he got citizenship within three months because he volunteered in the Army. And from there on, his career went on as a citizen.

CARUSO: Did you have a sense—and I do want to hear about the draft board coming after you, but when you were finishing up your degree, did you have a sense of where you wanted to go? I mean, you know, there are options: academia, industry, government research.

WYMAN: Academia was out. Everything was out except war industry. Otherwise, the draft board will get you. In other words, if you say, "All right, I'd like to go and work at the Mellon Institute [of Industrial Research]," out. Three months later, you're in the Army. You want to work at, I don't know, University of Chicago. Maybe there's a choice job there for a Hungarian speaking organic chemist. Who knows? No. Teaching jobs were out. Had to be war industry. And on the other hand, that was in the days when no chemist was going without a job. So Bill

Miller found me a job in the war industry before I ever finished. And I started with General Chemical, which was a division, at the time, of Allied Chemical and Dye [Corporation], which later on merged—God knows with whom.

CARUSO: If you had had your choice, would you have wanted to stay in academia, or did you…

WYMAN: Yes. Yes, I think I would.

CARUSO: Okay.

WYMAN: And I tried a couple of times later on to find something in academia, but I couldn't. I'd interview—when I was in industry—this was after the war now. No restrictions. You could be a street sweeper if you wanted to and you wouldn't get drafted. And I had an interview with Swarthmore College, which was a very good…

CARUSO: I live in Swarthmore, [Pennsylvania].

WYMAN: All right, so you know about Swarthmore. All right, well, the pay there would have been half of my industrial pay. And I said, "I can't afford that." When I took the job with the federal government, I took a cut. Not much, but slight. So it…well of course I got into the academia, but the—when I wound up dishing out money to academic sciences, which was not until 1960. The net result is that **<T: 05 min>** I still have many, many academic scientists as personal friends, not because of the money I gave [them], but because I think we had a very good relationship.

One friend is dead now. We got acquainted because I supported his research, and we discovered we both liked to ski, and we went on many ski trips together until he—he was two years older than I was, so he gave up a little before I did, about the same time.

And—oh, I had one very interesting experience. I don't know if you guys know about this. I liked to ski. You got that message. And I liked to ski in Switzerland. So I went pretty regularly. I found the place that was quite inexpensive for me in the best ski area in Europe, probably, in Davos, [Switzerland]. This is after I retired. And I, also, by then established, and I had some very good relations with some high-ups in Hungarian chemistry. And the reason for that is this.

In [1987], there was a rotating meeting of photochemists, an International Conference [on Photochemistry], which is held here and there. In 1987, it was held in Budapest and of course, I went to Budapest, in part because I grew up there, in part because I used to be a photochemist.

So it was appropriate. And I got to be quite friendly with the people who put on the meeting, and especially one, [Professor Ferenc Márta], who was a member of the Academy, Hungarian Academy [of Sciences], and the director of the Academy's Photochemistry Institute. And so after that, after the meeting, every time I go skiing in Europe, I make a point to stop in Budapest for a few days just to see my friends. […]

And in the meantime, in the U.S., I was consulting for these universities—it added up to about sixty-five—about where the money is, and the individual faculty member, […] what are the best chances of approaching the AEC [United States Atomic Energy Commission], well, DOE [United States Department of Energy] now, for funds. Or what are the best chances of approaching, maybe, the Navy for funds and things like this. And to do that, I'd spend a couple of days in Washington, [D.C.], every year, and the biggest, the most money was from National Science Foundation. And I had a good friend who was director of chemistry at NSF, Ken [Kenneth G.] Hancock, who died, unfortunately, very young.

So in 1990—I think it was 1990. I may be wrong by a year or two. I'm visiting Washington, have lunch with Ken Hancock. We talk about what their interests are and [what are not]. […] After Washington, I was going to stop somewhere else and then go to Switzerland to ski for a while. "Oh," he says, "Are you going to stop in Hungary?" He knew I had good friends.

"Yes," I said, "I am."

"You going to see any chemists?"

"Oh yes, this guy is very VIP in chemistry."

"Well," he says, "you know communism is over now, and the Hungarian chemical community doesn't know much about the U.S. and NSF and anything we do internationally or otherwise. Maybe we could somehow try to get—educate them. Maybe **<T: 10 min>** you could have some kind of a workshop, a joint workshop, a half a dozen Hungarians, a half a dozen Americans on a deserted island somewhere." He was thinking out loud.

I said, "Ken, I bet you that the Hungarians would be delighted to have a get-together like that on an island, on a mountaintop, it doesn't matter. They'll do everything they could to make it a success, but—except for one thing. They probably won't be able to contribute a nickel," because Hungarian currency wasn't worth a damn.

So he says, "Why don't you look into it?" So, all right, I look into it. I stop and visit my friend in Budapest and I tell him this idea and whatnot, and it's exactly as I predicted. They'd love to have it. They'd love to get together. They'd been eastward oriented for twenty years, forty years, whatever it was. They wanted to, at least, balance orientation, but they don't have anything to contribute to start it.

So I come back—do my skiing, come back—talk to Ken and I said, "Ken it was exactly like I predicted."

"All right, let me think about it," he says. And about a week or ten days later, the phone rings and it's Ken Hancock. He says, "I think I got the answer." He says, "How would it be if we sent an American delegation to Hungary? They wouldn't have to contribute any kind of hard currency. How would they take to that?"

I said, "I bet they'd love you to pieces." So the next year I went back, saw the same guy; yes, he loved us to pieces. They'd open up. They'd have a red carpet, everything. Now, the thing for us to do on this end is put together a delegation representing the whole broad field of chemistry. And Ken and I did that together. And I think that was a remarkable success and he repeated it in other countries.

And what we did was, we took two organic chemists, because it's a big field, two physical chemists, because it's a very varied field, one inorganic chemist, and one polymer chemist. [And representing analytical chemistry was Allen J. Bard, from the University of Texas]. That covers chemistry pretty well. And then there was Ken, who was a generalist for NSF, and myself, who was sort of the journeyman on this. […] So we went, and we were planning to go for a week, except the Americans, if they wanted to, they could come a week or two, a few days earlier and give a lecture in Debrecen, [Hungary], a lecture in Veszprém, [Hungary], or whatever, if the Hungarians wanted them to. And it was terrific. The Hungarians took us to every university and every research institute in the five working days we had. And in the evening we had a couple of dinners. They treated us to dinner and things like this.

And the delegation was very, very good. It was mainly young people. The only real senior person was Al Bard, who was a member of the Academy and whatnot. But of the other five, four of them got elected to the National Academy of Sciences within the year. So they were right on the verge of it, and a couple of them were Hungarian born.

By the way, I don't know if you know this, […] [one of the delegates, Peter J. Stang, just received the Priestley Medal]. It was—anyhow, the big award and the guy who got the award is, (a) Hungarian born, (b) he was on this delegation. […] **<T: 15 min>** He's at [University of] Utah. He's an organic chemist, very much a mechanistic organic chemist. […] He just got the award, and of course Hungarians have gotten big, big awards in chemistry. I mean, George [A.] Olah is a Nobel Laureate,[10] Hungarian, and Peter Stang is hoping to get a Nobel Laureate—Nobel Prize or whatnot. Peter Stang got the—yes, it's the Priestley award that he just got.

And Peter was on that delegation, one of the organic chemists. Julius Rebek [Jr.] who was born and raised in Hungary was the other organic chemist. Veronica Vaida, a physical chemist from University of Colorado, was a physical chemist. Another one was from Brandeis [University]. I can't remember his name. […] My friend Tom [Thomas J.] Meyer from UNC [University of North Carolina] was [the] inorganic. And […] the polymer. We had inorganic and the polymer yes. And we had quite a hassle for polymer chemists because there was a fellow named [Joseph P.] Kennedy at one of the Ohio universities, he's Hungarian born and we offered him to come on the delegation. And he had some conflict, couldn't come, but he was

[10] George Olah was awarded the Nobel Prize in Chemistry in 1994.

dying to go. And then we switched and we got [Eli] Pearce from Brooklyn Poly [Polytechnic University, now Polytechnic Institute of New York University]. He's not Hungarian born and after we offered it to Pearce, then Kennedy came back. He said he found some other way of changing his schedule. He'd still like to go. We said, "Sorry." It was taken.

But Hungarian-born chemists, among the prominent chemists in the U.S., are knee deep. I mean, an awful lot of them here. And they've—many of them have done very, very well. And George Olah comes to mind. Peter Stang. Although Stang was something like four years old when he left Hungary, so that's not exactly Hungarian born, but George Olah had all his education in Hungary.

CARUSO: So you can you tell us a little bit more about going from your—you finish your degree at Cornell and you—Miller found you this position at General Chemical Company in New York City. By this time, I think your family had made it to New York City, your father and your sister.

WYMAN: Yes. Yes, my father and sister were there.

CARUSO: Okay. How was it transitioning from your graduate career into this industrial position?

WYMAN: Well, General Chemical at that time was quite interested in fluorine compounds. And fluorine compounds was a very new subject in industry. I mean, there are not many industries doing fluorine compounds. Some of it was still classified, and some of the fluorocarbons that are used as coolants in the nuclear reactors, things like that, that was still—couldn't be touched. I mean, General Chemical was not involved in that. I got one patent out of my ten months or so in—with General Chemical—and that's the silliest patent probably in the patent literature.[11]

CARUSO: What's it for?

WYMAN: […] All right, my job was…. **<T: 20 min>** Remember now, we're doing fluorine chemistry. What the people at General Chemical are interested in was, you have a—what was known is that if you had CF_3 here, you could oxidize the CH_3 group and get the acid. Okay? This becomes the carboxylic acid. And—but to get CF_3 in there is very difficult and expensive. I think it still is. This is through 1944. Okay.

[11] George M. Wyman. Manufacture of 1,1-Chlorofluoroethylenes. U.S. Patent 2478933, filed on 2 April 1947, and issued 16 August 1949.

So my boss had the bright idea if you have CF_2Cl, which is almost as electronegative as this, and you have CH_3 here, you oxidize this and you get the acid, and that would be a very strong organic acid. And that's known, that this is a very strong organic acid. This is a little bit stronger. Okay. So the boss comes to me. He says, "I want you to do this." I said, "All right, how do you want me to oxidize it?" He says, "That's up to you."

But it would have to be a vapor phase oxidation. And how do you oxidize organic compounds in those days? Again, talking 1944. You need a catalyst. And what's the catalyst? The metal oxide. And the best results that have ever been, at the time and probably since, [use] vanadium pentoxide. Vanadium pentoxide is a high level, high oxidation state, oxide. And you mix vanadium pentoxide, heat the hell out of this, and it should work. And General Chemical had masses of supplies of vanadium pentoxide because they were the country's largest supplier of sulfuric acid. And you make sulfuric acid by taking the trivalent—or it was four valent—sulfur and oxidize it to six. But this vanadium pentoxide that we used, the company used, for oxidizing, making sulfuric acid in truckload quantities, was kept under lock and key. In fact, it was kept under such lock and key that our division couldn't get hold of it.

And the boss comes around a couple weeks later, "How was the oxidation?" I say, "I can't get any vanadium pentoxide." He says, "Well, use any damn oxide." So, all right, use any damn oxide. The nearest oxide I could get was aluminum oxide, $A1_2O_3$. Nobody cared about Al_2O_3. So I mixed up the difluorochloro acetic—what is it? Hydrocarbon. Well, not the hydrocarbon anymore—with aluminum oxide, and see what comes out of it, and something new and different came out of it. That's where the patent is. The only catch is aluminum has a very strong affinity for fluorine. So the aluminum pulled the fluorine out. And there's no—so they insisted having a—getting a patent on it. No one is ever going to use it because fluorine is very expensive to put in. You sure as hell don't want to pull it out and get—and pay somebody a royalty for it, so I don't know if they ever used vanadium pentoxide for it. But, I mean, elementary chemistry said that for oxidation with oxides, vanadium pentoxide, because it's a high valence state, is the one most likely…. If it doesn't work with vanadium pentoxide **<T: 25 min>**, it's not going to work with anything. We never tried it. I didn't stay long enough. I don't think they ever tried it either.

All right, so that was the funny one, here. In the same company, we couldn't get—we didn't need much, maybe 5 grams or something to try it. Couldn't get it. They probably bought it from the Russians or something.

CARUSO: So you stayed at General Chemical Company for approximately one year?

WYMAN: Yeah.

CARUSO: [Nineteen forty-four] to '45? And then you went on to the General Aniline and Film Corporation. How did that come about?

WYMAN: A year earlier, I was still at Cornell, I registered at the employment clearinghouse, [at] the ACS [meeting], and the head of this department at General Aniline interviewed me and then he looked at my draft status and said, "There's no point in my thinking of hiring you, because we hire you, and three months later, you're in the army." So he was not interested, but between these two events, the war ended. So he didn't have to worry about it.

And I remembered this guy. I wrote him back. I said, "You didn't offer me a job at the ACS meeting a year ago because of the draft board, but draft board's gone, the war is over. Are you interested?" And he was and offered me a thousand dollars more than General Chemical was paying me.

ROBERTS: So did you want to leave General Chemical?

WYMAN: Everybody left. About 40 or 50 percent of the people left there. They were—it was the worst laboratory conditions that you could ever imagine.

CARUSO: Is it just because the groups weren't working well with each other or were there other problems?

WYMAN: No, they had some excellent people. The laboratory was not air conditioned. It was—so all the windows are open. It was next to a glue factory, and the glue factory was sending out glue vapors right and left. And all and the pay was low. So as soon as the war power restrictions were lifted, about 60 percent of the people at General Chemical left. This was a…I mean, as an example, when they offered me the job, they never showed me the lab. The job offer came in headquarters in downtown Manhattan, [New York], in the executive suite. But they had some very, very good people at General Chemical and they all left. And they didn't really try to keep you. I mean, my boss told me, when I told him I was going, wanted to leave, he says, "Well, if you're going to leave, we're going to tell your draft board."

I said, "Oh, go ahead, tell the draft board. The war is over." The draft board didn't care. I didn't care. So nobody thought it was—they were working on a new lab in New Jersey, which opened, I think, about a year and a half, two years later. And it was the Allied lab and this was General Chemical part of Allied. I visited that lab later on in, probably, my other capacity as—but that was much better when I was working for the Army, yes.

CARUSO: So when you went over to the new company, were you just assigned a division to work in? Did you have any choice?

WYMAN: No.

CARUSO: Okay, you were assigned where to work. Where did you wind up—where were you focused for your research?

WYMAN: Well, this was the so-called Process Development Division and they were working on dyes. And dyes seemed like very interesting molecules to me.

CARUSO: Why is that?

WYMAN: Color.

CARUSO: Just simply because "color"?

WYMAN: It's—well, there's an awful lot of—to get color, you have to have a lot of conjugation and those are much more interesting than just having, say, a **<T: 30 min>** CH_3, CH_2 ten times, and then CH_3 at the end. There's nothing very exciting on that. Some of those dye structures are fascinating. And I enjoyed them later on at the Bureau of Standards when—which brings me back to my question to you about indigo.

CARUSO: I have the research librarian at CHF looking into it.

WYMAN: I know, that's very nice of you. It is a fascinating question. I mean, indigo is a fascinating molecule.

[Drawing] That's indigo.[12] In a *trans*-form. And there is—as of 1990, at least—there is no *cis*-form. But you take this and mix it up with—heat it up with oxalyl chloride, you get the oxalyl derivative of *cis*-indigo. You hydrolyze the oxalyl derivative of *cis*-indigo, what do you get? *Trans*-indigo. So it's a very puzzling thing and it's…. Well a lot of things are puzzling. This absorbs at 600 nanometers. The—it's not soluble. Never mind. I tried various indirect approaches.

And one of the things that got me started on this is many years ago, I walked into the chemistry building at Columbia University and in the lobby as you come in, they have a little—at that time—they had a little exhibit, just at that time. And they had a formula for *cis*-indigo, and under it, it said "indigo."

[12] See appendix.

So Nick [Nicholas] Turro's a good friend, and at that time he was a senior professor there. I said, "Nick, you guys have the wrong structure for indigo in your lobby." He went down there and was very embarrassed. Changed it real quick. But the *cis*-indigo is a mystery. See, I worked a lot with thioindigos; instead of NH, you've got sulfur. There you've got *cis* and *trans*, but we discovered that first at the Bureau of Standards. The others thought that there was only one kind. And when I was in the dye industry, they only thought there was only one kind. There's not.

So there's still, well, in 1970, 1975, there's still a lot to be done about it and I don't know how much has been done. But the *cis*-indigo is—a couple of papers in the literature going back to 1930, somebody thought he saw *cis*-indigo, but no, he did not see *cis*-indigo, so it's a mystery.[13] And that's why I'd like to know if—I mean, then came flash photolysis and then came, the super flash photolysis, and they could get to a real fraction of a millisecond, all these things, and you still don't see that damn thing. And I had friends working on it, and the Russians were working on it, […] we were working on it. The Germans were working on it, or the Max Planck Institutes, and it's a mystery. It's really a mystery and…**<T: 35 min>**

CARUSO: So did you start your interest in the indigo while you were at Aniline or was that something that started at the National…

WYMAN: It's funny. Yes and no, both. I was at General Aniline and I was leaving […] to go to the Bureau of Standards and work with Wallace Brode. And I knew what Wallace Brode had in mind, and General Aniline had no objection to my—to giving me some samples. And Wallace Brode, at that time, what he had in mind was [to study the effects of] substitution on [dyes]. Here's a dye with no substitution and it's—there's a spectrum. You put the chlorine in the five position, the spectrum changes like that. Put the chlorine in the six position, the change is different, that sort of thing.

So I had worked with thioindigos at General Aniline just before leaving. And there was quite a collection of thioindigos with this substitute, and that substitute, and no substitute, and whatnot. So we could have a—could make a study of a series. And I asked them, and they were perfectly happy for me to take a small sample, which is all we're talking about, of a bunch of these thioindigos with different substituents and look at the spectra, which they were not interested in, and see what we could find. So here I am at the Bureau of Standards with a very nice collection of this thioindigo, and this thioindigo, and so forth, and looking at them. And then we see, with a new Cary spectrometer, that the spectra vary. If you—you get a spectrum today at one o'clock, you get a spectrum. And you go home and you come back the next morning and the spectrum's going to be different. That didn't make any sense. The Cary was foolproof. Something's happening to the dye. What's happening to the dye?

[13] Gustav Heller, "Zur Kenntnis des Indigo; *cis*-indigo" *Berichte der deutschen chemischen Gesellschaft* 69 (1936):563-565.

What we discovered that was happening is the thioindigos, which has a central double bond, they always have an equilibrium between *cis* and *trans* depending on the lighting. And if you leave them in the spectrometer where it's dark, then it goes from a mix of light plus dark to only that which is stable in dark conditions. And we unscrambled the whole thing and found out what it was and what the pure *cis* and what the pure *trans* would look like. So this was all great for thioindigo because thioindigo does this.

So let's take a look at indigo. Indigo doesn't do it. And then let's take a look. Indigo and thioindigo, the difference is the half the molecule, the half indigo, half thioindigo you have one NH group and one S. The half thioindigo doesn't do it either. So we had to find out why and how, and we did to a certain point. And we know why indigo doesn't do it, because what I carefully neglected to do here, this oxygen is really out here. This hydrogen is really out here, and the hydrogen bond stabilizes the *trans*. Now, the thioindigo, the NH group is replaced by a sulfur, no hydrogen. No hydrogen. No nothing. So this is what's fascinating, that even one hydrogen will stabilize this, so if it will stay in this position, and it will not go to this position. And I don't know if that's still true. I mean, it's—I got to Bureau of Standards in 1950, I think, so that's seventy-five years or something.

CARUSO: What made you want to go to the National Bureau of Standards?

WYMAN: Well, they were doing basic research. Government **<T: 40 min>** research is basic research. Industry's interested in applied research, a dollar sign at the end of every equation. And the government is not interested in dollar signs, as we all know. And the—it seemed like a good opportunity, and as I say, I took a salary cut to go there and then I stayed with the government for thirty-five years.

ROBERTS: So I'm curious—before we get to the Bureau because I think there'll be a lot to explore there—I'm wondering what new terrain you covered while you were at General Aniline that built upon what you had done at Cornell? It seems like the year at General Chemical—there wasn't a whole lot of new learning that went on. But it does seem like at General Aniline you had an opportunity to apply some of your mechanistic training around organic synthesis.

WYMAN: No.

ROBERTS: No?

WYMAN: No. This old German Austrian department head, he was very much, was very, very classical sort of a chemist. I still remember one time, one of my colleagues went to show him something. General Aniline at that time acquired a spectrophotometer which was not a Cary. It was—it did not measure absorbance. It measured transmission. And he wanted to show the boss

that here's a spectrometer shows that this and this are the same. The curves were the same in the spectrometer and the boss said, "Ah, spectrometer, who cares about—we never had those when I was going to school." And obviously nobody could convince him that science was really progressing beyond when he was in school in 1910 or whenever. No, I never had much real interest in what was going on. I did what they wanted me to do. I got some dyes for them and that was it. And the dyes got much more interesting when we could compare them at the Bureau of Standards with a real good instrument and see what's going on on the micro scale—micro, even smaller than micro.

ROBERTS: Maybe that'd be a good place to start tomorrow morning.

CARUSO: I think so.

WYMAN: Hmm?

ROBERTS: Maybe that's a good place to start tomorrow morning.

WYMAN: Up to you guys.

CARUSO: Sure.

WYMAN: I suggest maybe you read this before tomorrow.

CARUSO: Sure.

WYMAN: It's only a couple of pages. And this tells you what happened as I got to Bureau of Standards and what I did pretty much at the Bureau up to a certain point. And then I'd be glad to answer any questions if you have them, but…

CARUSO: Sure.

ROBERTS: Well, I think…

WYMAN: I think I have two of them, if you want two.

CARUSO: I think one is fine.

WYMAN: One is fine. It's not long. Now, the other thing that I did, and I don't know—well, that comes later, of course. When I started working for the federal government, I had some opportunities that others could not—wouldn't have had. I started a series of international conferences. And the first one was held, again, in photochemistry because I happened to fall into photochemistry. And the first one was held in 1962, and in 1962 a lot of new things were happening in photochemistry. It so happened that I picked a good time. The Army gave me money to bring people from all over the world to Durham, [North Carolina]. The meeting was held here in Durham and the photochemists liked the meeting. And I think that series of meetings, first it was every couple—every three or four years, and then every couple of years. I think that series of meetings is still going on. So it was something that **<T: 120 min>** was needed. I'm not saying that somebody else may not have started it a couple years later.

Here's what I looked like in 1962.[14] But that's only about conferences, so conferences are something that…Sometimes very significant things come out of conferences, but not always. But this is the one I think you should read. Do you have the other one?

CARUSO: Yes. I do.

WYMAN: All right, you've got it.

CARUSO: Okay.

ROBERTS: Thank you.

CARUSO: Thank you.

[END OF AUDIO, FILE 1.3]

[END OF INTERVIEW]

[14] George M. Wyman, "History of Photochemistry: International Conferences of Photochemistry. Duham, 1962-Durham, 1999. A Subjective History," *EPA Newsletter* 67 (1999): 7.

INTERVIEWEE: **George M. Wyman**

INTERVIEWER: **David J. Caruso**
Jody A. Roberts

LOCATION: **Carolina Meadows**
Chapel Hill, North Carolina

DATE: **2 May 2013**

CARUSO: Today is May 2nd, 2013. This is the second session with George Wyman. We're again at Carolina Meadows in Chapel Hill, North Carolina. I'm David Caruso, and here with me, also, is Jody Roberts conducting the interview. Yesterday, we kind of wrapped up hearing about your time at General Aniline and Film Corporation. You were in Linden, New Jersey, working there for about four years. And I think that you had started to tell us a little bit about your transition to the National Bureau of Standards, but just for consistency's sake, can you tell us how it came about that you left Aniline and joined the National Bureau?

WYMAN: I didn't like doing applied research, research with dollar signs in front of it, which is what industry does, and industry has good reason to do that. And I thought I'd like to get back to basic research, where the intellectual curiosity is what drives the research. And a friend suggested I look into government research possibility. Well, there were three possibilities, then and still. You work for industry, you get the most money and probably least intellectual challenge. The government, where you have less money, more intellectual challenge. And then the last one is university, like Joe.[15] I think Joe has pretty good money, but the—in general—academic jobs pay the least, but you have complete freedom to do what you want.

So I wanted a compromise. I didn't want to step down into academia, which seemed like a fifty percent salary cut, but I was willing to take a smaller cut and get more freedom, but not complete freedom, and start to work for the government. And I was lucky, because the Bureau of Standards offered me a position which gave me a great deal of freedom in the research, and a lot of fun with it, and a salary cut which was tolerable.

CARUSO: You mentioned yesterday, in some ways, your father was the one that told you—or got you started on the idea of going into chemistry. I asked you about getting your Ph.D., and you said, "Well, my father said I should get…"

WYMAN: "Go as far as you can."

[15] Wyman refers to Joseph M. Desimone, whom Roberts and Caruso interviewed the same week they conducted the Wyman oral history. Desimone's interview transcript is in process.

CARUSO: Go as far as you can go. Was he still providing advice to you about your career, or was he letting you…

WYMAN: No, he was—he was satisfied with what I was doing.

CARUSO: And your sister was—I know she was in the country at this point. Was she pursuing a career as well?

WYMAN: Well, yes and no. Women at that time, both in Europe and the United States, were—had very minor sort of careers. She was trained as a cosmetician, and she worked for a while for Helena Rubinstein, [Inc.], which was a big cosmetic firm. And in New York, they actually had a salon where the richer women would come and have their hair done and what not. And she was busy looking for a husband, and found one when the war was over, and got married, and that was it.

CARUSO: Okay. All right. So how did you actually meet Wallace Brode, and…

WYMAN: I mentioned that yesterday. I had the intention of looking for a government job in Washington, and the first place to go to—my friend suggested—was the Bureau of Standards. I went to Bureau of Standards, the personnel office. And of course, they didn't really know what kind of chemistry I was doing. They knew they had a bunch of chemists, and they knew they had a division called Organic and Fibrous Materials Division. And the word organic caught them, because I had a—I told them I was a doctorate in organic chemistry. All right. So they sent me off to talk to the division director of this Organic and Fibrous Division. […] And he was very helpful. He called some people at NIH [National Institutes of Health], arranged for me to go—those were the **<T**: **05 min>** days when chemists were—there was a shortage of chemists, so a chemist walks in, it was a big thing, looking for a job.

[…] He remembered that Wallace Brode, [who] had recently come to the Bureau, and he was—Brode was this guy's boss. So Brode intended to start a project, a small research project, on dyes. And here I come in from a dye company. So he arranged to have me meet Brode the next day, and that's how I got a job at the Bureau. Brode, it turned out, was interested in me because of the dye background, and also because my fluent German, because [the former] student he brought with him, I told you, couldn't pass the German exam. And those days, 1950, 1949, German was a required language for a doctorate in all universities in the U.S. But ten years later, that disappeared. But it was my luck that Wallace Brode was looking for somebody with experience in dyes and fluent in German, and I had both of those.

ROBERTS: What was the work that they were hoping to do on dyes?

WYMAN: Wallace Brode was interested in correlating the structure of dyes with spectra, and doing spectra was a new thing. The instruments were new. Beckman [National Technical Laboratories] DU spectrometer had been on the market for maybe four or five years, but that was a point by point measurement. And Cary, Howard [H.] Cary, brought out—his company was known as Applied Physics Corporation. He started making recording spectrometers, the same as—get the same—there's really no difference in what one would do versus the other, the Cary and the Beckman, except Cary, you get a spectrum, a line, and Beckman, you get point by point. It's—you have wavelength 340, you get a point, 350, the next point. You have to do it just like this, and it was very, very time-consuming.

So the Bureau was very proud that they've got the number thirty Cary [Model 11] recording spectrometer, the thirtieth that Howard Cary had put on the market. And we—and John [H.] Gould, a fellow who worked with me, and I—we had the Cary all to ourselves. So Brode was very interested in getting the spectra of these dyes, any dyes. And of course, what he wanted, because he was a physical organic chemist, was a correlation. The—correlate the structure of the dye with what the spectra will show. But instead of that, we found that the dyes that I had [had] changed, because there's a chance for *cis-trans* isomerization, and this was red hot. Nobody had ever seen *cis*—never heard of *cis*-thioindigo. We published two or three papers in a hurry, and this was something new.[16] And then we pursued that. We looked for *cis*-indigo, and if I'm right, nobody still found *cis*-indigo, because of the hydrogen bonding. You've got half and half, thio and indigo. And that—you don't find *cis*- half thio-half indigo either.

And then we went to the azo compounds. We cleaned up some things in the literature that were sort of ambiguous.[17] We got—my friend John, my colleague, he devised a gadget for the instrument, so if there was something in there that was unstable, that was maybe stable only for maybe 50 microseconds, you could still find evidence for it. It was something called a shutter-illumination device. We published a paper with that, John Gould and Wallace Brode and I.[18] We had—I'm sorry I don't have reprints anymore. I threw them away.

CARUSO: Oh, that's fine. We can always get the…

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[16] G. M. Wyman and W. R. Brode, "The Relation between the Absorption Spectra and the Chemical Constitution of Dyes XXII. cis-trans Isomerism in Thioindigo Dyes," *Journal of the American Chemical Society* 73 (1951): 1478-1493; W. R. Brode and G. M. Wyman, "The Relation Between the Absorption Spectra and the Chemical Constitution of Dyes. XXIV. Absorption Spectra of Some Thioindigo Dyes in Sulfuric Acid," *Journal of the American Chemical Society* 73 (1951): 4267-4270.

[17] W. R. Brode, J. H. Gould, and G. M. Wyman, "The Relation between the Absorption Spectra and the Chemical Constitution of Dyes. XXV. Phototropism and cis-trans Isomerism in Aromatic Azo Compounds *Journal of the American Chemical Society* 74 (1952): 4641-4646; W. R. Brode, J. H. Gould, G. M. Wyman, "The Relation between the Absorption Spectra and the Chemical Constitution of Dyes. XXVI. Effect of Solvent and of Temperature on the cis-trans Isomerization of Azo Dyes," *Journal of the American Chemical Society* 75 (1953): 1856-1859.

[18] W. R. Brode, J. H. Gould, and G. M. Wyman, "The Relation between the Absorption Spectra and the Chemical Constitution of Dyes. XXV. Phototropism and cis-trans Isomerism in Aromatic Azo Compounds."

WYMAN: You can find them? This is the—well, just search by my name and you'll find them, about **<T**: **10 min>** twenty reprints for the period I was with the Bureau of Standards. I mean twenty articles. And John Gould has his name on some of them, and we had a great deal of fun finding out a lot of new things about some of these dyes, the thioindigo dyes, indigo, half indigo dyes, azo dyes. I don't know what else. A few other things.

ROBERTS: Well, you mentioned this just briefly yesterday about the role of the new instruments that were coming out after the war. So was this new opportunity at the Bureau of Standards really about applying some of this new equipment and this new instrumentation to an area?

WYMAN: Well, they were applying it for this particular project. That instrument was used just for this project, the spectra of dyes, Wallace Brode's project.

ROBERTS: What kind of experience had you had with any of this equipment before you got to the Bureau of Standards?

WYMAN: Zero.

ROBERTS: So what was that transition like?

WYMAN: But so did everybody else, you see.

ROBERTS: Right.

WYMAN: There was no recording spectrometer. John Gould, my friend, and—he's dead now. He's a former student of Wallace Brode's from Ohio State [University]. He was one of these chemists who had a very good instinct for instrumentation. He could modify instruments. He knew how to fix something when something was wrong. He was very, very good at that. I am completely—I was always a complete dunce. So John and I complemented each other. I knew more about the chemistry, and he knew more about the instruments. And as a result, we had some—got some very good results.

The funny thing about the Cary [12] spectrometer, […] it was the workhorse, everywhere, after Cary brought it out. And it was—Cary's company was known as Applied Physics Corporation. And at UNC, when I became an adjunct professor, I got involved in some

of the photochemistry at UNC. This was now in the 1970s. My friend David [G.] Whitten, who was a faculty member then, discovered the Cary sitting in a shed somewhere. Nobody was using it. By then, the Cary had been superseded by more sensitive instruments. But this Cary that he found in the shed was—gave him a lot of good results. And, I mean, good meaning reliable results, for some of the things that David was interested in, which was energy transfer and the sensitized isomerizations, things like that. David is now in New Mexico, having left here to go to [University of] Rochester. I don't think Rochester, [New York], is the shortest way to New Mexico.

ROBERTS: Probably not.

WYMAN: Anyhow, he was—he was a terrific guy, and it was a big loss to the department when David Whitten left.

CARUSO: When we were speaking yesterday, and this was during the time that you're at Cornell, you brought up that in some ways there was this old guard of chemistry…

WYMAN: Inorganic chemistry.

CARUSO: …inorganic chemistry, that, you know, they were very stuck to the way things had been done for so long. There were some new professors coming in to push things in a new direction, right? And it sounded like those groups—I don't want to necessarily frame it as they didn't get along, but they had different perspectives on what…

WYMAN: Let me tell you what happened. This is interesting. These two guys came in—I told you, Rubin and Taube. And after five years or six years, whatever, this question came up, which of the two guys get tenure? And the old guys had to—making the decision about the tenure for the new guys, and Henry Taube, who got a Nobel Prize twenty years later, almost didn't get tenure. He did get tenure, and Rubin did not. And they both were very bright. Rubin was kind of—**<T**: **15 min>** well, he ruffled the feathers of the old guard more than Taube. Taube was more diplomatic about knowing it all and knowing all the new stuff. Rubin was much less diplomatic about it. So this was a personality case, and Rubin—Rubin then got a professorship, though, at Ohio State. This is the last time I saw him, he was at Ohio State, which was 1955, maybe. And Taube stayed at Cornell. And in 1960 or so, he moved to [the University of] Chicago, stayed there maybe eight or ten years, and then he went to Stanford [University], and at Stanford, he got the Nobel Prize.

And Henry Taube is dead now. But […] the reason he got the Nobel Prize is because he applied that new inorganic chemistry to a lot of systems that had been treated with the old sort of—with the old approach. And the new approach gave much more interesting results, much

more good stuff. I was greeted by Henry as soon as he got to Cornell, because he—well, he applied kinetics to inorganic chemistry. The old guard [was] not interested in kinetics.

CARUSO: The reason I raised that was, I'm curious to know, with new instrumentation making its way into labs, were—I'm assuming you were going to conferences. I mean, clearly, you were publishing papers. Was there any sort of division in the chemistry community more broadly over the use of these instruments? Were there people who said, you know, we don't need these things, and others embracing them, or was it just, you know, this is a new toy to play with?

WYMAN: You see, I can't really tell, because by then, I was not in the University. At the Bureau of Standards, where I was at the time, let me think. Well, again, there were old timers and there were the new guys, and the Bureau of Standards was quite an old line agency. I mean, there was—I gave my first paper at an ACS meeting while I was at the Bureau, and I liked it, so I decided I'd try to get a paper ready for every ACS meeting.[19] But that didn't go so big with the old timers. The—one of the division directors had a wife—I guess they all had wives. And this wife was the division secretary. And I came in and I said, "I've got a paper for the Chicago ACS meeting." And, "Oh, but you presented a paper at the last meeting. You can't go again. You can go, but we're not going to pay you."

And I said, "Well, isn't the paper—" "No, it's not." So I hitchhiked, or whatever. And then the next meeting, I had three papers. That wasn't enough, either. So I got a carpool of other people who didn't get money. That was a Chicago meeting again. And we—it was three people. We all had papers in the program. None of them got money from the Bureau to go. So the old tradition was that going to a meeting was sort of a privilege, and if you go this year, you sure as hell don't go next year, and probably the following year, either, and so forth, and never mind if you, in between, got a Nobel Prize. I mean, that's irrelevant.

CARUSO: So it does sound like, though, that—I mean, since your papers were getting accepted at the society's meetings, the society was interested in what you were doing. So it sounds like—at least in terms of the interest in that form of science—there was interest in the community broadly.

WYMAN: There was. Yes.

CARUSO: Okay.

[19] G. M. Wyman and W. R. Brode, "The Relation between the Absorption Spectra and the Chemical Constitution of Dyes XXII. cis-trans Isomerism in Thioindigo Dyes."

ROBERTS: And I think—more and more, perhaps in addition to the use of the instruments, if you could talk a little bit about what you were seeing because of the instruments, and **<T**: **20 min>** what that was doing to the larger community. So you've obviously spoken a lot about suddenly seeing isomerization, where that wasn't something that people were really thinking about with natural dyes and synthetic dyes, that this equipment was providing a new perspective. And I think this is where you end up spending a lot more of your time going forward, but how much of that you were allowed to participate in or that you were able to see from the Bureau?

WYMAN: Well, I finally worked out a system as follows. You guys are too young, but for a period in the 1950s, the ACS only had national meetings in three locations: New York, Atlantic City, [New Jersey**Error! Bookmark not defined.**], and Chicago**Error! Bookmark not defined.**. No—none of this Philadelphia. "Philadelphia's a hick town," you know, and all these others. So the Bureau, I was working in Washington, so I could easily afford to get a cheap hotel room in Atlantic City. I sure as hell couldn't afford travel costs to Chicago, and a hotel room. So I concentrated—I paid my own way to the Atlantic City meetings, and tried to get the Bureau to pay my way to Chicago. New York was a toss-up. But I think I had a paper every year—I mean, not every year, but every ACS meeting. But this is one reason I left the Bureau, is because that was pretty niggardly, I thought. And here there were people who never had a paper, and yet you're sending them at Bureau cost to Chicago, because it was their turn. They hadn't been in eighteen years, so it was time for them to go. I mean, see Chicago.

All right. Now that's a sideline. At Natick, [the Quartermaster Research and Development Center], it was easier. I mean, there, you—if you had a paper, they'd send you. We were a new organization. We were trying to make a splash on the horizon, and we did it by having papers at the meetings, and not just having attendants.

CARUSO: Now one thing I do want to ask about, and you haven't—you haven't mentioned this, so I don't know when this started to happen, but you did get married in 1951 to…

WYMAN: Happens to the best of us.

CARUSO: …to Mary [Ellen] Truitt.

WYMAN: Yeah.

CARUSO: Was she someone you had met when you were down at the Bureau of Standards? Was this…

WYMAN: Yes.

CARUSO: Okay. Was she working there, or she was…

WYMAN: No, she worked somewhere else. So she worked for the government. She worked for the Army, actually. And, yes, she died, what, five years ago, something like this. And we had one child, and the child is in Raleigh, [North Carolina], just around the corner here. But the child has a child, a grandson, my grandson. And the—my daughter raised him alone because her husband, the boy's father, died very young. And the boy went to Duke [University], and Duke paid most of the expenses, because she didn't have much income. And the kid's got a job with Microsoft [Corporation] in Seattle, [Washington]. And he just graduated from Duke last year. You know what they're paying him?

CARUSO: A lot.

ROBERTS: More than we're making.

WYMAN: Ninety-five thousand.

CARUSO: Yeah.

WYMAN: It's unbelievable.

ROBERTS: Your daughter must be very excited. Very proud of him.

WYMAN: I mean, my daughter works for the school system, and they don't pay much. And I was worried, well, I can leave some for my daughter, but the grandson will have to make his own way. Make his own way, hell. He flies right and left all the time. I mean, he's getting to be a world traveler. He loves Microsoft.

ROBERTS: What does he do for them?

WYMAN: I can't tell. It's not—he's not a computer guy. He's more—he's not a salesman. He's just sort of a—put on a good face and…

CARUSO: Public relations?

WYMAN: That sort of thing, but individual—I don't know. I mean, he's—he majored in business **<T**: **25 min>** and psychology, and this is sort of the psychology of business. And, now, it may be that he's lucky that he went to Duke. Bill Gates's wife [Melinda F. Gates] went to Duke. And in fact, she gave them the whole science complex.

ROBERTS: Wow.

WYMAN: And she's on the board of trustees. She's everything. And whether there's some kind of a special connection between Microsoft and Duke, I don't know. I wouldn't be surprised if there is a little something there.

ROBERTS: Sure.

WYMAN: But certainly as far as my family is concerned, well, when I'm gone, he can look after his mother.

CARUSO: Did getting married—when was your daughter born?

WYMAN: [Nineteen] fifty-seven.

CARUSO: 'Fifty-seven. Okay. So that's after you were at…

WYMAN: She was born in Frankfurt, [Germany].

CARUSO: …in the Army. So did getting married change your work schedule at all? Or did it stay relatively standard when you were at the Bureau?

WYMAN: Well, wait a minute. I had left the Bureau. But no, I was still at the Bureau when we got married, yes. But shortly after that, I left the Bureau. I went to work for the Army. All right. And the answer is no, the getting married didn't affect my work in any way.

CARUSO: You mentioned that one of the reasons you left the Bureau was their reluctance to support you going out into the community to give papers. Were there any other reasons why you didn't want to stay at the Bureau?

WYMAN: Pay, pay, pay.

CARUSO: Pay.

WYMAN: I mean, the—a friend came up to me, and he said, "I am now working for the Army, the Quartermaster Corps, and the Quartermaster Corps is paying me real good money. They're building a brand new research facility in the Boston, [Massachusetts], area, and they'd like you to come. They told me that—to staff that facility, and I would like you to come and add a research laboratory, a small one," doing much the same what I was doing at the Bureau, but giving me a 20 percent pay raise. I mean, wouldn't you go?

CARUSO: Yes. Probably.

ROBERTS: But it had to be…

WYMAN: Furthermore, I was a skier, and the Boston area is a great place for skiers to live.

ROBERTS: And it seems like it was an exciting time. You started your work at the Bureau, you said no experience with some of this new equipment, and now you're headed to this new office up in Natick, [Massachusetts], and you're…

WYMAN: New facility.

ROBERTS: …and you're running the whole operation of that division.

WYMAN: They had fantastic instrumentation.

ROBERTS: Right.

WYMAN: We had a funny thing there. I mentioned to you the Cary spectrometer. Well, this was a new facility. When the government builds a new facility, there's a lot of money around, usually, and they don't really know what the money is going to be used for, but they want to use it. So if it's a research laboratory, they go out and see what's on the market, and they buy the most expensive instrumentation, and they don't know what they intend to do with it. So I arrive on the scene there, and there is a—the latest model Cary spectrometer. Well, I knew the next latest model from the Bureau was not much different. Okay. Well, we know what to do with the Cary spectrometer.

But because the extra money, the Army spent thousands for a fluorescence attachment for this, and I said, "What am I supposed to do with that?" And they said, "We don't know, but we just had the money, so we just…" Well, I hated to see the thing sitting in a cabinet, so I started reading about fluorescence, and I published a couple of papers after a while, and fluorescence spectroscopy is a very fascinating part of physical chemistry.[20] And the grandfather of fluorescence spectroscopy became a very good friend of mine, a German professor who's dead now. And what happened is that here I am in Natick, and here is the fluorescence instrument, and we had access to the MIT [Massachusetts Institute of Technology] library, and of course, the MIT library had everything. So I said—I go to the MIT library and read about fluorescence. I mean, **<T**: **30 min>** I knew nothing. And I get this one book which was in German, and by Theodor Förster. I say I've got to read—and it's—the title was *Fluorescence of Organic Compounds*.[21] And there was nothing like it in English. I say, "I've got to read this, because what I'm dealing with is—you know, I'm going to use this. I'm going to use an organic compound." That's what the book is about. And the book told me all about fluorescence, and I learned about fluorescence. And then when I got to Europe a year or two later, I got to be real friends with Theodor Förster. And he should have had a Nobel Prize.

Anyhow, so I published one or two papers on fluorescence, and I learned something where you could use that as a special—an additional tool to the spectroscopy that you'd normally be doing. But the only reason I got into it is because the Army had the money. They bought the instrument, and nobody knew what to do with it. All right.

CARUSO: One thing that I find—or, I'm interested to know more about, with regard to your career—is that you're in the Army. You're working as a civilian, but you're in the Army, or the Armed Services…

WYMAN: I worked for the Army. I was never in the Army.

CARUSO: You worked for the Army. Right You worked for the Armed Services. And during the time where—I mean, the Cold War is framing much of what's going on in American science.

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[20] D. A. Rogers, J. D. Margerum, and G. M. Wyman, "Spectroscopic Studies on Dyes. IV. The Fluorescence Spectra of Thioindigo Dyes," *Journal of the American Chemical Society* 79 (1957): 2464-2468.
[21] T. Förster , *Fluoreszenz organischer Verbindungen* (Göttingen: Vandenhoeck & Ruprecht, 1951).

And I was wondering, as we discuss your career, and I think you finished in 1985, if you could let us know if there are times where you see the science changing in response to the Cold War going on, or things along those lines. And we don't have to cover it now, but just something to keep at the back of your mind as we discuss…

WYMAN: One instance that comes to mind immediately was—this was much later, when I was an administrator for the Army. And every now and then you've got these leads from headquarters, and—wait a minute. This was not the Cold War. This was Vietnam. This was worse. Ah. Somebody in Washington—we were worried about the "good Vietnamese" and the "bad Vietnamese." And somebody in Washington had the bright idea that we should be able to tell them spectroscopically by chemistry, because they have—they eat different things. And the good Vietnamese are going to—I don't know, they eat steak dinners or something, and the—and the vapors that they exude, we could pick up on a sensitive instrument, and you could tell that the guy behind the bush is a good Vietnamese. But behind that bush is the bad Vietnamese, because he doesn't eat steaks. He eats, I don't know, little pigs or something. And they wanted us to try to work out a system by which you could actually detect this.

Well, this was ridiculous. But you get things like this from the uninitiated, from the uneducated—I don't know who had the bright idea. This was—so no, this was not the Russians. This was the Vietnamese, the good Vietnamese and the bad Vietnamese. And so that would have been 1970—1975 is when the Vietnam War was over, so this probably came pretty late, 1971, 1970. And we had this thing.

Oh, we had all kinds of crazy things from headquarters. Flame retardants. There was quite a bit of research going on. It was mainly halogen, the—let me think of—oh, I can't remember exactly. It was halogens in the gasoline. It **<T**: **35 min>** wasn't really chlorine or bromine or anything like—bromides. Something like this. Put that in gasoline, and then you—it'd be a fire retardant, and the—your gasoline would last longer. Your engine would last longer, whatever. Actually, later on, after a year or so experimenting with this, then they discovered that it all worked out fine and you'd get less flammability. There was less flammability from the gasoline. But the engine was eaten up. And the engine was gone. The gasoline was fine. That was not exactly what they were looking for.

ROBERTS: Where was that research happening? While you were up at Natick?

WYMAN: No. I can't remember. It was—I think it was mainly at [the U.S. Army] Edgewood [Chemical Biological Center]. Edgewood was the Chemical Corps…

ROBERTS: In Maryland, right?

WYMAN: Maryland. Yeah. Edgewood, Maryland. So every now and then you would get these bright ideas that fizzled. And sometimes there were some bright ideas that worked well, but—and you had to try them all. The reason I remember the flame retardant thing is because I remember being at a—I was director of chemistry division then at the Army Research Office, and some big wheel was coming down from Washington. We had to give him a briefing about all these programs that we had instituted to make a difference in the war effort. I think this was—this may still be in Vietnam, probably.

And my boss asked me, did I have anything going in the—this thing about keeping the engine from—reducing the flammability of the fuels, things like that. And I said, "No," I said, "I don't"

He said, "Well, why don't you have one?"

I said, "Because you didn't give me any money."

"Well," he says, "maybe we'll give you some money." But then about six months later, the word came out that yes, the flammability was reduced, but the engine was going to hell. So that was the end of that project.

ROBERTS: Was that research that was happening internal to the U.S. Army, or were these…

WYMAN: Yes.

ROBERTS: …kind of corporate partnerships?

WYMAN: No, this…

ROBERTS: Just all internal?

WYMAN: A little bit of both. Both.

ROBERTS: Both?

WYMAN: Both. It was—the corporate research was done mainly from the outside. The university research was done here—I mean, was administered from Durham. The Army Research Office, which handled all research support in universities, was here in Durham, at first on the Duke campus, and later on at—out in the Research Triangle [Park], and I think it still is

out there. The U.S. Army Research Office is very near the airport. And that's where I came back from Frankfurt, because they offered me the job of being director of their chemistry program.

ROBERTS: So I want to make sure we cover what you were working on while you were up in Natick for those years.

WYMAN: Natick.

ROBERTS: Natick. Sorry.

WYMAN: Natick**Error! Bookmark not defined.** is a suburb of…

ROBERTS: Of Boston.

WYMAN: …Boston. Twenty miles west.

ROBERTS: So should we just say Boston?

WYMAN: Twenty miles west.

ROBERTS: So what were you doing—what were the main projects that you were working on while you were in Boston?

WYMAN: The main project was pretty much what I did in the Bureau of Standards, was these dyes, the […] *cis-trans* isomerization of dyes. And then I incorporated, once I educated myself, fluorescence. Now interestingly enough, my *cis* and *trans*-thioindigos: the *trans* fluoresce, the *cis* do not fluoresce. And that could be predicted based—if you read my friend Förster's book carefully, that's to be expected. And I—what else did we do?

ROBERTS: So why was the quartermaster interested in…

WYMAN: Well, the quartermaster, among other things, provides all the textiles for the…

ROBERTS: Well, sure.

WYMAN: …for the military.

ROBERTS: But why as an internal matter to the Army are they worried about dying clothes? I mean, is this in terms of thinking about—you know, is this technology that has potential for thinking about camouflage? Is this…

WYMAN: Yes. Yes. There's some basic research that they're interested in on dyes, and then there's **<T**: **40 min>** some applied research. And they have a pretty strong applied research program at Natick, where it comes down to the dye and the cloth together. I mean, it's—again, it's not good to have a dye which is perfect, if you cannot put it on a cloth, or vice versa. And the applied people, we had some interaction, not as much as there should have been. Actually, I didn't stay at Natick very long. And that was kind of a fluke that I left.

ROBERTS: How did that happen?

WYMAN: Well, in every major installation, perhaps even at your place, you get a weekly bulletin of some sort, or activities, visitors, that sort of thing. And management can put any kind of an ad in there, if you want to call it an ad. And one day I pick up one of these weekly bulletins, and it says the Army is looking for a senior physical scientist who's knowledgeable in the major European languages for their new European office. And the war had been over five years, six years. No, more ten years now. And I was interested in Europe. I hadn't been back. And it seemed like a tempting—and the grade level was top. The salary, top. And so I showed this ad to my wife, who was pregnant. She was not at all happy, the idea of traveling while pregnant. And decided I'll apply for this. I didn't know…. I wasn't a senior scientist as they constitute this. […] I was a thirteen then, and this was a fifteen. That was a big jump. Fifteen was the very top of the Civil Service. But it was tempting.

And I didn't know if they'd consider my languages adequate: Hungarian is not a major language. German, yes. English, everybody speaks English. My French and Italian were very sketchy. All right. But I told them. And then I wasn't a senior scientist. I was a thirteen out of fifteen. They knew that. And the recruiting was done directly from the Pentagon. And I made about three trips down to the Pentagon, and most of my interviews were concerned with my wife's pregnancy, because the Army has a strict rule: no pregnant women are transported overseas after the [sixth] month of pregnancy. Then you have to wait until the child is six months old, and then you can go again, but there's a period where we will not transport.

Well, so the question is, when were the six months up and all this? Nobody here tested me for French or Italian or anything. And finally, I had the offer, but they couldn't have me as a

fifteen, because there's a rule that you can only go up one step at a time. But they promised me a fifteen after one year. Well, that was still very attractive.

And I took the job, and I was the only civilian on board. They had a bunch of lieutenant colonels, and one colonel with some technical background. And they didn't know where to go, didn't know what to do, really, because what they wanted is to establish interaction with the European scientific community—science meaning all sciences. And for that, they wanted one guy who would cover all sciences. Well, we may have a few of those in the country, but I don't know if there are many.

And I took it, and it was great fun. And **<T: 45 min>** then I don't know if they thought I did a very good job, and that's why they decided to hire another civilian, or if they thought that I did a bad job, and that's why they felt that they had to hire a second civilian. But they did, they got a fellow who was a computer sort of a guy. This was 1950s. There were not many computer guys in the 1950s. And he stayed a couple of years, went somewhere else.

And the following year, they hired two more civilians. So by the time that I left, they had four civilians, and in quite different fields. I was the chemist. This guy was the math. There was a fellow who was biology, and then a fellow from ONR [Office of Naval Research]—not ONR—NRL [Naval Research Laboratories], who was a physicist. So there was one for four different sciences, which makes sense. I mean, this is the physicist, follows leads in physics. I follow leads in chemistry, and so on.

And when they hired me, they really got a bargain, because my former boss at the Bureau of Standards, Wallace Brode, he knew everybody who was anybody in science in the world. I mean, coming with—starting with Nobel Laureates. I mean everybody. He was the former president of the Optical Society of the ACS. And he gave me a long list of people where I could look them up and say, "Wallace Brode suggested I look you up."

And I'll give you a very good example here of how that really paid off. I arrived. I'm the only civilian scientist. The war ended six years ago, eight years ago, whatever. And my military colleagues are wondering, "What could we do to have interaction, U.S.—between the [United States] Army and some of the formerly neutral countries?" I mean, you could easily have interaction with the Germans, because we liberated Germany, what not, and some of the other countries. But Sweden and Switzerland and those countries, they didn't want any part of us during the war, and they didn't feel much—any closer than that ten years later.

But Wallace Brode's list had a long string of important people from Switzerland, Germany, what—I mean, Switzerland, Sweden. And so when this problem came up in one of the staff meetings, I said, "Well, let me try." I had the name of a recent Nobel Laureate in Sweden on Wallace Brode's list [Arne Tiselius].[22] I said, "I'll go up, set up an appointment to see that guy. And if he sends us a proposal—" we had a little money for research proposals. "If he sends us a proposal, when we tell them in the States, when they evaluate, that never mind what he sends in, approve it, because there'll be nothing better than to be able to say that—"

[22] Tiselius was awarded the 1948 Nobel Prize in Chemistry.

[…]. Anyhow, he had a Nobel Prize two years earlier, so everybody knew of him. And then we could say the U.S.—that this guy, whom everybody knows, is getting U.S. Army support. Why wouldn't you? And all the other Swedes think that this guy is their top scientist at the time.

So sure enough, I write him, a very nice guy. "Come visit me." I visit the university. He takes me home for lunch, tells me about his grandchildren and what not. And then he said—talks to me about some of his research interests. And then he looks to me—he was a very modest sort of fellow. I mean, never mind the Nobel Prize. And he turns to me, he says, "Do you really think the Army would—the U.S. Army—would be interested in supporting this?" And I said to myself, "Hell, if this guy will give me two pages **<T: 50 min>** from the Uppsala [Sweden] telephone book, we'll support that, because it will open up the whole country." "Yes," I say, "Sure." I didn't know much about what research he was talking about. "Sure."

So then we got a proposal from him. We sent it with red flags back to the States for evaluation. "You can either evaluate this to support it, or else support it." I mean, there are no ifs and buts about it. And sure enough it came back approved, and he got his money, and the Army got its money's worth. But from there on, Sweden was open. And again, I can take no credit for it. Wallace Brode gave me his name. I could write to him and say, "Wallace Brode suggested that I look you up." And the same happened in Switzerland, except there it was a geographer [Hans Heinrich Boesch]. Well, I didn't know anything—I mean, I liked geography, but I had no claim to fame in geography. And again, he opened up Switzerland for us, because he also sent in a proposal. And we became friends for quite a while.

So I had this very special package with me that I could take no credit for, because Wallace Brode is the one who—he was thirty years older than I was—I mean, he'd been around, and he—as I said, he knew everybody who was anybody.

CARUSO: One thing that I'm not entirely clear on is what the Army thought it would be getting out of these relationships. Was it looking to use these scientists to conduct more research for the Army? Was it just looking for potential future collaboration? I don't fully understand what they were…

WYMAN: All right. It varies, and it still varies. The offices still exist in Europe, and in fact, the Army/Navy/Air Force also has a presence in Japan. I don't know where else. The money available for research support in Europe…In the first place, ONR never got interested in research support in Europe. ONR in London is a place for American university scientists to take a sabbatical, go to ONR in London, and then develop contacts with Rome, Paris, whatever, and what not, and then go home, after one year. And they don't really do anything for the Navy. They establish some contacts between American science and the guy doing "blah, blah" in Rome, and somebody else in Paris, and anywhere else. And they exchange reports, and get—frequently get—the professor in Rome may have a senior graduate student whom he would like to send to the States. They establish this contact so the senior graduate student can go to the States a year later, that sort of thing.

I had a very good friend, spent a year with ONR London, and he was with Wayne State [University], and he had—for a couple of years, he had all kinds of students, graduate students, postdocs, what not, coming from Italy, from Germany, from wherever. He made the contacts in his year in London, and the contacts paid off by getting these students to work for him in Detroit, [Michigan], a year, two years, three years later. So this is—now that's what the Navy does. It's completely different from the Air Force. The Navy does not have any of its own staff. They're all these transients, these professors, a professor from UNC may be working for ONR London next year. You and I don't know. We could find out. It's no great secret. But you establish these contacts, and they're good. The Air Force—which also means that ONR London, I think, had one or two scientific personnel of their own. I'm not counting the guy who cuts the payroll and that sort of thing.

All right. Now the Air Force **<T: 55 min>** does just the opposite. The Air Force is in Brussels, [Belgium]. I'm sorry. They're all now—they are all in London now, but in the beginning, they were in Brussels. And the Air Force had no civilians. They had only Air Force officers. Now Air Force had a fair number of Air Force officers with Ph.D.s in this, or that, or what, in sciences. And the Air Force does a little of both. They have some money to support Joe Does in Sicily, [Italy], to—if they have something of great interest to the Air Force, they can give him twenty-five thousand dollars or something, and have him work on that project and get the reports to the appropriate Air Force lab in the U.S. that has an interest in whatever this guy is doing. And the Air—so the Air Force staff is all military, but they're all technical military. They're scientists, although they're wearing uniforms. Now the Army—oh, the other thing is the Navy started it in 1948. The Air Force got into Brussels in 1952 or 1953. And the Army, when I got in, it was in 1956, they established the office in 1955, and the Army probably established the office because the Navy was already there, the Air Force was already there. They didn't want to be—that didn't know what that was, but they didn't want to be left out.

Okay. Now the Army, on the other hand, in 1956, decided they'd have a little bit of money. And a little bit of dollars went a long way in 1956, to support a German scientist in Germany, an Italian scientist in Italy, and what not. If the subject was of interest to the Army, the reports would go to the Army. And every now and then—and there'd be one laboratory in the U.S. that has primary interest in the work in Frankfurt. I shouldn't say Frankfurt, because that's where the office was. Coincidence. Say, Munich. And say at the end of the research, or maybe even during the research, they'd encourage the two investigators, the one in the U.S. who was interest—the primary interest in the project—and the one in Munich, to get together somewhere. Probably—usually—in the U.S., in wherever the U.S. scientist is located. Get better acquainted, and maybe shape up the research even more so to the Army interest.

Now later on, the Army research money dwindled, and when I was there the second time, I served two tours there. I was there in—when my daughter was born, which was 1950s.

CARUSO: Right, 1956 to 1960, and then 1977 to 1983.

WYMAN: Right. And then I went back in 1977. But by 1977, there was very, very little money. And what we did have was travel money. And we drove around Europe, go to meetings, and then we'd find there is a professor in Stuttgart, [Germany], who is doing the sort of thing that is of great interest, say, [to] the Army's Edgewood laboratory. Well, then we would get acquainted with the guy in Stuttgart and say, "Well, we know that there's some people in Edgewood who are real interested in the stuff you're doing. How would you like to spend—we pay your way—spend a few days in Edgewood, the next time you want to visit your daughter-in-law in California or something?" Or whatever reason he may want. And then we pay his way, pay his expenses while he's at Edgewood, and get these two together.

And then later on, if there's real compatibility, I mean real—they loved each other, […] the guy at Edgewood may find money in his own budget, in Edgewood, to support the guy in Germany. But the office in London doesn't really have money to support these guys. It has two thousand dollars to **<T**: **60 min>** send him to Edgewood for three days, and that's it. If any research support comes out of it, it will have to come out from the stateside budget.

ROBERTS: So your job primarily was identifying and helping to set up these relationships?

WYMAN: That's it.

ROBERTS: So were there…

WYMAN: And we did. We had some people who—some European scientists—who'd come regularly, say twice a year, to a particular location, a particular Army laboratory, and they were received with open arms and what not. And it really helped the Army lab, who are otherwise somewhat…what? The Army—scientists in the Army labs, and probably in Air Force labs also, are somewhat isolated from the scientific community. They don't do much that's in the public eye. So this really helped the American science quite a bit.

ROBERTS: So were there places or people you could not approach?

WYMAN: What do you mean?

CARUSO: Given that it was, again, the Cold War, there were probably certain scientists in countries that you could not approach…

WYMAN: Well, only Western Europe. When there was the Cold War, it was only Western…

ROBERTS: So when you were established in Frankfurt, you couldn't cross Western Europe…

WYMAN: No.

ROBERTS: …in terms of establishing these connections?

WYMAN: I could go to a meeting in Eastern Europe. I went to—oh, some of these meetings were kind of funny. There was a meeting in—a spectroscopy meeting—in what was then still the Soviet Union, in Tallinn, which was Estonia originally. And the meeting was full of Hungarians, for example. And what do you think the Hungarians were doing in Estonia at the meeting? […] Where would you think you'd find the most of them? The beaches. There are no beaches in Hungary. There are no oceans. This isn't an ocean; this is the Baltic. But they just loved the beaches.

ROBERTS: Yeah.

WYMAN: So I discovered the Hungarians were not at the meeting. They were at the beaches. I was interested in talking to some of the Hungarians. And of course I had a special—to talk to them in Hungarian is different than if you have questions or something in English. And that was in Tallinn.

Where else did I go? I actually became quite friendly with an East German scientist who was also working on indigos, and he was from the University of Jena. And he invited me to give a seminar at the University of Jena, which was East Germany. And I gave a seminar in German about indigo dyes. This was, I don't know, 1977. Then later on, I helped him go to a meeting in England. I mean, these were sort of side things. And then much later, I heard he committed suicide, but that was mainly because I think his wife left. And he—but having—for U.S. Army scientist to give a seminar in East Germany, it was not very often.

CARUSO: Yeah. One thing that you had mentioned about your time once you went abroad was the fact that that was pretty much the end of your research.

WYMAN: What?

CARUSO: That was pretty much the end of your research career. You did little bits occasionally thereafter, but you were pretty much now in this administrative role. And I was wondering how you felt about that.

WYMAN: All right. In—I went to—all right. I did research at Natick. I did no research when I went to Frankfurt for three years. Well, a three years' gap is permissible. I mean, you could still come back to research. But I did some soul-searching, and I said to myself, I said, "Well, I'm a pretty good research scientist. I'm not likely to win the Nobel Prize. But I am a very good administrator," and I **<T**: **65 min>** have the—some other pluses—in terms of foreign languages and things like that to an administrator. I was thoroughly acquainted with the Army R&D system. So it seemed to me that I could do better as an administrator.

[…] But then the offer of the job in Durham came up, and that had a lot of appeal. Because at that time we were encouraged, the senior staff at the Army Research Office, was encouraged to do research on a part-time basis at the neighboring universities, neighboring—we were knee-deep in universities, as you know. We've got UNC here. We've got Duke. I was living in Durham. We've got NC State [North Carolina State University] a stone's throw away.

So—and I took advantage of that a fair bit, and the Army would pay for a postdoc. And I had a couple of very good postdocs in my research career. So I still had research going on at UNC, especially I mentioned David Whitten. I learned a hell of a lot of chemistry from David. And photochemistry was just—organic photochemistry—was just beginning to…what shall we say? It made a big splash in the 1960s. And the man who made the big splash was a fellow named George [S.] Hammond. And Hammond was mainly at Caltech in that period. He was at Iowa State [University] for a while.

And it—and David Whitten came from George Hammond's laboratory, from Caltech. So I learned a lot on the modern photochemistry from David, just like we were talking about the modern inorganic chemistry at Cornell back in 1940. And I had these postdocs working in the lab. They worked full time. I could come out to the lab about once a week to oversee them. And I think I had twenty publications from UNC. And one—that includes three publications I had when I was on—the Army Research Office also had a sabbatical system, and I took a year's sabbatical, went to Germany, [at the Max Planck Institute for Biophysical Chemistry].

CARUSO: The Max Planck Institute?

WYMAN: Yes. And that was great fun. And there I got about three publications.[23]

ROBERTS: So you still stayed pretty active in publications, despite the fact that you were playing a largely administrative role.

[23] For instance, D. Shulte-Frohlinde, H. Herrmann, and G. M. Wyman, "Excited State Chemistry of Indigoid Dyes IV. Triplet State Spectra and Lifetimes from Flash Photolysis at Low Temperatures," *Zeitschrift für Physikalische Chemie* 101 (1976): 115-121.

WYMAN: Yes. And I thought that was—well, you see, the theory the Army has, and it's a good one, an administrator who does nothing but administrative work gets stale on his science. And a researcher cannot do administrative work on the side. So they say, do your administrative work, and so that you don't get stale, keep your finger in research, and have a helper, a postdoc, who we'll pay for, as long as you and he interact, you and she interact. I had a Belgian girl [Andrée "Fanny" Kirsch-De Mesmaeker] who was very, very good, and we—actually, it was a married couple. David Whitten had the husband, and I had the wife. Somebody took care of the baby. The […] baby was born in Europe, and then came here. Yeah. And we're still in touch. I mean, I had a Christmas card from her. And she was here in—the year I spent on sabbatical was—my daughter was fifteen. So, well, she's—about forty years ago, she was—the postdoc was—she is retired now. She became a professor at a university in Brussels, and sends a Christmas card telling me **<T**: **70 min>** what's new with them.

ROBERTS: So did your family travel with you when you went back and forth to Europe for the sabbatical?

WYMAN: Yes. My family was—yes. Yes.

ROBERTS: What was it like being back in Europe after all the years in the United States?

WYMAN: Well, it was hard on the family. I had no problem, because I went to Germany, my German is fluent. The Max Planck Institute, everybody spoke English. I gave a couple of seminars there, and the seminar would be at 4:00 pm, and the Institute director [Albert Weller] at 3:30 pm, I'd ask him, I said, "Albert, do you want me to give the seminar in German or in English?" And he'd go one way or the other, or he'd say, "Well, it's up to you." And—but my daughter was fifteen, and she didn't know any German. I mean, she knew German when she was two, because she was born in Germany, and we had a German maid. But at fifteen, she'd forgotten it all. And dumped her into a German school. But she had great fun. And at the end of the year, we had a couple of the German girls she was friendly with come over here for a month.

And the—my wife, it was hardest on her, because she didn't speak German, and she was too old to learn. My daughter picked up German very well, very fast. But this is true of all American scientists who go on sabbaticals to other places than England or Australia, places like that.

ROBERTS: What about for you leaving Europe under the conditions that you did at seventeen, and then returning that first time to Frankfurt? Was that strange, to be returning to Germany all

grown up for work? Or had enough time passed, and the conditions were so different that you didn't think about that?

WYMAN: Well, I was very interested in what caused the war, and Nazis, and how it all came about. It was interesting to see things, talk to people. And some things didn't make sense, and still don't. For example, it wasn't—well, this was much, much later. I made a lot of friends there, and one of my friends was a German professor about my age. And I saw him develop from assistant professor, full professor, what not. And this last time I was in Germany was in 1998, so it's been, what is it? Twenty years, now. No, fifteen.

And we stopped, and he invited us to his house, and I asked him about his wartime years. Well, I had no idea the man was half Jewish. And how did he survive? His mother was Jewish and his father was not. And when Hitler was taking power, his mother decided she'd escape early and move to France, left her husband. Of course, Hitler took France, they found her, and she was executed. But he, the half-Jewish son, nobody bothered him. And this was a big surprise. They wouldn't put him in the Army, because he was half Jewish. And they had him in a labor battalion for a while, but then they decided he wasn't a very good laborer, so they sent him—they let him go to university. And he had a perfectly normal university career.

ROBERTS: Wow.

WYMAN: Which surprised me. I thought that all the ones who were half Jewish were also treated like the Jews. But no, his mother was, but he wasn't. And he lives very comfortably now, retired, in Göttingen, [Germany], university town. And a lot of these things you pick up as you—as you go along, as you get friendly with them.

I was actually—my home during my sabbatical was the Max Planck Institute. But I also, because I knew this guy, who was a university professor—university and Max Planck, they're completely different. There's a lot of animosity and jealousy between them, but I could get along with both of them. I knew both of these professors a long, long time. And I really enjoyed—I had a field day.

CARUSO: When **<T**: **75 min>** you became the director of chemistry in 1960 at the DurhamU.S. Army Research Office, what were you supposed to—well, how big was the group that you were directing?

WYMAN: All right. We had four people, including myself, four Ph.D. scientists, Ph.D. chemists. At the beginning, we only had two, and I had a chance to fill the other two slots very quickly. And what made a lot of sense to me was most of the program—I inherited the program that was existing. Most of the program was organic chemistry, so I went out and looked for the

organic chemists, and I was very fortunate in getting a very good one who was interested in doing administrative work.

And I had a chemical engineer on hand, but that was—he didn't stay very long. They found another slot for him. He really didn't fit in. And then I could hire an inorganic chemist and a physical chemist. I mean, you could only have four people, and I was sort of a jack of all trades. Organic, physical, and inorganic made sense, which is what I had, and for much of the time that I was director, these guys continued. And when I went on sabbatical, I went over to the European office, the organic chemist, who was my—who was by then the senior person there, he would pinch hit for me. He would be acting director. He…yeah.

Now again, because it's a small office, organic chemistry, to me, as an organic chemist—I mean, we talked about this—is in two pieces. There is synthetic chemistry and there's mechanistic chemistry. I was very fortunate that this guy, who worked for me for—or worked for the Army for thirty years, had experience both in synthetic and in mechanistic chemistry. He had a Ph.D. from Caltech, and then he worked with Jack [S.] Hine at Georgia Tech [Georgia Institute of Technology], who was a mechanistic guy. And then I hired him. No, he went to Turkey for a couple of years to teach chemistry for—at an English-speaking Turkish college. And then I picked him off the boat when he came back from Turkey, at an ACS meeting in New York. The boat landed, and he registered in the employment clearinghouse, and landed here, stayed thirty years. We're still good friends. He's retired now in Hilton Head, [South Carolina]. But I was very fortunate with him. […] And later on, the physical chemist, again, he stayed with me twenty years, maybe, then branched off. So we had four people. I mean, I was an organic chemist, so in a way I was—Bob, the Caltech guy, was redundant, but the program was about 60 percent organic projects that I inherited, so having two of us at the beginning was not too many, because it was mainly organic chemistry people. My predecessor, [John W. Dawson], was also an organic chemist. He was moved up to be chief scientist, and that's why I got the job as division director.

When I was on sabbatical in Germany, I…. The chief scientist retired, and I fully expected they'd offer me the job, and they did. And I knew the answer. I didn't want it. **<T: 80 min>** I enjoyed interacting with chemists. As chief scientist, you'd be interacting with mathematicians, with physicists, and what not. And how could I? It was not for me.

ROBERTS: But you had years of experience doing that while you were in Frankfurt. So why…

WYMAN: Well, but that was different. That was more a political thing. I mean, that was more a matter of—all right, I mean, opening up—I'm talking to a geographer in Switzerland mainly because we want to be able to advertise we got a contract in Switzerland. Not because I was interested in [the] geography that he had [to offer].

ROBERTS: So even though you weren't doing research, you very much enjoyed still being a part of the more specifically chemistry community?

WYMAN: Yes. Yes.

ROBERTS: So what kind of—when you say you inherited 60 percent of organic chemistry projects, what were the projects that you were overseeing, and what did that involve?

WYMAN: He now wants me to tell him what projects I had in 1960. [laughter]

CARUSO: Just generally.

WYMAN: What did you do in 1960?

ROBERTS: I can't tell you. [laughter]

WYMAN: Yes. Well, that's even. I can't, either. No, it was—we had some very good…

ROBERTS: What was the nature of the work, besides just the admin…

WYMAN: …university…Well, let's take a bigger look: 1960, the National Science Foundation I think was established in 1950, something like that. ONR was the first research supporting agency in the U.S., in 1948. The Army's always a Johnny-come-lately to these things, so they saw everybody else, and they got in, too.

And what I do remember, in 1960, our program was about one million dollars, which was a fair bit of money in 1960. And the NSF budget was a little more than one million, not much. Ten years later, the NSF budget was ten million, and ours was still one million. NSF was growing exponentially. All right. But we had everything.

The idea was [that] we support research in areas of interest to the Army laboratories. The Army has about fifteen laboratories scattered around the U.S. Most of them have some chemistry in them, not all. And the job of the Army Research Office here was to keep in—to put—sort of be a transmitter of information between the academic world, say in chemistry or physics or whatever, and the Army laboratory world of chemists, and physicists, and what not.

ROBERTS: So all of the Army chemistry labs, not just the one here in Durham? So Durham was…

WYMAN: No, no. We didn't have a lab here. We just had an office.

ROBERTS: So you were just facilitating these communications.

WYMAN: We were just facilitating. We would look for research projects that have some interest to one or more Army laboratories around the country. And Army labs were things like the Missile Command in…oh, where is it, where they shoot missiles out? In Alabama. All right?

ROBERTS: Oh, right. Right.

WYMAN: The Edgewood Chemical Command, where they have chemical warfare stuff. At Watertown, [Massachusetts], there's the Army Materials Laboratory. They're interested in materials. They're very different interests. I mean, the Missile Command wants unstable things. The Material Command wants stable things. And so getting very stable materials of a certain—that will stand up to high temperatures—they are a great interest in one lab, and no interest to the other lab, and vice versa. So we were out on the lookout for things, and encouraging the people in the universities to submit proposals to us that **<T: 85 min>** may be of interest to this lab, or this lab, or this lab. And in doing so, we had to get acquainted with what the labs were interested in, and also get acquainted with the—well, we have two hundred Ph.D. granting institutions in the country, and there's typically maybe fifteen chemists in each one. That's a lot of chemists, and what they were doing, and try to match. And it was great fun to do it.

And the same sort of thing is what the Navy does and the Air Force do. The Navy and the Air Force each have a research-supporting agency like the Army, and they each have a little more money than we do. But in the last fifty years, NSF has leapfrogged them all money-wise, deliberately so. I mean, this was done by Congress deliberately, that NSF should be the primary research supporting agency. And they are.

ROBERTS: How did that impact the kind of work that you were trying to do?

WYMAN: Well, it made us look harder for the Army relevance. If NSF will take anything—I mean, they have their own criteria, but Army relevance is not one of them—then we want to be sure that none of the ideas of people in universities are going to fall to the wayside that may have an interest to an Army laboratory somewhere, because NSF couldn't care less about the Army labs. And that's as it should be. NSF looks at scientific merit. And the Navy is interested in what the Navy—the Navy had big problems of corrosion on ships, the paint on ships and things.

And, you know, we had—the Army has no interest in that. And NSF wouldn't give it any priority, I'm sure.

The DOE now has extramural programs, and again, this is things that are of interest to the Department of Energy, what used to be Atomic Energy Commission. And twice a year—I don't know if you guys know this—all these agencies get together, sit around the table, and exchange information. So NSF says, "We've got a program of seventy million in chemistry," and the guy representing the Army Research Office probably says, "We've got seven million," and the Navy says, "We've got seven and a half million," and that sort of thing. And, "What are you interested in?" "This is what we're interested in. This is what we're not interested in."

CARUSO: During this time period, again, it's—I mean, you mentioned—earlier you mentioned the Vietnam War. This is also—when you're in this office, there's the space race going on. Did you see any of these offices around the country in any way come together on certain projects related to these larger areas of science being pursued in the U.S.?

WYMAN: Certainly not my office. No.

CARUSO: Not your office? Okay.

WYMAN: No. Let's see if anybody else got into this. No, not really.

CARUSO: Okay. I also noticed that in 1962, you organized a symposium on reversible photochemical processes, and this ultimately led to the biannual series, the International Conferences on Photochemistry. And I was kind of curious to know how that came about.

WYMAN: Well, that's kind of funny. I was—I started here in 1960, and after I got my feet on the ground, and I was also quite hot about new things in photochemistry, and especially reversible things that I found and other people had found about that time. It seemed like this was a good time to have a symposium.

And then I discovered that […] there was [an] almost unlimited amount of **<T: 90 min>** travel money. At that time, we—there was something called Category Z, I think, Category something, which meant that if we had good reason, we meaning Army Research Office, a good reason to provide travel for Professor Price in London to University of Chicago, spend a couple of days with a colleague in Chicago, and then go home, we could do this. And the way we did it is a military aircraft, at the time. A Category Z was a ticket which cost very little money, and actually, if it was done for a purpose like this, it cost nothing. And Professor Price would otherwise have to save money for twenty years before he could afford a ticket to fly to Chicago,

where he has a friend, or a colleague, or somebody who's doing work with—the two of them would benefit greatly to exchange information. And we could do it in either direction.

So after I discovered that, I said, "What the hell?" I said, "We could have a real good meeting here, and also put the office on the map, and put Duke University on the map. And let's do it." And I knew all the players in the game. I decided in 1960 to have the conference. In 1961, I took a trip to Europe and visited some of the people I knew, told them about the meeting, and I'd like them to come. And some very prominent—well, some photochemists, at the time, at the beginning of their careers, who twenty years later became very prominent, all came. And it was their first trip to the U.S., which was a big thing. There's one of them who really—something that I never expected—I'll tell you that one of them that is not necessarily going to come up in your write-up.

All right. Here is this young fellow in Germany, and somebody tells me he is doing good work, and right on the topic of the conference. He's never been to the U.S. He was maybe thirty years old. Okay. Good enough for me. I write him a letter. I think—I visited him in 1961. He made a good impression. "How would you like to come to the meeting?" He'd love to come to the meeting. Never been to the U.S.

Okay. And he gives a paper, and the meeting had about 150 participants. And, now, curtain. And the next meeting, to come back to your question, how did this meeting get to be the first one of a series? Well, nothing happened in 1962, 1964, 1965. In 1967, an Indian fellow, Indian from India, a very well-known photochemist at the time, his [name] was [Rajanarayanan] Srinivasan. You may want to write that down. Okay. The German fellow I was just talking about, his name was [Dietrich] Schulte-Frohlinde. That's more complicated.

Anyhow, Srinivasan decides in 1956 that it's—that he'd like to have a meeting like I had. IBM [International Business Machines Corporation] would give him the money. He was going to be in Yorktown Heights, you know, 50 miles from New York City, something like this. And Srinivasan was a go-getter. He got money from IBM, and he had a damn good meeting. I was there. And even though there was a five-year interval, he decided that the people who organized the previous meeting and this meeting should all get together, in sort of an international committee, and that this should be the beginning of a series, which traces its origin to my meeting. I said, "Fine."

So in 1967, we **<T: 95 min>** had the meeting in Yorktown Heights. And Srinivasan has disappeared from the scene. He may be retired. He may be dead. I don't know. But he was very active in photochemistry in the 1960s and 1970s and 1980s. Okay. All right.

But now to come back to the German fellow, all right, the German fellow comes in 1962. Never been in the U.S., and nobody ever heard of him very much. But later on, he starts making waves, and within about fifteen years after the trip to the U.S., he becomes institute director at one of the Max Planck Institutes, for photochemistry, where the previous institute director—the

Germans have a very strict retirement age.[24] You hit seventy, you're out. So Schulte-Frohlinde is the name of the guy who had his first trip in 1962. In 1970 or 1975, whenever, he becomes the successor of a very well-known older man named [Günther O.] Schenck, at one of the Max Planck Institutes. And in Germany, Max Planck Institute is the top of the world for research. All right?

And I get to know Schulte-Frohlinde some more. He still looks upon me as—well, he's still appreciating my inviting him when he wasn't known, back in 1962. We collaborate on some projects. I go to his institute many times. I think I published three or four papers with either him or some of his associates. So I was a welcome guest to his institute.

But then a funny thing happens. He invited me to his house in—where he lived in Germany, and one of his sidekicks that I collaborated with, a fellow named [Helmut] Görner—he was younger than either one of us. And I bumped into Görner at a meeting, which was the, I don't know, eighteenth or whatever meeting of the series in Vancouver, [Canada]. And Görner says, "You will be hearing from Professor Schulte-Frohlinde when you get home." Here we are in Vancouver. He knows my home is the East Coast. I said, "Well, we're going to Alaska, but all right."

And what happened to Schulte-Frohlinde, he fulfilled his requirements. He retired from that institute when he had to retire. And apparently at that point he left his wife, and got involved with an American artist who he met in Italy somewhere. And he left the wife and moved into the Washington, D.C., area. And what he was writing to me about is—I got his letter. I don't think I still have it. He says he's—he tells me the story. He now lives with so-and-so woman, and she was an artist in the Chevy Chase [Maryland] area, and everybody among his neighbors, they're all members of the Cosmos Club, so he'd like to become a member of the Cosmos Club.

And he went through the directory and found that I was a member of the Cosmos Club, and I'd known him all these years. Would I recommend him for membership? I said, "Sure." So I filled out the forms, and he became a—he got elected. And then about six months later, the ACS met in Washington. I thought I'd surely meet the guy. No, he wasn't registered, nothing. So I pick up the phone and call him. I had his number at the time. And the lady answers. Well, she thought—yes, he wasn't home now, but she thought he'd probably be at the meeting sometime. He never showed up at the meeting. He gave up chemistry and every—his life in chemistry and everything, and moved in with this woman in Chevy Chase. And I don't think that my giving him a trip in 1962 had anything to do with that, but…

ROBERTS: I don't think you can take responsibility for that. So did you ever see him again?

[24] Schulte-Frohlinde was Executive Director of the Independent Department for Radiation Chemistry at the Max Planck Institute for Coal Research, which evolved into the present-day Max Planck Institute for Bioinorganic Chemistry.

WYMAN: I haven't seen him since. No. I—**<T**: **100 min>** he's a nice guy, good-looking, good scientist. These Max Planck Institute offers are—some way he suddenly gets one from the sky. I mean, it just falls on his lap. I was at a meeting in Germany once, not too long ago, thirty years, maybe, and he was a professor in Switzerland. And he says—I asked him if I could get a ride with him to the nearest railway station. "Yes," he says. "You can help me decide." He says, "They invited me to become an institute director," at the same institute where the other guy retired from. I think he is the—actually the successor of the guy who moved to Chevy Chase. "And I don't know if I should leave Switzerland, and it's an ugly part of Germany, where that institute is, in the Ruhr District."

And, well, I rode with him for two hours, and he hadn't decided, but later on, he took the job. Max Planck—and they give you research support, everything. I mean, you don't have to do anything the rest of your life. Okay.

CARUSO: So you stay in Durham until 1977. You have your one-year sabbatical at the Max Planck Institute.

WYMAN: That was in 1972.

CARUSO: [Nineteen seventy-two, 1973]. I know during this time period you also re-approached the *cis*-indigo problem, initially with A. F. Zenhäusern while at Duke?

WYMAN: Yeah. He was one of my postdocs. Yeah.

CARUSO: Okay. And then David Whitten at UNC.

WYMAN: No, David Whitten was my mentor. He was a faculty member at UNC.

CARUSO: Oh, okay. But in 1977, you decide again…

WYMAN: Go back to Europe.

CARUSO: …to go back to Europe. And in that position, you were going to be the chief of chemistry branch?

WYMAN: Yeah.

CARUSO: Of the U.S. Army European Research Office.

WYMAN: Chief of a one-man branch.

CARUSO: So (1) why is it that you wanted to head back, and (2) what were you doing as chief of a one man branch?

WYMAN: All right. I went—1977, my daughter was ready to go to college.

CARUSO: She was twenty.

WYMAN: She was born in 1957, so she was already in college, I guess. Okay. And the office had moved from Frankfurt to London, which made life much easier—which would make life much easier for my wife, because the street signs and the store signs and the sales signs are all in English, not in German. So this seemed like a…. And number three, the head of the office was a good friend. And he just assigned—he was the commanding officer here, and his next assignment was London. And I knew I could work with him very well. So everything was in favor of it. The only thing—well, I think I got a thousand dollars less, or something, salary. But again, the job was a wonderful job. You traveled all over Western Europe, and go to meetings. I enjoyed meetings, and seeing a lot of my old friends, like people I had known for fifteen, twenty years before that.

CARUSO: So was this position similar to the one that you had when you first went to…

WYMAN: Everything changed.

CARUSO: Okay. How did it?

WYMAN: Europe changed. I mean, Europe in—when did I first go? My daughter was born…

CARUSO: [Nineteen] seventy-seven?

ROBERTS: First time you went was […] 1956, 1957, was when you first got there.

CARUSO: I think when—I think he's talking—when you were a child, when you first went there?

WYMAN: No, no, no.

CARUSO: Oh, okay.

ROBERTS: No, the first—when you were based in Frankfurt—you went in December 1956.

WYMAN: 'Fifty-seven, 'fifty-six. Yeah. Okay, 1957. So Europe in 1957 and Europe in 1977 is day and night. In 1957, they're trying to recover from the war, rebuild, what not. In 1977, all that's behind them. Now Europe is—you also have the European community, and Europe is getting to be a **<T**: **105 min>** big economic power and what not. Everything is all well settled. The dollar is worth less in terms of research support. You don't have any money to—it was mainly establishing contacts, travel, bringing people together, going to meetings, finding interesting—things of interest to the Army. So it was a completely—it was not just a different location. It was a different job, really. It was the same office, nominally it was the same job, but it was not, because you had—I mean, one of my fellow members in the office was—spent all his time on computers. There were no computers twenty years before. And we—we had more freedom to travel. We could go to meetings in Eastern Europe, not just Western Europe. There was—it was much more emphasis on liaison, and far less on buying research.

And then the surroundings were different, of course. You operate out of London versus out of Frankfurt…

CARUSO: Did you see a transition in the funding that you would get during—that you got during this…

WYMAN: Did I what?

CARUSO: Did you see a transition in the amount of funding that you got during this period? Because this is of course the Carter presidency to the Reagan presidency, and I wasn't…

WYMAN: No, no. We—the Army European Office—had almost zero funding for research support. We had enough to pay for the staff. We had enough—we had a good travel budget, because that was it. I mean, we could go to meetings and make friends. And some of these—

and then we had travel budget for once you made friends, it's to get them together with their American counterparts. Yes. But there was almost zero […] budget for research support. None.

CARUSO: Okay.

ROBERTS: But there is also—I think what Dave's pointing at, too, there's a change in foreign policy rhetoric from when you get there in 1977 to the Reagan administration and its relationship to the Soviet Union and Eastern Europe. And you're there at the bridging moment of the late 1970s and early 1980s. And so did you—you're traveling all over Europe. I mean, is this something that you sense in the meetings that you go to, or in your interactions with other scientists?

WYMAN: I'm trying to remember. There was a meeting in—actually, a couple of meetings, Soviet Union, we could go to. There were—I'd say there was a slight relaxation of where to draw the line in between Eastern and Western Europe. Yes. No, that was in East Germany. There was a meeting in East Germany. Yes. And my main claim to fame in that one, I arranged a wedding. I mean, a marriage, an American chemist with a Hungarian lady chemist.

[…]

CARUSO: So you were in that position as the only—you were the chief of a one-man chemistry branch.

WYMAN: Well, there was—again, we had—they established this, what do you call it, organizational structure, back in 1960, 1961, **<T**: **110 min>** something like this. They would have four or five—three or four scientists. There'd be a colonel who's a chief, and there'd be probably one military guy, who was frequently from the Signal Corps, because they had their special needs for signal communications, things like this. And then four civilian scientists covering chemistry, physics, frequently biology, and maybe computer—by then, computers got into mathematics, and computers, you know. So it'd be four civilian scientists at the time.

And then there was a fifth one who was sent from—the Army has a Waterways Experimental Station. It's down in Mississippi, on the Mississippi River. And a good friend came from there, and he was added to the civilian staff. And he handled the—what would you call it? Geography and whatever is beyond geography, how the land would stand up to water, and things like that. So there would be five civilians at the time. Yes.

CARUSO: Okay. And you stayed there for six years, and then came back to the same office here in Durham?

WYMAN: Yeah. I got my old job back.

CARUSO: But they expanded it to—instead of just chemistry, it was now Chemical and Biological Sciences?

WYMAN: Well, that was changed in 1973.

CARUSO: Okay. What was the reason for that change?

WYMAN: All right. The Army Research Office here was established gradually in nineteen—let's say 1965, maybe. And then the—there was also an Army Research Office in Washington. And in Washington, pretty soon there was nothing left for them to deal with except the life sciences. And there was a small group of two or three people who had the responsibility for that in Washington.

In 1972, somebody finally discovered that this is silly, you have all the sciences in Durham except one, and one is in Washington that's got no foothold, nothing. So they decided to move life sciences to Durham, except the people didn't want to move, except one guy. So [F. W. "Bill" Morthland] came, and everybody else disbanded, sort of. They went to work for other people.

All right. So then Bill Morthland was down here, and he was life sciences, and the other divisions all had four or five people in them. And the question was—the High Command had to cope with this question of what do you do with Bill Morthland? Set up a life sciences division like they had in Washington? That meant hiring three more people, probably. Or absorb Bill Morthland somewhere in the existing structure, which is a damn sight easier. You've got one guy, you could…And so they looked around, and they saw me, and they said, "Well, life sciences is closest to chemistry." It's closer than to physics or math or earth sciences, something like this. "Will you take Bill Morthland?"

I said, "I don't know beans about life sciences, but on an administrative end, I can help him. Yes. I could sign his time card and that sort of thing. I'll take him."

Well, after that, Bill was mad at me. He wanted to have his own division. Then later on, we became good friends. […] The division program got larger by 20 percent, **<T: 115 min>** which was his program, life sciences program. And nothing had really changed. I signed his time card. And then later on, what surprised Bill was when I went up to him about six months later, I said, "Bill, I think we ought to go to bat for adding a second person in life sciences." "What? I thought you were against life sciences." I said, "I'm not against life sciences. I just don't know anything about it."

So we made a big pitch for a second person. And we had this CO who went on to London later on, […] he had a Ph.D. in math from Caltech, even though he was a military guy. And a Ph.D. from Caltech in the sciences is nothing to sneer at. And the guy was real sharp. So we made a presentation, "Hey, we need more," and the CO stood up and he says, "Well," he says, "you guys made a good case. You've convinced me that you really need more than one person, but you didn't convince me that you needed two. So how about one and a half?" Like Solomon. And we hired a half-time Ph.D. in bacteriology, a woman who—married woman—whose husband was head of biochemistry at NC State. So she was only—she didn't need the money in the first place, although she negotiated for the money. She and I are still good friends. She still lives near Raleigh. And Bill is dead.

And we—so at ARO, they still have, I think, a combined chemistry and biological sciences. And—because biology is not all that big. Chemistry—nothing is big. But, say, if you have [an] eight million dollar chemistry program, and a two or three million dollar biology program, then it doesn't make sense to have a separate division.

CARUSO: So after two years back here as director, you decide to retire from…

WYMAN: Oh, I decided that long before then. I just came back to reestablish the position as a super grade position. And I stayed there for about a year. I straightened out a few things that had gone bad, and then I retired. I mean, I knew I was going to work a year and a half, roughly, something like that. Yeah.

CARUSO: Okay. And when you finished off your work with the Army Research Office, you began consulting, and you did that for about a decade?

WYMAN: Yes. That was fun.

CARUSO: Can you tell us a little bit about what you were doing in the consulting?

WYMAN: This was funny. It wasn't my idea. I went to an Army-sponsored meeting at Edgewood, which was the Chemical Warfare Center. Every year, they had a big meeting, a lot of university people. And this was—let's say early December. By then, I had announced I was going to retire January 3rd. And one of the professors from Florida, an English-born professor, can't remember his name, came up to me, he says, "Oh," he says, "you're going to retire, huh?" He says, "Boy, we'll have to have you come down to University of Florida, because you must—you can probably give our faculty lots of good advice about how to get research support." I hadn't thought of that. I said, "Oh, I'd be glad to." I mean, I had a cousin living in Florida, along with other reasons to go to Florida. Why not?

So he had a very naïve idea of what I could do for them. But anyhow, we had a meeting for two days. He says, "Come for two days. The first day, you tell us what the different agencies are interested in, and we'll have the faculty busy with notebooks and things, writing down all the brilliant ideas. And the second day, they'll come in with proposals that you can critique, and they'll send them off." It's not that simple. All right.

Anyhow, they had me, and they thought it **<T**: **120 min>** was worthwhile to talk to the faculty about my vision or how I see the best chance of getting research support. And then I thought about it some more. I decided—I always enjoyed interacting with university faculty in chemistry. And I said, "Now if I did something like this, I could offer my services to the department, and they could pull in those faculty members who they think are—need the most advice or get most benefit from advice." I mean, they wouldn't pull in the Nobel Laureates, because they already know what to do.

And I didn't need the money. The government retirement system is very good. But I could collect frequent flyer miles. And so I advertised this to various chemistry departments, and what I really enjoyed most is interacting with chemistry faculty. So—and that I could maintain.

So I started this, and wrote letters to some people I knew, mainly either senior people in chemistry departments or chairmen. And the next thing I knew, every year for something like ten years, I had about ten customers, where they'd pay my way, I'd spend a day or two at the university. How the university arranged my visits was up to them. Typically, it would be—except what I first offered—it was, I'd give a talk first. I don't know, a one-hour talk or something like this. And then they would bring in individual faculty members who they thought would benefit by having a…

[Recording paused]

WYMAN: They would meet with me one on one, and for as long as they chose. And this worked out very well. And by the time I got through, I quit the whole thing in 1996 because we had a big family crisis. My daughter had a child, she was pregnant, she had a second child who had all kinds of birth defects, at the same time that her husband came down with a brain tumor, a non-operable brain tumor. So it was a big, big crisis.

But for ten years I did this, and I went to such schools as Caltech, Pennsylvania twice—Palladino was the guy's name who organized my visit. George Palladino at—he was assistant to the chairman at Pennsylvania. And he had me come for a second visit—that surprised me—just to talk to one person. I had the usual thing for one visit, talked maybe to six people in a group, maybe ten. And then six months or eight months later, he says, "Can you come sometime in the fall? There's somebody I want you to meet." And I said, "All right." I mean, if he pays my way, I'll come and talk to the devil, you know.

But I don't know who the person was that I talked to. But I was surprised he had just one person. And the university—as I said, the universities were about half the Ivy League schools, half the Big Ten schools. I had preference for the University of Utah, because I could combine it with skiing. I went two or three times. And—what…

ROBERTS: So did your wife accompany you sometimes if you were making…

WYMAN: Hmm?

ROBERTS: Did your wife accompany you sometimes if these were kind of combined business/pleasure?

WYMAN: Sometimes, yes. I mean, I was at Caltech when we—they put me up in the faculty club there to **<T**: **125 min>** spend the night, at the same time when they were making the movie with what's his name, one of the big movie stars. It was interest—a lot of these things were very interesting. And then there were places like Arkansas. I didn't even know how to fly to University of Arkansas. The—and I talked the University of North Carolina here, in paying my way to Washington every year, for me to talk to the agencies and get updated on what's new. And for that, they could consult me any time, and they did. And it worked out very nicely for ten years. And I really enjoyed the meetings, the—with the faculty. It was great fun.

[…] Yeah, I was at MIT and Caltech and half the Ivy League. So on the other hand, there were schools like—oh, what, the University of North Carolina [at] Greensboro, which is not a research institution, and East Carolina College, which is also not a research institution. But the price was low. I mean, I got something like three hundred dollars a day. Usually it was one day for a smaller school. And the travel expenses. And like Greensboro or East Carolina, it's six cents, eight cents a mile or something. You're there and back. Didn't even have to spend on the hotel. The—but the Pennsylvania visits, the two of them, I remember, because the second one was so unique, just one person. And Palladino is the guy who had me.

My other connection to Philadelphia was this. As usual, I took the job [while I was still] at the Bureau of Standards to work for the Army's Natick laboratories. Natick was still being built. And as usual in government projects, the building was behind schedule. And the Army had a—the Quartermaster Corps—had a pretty sad-looking research laboratory at the Philadelphia [Quartermaster] Depot, which is somewhere between the center of Philadelphia and the airport. And it's—so when they told me, they said, "Well, we're running behind schedule. You want to start in Philadelphia? You want to just come later?" I said, "What the hell. I'll start in Philadelphia."

So I actually lived in Philadelphia about six months and worked in the Philadelphia Army Depot, and it also meant that for several months, there was no work to be done. The Natick lab was not finished. The Philadelphia—the chemistry was out of it. And I wrote a big review

article using the University of Pennsylvania library.[25] […] Must have been 1954. I think it was published in 1955. "The *cis-trans* isomerization of conjugated compounds." And I was kind of surprised, because [*Chemical Reviews*] at the time was known to be pretty fussy about what they'd publish, but it was published in 1955. And at that time, we didn't know much about—there was a sporadic knowledge about *cis-trans* isomerization of conjugated compounds. And yes, they were quite different from non-conjugated compounds. And I needed a library, and of course, University of Pennsylvania, [has a] fine library.

ROBERTS: Sure.

WYMAN: I could have had a library anywhere else just as well, but it so happened I was in Philadelphia with **<T**: **130 min>** no lab and in Natick…

ROBERTS: And a lot of time on your hands.

WYMAN: …and in Natick, no lab. I was still getting paid. I had to do—felt I had to do something. And it was a lot of work, getting a review article together. Ooh. That's my knee.

ROBERTS: So we've covered a lot of ground in two days. Do you think we've missed anything?

WYMAN: Well, let me go to the little boys' room and we'll check, okay?

ROBERTS: Sure.

WYMAN: And I think…

ROBERTS: Yeah, we'll…

[END OF AUDIO, FILE 2.1]

WYMAN: The—I don't know what else…

[25] G. M. Wyman, "The Cis-Trans Isomerization of Conjugated compounds," *Chemical Reviews* 55 (1955): 625-657.

ROBERTS: I feel like we've been pretty thorough, unless you can come up with something.

WYMAN: I'm trying to think if the—the one thing that—my dabbling into photochemistry was really accidental. It just happened that we had this sensitive instrument, and the instrument says something's happening. And then we dug into it and see what is happening. Okay? So far, so good. But what we did not do at the time, because we didn't know enough, is "How is this happening?" And that's—that came along about ten years later. And for this, for other things, the—that's sort of—that's George Hammond's contribution to photochemistry, that he cleared up the intermediates that may be happening. I mean, if a molecule starts like this and you shine light on it and it goes like this, why? What could be happening in the middle? And it turns out that it doesn't have to be light. It also could be sensitizers, and the sensitized isomerization, is George Hammond, is all his baby.

And an awful lot has been done since then, and—on these isomerizations, things like that. What this was—these two things I gave you were submitted was a very informal publication, as you could tell. The photochemists have all kinds of societies, not as big as the ACS, but sub-societies. And this is the…

ROBERTS: Is that the European Photochemistry Association?

WYMAN: The Europeans have a Photochemistry Association, and then the Americans have one. And there is a fair bit of interaction. And the Americans also have a newsletter, and frequently, what's published in one newsletter gets published in the other newsletter also. I was—I think it was when I was on sabbatical that I got to be real friendly with the secretary of the Europeans, so I submitted some of these—like I submitted the how did I take up pipe smoking. I mean, some of it is funny like that. The—and they published it—so this doesn't really count as a publication. The pipe smoking is gone. I don't know why—I gave [them away]. I was telling you how I took up pipe smoking. In self-defense. And…

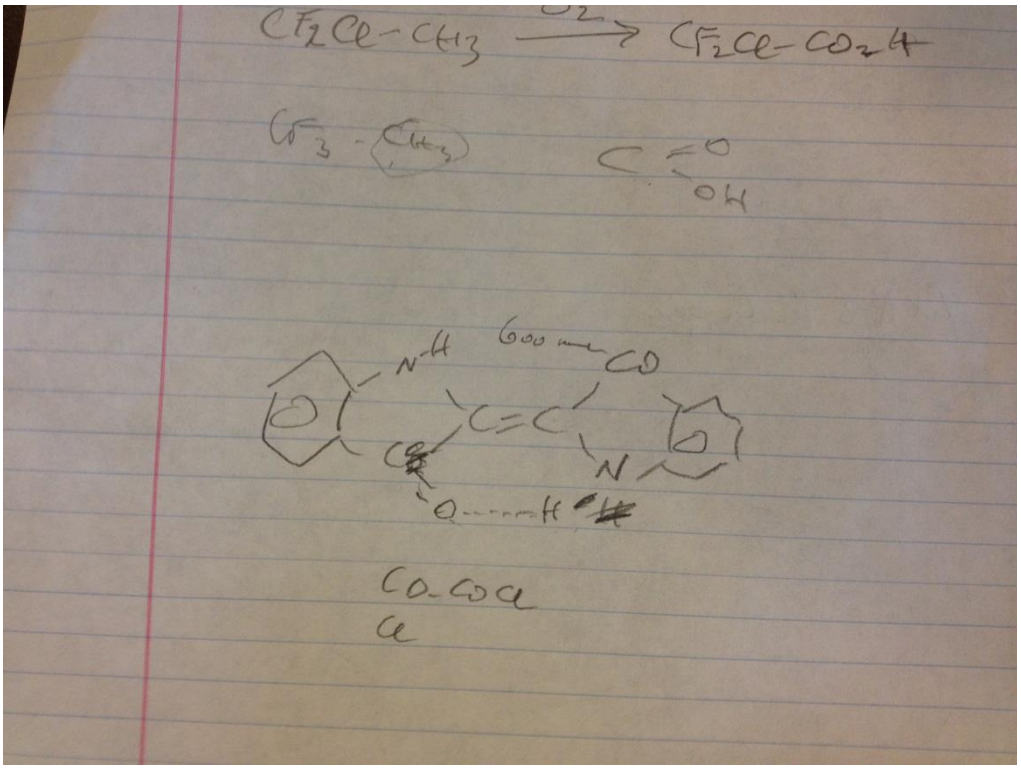
ROBERTS: It was a pretty good tactic.

WYMAN: It was necessary. All right. I want—so you guys are going to—you're collecting how many of these, roughly?

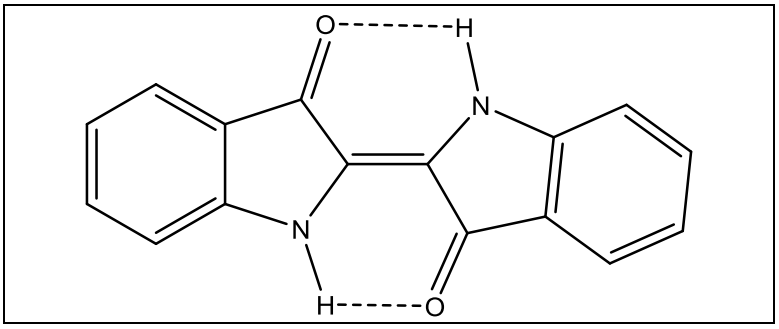
[END OF AUDIO, FILE 2.2]

[END OF INTERVIEW]

APPENDIX



Wyman's drawing of *trans*-indigo



***Trans*-indigo**

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