

CHEMICAL HERITAGE FOUNDATION

N. BRUCE HANNAY

Transcript of an Interview
Conducted by

James J. Bohning

in

Baltimore, Maryland

on

9 March 1995

(With Subsequent Corrections and Additions)

ACKNOWLEDGEMENT

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THE CHEMICAL HERITAGE FOUNDATION

Oral History Program

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(Signature) N. Bruce Hannay
N. Bruce Hannay

(Date) March 21, 1995

(Revised 17 March 1993)

N. BRUCE HANNAY

1921 Born in Mt. Vernon, Washington, on 9 February
1996 Died in Bremerton, Washington, on 2 June

Education

1942 B.A., chemistry, Swarthmore College
1943 M.A., physical chemistry, Princeton University
1944 Ph.D., physical chemistry, Princeton University

Professional Experience

Bell Telephone Laboratories
1942-1960 Research Chemist
1960-1967 Chemical Director
1967-1973 Executive Director, Research, Material Science and Engineering
1973-1982 Vice President, Research and Patents

Honors

1976 Acheson Medal, The Electrochemical Society
1978 Honorary Ph.D., Tel Aviv University
1979 Honorary D.Sc., Swarthmore College
1981 Honorary D.Sc., Polytechnic Institute of New York
1983 Perkin Medal, Society of Chemical Industry

ABSTRACT

The interview begins with Dr. Hannay describing his family background and his early education in Washington state. Both his high school chemistry teacher and his older brother greatly influenced his decision to pursue chemistry and to attend Swarthmore College, where he received a B.A. in chemistry in 1942. With the advent of World War II, Hannay received a student deferment from the draft because his doctoral thesis at Princeton University—involving the measurement of dipole moments—related to the synthetic rubber program. While still at Princeton, Hugh Taylor involved him in the Manhattan Project and after receiving his Ph.D. in 1944, Hannay took a job with Bell Laboratories, where he continued his work on the Manhattan Project. Once the war ended, Hannay began research on the mechanisms of thermionic emission from oxide cathodes. The invention of the transistor in 1947 led him to focus on silicon, which was deemed more useful in semiconductor research than single crystals of germanium. This work resulted in Hannay's development of a mass spectrograph to analyze solids. Soon after, Bell Labs asked him to coordinate the silicon research. In 1954, Hannay became a research supervisor, and he discovered a preference for management. Following this inclination, he continued on at Bell Labs in various management capacities until his retirement in 1982. This interview concludes with Hannay's brief assessment of the chemical industry and its need for more research autonomy.

INTERVIEWER

James J. Bohning is Professor of Chemistry Emeritus at Wilkes University, where he was a faculty member from 1959 to 1990. He served there as chemistry department chair from 1970 to 1986 and environmental science department chair from 1987 to 1990. He was chair of the American Chemical Society's Division of the History of Chemistry in 1986, received the Division's outstanding paper award in 1989, and presented more than twenty-five papers before the Division at national meetings of the Society. He has been on the advisory committee of the Society's National Historic Chemical Landmarks committee since its inception in 1992. He developed the oral history program of the Chemical Heritage Foundation beginning in 1985, and was the Foundation's Director of Oral History from 1990 to 1995. He currently writes for the American Chemical Society News Service.

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INTERVIEWER: James J. Bohning

INTERVIEWEE: N. Bruce Hannay

LOCATION: Baltimore, Maryland

DATE: 9 March 1995

BOHNING: Dr. Hannay, I know that you were born on February 9, 1921. Could you tell me something about your father and mother and your family background?

HANNAY: I was born in a small town in the state of Washington. My father and his parents and his brothers and sisters had descended from a Scottish family who came to Nova Scotia. After a few years there, they said it wasn't any better than Scotland as far as the climate, so they started migrating west. [laughter] They spent a little time in Chicago, where my father was born. Then they lived in South Dakota for awhile.

Finally they came to the state of Washington, where my father, leading the family, acquired a small town bank or one that was just starting. He was a banker by profession, and so were his father and his brothers. That was his business in this small town, in Mount Vernon, Washington. It was a successful bank and it's a rapidly growing region.

I might comment that the thing that really gave that part of the country a big boost was the Alaska gold rush. People came out there and they poured through the state of Washington because that was the closest point to getting to Alaska to dig for gold. All the towns grew—Seattle grew, everything grew.

My mother was the offspring of a family of pioneers who had come from southern Indiana. She was born in the state of Washington in 1887, before Washington was a state. I'll tell you, the first settlement in Seattle was, I think, begun in 1859—so she was a real pioneer. [laughter]

BOHNING: I'll say.

HANNAY: Her ancestors came from the Daniel Boone family in Kentucky, across the river from southern Indiana.

We lived there and it was pleasant, being born and spending my early years in a small town. But when the Depression came along, the bank's business was largely mortgages on farms that were owned by the Swedish farmers who had left Minnesota because they were too crowded there. They had all come out to Skagit County in the state of Washington, and they all had mortgages. [laughter]

My father said, "There's no way we're going to foreclose any of these mortgages. These are honest people; they'll pay it back when they have the money." But he couldn't float it because of the money requirements, so he sold the bank. I might say his father was working in the bank, and his brothers were working in the bank, and his brother-in-law was working in the bank.

He sold the bank to the National Bank of Commerce in Seattle, a large bank which had a much better capitalization. He became an officer of that bank and we moved to Seattle. That was fine, except that my father was killed in an accident soon after we got there. My mother had three sons of varying ages, from high school on down. I was the youngest. She was left with the job of bringing up three boys, and she did it.

I lived in Seattle through my high school years. I was the youngest; I was the last to leave. Then I went east to college. The first time I ever crossed the Mississippi was going east to college, in the outskirts of Philadelphia. [laughter]

BOHNING: You received your grade school education in Mount Vernon, then.

HANNAY: Part of it, part of it.

BOHNING: However, you received most of your education in Seattle.

HANNAY: Yes. I got through the fourth grade, or something like that, in Mount Vernon. Then I went to Seattle and got the rest of the grammar school education and high school there.

BOHNING: I notice that on this form, you've mentioned that during your high school career, both your second brother and your high school chemistry teacher had an influence on you (1).

HANNAY: Yes. The teacher was very good. It was one of those public high schools, Garfield High School in Seattle. I was a pretty bright student, and the chemistry teachers—one in particular, but actually both of them, I think there were two—were very encouraging. I remember that they asked all the students— maybe this was juniors and seniors—to go to a

little class once a year where they could spend a couple of hours hearing about a profession. They'd invite somebody in from the local community to tell them what it was like.

I just assumed I'd be a banker, everybody in my family was a banker, and I went around and I listened to the one in banking, and so forth. But by the time I finished high school I'd been so enamoured of the possibility of going into chemistry. These teachers were so careful to cultivate my incipient interest which began to emerge, taking all the courses offered in high school. In fact, they offered me the chance to go to courses that weren't even listed as courses—just go and work in the back room, and do experiments and study ahead and learn things. It was really quite nice, because they saw that I could get through their assigned work in their chemistry classes. The people in mathematics did the same thing and also, to some extent, the people in physics. It was very encouraging.

For example, why did I become a chemist rather than a physicist, when I ended up later being at least as much of a physicist as a chemist? It was simply because those teachers were more open in their cultivation of me, so I regarded them as being important.

The oldest brother had gone off, he had taken a job and lived far away. But the next one down was a pretty scholarly one, and he spent a lot of time trying to encourage me to learn things and do things and so forth. So I'd say it was very fortunate.

BOHNING: Was this a large high school?

HANNAY: Oh yes, it was a big public high school. Seattle at that time I think had a population of three hundred sixty thousand or something, and there were nine public high schools. These had something like three thousand students. My high school had the most diverse collection of people racially, and that was good. There were a lot of black students, and there were oriental students, and it was fine. We all got along very well.

It was a good school to go to, Garfield High School. People who went there, I run into them from time to time, not the ones I knew then—but I think that all the people who went to that place are proud to claim it as their high school.

BOHNING: By that time, both of your older brothers had gone on to college.

HANNAY: Yes, they went to the University of Washington.

BOHNING: Was it a foregone conclusion then, that you would be doing the same thing?

HANNAY: They both encouraged me not to go to the University of Washington. They felt that I would do much better at a small liberal arts college with strict academic standards. So while they didn't force me to do it, they whetted my appetite in this. They weren't particularly unhappy at the large state university, but the University of Washington today is a much stronger institution than it was then. It was not all that strong, academically. They both got a good education; they just encouraged me to do something different.

We talked about it with my next older brother. We talked about places to go, and he said the best in the country was a place called Swarthmore College in [Swarthmore, Pennsylvania, a suburb of] Philadelphia. So I only applied two places; I applied to Swarthmore and Caltech. I was admitted to both of them, but I chose Swarthmore and went there as an undergraduate.

BOHNING: Why did you choose Swarthmore if you were admitted to Caltech?

HANNAY: Caltech had a tremendous reputation in science, but I thought that Swarthmore would provide a more broadly based liberal education. Also, it was coeducational, which sounded like a good idea. I was shy with girls and I thought that maybe it'd be good to be exposed to girls. As it turned out, Swarthmore allowed one to specialize so much, I think I would have gotten more liberal arts courses if I'd gone to Caltech than I did get at Swarthmore, where I took chemistry, physics, and mathematics until it was coming out of my ears. [laughter]

BOHNING: That's interesting. I wouldn't have thought that. [laughter]

I'm curious as to why your brother thought Swarthmore was the finest. Did your family have any connection with it?

HANNAY: No, we have none at all! There are no Quakers in the family. But you could read articles, and when you read articles about college, small liberal arts colleges were mentioned prominently. Invariably, Swarthmore was either the top one that was named or in the top two or three.

The others were, I'm sure, very good. Haverford was another Quaker college, but that was all men. I could name some more—Carlton in Minnesota, and Reed College in Oregon was a possibility; and maybe one or two others. But Swarthmore was always up there and usually named number one, so I thought that I ought to go there.

BOHNING: How did you finance going to Swarthmore?

HANNAY: Things didn't cost as much in those days, and I had a small scholarship, but my father had left enough for my mother to take care of things pretty well, so we paid for it.

BOHNING: You had said the first time you crossed the Mississippi was going to Philadelphia. Did you make that trip alone? How did you react to Philadelphia when you got there?

HANNAY: My oldest brother was now working in Texas. [laughter] The middle one of the three of us was in graduate school, and he was probably at Stanford at that time. During the summers my mother was eager to see her oldest son, so we spent a lot of time driving around. We'd just get in the family car and go for a trip; we'd drive down to Texas to see my brother. Nobody had any pressing obligations. So I'd been around as far as Texas, but I'd never been as far as the Mississippi River.

When the time came for me to start at Swarthmore, my middle brother by that time was a graduate student at Princeton, in economics. We just took the family car and just kept going. I had my stuff in the back of the car.

My mother was on the trip. She wanted to see where I was going to school, so the two of them dropped me off at Swarthmore, and my mother shed a few tears leaving me to the tender mercy of the East. I believe that it was probably her first time in the East.

I was sort of excited by it. I'd heard of these places like New York and Philadelphia and Baltimore and Washington and Boston, and never seen them or been anywhere near them—I hadn't even been to Chicago—so it was all very exciting.

BOHNING: That was 1938?

HANNAY: That was 1938, yes.

BOHNING: What was Swarthmore like at that time, when you started there?

HANNAY: One thing about it was, we had very distinguished president at that school. He was Frank Aydelotte. He was a famous American educator, and that's why Swarthmore was

always at the top of these lists. He later became head of the American Rhodes Scholar Committee and Director of the Institute for Advanced Study in Princeton.

This was part of my reason for choosing Swarthmore. He had instituted a program of study called the Honors Program. Everybody took courses for the first two years. Then, generally speaking, the top half of the students—pretty close to half—would only take two subjects at a time each semester. They'd have seminars that might last all afternoon, or they might last a good part of the day. It would be a small group of people, maybe six, seven students meeting with the professor. It wasn't an assistant professor or somebody else, it was the professor. You'd meet with him, and he'd pick your brains and cram things into your head. It was an informal setting. Boy, you'd really learn things that way because he was giving you individual attention. We had a teacher to student ratio of maybe six.

BOHNING: That's incredible! [laughter]

HANNAY: These were professors, and they were smart people who loved teaching. They weren't hired because they were great research scientists; they could have done research, but they loved teaching. Boy, that was good. If you went to a large school at that time, there'd be a big lecture, and the professor might give it once a week.

In fact, when I was later a graduate student at Princeton, I had a scholarship which got me teaching physics. I'd go once a week to the lecture that the professor gave, and he had a student group of one hundred people or so. He gave a lecture for an hour—that's what they saw of him. Then they'd break up into smaller groups, like twenty or twenty-five. I was a graduate student in chemistry and I was teaching these freshmen physics, in groups like that. Why, I was only a graduate student in chemistry. That's quite different from Swarthmore style.

I'm not knocking Princeton. It was a very good place, but I'm just saying that Swarthmore gave an attention to the students which was remarkable. When I finished Swarthmore and went to Princeton—and Princeton is a good university and it was well known in chemistry—I knew everything that they taught in the first-year courses. I started with just a couple of the second-year courses because I knew everything in some of the second-year courses in chemistry. I was ready for my Ph.D. exam—the written exam—after one year at Princeton.

BOHNING: That's quite a testament to your training at Swarthmore.

HANNAY: At Swarthmore.

BOHNING: Who were some of the chemistry faculty?

HANNAY: Well, the chairman of the department was H. Jermain Maude Creighton. He was a well-known electrochemist. He gave the freshman chemistry lectures at that time. We all remembered, he'd pace back and forth at the front of the lecture hall as he talked. Once in a while, he'd stop and write something on the blackboard. Then he went back and forth. Some of the students would tick it off; they'd want to know how many times during the course of the hour lecture he went back and forth.

There was a younger one who was a Ph.D. in chemistry. His name was Walter B. Keighton. He was just remarkable in his devotion to teaching the students. He had a great influence on me.

There was another fellow, the organic chemistry professor, whose name was Ed Cox, Edward [H.] Cox. He was a tough fellow and he wanted everybody to do better than they were already doing, and he stuck it to us. I didn't want to be an organic chemist, but boy, he was—. [laughter]

Those were the three principal ones. There was an analytical chemist, but those were the three big names.

BOHNING: What were the laboratory facilities like?

HANNAY: Not bad. It was an old building, built in the Dark Ages, and the laboratory stuff was not new and not remarkable, but it was very adequate. We had plenty of laboratory work.

The professor would come to the laboratory! There wasn't a graduate student there telling you what to do. When I was at Princeton, I sometimes assisted in the chemistry labs, but the professor wasn't there most of the time. These guys would come in a white coat to the laboratory, and they were going around looking over everybody's shoulder. I felt that they did an outstanding teaching job. I couldn't speak more highly of the quality of the education at Swarthmore.

BOHNING: You said you'd taken a lot of physics and math at the same time.

HANNAY: When you arrived at Swarthmore, they said, "Do you know what you want to major in?" I was pretty sure I wanted to be a chemistry major, but the system was, the chemistry major only started physics in his second year. I wanted to start physics right then and there, so I told Professor Creighton I didn't know whether I wanted to be a chemistry or a physics major—

distorting the truth a little bit. He said, "Well, take introductory physics in your freshman year, so that you can go either way," which is just what I wanted. [laughter]

I started with both physics and chemistry. [laughter] I guess my interest in physics increased a great deal when I was there because there was an assistant—he probably was an associate professor by the time I left, and the name was [William C.] Elmore—and he was just as good as these chemistry professors who were working with the students. I found him an outstanding teacher, and our mathematics professors were outstanding. I was enjoying myself thoroughly in all this.

When I finally started a working career after graduate school, and went to Bell Labs. I found myself doing mostly physics. Then I sort of regretted that I hadn't been a physics major, but that was all right.

BOHNING: I'm intrigued by your comment earlier that you didn't get many classes in the liberal arts. [laughter]

HANNAY: Well, you can see how, if I was taking physics instead of some liberal arts course, and working to take more mathematics and more physics and more chemistry than were really in the book, there was only so much time during the day. Also, with this honor system where you just had the two seminars, you should have gotten your liberal arts courses in your first two years, your freshman and sophomore years. I took what was required, which was to learn German, and I had to take something from the humanities, so I took a year of English. That was about it. My liberal arts consisted of two semesters of English courses—I picked out a couple—and German. The rest of the time I was taking chemistry, physics, and mathematics.

BOHNING: That means you had the flexibility to make those choices.

HANNAY: That's right.

BOHNING: At a number of liberal arts schools, you don't have that kind of flexibility.

HANNAY: That's true, that's true. I'm not sure that Swarthmore would give it to anybody now, but I worked the system to what I wanted to do at the time, and I think they probably were a little startled to find that I hadn't taken more things. Later on it would have been very nice to have done it, but I was in a great hurry to get out of my subject and get on to graduate school.

BOHNING: How many chemistry majors were there? Were there a large number?

HANNAY: I would guess that there must have been about oh, fifteen or so who started at the same time I did. See, the classes at Swarthmore had maybe eight or nine hundred students at that time, and the classes were all quite small. I would say that that was about the number.

About half of the chemistry students, let's say the top half, went into this honors program. The other half continued in regular courses in the same subjects that we were taking, but not with the same intensity and not the same level—so while we remained friendly with them and knew them as fellow students, the ones I was in close association with were the eight or so who were in the chemistry honors program. I got to know those people very, very well. We spent all our time together because we were supposed to take the same things—the same physics seminars and the same mathematics seminars—so we knew each other pretty well.

BOHNING: Did you get much laboratory experience during this seminar time?

HANNAY: Yes. Yes, they had laboratories. They didn't neglect that. I'd say that the typical one in physical chemistry or organic chemistry was, we would have one morning or afternoon a week in a seminar session. That might be up to four hours. Then there would be a laboratory once a week and we'd spend the whole afternoon in it, let's say from one o'clock to five o'clock. That was true in physical chemistry, it was true in analytical chemistry, it was true in organic chemistry, it was true in physics—so we had a full quota of laboratory experience.

BOHNING: Did you have a chance to do any independent research?

HANNAY: Not really, although some of the teachers had encouraged us to do a little bit more than the prescribed thing. They were trying to have kind of an open-ended thing and would encourage us to do such things as, "Here's something you want to synthesize or something you want to do, why don't you go read about it and find out a way to do it and then come and tell me," so that we didn't waste our time on a dead end or blow ourselves up or something.

That isn't independent research. They were encouraging us to be independent in our thinking, but it wasn't that they set us to work on long projects where we had to plot the course—that didn't happen. I guess later on it did, but it wasn't the fashion at that time.

BOHNING: The war started. You were in your senior year.

HANNAY: The war started. I can remember exactly where I was. The young girl who was studying with me one evening in the chemistry library—and who has been my wife since not too long after that—and I were studying and we were the only ones in there, when one of the other chemistry majors and his girl—the one whom he's married to now—came into the library. They looked kind of ashen and they said, "Did you hear the news?" "No, what's the news?" They told us about Pearl Harbor; they'd just heard it on the radio.

That was December of our senior year, because that was 1941 and I graduated in 1942. It was December of our senior year.

BOHNING: What effect did this have? Were you worried about being drafted at that point?

HANNAY: Yes. Of course they had the draft going, and both my brothers had been drafted. But the first students—we were of an age that they weren't drafting us yet. I went up and I started at Princeton only about the length of time that it takes to take the train out to get from one place to the other. [laughter] I started at Princeton.

Then, I was quickly swept in. I had a student deferment at first. Then they started drafting everybody, but the work that I was doing—and I had already started my thesis work—was considered worthy of deferment because it related to the synthetic rubber program.

[END OF TAPE, SIDE ONE]

HANNAY: Then, partway through my graduate work, I was swept in on the Manhattan Project. The chairman of the chemistry department at Princeton was a big wheel in the Manhattan Project.

BOHNING: Was that Hugh [S.] Taylor?

HANNAY: That was Hugh Taylor.

BOHNING: Okay.

HANNAY: That part of it was called the K-25 project, which was the Oak Ridge gaseous

diffusion. It was at Columbia University [and] at the Kellogg Corporation, which was something derived from the M. W. Kellogg Company—I think I got the initials right. Hugh Taylor had all the graduate students working on it in chemistry whom he could get, and he was a hard driver. He wasn't my thesis professor, but one of the stories about him was, we were doing our Manhattan Project work in one of the freshman laboratories or something—a big laboratory, and every one of the graduate students had a piece of real estate in that place. There were twelve of us, I think, who were using that laboratory. He came in one night at midnight—he'd just come back from Columbia, or Kellogg—and he looked around and there were six people working, hard at work. He said, "Where is everybody?" [laughter]

That was the style. I'll tell you, we really worked hard. When I finally moved to Bell Labs, they had a small group working on the Manhattan Project. I transferred up there working on the same area that I had been working on.

Another thing that isn't generally known is, the material—the diffusion barrier that was finally used at Oak Ridge and that allowed them to separate U-235 and U-238—was Bell Labs'. It was not done at Columbia or Kellogg or Princeton.

BOHNING: Oh, really?

HANNAY: It was done at Bell Labs. They've never wanted to advertise it particularly, because they didn't want to associate the telephone company with such a fearsome weapon, but in fact, it was a Bell Labs project. I don't mean the idea of having a diffusion barrier, but the particular metallurgical processes and so forth were the idea of somebody at Bell Labs, and we worked on it together.

My job was measuring the reaction rate between uranium hexafluoride and these barriers. I had a great big tank of fluorine at about two thousand pounds of pressure in my lab. [laughter]

I'll tell you kind of an amusing story on that that comes to mind. I had a canned pressure tank of uranium hexafluoride. Where I'd get the uranium hexafluoride is, I'd go up to SAM Lab, to Special Alloy and Materials or something, that was the code for that place up at—I've forgotten whether it was at Columbia or at Kellogg—and I'd get the thing filled.

I wasn't given a limousine to go up in. I took a bus from Bell Labs, which was about thirty miles west of New York. I took a bus to Summit, New Jersey, got off that, took the [Delaware,] Lackawanna [and Western] Railroad to Hoboken. I took what we called the tube, the Hudson and Manhattan tube, over to Thirty-third Street and Sixth Avenue, changed to the

IRT, and took that up to Columbia University. I went in with the can, and they would fill it, and

then I reversed the whole thing. It was the better part of a day. That's the way I got the supply of uranium hexafluoride.

I was coming back one time from this, and I was sitting on the IRT. I began to smell uranium hexafluoride. The valve on my little tank obviously was defective—not badly, but enough so that I smelled something, and I was pretty sure it was sulfur hexafluoride.

Hey, what was I going to do? Here I was a young new employee, and I was leaking uranium hexafluoride into a subway car on the IRT! [laughter] Well, being cowardly, I just went on with it and went right through that whole procedure, I didn't call the police or anything. When I finally got to Murray Hill, I attached it to my system and then that put a stop to the leakage out, but holy crow! [laughter]

I've told that story to people about it, sitting in there for the Manhattan Project with a leaking tank of uranium hexafluoride, and people are sort of aghast! [laughter]

BOHNING: That's amazing!

Let me go back a little bit. Your brother, I think you'd said, had gone on to Princeton. First of all, when did you decide you wanted to go on for the Ph.D.?

HANNAY: I just sort of assumed that I would. He encouraged me to do it, my oldest brother did; my middle brother was himself working through to earn a Ph.D. He didn't finish it, by the way, because the war interfered and he didn't have the heart to go in and finish it; he got partway through the program. Also, he was kind of an intellectual—I shouldn't use the word dilettante, but he was interested in so many things that he didn't concentrate. I had a much stronger interest in concentrating on the target. We were after him all the time to get on with it and start his thesis and so forth. He could always find ways to go off on side issues.

He didn't finish it. That was too bad, because he had the brains to do it, but he didn't have the drive. My oldest brother went into commercial activities right away, and he wasn't the student that the other two of us were. He had no interest in doing that. Not that he wasn't intelligent and successful, but he was much more conventional in his ambitions about what he wanted and his working life.

BOHNING: Had you selected Princeton because of your brother?

HANNAY: No. At that time, I had made up my mind I was going to be in physical chemistry, not organic chemistry, so I had advice and my own observations as to which were the top

universities in the country—graduate schools—in physical chemistry. The list would have been somewhat different if it had been organic chemistry. Those were the two primary fields, and lots more people took organic than took physical chemistry. If you were going to be an organic chemist, you might have said the University of Illinois, which is running over with stars in organic chemistry, but not in physical chemistry.

Princeton and Caltech and MIT and maybe Harvard—they were the top graduate schools in physical chemistry. I was somewhat influenced by the size of the fellowships I could get at each one, and I kind of liked the looks of Princeton. I was also very impressed with the reputation of Henry Eyring, who was a theoretical chemist.

You're a chemist, aren't you?

BOHNING: I'm a chemist, yes.

HANNAY: Well then, you know Eyring. Henry Eyring was at Princeton at that time. I find him a dazzling figure. I went there thinking that when it came time to do a thesis, I would do it with Henry Eyring. He gently steered me away from that, or told me to wait a while and make up my mind, and I ended up doing my thesis with Charlie [Charles P.] Smyth.

BOHNING: I see.

HANNAY: Hugh Taylor had a big reputation in catalysis, and so did Henry Eyring. But an organic chemist, I would say, should not have gone to Princeton at that time, because there were people who were respectable organic chemists, but that wasn't the thing—by all odds, it was physical chemistry at Princeton.

BOHNING: What was it about physical chemistry that attracted you, rather than organic?

HANNAY: Probably the incipient interest in physics.

BOHNING: I would think so.

HANNAY: Well, I don't know. At that point in time, I hadn't particularly thought about whether industry or an academic career was of interest to me. It just seemed to me that there were more exciting opportunities that might come my way—exciting to me. Don't

misunderstand me, I have great respect for the organic chemists, and they've done a tremendous job over the years. But you make your choice.

BOHNING: When you arrived in Princeton in 1942, shortly after you left Swarthmore, you had a teaching assistantship. Was that the first position that you held?

HANNAY: Yes, it was. In the first year of graduate work, the people with the assistantships were helpers in the lab. We'd attend the lectures, but we'd help in the lab. We didn't teach sections or anything.

But then I got a fellowship that was, I think, the Allied Chemical and Dye Fellowship, which I never took up because they came around—I guess Hugh Taylor came around—and said that the physics department needed teachers because all of the professors were at Los Alamos. He didn't mention Los Alamos, but he said they were all away. I found out after I got over there. There really were only one or two professors left. Everybody else was in Los Alamos, including the instructors or people who might teach the sections.

They were rounding up graduate students out of other departments who might be qualified to teach sections. They wouldn't have gotten particularly an organic chemist, because he wouldn't have been interested enough in physics; but I had had enough physics, and was interested, and I was a physical chemist, so they got me over there.

One of the remaining physics professors was giving the lectures. They were a once-a-week kind of thing. He was an outstanding lecturer. Then I had a couple of sections and I would meet with them, I've forgotten whether it was two or three times a week. We would review the things that had been covered in the lecture and then there were problems and all kinds of things, and they'd ask questions. This is where there was an interactive session.

I would suppose there were probably twenty to twenty-five people in the section, and they were freshmen. [laughter] I guess by that time, I was probably a second-year graduate student. They were either Princeton freshmen, regular students, or they came out of one of the military programs, the ASTP—I've forgotten what it was, but they were army. Then there was the navy program, the V-12 program. They had, let's say, different backgrounds in their high schools, so it wasn't an identical job teaching them.

I taught the Princeton freshmen for a while, and then I went over and taught the army—the ASTP program—but not the navy people. We all got divided up. [laughter]

They divided the students up alphabetically. I had a class in which I think half of the people were named Moskowitz. [laughter] I had all the Ms, everybody in the section had a name beginning with M.

BOHNING: You said that some of your first thesis work had to do with the synthetic rubber program. That was Smyth at that point?

HANNAY: That was with Smyth, yes. Of course I actually started it when I first got there, in a mild kind of a way.

I was measuring dipole moments and the electronic structure of molecules. The particular topic that we agreed on and set had to do with unsaturated compounds of the butadiene class or derivatives of butadiene, and there was the electronic structure with those because of the resonance, as they called it then, between various electronic structures. You kind of artificially would end up saying, "You've got a certain amount of this and that. Where are the electrons in there, and why does it have a dipole moment of such-and-such?" I measured a whole bunch of those things.

These were materials that were used in the synthetic rubber program or related to it— isoprene and the whole bunch. The rubber program didn't depend upon my work, far from it. It was work on a class of compounds, trying to understand them, not just commercial potential but actually also defense work, because it was related to the synthetic rubber program. That got me deferred, for a while anyway.

BOHNING: You had mentioned that you had wanted to work with Eyring, but he steered you away. May I ask why?

HANNAY: I don't know, you'd have to ask Henry. [laughter] Henry's in heaven with [Cyrus] McCormick and [Thomas] Edison; basically I learned mechanics from him. He probably figured I wasn't smart enough or something. Anyway, he wasn't convinced that I knew what I was doing, so while we were extremely friendly and I enjoyed his classes and got along very well with him and, I think, learned a lot from him, in looking back on it, I was much better off with Charlie Smyth.

I think it's very hard for a student at that age to see very clearly what you really should do. You're too much influenced by the man himself, and Eyring had this big reputation.

I wouldn't have wanted to be an Eyring student in the long run. Charlie Smyth was a marvelous person to do a thesis with. He was such a fine person, such a gentlemen. I learned a lot from him, but he wasn't flashy the way Eyring was. I think Eyring did me a favor in the long run.

I was pleased with my association with Eyring, but it was strictly in classroom context. He was quite a teacher, too.

Charlie was a great name, too, in chemistry, because there wasn't anybody in the world who was better known in the measurement of dipole moments and the determination of molecular structure through the dipole moment measurement. He had a succession of graduate students who had—you'd take a particular class of compounds the way I had these conjugated diene structures, and work your way through that with whatever suggested itself to see if you could work out a coherent explanation of the effects of where the electrons were. Other people had done other things.

Another thing I did with him was, he was very interested in knowing whether somebody could measure the dipole moment of hydrogen fluoride. Hydrogen iodide, hydrogen bromide, and hydrogen chloride had been measured, and Linus Pauling had derived from those three a formula for electronegativity—an electronegativity scale. But nobody had ever been able to measure hydrogen fluoride because it would chew up the apparatus.

The opportunity came when I was a graduate student with Charlie because Teflon was invented, I guess, at DuPont.

BOHNING: Yes.

HANNAY: Finally, there was an insulator that wouldn't be attacked by hydrogen fluoride. Charlie suggested that I go to work on hydrogen fluoride. We drew up a little cell and the system that could be used to insert the cell into the apparatus that was going to measure the dielectric constant. We took advantage of the fact that he could use his connections and get some Teflon. That was the insulator that we used to separate the electrodes. We'd have to have two electrodes, there's no getting around that! [laughter]

We did this and I measured the hydrogen fluoride, and he was astonished because it was way off from what had been expected from the extrapolation of the other three. He was too nice a fellow to tell me that I'd made a mistake or something.

When I left, we wrote a paper (2). It was obvious that he was still suspicious of it, so he got the student who came along and inherited all my laboratory and equipment while I'd gone off to Bell Labs—he got him to measure it, and he got precisely the same number. I felt relieved. [laughter]

We wrote a paper which far more people read and paid attention to than any of this stuff on the strictly organic things, because we essentially provided a new formula for electronegativity different from that Linus Pauling one (3). It was one of these things where you had three things that didn't change all that much, and then hydrogen fluoride was way up here because it was so extraordinarily different. You badly needed that one in order to say what shape is the curve. It's pretty hard to find it from three points that are clearly not linear.

Anyway, that was a little offshoot of the thing. I was pleased to have participated in that with Charlie.

BOHNING: Teflon was discovered in 1938, but it was more of a curiosity until wartime.

HANNAY: That's right.

BOHNING: Just knowing that Teflon had to come out of the wartime effort would have made it easier for you to acquire it.

HANNAY: Oh, it came out of that, and everybody there at Princeton in the chemistry department was involved in one way or another with the Manhattan Project. That's how he got it.

BOHNING: That's fascinating. You then switched over to the Manhattan Project. What year would that have been?

HANNAY: That's sort of murky. I gradually got involved in Manhattan Project activities while I was a graduate student, after I had really finished a thesis.

I hadn't gotten a Ph.D. yet because Hugh Taylor wouldn't give it to me. He said, "You can't come in here and expect to get a Ph.D. in that short a time period. You've got to put in your time." [laughter] He was a great believer in the Oxford-Cambridge kind of system, so I said, "I thought it was what you had learned and whether you could satisfy the examiners." Well, he didn't want this to be some place where an upstart from Swarthmore could come in and breeze through, so he wouldn't give it to me. He had to sign a release to get me to move out of Princeton.

So there we were. We were at an impasse for awhile, and I started to work at the Manhattan Project at Princeton. But as I explained to you earlier, what was going on at Bell Labs was part of the same project that Hugh Taylor was on. In fact, I have to say that the work at Princeton had less of a focus and led to less than the work at Bell Labs.

I even got myself hired. I went around and got interviewed and got hired at Bell Labs, but I couldn't get a release to move. So the fellow who had hired me, who was the Chemical Director at Bell Labs—Robert M. Burns, R.M. Burns—he laughed about this, and he got hold of

Taylor, and he said, "Well, we'll put him to work on the Manhattan Project when he comes up here."

Finally, Taylor just got sick of me, and he signed the release and I could leave. I went up to Bell Labs, still on the Manhattan Project. So that transition was made that way. [laughter]

BOHNING: That was 1944.

HANNAY: Yes.

BOHNING: As it was, you got your Ph.D. degree in two years.

HANNAY: Yes. People in those days, they spent a lot of their time working. We worked. It was seven days a week, around the clock, so there was no goofing off. [laughter]

I would say that the Swarthmore experience had certainly saved at least a year. All the other people started graduate school, took the first year of graduate courses. I started to go, I sat in on them, and then moved on.

There were no grades or anything in the Princeton graduate courses. You'd just sit in—in fact, audit the course. I sat in on them, I said, "Holy smoke, I've covered all of this way back when I was a junior at Swarthmore, so why should I sit through these lectures? I already know all this." So I moved on to the more advanced courses. I give Swarthmore credit for having exposed me to this material before I got to Princeton.

BOHNING: It was Hugh Taylor who got you into the Manhattan Project, then.

HANNAY: Yes. I think it was Charlie Smyth who suggested that I should go over and help those people. But I'm sure Hugh Taylor was, in effect, drafting graduate students. "Here's a guy who's on the loose. Let's put him to work on something that Hugh Taylor really cares about." Hugh Taylor didn't care about dipole moments then. Hugh Taylor was a very patriotic fellow, and he was pushing very hard to get the Manhattan Project—his part of it—successfully advanced. I ended up being quite good friends with Hugh Taylor, but at that time it was a kind of touchy relationship because I was a thorn in his side. [laughter] Hugh Taylor was a very good scientist, but he was a pretty hard-headed fellow—I mean, he was convinced that he was right about things. He wasn't very flexible.

[END OF TAPE, SIDE TWO]

HANNAY: He came up to Bell Labs once when I had been up there for several years. He came up, and it fell to me to show him some of the work in chemistry. He almost exploded. He said, "You people have got your head in the clouds up here! You're really doing all this basic research, you're not working on products." [laughter] Here was a university professor coming in accusing us of being too basic in our research! [laughter] Why weren't we doing something that's immediately apparent as a useful activity?

BOHNING: When you started the work on the gaseous diffusion, were you aware of the ultimate goal of the Manhattan Project?

HANNAY: Well, the graduate students talked with each other and compared notes. They were very careful; they never talked about Los Alamos, and they never talked about Oak Ridge. There was Location X, and there was Location Y. The professors would drop little hints, and so forth.

We'd put our heads together and we had a pretty good idea of what it was, but I think we were as surprised as anyone when it actually came to pass. I guess it was Hiroshima that brought it to our attention, just like everybody else. We had a pretty good inkling about it, but we didn't have any information whatsoever about what was being done at other places except the ones that I mentioned.

I didn't know that Oak Ridge and Los Alamos were involved; I didn't know anything about those places. I didn't know that there was a project in Hanford because there were different things that were being pursued. We had the gaseous diffusion project.

But then, I think there was the Y-12 project. That was essentially an electromagnetic separation using a spectrometer, if you like. That was separating out the isotopes. Nobody knew how, and here we were working on making these miles and miles of tubing that would do it. All the ideas were pretty fanciful at that time, and at least those two that I mentioned ended up as being part of the final process.

I really wasn't completely aware of it, but neither were we completely ignorant of what was going on. They certainly had a tight security; they were not mentioning lightly what we were going to do.

BOHNING: Were you cautioned against saying anything to anybody about what you were doing?

HANNAY: Probably, in a mild way. It was certainly said there was a secret project that shouldn't be talked about; but they gave us so little information, we really didn't know much of anything except what we were really working on. I guess the caution was heard and observed.

After all, we were aware of the fact that there was a war on. I had two brothers who were getting shot at. I would say that the attitude of the graduate students whom I saw, and the people working on the Manhattan Project, was one of total cooperation with the whole thing. There wasn't any looseness in the way they approached the security. There wasn't any attempt on anybody's part to say, "Is this thing what society needs?" We were in a war!

When we finished and the project was successful, I think all of us were very proud of having been part of it. I am today proud of it. The people who think it was wicked to have done that—well, they didn't know about how many hundred thousands of people would have gotten killed going into Japan. I just disagree with the protesters completely. It's misguided.

BOHNING: Yes, this recent business with the Enola Gay exhibit at the Smithsonian is a good example of that.

HANNAY: Oh gosh, yes. [laughter]

There was some of that going on at the time, but not at our low level. After all, [J. Robert] Oppenheimer and other people had great willingness to pass the information to the Russians. I haven't heard of anybody passing it to the Nazis, but they were certainly sympathetic to the Russians. Of course, it ended up that almost immediately after the end of the war, we found out where we stood with them.

My boss at Bell Labs—I wouldn't put this in the account—but when the end of the war came, he was working on that project too. In fact, he was the one who conceived the nature of the metallurgical process that produced this successful barrier. The Hiroshima bomb had been dropped, then another in Nagasaki, and then the end of the war came. My boss said, "If we had any sense now—and it will never come to pass, because the country wouldn't do it—what we should do is to go around and drop one on the Russians right now. We'll save a lot of trouble later on." [laughter]

He was saying that to startle us and to be humorous, but I often thought afterwards that at that time, for a brief time, we had admitted. . . . [laughter]

BOHNING: Yes. Did you initiate the contact with Bell Labs?

HANNAY: No, Charlie Smyth did. Charlie Smyth said, "Where are you going to go to work?" I said, "I want to go to an industrial laboratory." This was during the war, and a lot of them were not really doing research. I thought I wanted to do research, real research. We talked it over and came up with a list of three places that I was interested in. One was DuPont, and one was Shell Development in Emeryville [California], because there'd been some exchange in the good work done by some people I knew about out there, in catalysis and so forth. The third one was Bell Labs. I knew less about them.

It was really Charlie who got me straightened out on this, and I was interviewed at all three. The one at Shell gave the show away because one of the people I saw during the course of the day said, "You know, none of us are doing research here. We're just all working on engineering projects because of the war." The war still was going on.

We washed out Shell right away. I went down to DuPont and visited two or three of their divisions. They probably didn't want to make me an offer, anyway. It didn't lead anywhere. But I went up to Bell Labs, where Burns was Chemical Director. He had gone through chemistry and gotten his Ph.D. at Princeton, so this was the Princeton crowd at work.

I went up there, and I instantly knew this was the place I wanted to be. They offered me a job and I said to Burns, "I'm very interested, but is this a permanent job?"

They were really only hiring people on a temporary basis till the end of the war. I said, "I'm not interested, and I don't want it. It's a permanent job or nothing." We talked about that some more and finally, they offered me a job on a permanent basis. I think that there were only three of us hired at Bell Labs as chemists, during the war, on that basis.

The rest were laid off at the end of the war. That's that way it is if you've been in war work. I had a greater desire for security in my employment than that.

I was working on the Manhattan Project, and that came to an end at the end of war. Then I went to work on Bell Labs work after that. The other two also stayed. But most of the people who got hired during the war, left at the end of the war.

BOHNING: Well, that was 1944. You were there at least a year working on the Manhattan Project, when the war ended in 1945.

HANNAY: Yes. It was about a year, I guess.

BOHNING: To use a popular term, did you sense anything of the culture of Bell Labs at that time, or was it completely overshadowed by the war effort?

HANNAY: No, I learned something of it—certainly, a lot more than I knew about it when I applied for a job there and got hired. I discovered that there were all kinds of people around who, when you'd meet them, you didn't know—and then you discovered how bright they were. Then you found out those are some of the brightest people you'd ever meet! They were all very modest and unassuming.

I don't mean they all were, but you didn't get the impression of the intellectual power that was around the halls there until after you'd been there awhile. Then you began to realize that you came in thinking you were pretty smart because you'd just gotten a fresh Ph.D. from a prestigious university, so these people were obviously going to be impressed with you. They'd come around, "I'm not going to ask what you did," and then they'd ask me some questions about what did I think about this or that. I'd say, "Gee, I can't even think about that subject. That's way beyond me!"

One of the things I learned was something that was reinforced in later years, that Bell Labs was probably—and I think this is an honest and fair statement—the most widely admired industrial research laboratory in the world. The chemistry was its specialty, but it was a model for a lot of other companies that used to beat a path to our door trying to find out, "What's the secret of your success?" I don't mean to imply that I knew all that when I was just being interviewed.

BOHNING: When the war ended, how quickly did things change?

HANNAY: Well, we got off the Manhattan Project pretty darn fast and got onto Bell Labs' work. Nothing changed, so people were leaving war work and getting back to telephone company work. What took over then was the traditional Bell Labs approach to things. Things had been pretty well dictated by the needs of the war work during those years.

Then it came back to being things that would help the telephone company, and Bell Labs had a very long-term view of that as to what was going to help the telephone company. Their tradition was to do things which had a long-range potential for helping the telephone company. They weren't pushing you to get a result this year or make a progress report every Friday afternoon—nothing of the sort.

As the system became clearer to me over the years, and as I became an administrator of things, it was a good place to go. You had all kinds of freedom, but one of the freedoms was to hang yourself, in effect! They'd support you in all kinds of ways, but if you didn't see whether you could use those freedoms and so forth, it was sort of, "Well, it's been nice knowing you."

We quietly separated plenty of people. It was like teaching a bunch of children to

swim by throwing them all into the water. They'd either swim or they wouldn't swim. The ones who couldn't, why. I don't mean that we'd let them drown. But at Bell Labs, if they couldn't make it in that kind of environment, well. . . . They had to have the initiative and ability to produce, not in a competitive environment, but in a very cooperative environment. It was competitive in the sense that we wanted to be the best and to do well.

It was a great place to be, but the judgements were harsh—or the standards were extremely high. People said they would have been happy to work there for nothing if they didn't have to eat, because it was so much fun. Great place.

BOHNING: Ray [H.] Boundy of Dow Chemical Company described the same thing in the 1930s at Dow. "Accountable freedom" is the term he used (4). You're essentially saying the same thing.

HANNAY: Yes, I think that's right. I didn't know him; I met him. There was freedom and they were tolerant about it; but eventually, something had to come to pass. I mean, it didn't go on for five or ten years. In fact, later on I really discovered and helped create a kind of an attitude on this. If somebody wasn't doing something that looked really worthwhile in three or four years after he came, he's better able to get another job when he could still coast on his professors' recommendations.

If he'd spent eight years with us, and then another potential employer asked what happened, "Well, he failed." We didn't want to be in that position, so we'd help him find another spot.

We had very high standards on what they'd have to do, but we'd give them an awful lot of freedom—as I said, almost the freedom to hang themselves if that's what came most easily to them. [laughter]

BOHNING: [Reading] "Manhattan Project Stops Use." The next phase of your work was to look at the mechanisms of thermionic emission from the oxide cathodes.

HANNAY: The oxide cathode is the electron emitter in an electrotube—the vacuum tube as they were often called. A man named [A.] Wehnelt, obviously a German, had discovered in 1904—I may have missed it by a year—that the best coating for a thermionic emitter in one of those things was a mixture of alkaline-earth oxides. Notably barium, although some strontium and occasionally, sometimes calcium in there.

Now here we were in 1945, 1946, 1947. We presumed we were a whole lot smarter than they'd been back then. The vacuum tube had been hardly invented at the time that he discovered that. We couldn't believe that the very best thermionic emitter had been discovered in the really primitive days of the vacuum tube!

Well, as it turns out, it had been. [laughter] Semiconductors were a hot new idea in physics. Fred [Frederick] Seitz had talked about some of the defects in solids. This was a new concept for me. We learned about semiconductors and holes and electrons.

It was in a primitive state, because the semiconductors were compounds, like two-six compounds, and nobody was quite sure of them. They didn't even know about n/p-type semiconductors.

We had an idea. It was really implanted in me by my boss, but it fell out of discussions that he and very bright theoretical physicists had had who gave us some help. We worked out a program trying to find out whether these oxide cathodes weren't semiconductors, because you could have explained things about their thermionic emissions—not just from the low work function or the energy level difference to get an electron out of the interior of the solid.

I set out to do that, but these are powders. Boy, it was a messy business. I was flailing away at this, making some progress and publishing a couple of papers, but it was kind of depressing at how you couldn't make single crystals or anything of the material. I and a few other people around the world—some people in Holland, Bob [Robert L.] Sproull—were up at Cornell [University] working on it, and a few others.

The salvation came when some of our colleagues at Bell Labs invented the transistor. At that point, nobody was going to care any more about the oxide cathode. We couldn't care less! [laughter] So the bandwagon was going—that was in germanium. It was a great relief because at that point, it made my work obsolete. [laughter] I didn't have to continue it. It'd been moderately successful.

I'd have hated to spend my life trying to work on oxide cathodes. God, what a mess! Trying to measure things like Hall effect and to learn the properties of the carriers—no single crystals.

Do you know Bob Sproull?

BOHNING: I know who he is, yes.

HANNAY: Well, he was a graduate student up at Cornell at that time, and he made the first single crystals of barium oxide—maybe it was barium, maybe it was a solid solution of

barium and strontium oxide—and he got some measurements made, but it was hard work for all of us.

What happened, then, was that the first transistor was germanium, and it was not a single crystal. The great genius who had put together a group at Bell Labs of physicists at the end of the war—really, he knew that they wanted a transistor. All I knew was, there were strange electronic effects in classes of compounds—including the silicon, which was not a compound, but was a polycrystal material that had been used as a detector for radar, in World War II. He wanted to explore those. I think he had in mind that it might have an amplifier.

If you think about it, here's the telephone company, which had a million employees within a few years, and we had a network all over the whole country, and it all depended on electrical signals being amplified, and we're going to do all that with vacuum tubes. That's doing it the hard way. If you could get a solid state amplifier, it would revolutionize communications and all of electronics. So he put this group together in the end of the war, in 1945. In December of 1947, they made the first transistor.

I think it's arguably the greatest invention of the twentieth century because everything that you now think of in the way of electronics is derived from the invention of the transistor. Without the transistor, none of it would have happened—the whole computer revolution, and all the other electronic things that are changing life today in communications and so forth.

They invented it. But [William] Shockley, Bill Shockley, who was a controversial person, but a real genius—he's a theoretical physicist—he had some odd ideas about race, which got him into hot water after he went out to Stanford to be a professor. Shockley didn't think it was necessary to have single crystals. He thought they could do everything they wanted with polycrystalline germanium. The genius who had put this group to work inventing the transistor didn't see how important it would be to have single crystals!

Two or three chemists at Bell Labs, under Bob Burns, just went to work to make the first single crystals of germanium. Then it proved to be such a great thing when they finally did do it.

Gordon [K.] Teal, who ended up going to Texas Instruments, made the first one. It was in a laboratory across the hall from me at Bell Labs.

They made these single crystals. Then everybody knew as semiconductors began to develop. Shockley worked out the whole business of p/n junctions, n-type, p-type semiconductors, the doping, all of this.

That's all chemistry. It's chemistry to do it. The transistor itself and the qualities of the p/n junction were physics. But making the stuff, you had to have high-purity material; you had to have single crystals. The material had to be free of defects, free of all kinds of very small quantities of things that would kill the electronic action. That was a job for chemists.

It's just that there were more people who were chemists working on semiconductors all through the years than physicists, although the idea of the transistor had come from physics.

Everybody could see that silicon was going to be better than germanium. That's because of what's called the energy gap, there between the electronic band that's filled with electrons and the empty one. You want to promote a few. That was a bigger gap, so silicon was going to move it; it would be usable at a higher temperature than germanium. Germanium was just barely usable if the thing warmed up. Furthermore, the mobility in silicon was very high; that is, the electrons and holes move very rapidly in a field.

Silicon was obviously the material we wanted. But it was very difficult to handle, and nobody could make single crystals. They couldn't make a transistor. So I went to work masquerading as an analytical chemist and designed a mass spectrograph to analyze solids. It ended up later as a commercial instrument that was sold to people for various little things. I worked at that for a couple of years and published it, and then [I was featured in] one of your books (5). There's a picture of it in there.

That certainly didn't solve all the problems, but it gave some useful information.

There were people working around Bell Labs in research on silicon, trying to do this or that—probably a dozen or two. All the big effort was on germanium still. Then the powers that be asked me to coordinate the research on silicon. That was a concept that hadn't quite occurred to me before, which was not only to do some work on my own, but to go around and try to goad people into doing work that would reinforce the work of the others. I had a smaller group of people, very talented—two or three fellows who were going to try to grow single crystals.

That work had been started by Gordon Teal. You remember, I told you he'd done the germanium work. Then Gordon Teal, going to greater opportunity, as he saw it, meaning more importance and financial things—Gordon's a good friend of mine—he went to Texas Instruments because they wanted to lure somebody who knew this away to TI.

I was coordinating the silicon work at that point. I had this group of crystal growers who were trying to grow single crystals of silicon.

[END OF TAPE, SIDE THREE]

HANNAY: I'd have to look it up to find out what year it was, but it was probably about 1953. About 1953, we made a silicon transistor. As far as we know, it was the first one.

Well, TI had Gordon Teal down there with the materials work. They made one too. We have to call it a tie for the first silicon transistor—because we and they first reported our construction of a silicon transistor, at the same meeting, in back-to-back papers (6). It was the

IRE, which was the Institute of Radio Engineers, in Minneapolis. The TI people have sort of forgotten that it was the back-to-back papers. [laughter]

Anyway, it was. They and we, running neck and neck, made the first silicon transistor. I'm sure you know that pretty soon silicon took over the world, and still owns the world. [laughter] I think that was probably my individual technical contribution. I don't mean that I made that transistor, although I'm one of the authors on the first paper. That was largely because I was guiding the materials for it.

In terms of its importance, ultimately the project that we were on was—somebody else would have done it, but somebody has to do it first, and we and TI did it.

TI is, in the popular mind, much more likely to be thought of. The reason was that they made a transistor radio very soon. They were selling a little radio. The telephone company couldn't sell anything like that! We were going to have to put it into telephone equipment. So they had the first commercial use of it. TI had a lot of good people, and they deserved a lot of credit. It wasn't of interest to us to make a little radio. But the fundamental thing was that you got the silicon transistor.

From then on, my course was set as far as my contribution to technical work. It was the solid state work, as we called it.

Let me just say a little bit more about that. At that time, the end of World War II, I don't know how the people were divided up in physics, but a tiny fraction of them were in what we called solid state physics. They were in gaseous electronics or something. These were the people who were looking at the flight of electrons through space or through gas discharges. Then, very soon after the war, they got into nuclear physics—not yet high energy, but nuclear physics. They were all in nuclear physics—really sopped up by probably well over half of the physicists for that. There weren't any solid state physicists around.

After the invention of the transistor, probably within five years, a third of all physicists in the country were working in solid state physics. [laughter] The chemists had all kinds of other things that were on their plate, plastics and everything under the sun. When an awful lot of chemists went—and metallurgists too—into the materials aspect, it never achieved the same unique position in chemistry that solid state physics had in its larger field. I spent a lot of time in subsequent years promoting the idea that solid state chemistry was an important field because there were just as many chemists who were doing this kind of thing as there were physicists over there doing their solid state physics.

We didn't have anything. The American Chemical Society, if you sent them a paper in solid state chemistry, they didn't have any place to put it and they couldn't have cared less! They just passed the thing by completely, all of this business. They didn't recognize it as something that would be of interest. They had too many other things they wanted to do. The American Physical Society started off with that attitude, but they changed pretty fast when their

meetings began to be dominated by the advances in solid state physics. It didn't happen for the chemists.

The home for any solid state chemist was hard to find in the professional societies. My friend—R.M. Burns, my patron—he was an electrochemist. He was a big wheel in The Electrochemical Society, and he saw that electrochemistry was not growing. I mean, how many people can you put in batteries, and corrosion, and electroplating, and so forth. He was trying to figure out how to do that, and he saw this as a field that The Electrochemical Society should look on as a possibility for them—the circle of electrons, and so on.

He urged the sponsorship of The Electrochemical Society of a symposium. They were the first society and a long-time leader in providing a home for the solid state chemists. In fact, within a few years, half of The Electrochemical Society was doing solid state chemistry.

BOHNING: You've mentioned a number of things I've wanted to return to. Let's go back to the mass spectrograph for solids in the context of the Bell Lab culture. Was this an idea you conceived? You knew the analytical problem of determining very minute quantities and solids. Were you free to say, "I would like to try this?"

HANNAY: Oh, yes, yes. It was well known. It was common scuttlebutt around there that somebody had to figure out what's the matter with silicon. Why weren't we able to do anything with silicon?

My supervisor and I talked it over. A paper had been written by a couple of people at the University of Chicago during the war. They had built a mass spectrograph to analyze nuclear materials, and they had published a description of the paper in the *Journal of Scientific*

Instruments, or maybe *Journal of Applied Physics* (7). We called them and asked them about this thing, because they were determining trace impurities in solids.

They had used the thing for a couple of weeks, but they'd gotten so highly radioactive with long-life isotopes that they had to take it out in the desert and bury it. [laughter] But they'd be glad to talk to us about it. So my supervisor and I took the train out to Chicago and went to visit these guys, and they said, "Here! We'll give you diagrams of the things that we did."

We talked it all over with them, and I came back all fired up to see if I couldn't build one that would be somewhat similar to theirs, and use it on silicon. That's what triggered off the whole thing.

It wasn't immediately apparent to everybody that, "Gee, this is a brilliant idea." I know the director of research said, "Well, we're going to have to spend some money on this

thing." It wasn't a cheap thing to build. There was somebody who had some other project in mind, and the director of research called us in, the small group, four or five. He wanted to hear these two proposals about what we thought we were going to do. Shockley came in, and I proposed this mass spectrograph. Shockley said, "Well, we don't need it, because we can do everything we want with polycrystalline silicon." He was the great man, doing that. But Ralph Brown, who was the director of research, listened to him skeptically. He knew Shockley and knew that he shouldn't believe everything Shockley said.

The upshot of that meeting was that I got the green light to go ahead and start this—but it was a project that took, I don't know, certainly more than a year to build the machine. I didn't have any background in mass spectrographs. [laughter]

In fact, I went up to Wesleyan University, where there was a fellow measuring masses very precisely, and asked if I could come up and watch him with his machine and maybe play with it a little bit. It was used for precise measurements of masses of isotopes.

I went up there and I spent maybe two weeks up there, at Wesleyan University. He was very friendly and gave me all kinds of help—ended up as a good friend of mine. Harry [Henry E.] Duckworth, a Canadian.

That was the kind of preparation. These were not common machines. The only one that had ever been built for analytical purposes was the one that was buried out in the desert [laughter]. The Chicago people weren't about to do it again.

I went to work on it. In retrospect, it took a lot of temerity, a lot of ignorance on my part to plunge ahead with a project like this. There were all kinds of problems that hadn't been solved. But it worked!

In fact, there were two different models, completely different approaches to the mass spectrograph analysis—the source and how do you get the ions into the fields. I started two of them simultaneously. The other one came to grief; it wasn't working very well, and it was going to have to be redesigned. The one that ultimately worked, the one that's cited, came along—and it worked (8). I dropped the other and quietly shelved that part of the project.

BOHNING: How much reporting would you have had to do along the way in a project like this?

HANNAY: Very little. My supervisor knew certain things. There was no formal reporting. He'd ask me when he'd see me, "How's it going?" I'd go and talk over problems about it with him. I don't remember, but maybe out of curiosity, his boss might come once in a while in a friendly way, to ask how things were going—but there wasn't any reporting, as such. My relationship with my supervisor was such that it was terribly informal. I'd go to him right

away with problems that I had. He was very willing to give all the help he could, but it was more as a more experienced researcher. It wasn't any grilling or anything of the sort. It was just, he obviously felt some degree of responsibility because he'd recommended that we do this, so he had a stake in it too. A fellow named Joe [Joseph A.] Burton. He was a chemist, and a fine person, and very smart. Very modest.

BOHNING: You often had these informal discussions. Did you have any formal seminars or research group meetings where you shared your ideas or brought your colleagues up to date as to what was going on?

HANNAY: No.

BOHNING: Okay.

HANNAY: We really didn't. It was a very informal place. There was very little requirement that things be reported up the line periodically or on some sort of a schedule, or anything. Obviously, word filtered up. I'm sure that the chemical director would ask, or it would get asked, "What's happened to Bruce Hannay? Is he doing anything? How's he getting along?" They'd say, "Oh, he's all right. It's slow going, but it's definite." I'm sure he was aware of what was doing, but it wasn't because he came around or I had to march up and give up a report.

It was a very supportive environment—but again, as I say, you could have enjoyed yourself right up to the point where you failed totally. [laughter] I can only say that the people whom I saw in my line of supervision—I would have had a hard time finding people that I had more respect for. It was a very comfortable group of people, but boy, they really encouraged you to work very hard at success. I'm sure that that system was getting the most out of people. It wasn't scary or anything, but you could figure out that the challenge, it's up to you. A great bunch of people.

BOHNING: Let me ask you a more general question in this sense. Did you ever have a case in which you worked on an idea that you eventually knew would not succeed? How did you handle it?

HANNAY: Oh well, of course in a project like that mass spectrograph, yes. Things that weren't going to succeed, I didn't have any worry about dropping them. I had mentioned, I'd started two designs at once, and it was getting them made. I told my supervisor; I said, "I have this other thing; it isn't working." He'd give me some advice about that and we talked about it, but I'd say, "I'm going to push this other one." If I could get one out of two, that's better than zero out

of two! [laughter] It wouldn't have made sense to go on with that. I never had any feeling that—at least with him, and he was obviously the primary management contact.

I have to say that for the first three or four years, I probably didn't know the name of the president of the Bell Labs, or maybe the vice president of research. There wasn't some mighty figure who was towering over you. I didn't have any hesitation about saying where I was in trouble.

I might tell a little story.

Later on, when I was in the job that R.M. Burns had, chemical director, I went off and visited some European chemical companies, including the Germans' three pieces of I. G. Farben—the three principal ones. One of them—I wouldn't quote it by name, but it was Otto Bayer, who was the director of research at Bayer.

We had good relations with these companies all over the world, because we were not competitors and we were cross-licensees. I was going to spend the day seeing what they were doing at Bayer in the way of research that related to the solid state interests that we had.

They had this great edifice, about a twelve-story new building that was their research laboratory. He had the top floor, and it was the biggest office I'd ever seen—you could hardly see across it! [laughter] Not only that, he was a big man and he had a big cigar, which was the biggest cigar I'd ever seen! We talked for a little while, and he was interested in me. Bell Labs didn't mean a lot to him, because they were a chemical company.

I asked him about the kind of people they hired. "Well, we hire only Ph.D.s in chemistry." Well, we only hired Ph.D.s, too. [laughter] He said they had to have two years of post-doctoral work and then they'd hire them. I said, "What do you do when they come and report for work? Do they just work on a project that they pick out?"

"Oh no! I wouldn't let them do that! I tell them what to do!" I thought, "Boy, that's a different system!" [laughter]

BOHNING: That's good.

That leads me then to the question of the silicone business.

HANNAY: No. It's silicon.

BOHNING: Silicon. Excuse me.

HANNAY: That's a common mistake—silicone, one syllable, silicon. The average person who hasn't been in the semiconductor business hears a lot more about silicone than about silicon.
[laughter]

BOHNING: I've done interviews with Frank [J. Franklin] Hyde and Earl [L.] Warrick who were silicone pioneers, so I have to watch my terminology (9). [laughter]

When you were asked to coordinate the development of silicon as a material, was that something new?

HANNAY: By this time, Shockley was on board and said, "We need single crystals of silicon." Shockley thought that the way to do it was to hire a mechanical engineer, and he hired him. He told him to design a piece of equipment that would grow single crystals of silicon. This guy had under construction a very expensive crystal-growing machine that was doomed to failure. Couldn't possibly work. That was because Shockley didn't know anything about materials, and this guy was the wrong guy to be doing it. He was just designing things.

In the following sense, here are a couple of physicists over here and they're measuring lifetime. You inject carriers, electrons, or holes into a semiconductor; how long did it last before they recombine? That's crucial to the utility of the material. The thing that causes them

to recombine is usually an impurity in there, and it may be at a concentration of 10^{-7} or 10^{-8} . We called those, that element, deathnium, because we didn't know what it was.

As an example of the kind of trouble you got into, the first crystal-growing machines for germanium had lots of copper in them. Copper turns out to be the number one deathnium in silicon. You can't stand a trace of any copper in there, and you have to design it completely differently.

Furthermore, the crucibles that were used for melting the germanium-graphite crucibles—you try to melt silicon in them, it's five hundred degrees hotter. Set five hundred degrees—fourteen hundred and something instead of nine hundred and something—and it'll just dissolve that whole crucible. We were using silica quartz crucibles, but we had to unravel these things. What's the stuff in here that's causing the thing not to work? The carrier's kind of full. How do we dope the thing; how do we handle the thing? How do we make crystals that are not filled with lattice defects? Just a whole maze of problems.

Well, we were working in materials, and other people were measuring to get the

best crystal or the best solid sample that they could. They wanted to study the behavior of carriers in there and how long did they last. Other people were trying other physics experiments, but they're working with material which was poor. It's under poor control because it hadn't been purified. We didn't understand the role of which impurities were in there.

My job of coordinating was to go around and try to encourage the people who seemed to have a clue as to what to do—to help them by offering, for example, to make samples for them and work with them. Some people were trying to make a transistor out of it, but it was hopeless at that time.

There were all kinds of things. One of the most troublesome things was, if you even heated the silicon, it changed type. There were all kinds of changes in the electrical behavior of it, and even the type of semiconductor it was, which came about just from heating it! Well. It was a very mysterious thing, and there were a couple dozen people working on this and that phase of silicon around, but all with a material that didn't behave properly.

I was dealing with the DuPont company because we got our raw silicon from them. They had made it for radar detectors in World War II. We offered to buy five hundred pounds of it if they'd put the thing on the road again. That was the purest silicon we could find, but it was obviously highly contaminated.

I'm just trying to give some idea of what the coordination amounted to, was to try to do what I could. I wasn't in a position of authority, but I was only allowed to go around and try to persuade people, often who had more seniority than I did, "Why don't you do this or that? Can I do anything to help?" That sort of thing.

The belief was that this was an unconnected effort on the part of various people. So they put this young guy—me—to work, seeing if he couldn't help to bring some order out of this effort. The sort of central thing was, I had these crystal growers reporting to me, and I hired a couple of other people.

Anyway, we worked away at it. By 1953 or so, we were through the proof of feasibility for the transistor. From there on, everybody knew that silicon was going to be the thing. So we were off and running—not because we had solved all the problems, but at least we knew that the goal was obtainable and that silicon was going to be the material of the future. Within several years, germanium was really phased out and silicon has remained, since then, the material for all integrated circuits.

You know what an integrated circuit is. It's just a transistor with a whole collection, maybe a million transistors in it, and a million diodes, or something like that. [laughter] It's just an array of them on a little chip, as they call it now. It's a slice of a single crystal of silicon.

BOHNING: There's still a piece of ENIAC sitting at the electrical engineering laboratory at Penn [University of Pennsylvania].

HANNAY: Yes.

BOHNING: I've seen it, and it's just an amazing thing. [laughter]

HANNAY: I've seen pictures of it.

[END OF TAPE, SIDE FOUR]

HANNAY: George Stibitz had built the first electrical computer—that was before the ENIAC. He made it with telephone switches—a whole bunch of electromechanical switches in an array of them. This would close, that would close, and so forth. Oh, it was really a crude thing! It could add a few numbers, and that was about all it could do. [laughter]

BOHNING: You had mentioned that many of the people you were dealing with had more seniority than you did. You were still quite young at this point.

HANNAY: Yes.

BOHNING: You may have been in your late twenties.

HANNAY: Oh, no. Well, let's see. We were doing that in 1953. I was getting on toward thirty, but I was born in 1921, so the bulk of this work was in 1953. Then in 1954, and so forth. We, I think, published our first paper and some other things that came out of our work—a group of papers (10). They were probably published in late 1953 or early 1954.

BOHNING: All right. What I wanted to ask, then, was this. Was there any resentment to your being the coordinator?

HANNAY: Oh, no. I didn't have any authority; I couldn't reward them or punish

them. About all I could do was go around and urge them to do something, and they could tell me to go fly a kite if they wanted. No, we were a very cooperative bunch. They would badger me, "Why can't you make some crystals that are any good? The stuff that you're giving us is lousy!" [laughter] It was a give-and-take kind of thing. What had been seen was, "Is this effort to produce the materials feeding into reasonable experiments?" and to exercise some judgment on that.

No, I didn't see any resentment, maybe because I was too innocent to notice. But I'd say the level of cooperation and a friendly, cooperative relationship were notable—very notable! We were all in it together, and we wanted to be first. [laughter]

BOHNING: I would think that in a situation or in an environment with all this flexibility and freedom, there might be a tendency to carve out one's own turf.

HANNAY: Some of them would do that, but no turf was assigned irrevocably to anybody. If somebody else wanted to go in and said, "I can think of a better way of doing it," they could go in, so you might find that there's an internal competition. Not necessarily doing it exactly your way or just saying they had more dexterity than you have and would do the experiment right, but because, if they have a different idea, let them try it, and let the better idea win.

Bell Labs always took the attitude, "If we know you're working in the field, that doesn't give you an exclusive right to work. It gives you a right to work in that field. If you say you're going to want to do it, fine! We'll let you do it. But you don't own the field." You couldn't exclude other people from it.

We would exercise a reasonable amount of control. Supposing other people said, "I want to go in; I think I work more hours and I'm smarter than so-and-so." Sometimes, that situation would hold and we'd encourage somebody else to come in and give some competition.

I wasn't the one making such decisions. I might report that to my boss. I was reporting to them all the time, who's accomplishing something and who wasn't. "We're failing to do something or other, because it's too hard for these guys who are not—we're undermanned in an area."

It wasn't a team activity in the sense there was a czar over it all, because we did have the example of germanium. Everything that we knew about germanium, we wanted to know that about silicon. A lot of the concepts had been found in germanium. But I never had had anything to do with germanium. My job was to start with the silicon. Elementary physical principles told us that silicon was going to be better than germanium, and when it came along, it swept germanium out to the pastures.

BOHNING: As I understand it, one of the problems with the silicon was the dissolved oxygen.

HANNAY: That was the heat treatment, oh yes. That drove me out of my mind! Everybody else, we couldn't understand. You'd take a crystal of silicon and you'd want to heat it for something, maybe to diffuse an impurity and make a layer, and the thing would change.

The first transistor we did, because we found this heat treatment effect. We would dope the thing. Then we'd heat it and cook it, and it formed a p/n junction, a thin layer there. We made a transistor out of it by using this mysterious effect.

At that point, I had arrived at the point where I was supervisor of a small group. I hired a fellow named Wolfgang Kaiser, who was a German physicist with a very thick German accent. He had come to this country—a lot of Germans came then, because German institutions were pretty well wiped out—and he was working at the Signal Corps laboratory [now the U.S. Army Research Laboratory] in Fort Monmouth, New Jersey. That wasn't a very inspiring place to be. It was all right. It was a good laboratory, but.

Signal Corps had brought him over. He did spectroscopy and he was interested in silicon. He was doing some spectroscopy on silicons. Well, I hired him. We asked him if he'd want to be interviewed, and he said yes he did, and I hired him.

He found that the silicon we had was filled with oxygen. Why? [laughter] We were melting the stuff in a silica crucible! The silicon would etch enough of the oxygen out of the silica, and it was doping it with oxygen, and the oxygen was electrically active in the thing. We had to get it away from that.

Holy smokes, that really made a difference, because he cracked that problem! Wolfgang Kaiser, a good friend of mine—a good friend of mine today. He did it by finding the frequencies where silicon oxygen bonds, which were clearly identifiable for a spectroscopist, and he said, "This is filled with these bending frequencies." He found them all.

Then I took one of the best of the crystal growers, and we talked about it. Another German had done something that was called a "floating zone" method, and that was to grow a crystal without a crucible. If you take a rod of the stuff and put a coil around it, and move it slowly through that, you produce a molten zone. The surface tension kept it from running down the sides. You pass it through, the whole thing, slowly. In a floating zone you can see that the molten zone, between the two solid ends which are clamped at the ends, moves through the thing.

We started to grow because now it's not in contact with silica. When we did that, we set that up—cobbled together a floating zone apparatus—and we immediately got rid of all these heat treatment effects. These things were—oh, less than a half an inch in diameter—they look pretty ratty things—and they may be this long, and maybe a good section, like that.

It looks like a billy club now. It may be this diameter, and it may be five feet long. It's been handled that way—it looks as though it's been turned out on a machine. It's a single crystal of silicon with precisely controlled properties. That's the way. You don't bring the molten silicon in contact with anything except the vacuum. [laughter] Pretty soon, it won't take long to do it.

Working at Bell Labs had a unique advantage over any other research laboratory, really, and that was the key to some of our success. We were a monopoly. As you remember, the Bell System, being a monopoly, nobody else could go into the telephone business. I mean, we carefully encouraged the little companies and helped them, which meant that if we made an invention like the transistor, we would immediately license it to all comers. If you stop and think about it, I told you we couldn't make a transistor radio, and they couldn't make telephone equipment. By telling them freely what we did, letting them come visit us, look through the lab, and so forth. . . .

In fact, we had a symposium. Any company could come to that. We filled the auditorium with it. It was on the transistor and explained all the stuff that we knew and had learned. It was right in the first days of the transistor (11).

We got cross-licenses off of it, because the fear, if you're a monopoly, is that somebody else could hold us up. We couldn't hold anybody else up, because the Justice Department wouldn't let us, but we could license them. But if they got something, they could have held us up if it was something we needed. Therefore, we'd get cross-licenses. When I was later on vice president of research and patents, we had licenses around the world with maybe eight hundred, nine hundred, one thousand companies—every company that you could think of!

That didn't mean it was an even trade always. Usually they'd paid us some monetary sum in there. But it wasn't a money-making proposition; it just paid for our licensing operations and the lawyers.

That meant I could go into any laboratory of any of those companies around the world. All the Japanese laboratories—I could go into Hitachi and NEC and so forth. I could go into the German laboratories. I could go into the British laboratories. Wherever there was work, I asked to pay them a visit. "Hell, sure!" Because they wanted to visit us.

That was, in terms of information flow, the work of the world. Supposing somebody made an improvement on transistors. We're licensed on it. We only benefited from having somebody else capitalize on our invention because they're going to use it for some commercial purpose that we were excluded from. We could bring it back to us, and we could go and visit. That was a tremendous advantage.

When I said I've talked to Otto Bayer, I meant we could go to anyplace. They were licensed. They were licensees of ours. Everybody was. [laughter]

That was one thing. The other thing I was going to mention was, I think it was in 1954. I became the supervisor—or what we called the subdepartment [head]; it's called the department head now—of a group of maybe a dozen or so Ph.D.s in physical chemistry, or maybe physics, a couple of them. Some of them, two or three of them. Really, as early as that time—and it flowed out of my experiences in this coordination—I decided that I really enjoyed the management side of things. Not that as coordinator I'd been a manager, but I could see my spending my life more in a supervisory role where I could get my hands in lots of things, than in holing up in a laboratory.

This was partly because I thought that I would be better at it. Looking back on it, I think there's no question about it. I could do that more successfully than I could have been able to expect as an individual scientist. There aren't many of them. I mean, I would have loved to have been a spectacularly successful individual scientist, but I just knew I was better at the other and I enjoyed it more.

It was about that stage that I made the choice to follow the management route. I went from that up the pyramid over the years.

BOHNING: That was one of my questions (12). I'm glad you included that.

HANNAY: In fact, I got promoted every six years, it turns out, generally. On February 1, 1961, it probably would have been, I got made chemical director. On February 1, 1967, I became executive director of all the materials work, which was metallurgy and a whole bunch of things. That was 1967. Maybe it was 1972 or 1973, I became vice president of research and patents. So it was every six years. [laughter]

BOHNING: The transistor had been announced in 1953. In the late 1950s, before you really got into the more serious research management, was this the time when the gallium arsenide work started?

HANNAY: Yes. The gallium arsenide work got started when I had this group of a dozen or so people. The early semiconductors were known, were two-six compounds; zinc sulfide and things that were phosphors, the luminescent material. Then along had come germanium. That triggered off a smart German, named [H.] Welker, who was at Siemens, and he had suggested the three-five compounds. They weren't easy to make, but the easiest ones to make were the ones with the narrowest energy gaps—excuse me, indium antimonide.

Welker published a paper (13). He had some crude things; he didn't have single crystals, and he didn't have particularly good measurements. But it certainly laid out the

whole thing, and he deserved the credit, because it was Welker who thought of the three-fives. They had the same crystal structure as the silicon, if you just alternate all the atoms—I mean put the atoms in their relationship to each other. Never mind the identity of the particular one; they were laid out the same way. Zinc blende lattice—same as the diamond.

Well, Welker had gone on and done something else, and people had tried a little bit of this, but nobody was really working on it. So when I had this group, I said, "We ought to get to work on the best of the prospect of these." Now I'd studied the thing at some length as to which it would be, and I came to the conclusion that gallium arsenide was the most promising of the three-five compounds.

Who was going to do this thing? Well, there was a fellow in metallurgy who had been working on the floating zone silicon, and I'd worked with him on that. He was very unhappy in there for other reasons. The people he was working with, it just wasn't a happy relationship.

I cautiously inquired about his interest in coming over to my group and explained what I wanted. He was experienced at growing silicon crystals. He showed great interest in doing that, and so we did things properly through channels and so forth. The upshot of it was that he transferred over to my group with the explicit purpose of working on the gallium arsenide, which I had asked him to do. He set to work to do it with floating zone and so forth, and eventually, he was making gallium arsenide crystals.

As far as we knew at the time, we were the only people in the United States, or maybe even the world, who were working in gallium arsenide. It turned out we were the only ones in the United States working on the three-five at that point. The other things like indium antimonide, that were maybe useful, maybe not useful, as detectors for infrared—nobody cared about that.

We wanted to make transistors. Remember, the laser hadn't yet been invented. It turned out that, after we were well into this, had made single crystals of gallium arsenide, had made some observation about it—nobody had heard it claimed we could make transistors out of it yet, but we were trying to make the transistor. We discovered that there was a fellow in England, Cyril Hilsum, who is at the SERL which is Service Electronic Research Laboratory. It's a government laboratory. He had been thinking very much the same way as we had. Independently, we had both gone to work on gallium arsenide and we were at about the same place.

We visited back and forth, and he came—he was a fine fellow—so we can't claim that we were unique. But the Germans weren't working on it anymore; only that British laboratory and my group were working on gallium arsenide. We were going to make a gallium arsenide transistor.

We were struggling along. We got diodes made and so on. Meanwhile, a Bell Labs alumnus and a fellow at Bell Labs came up with the laser. To our eternal embarrassment,

somebody else found some things about the energy band structure of semiconductors that told us why you couldn't make a laser out of silicon, but you could out of a three-five compound.

So we weren't the first to make a laser out of gallium arsenide. There were three other laboratories that did it simultaneously; the Lincoln lab [in Lexington, Massachusetts] and GE, and somebody else, maybe it was IBM. That was a bad thing, but you can't win them all.

But we had the gallium arsenide! We made it. So then, gallium arsenide took off like a rocket because it was the preferred material for lasers. We'd made gallium arsenide in the first place thinking it was going to take over transistors, just as silicon displaced germanium. Here we had this new use—the laser—which was the excitement. I'd have to look up the year, but it was in the early 1960s.

Later on, we did in fact make gallium arsenide transistors. They didn't sweep silicon away though, because silicon got better and better, and gallium arsenide got better and better. Silicon was always moving along; it was getting to be a better material, purer.

It turned out that you do just about everything you wanted to do with silicon. Gallium arsenide, which would work at a higher frequency than silicon—there weren't enough uses to make it a big competitor for silicon, and it was obviously inherently a more difficult material to work with. Why work with that when you can achieve what you want with silicon? You only use it where you have to use it.

I've explained why, for a laser, silicon doesn't work, even. But for the transistor uses, silicon remained the king. Gallium arsenide was good, but that's how we got into the gallium arsenide business. I had the right idea on it, but the transistor came along only belatedly and I don't claim that I invented the concept of gallium arsenide. It was the German, Welker. They had dropped it, and everybody else had dropped it, and only the fellow in England and my colleague and I stuck with it.

BOHNING: Were there any other things that you were working on in the late 1950s before you became chemical director, or was the gallium arsenide your principal project?

HANNAY: Well, I was very interested in pushing the concept of solid state chemistry as a subject-defects in solids. I went around giving lectures on this subject and on lecture tour for the American Chemical Society. Well, I was a participant in it, but some of the people in that group that I had did some remarkable things. They could show that you could take electrons and holes and apply all the mass action laws that were known back then to chemists—where you had hydrogen ions, and hydroxide ions, and all the equations. Everything worked, all your old electrolyte things.

A semiconductor is nothing but that same thing. It's the electrons and the holes that

are the counterparts of those—mass action, everything applies. So it turns out that a chemist understands a great deal of the things that are going on in the solid, but it certainly wasn't apparent at first.

Two or three of my colleagues pushed this and we promoted the whole idea, because it was obviously chemistry that we were doing.

BOHNING: The first chapter of the book which you edited on semiconductors describes that (14).

HANNAY: Yes, that's right. It's in that.

BOHNING: I was looking at that and saying that applying mass action to semiconductors was incredible. [laughter]

HANNAY: The physicists knew that they could multiply the hole and electron concentrations together. They ended up with a constant, but it never occurred to them; they'd never heard of mass action. The chemist who was a theoretical chemist, who was primarily the one who triggered all this off, was a fellow named Harold Reiss. Last I heard, he was still a professor at UCLA. He's a very good friend of mine.

Gee, it was fun. [laughter] We had a marvelous time with this. My job in there where I was a supervisor of this group was not to tell them what to do, but to encourage them. They could bounce ideas off of me and I'd make suggestions, and so forth. I had my satisfactions out of having this whole thing flourish. It was really a red-hot group. We were publishing and people were winning prizes. One of the people who was in that, he made the first silicon solar cell.

BOHNING: I see.

HANNAY: That was in my group. That was Cal [Calvin S.] Fuller.

BOHNING: Yes, that was Cal Fuller.

HANNAY: Well, you know Cal Fuller.

BOHNING: Yes, I do.

HANNAY: Cal Fuller had been a polymer chemist. Then he switched over to semiconductors. He'd had some unhappiness in the polymer business and just dropped polymers entirely.

In any case he came over, and he was in this group. He was the guy who put together the first silicon solar cell.

BOHNING: Yes, he won a big award for that.

HANNAY: Oh yes, they won a prize. We had people winning prizes all over the place. Oh, it was an exciting time. I must say that, those years with that bunch of people—they were really a lot of fun. We had a marvelous time.

BOHNING: How did things change when you became chemical director?

HANNAY: Well, I had more administrative responsibility. I guess the chemical research organization probably had two hundred fifty people in it. It involved other things too—there would be a couple of polymer groups and so forth, because solid state chemistry was not in now.

I was moved over. I hadn't been in the chemical research; I had been in what we called chemical physics research, a smaller organization. I had to learn a lot of things but, you know, they weren't hard to learn. There was a group on electrochemistry, and there was an analytical group, and there were two or three polymer groups, and so on. I learned a whole lot of things. I learned not to call adhesive "glue." [laughter]

There were a lot of practical things. The chemical research organization had an engineering function as well as a research function. All of the materials that went into the Bell System—equipment manufactured by the Western Electric Company, which was one of the largest manufacturing companies in the country—those materials were specified by Bell Labs and by that chemical research thing. We would write the specifications and we would talk to Union Carbide [Corporation] or somebody about their polymers and what's the matter with their polyethylene, why we couldn't use that. I got to know a lot of that. I didn't find it difficult because after all, it was chemistry, but it was just a different area of chemistry.

[END OF TAPE, SIDE FIVE]

HANNAY: However, it was a big enough group so that I couldn't expect to have a close watch on everything. There were just too many different people doing too many different things. Then when I moved to the next level—which was called executive director, one step below vice president—it was not only the chemical research, but there was the metallurgical research and all the materials work, ceramics and everything else. So as the organization got bigger and bigger, I knew less and less about more and more different things. [laughter]

When I became vice president of research, that really was the case. I hadn't had computer science, and I hadn't had systems engineering and mathematics and behavioral sciences and some economics research, and then a whole lot of things. [laughter]

BOHNING: I've always been curious about this. As a scientist yourself, when you get to that level and you get further and further removed from really understanding the nitty gritty of what's going on, how do you feel about that?

HANNAY: Well, I told you, I had already expressed a preference for the management. It's not because I wanted to wield power or something. But I'd go over to the cafeteria and, by choice, not eat where the people who were, let's say, also the vice presidents of something or other would eat. I'd rather go out in the cafeteria and look around and see a familiar face of somebody in there, and sit down and ask them, "What are you up to?" Talk to them. I got a big bang out of talking in a very informal way with people. They, I think, recognized that I was genuinely interested in what they were doing. They could say anything too. They could complain, or badger me about anything, or tell me what they were doing. I could feel a connection with what the place was all about. But I was not in the executive dining room; I was in the cafeteria and looking for anybody who looked familiar.

I learned a whole lot from them. It was a good way to get information about what's going on. I enjoyed that too, because it did give me contact with so many different things.

So much of it was new to me. The computer science, for example. Operations research and all these things. Shoot, I was learning all about the things that were the wave of the future. At this point, I wasn't locked in as a chemist.

I'd say though that as a chemist—maybe I could contrast this to the mathematician and the physicist to their disadvantage—a chemist is used to messy and untidy situations, complexity. A physicist wants to reduce things to a simple system which he can solve exactly. [laughter] A mathematician has a mathematical purity about his thinking. But a chemist is much more ready to accept complexity and somewhat untidy solutions to things. They may be the best you can do, and you try to work your way up from that.

So I felt that a chemist had something to bring to the broad range of activities that we

had. As a matter of fact, my predecessor as vice president of research was a chemist. He was very well known, Bill [William O.] Baker.

BOHNING: Oh, sure.

HANNAY: Bill Baker had been vice president of research for eighteen years, and then I on top of that. So for something like twenty-seven, twenty-eight years, they had a chemist from Princeton in charge—first Bill Baker, and then myself. [laughter]

There were occasional complaints about the chemists having taken over in a laboratory that was not a chemical company. [laughter] I never felt that I was handicapped in this, but it meant that I was continually challenged to learn new fields and to learn what they were about. Some of the things that I was most interested in when I was vice president of research might have been dull, raw material.

For example, the optical fiber communication systems. That was probably, when I was vice president, the thing I pushed hardest which came to fruition. I was proud of it, because I was really ready to pour any resources into that. I hate to think how much money we spent on it. Nobody ever asked me to account for it. We poured money into it because I was absolutely convinced that it was going to be the whole system of the future.

The electrical engineers, systems engineers who were working on it, and I had set up a group effort on the optical fibers to purify those. We had gotten a coating company in and talked them into putting in an effort.

It was, you can see, the fibers. That wasn't all that far removed from my previous experience with silicon, so the materials were coming in. But I had to know, why are we doing all of this? It was very simple, but it seemed to me perfectly obvious that optical fibers would take over, and it's come out just that way.

I'm sure that in the research budget I must have spent a hundred million dollars on the optical fiber effort. Shoot, they got it all back in the first year! [laughter] That was one of the things about working for a company that had worked with big numbers.

BOHNING: As you moved up the management level, did you still have this flexibility and freedom that we talked about earlier?

HANNAY: Yes, I really did. The system there—I really had freedom.

I mean, people up the ladder were more aware of what was happening. We had certain

responsibilities we had to discharge. For example, when I was chemical director, and we were responsible for the specifications for the polyethylene that was going to be used in insulation, I had to make sure that we did the job and did it right. That wasn't just because we chose to do it; it was because somebody had to do it and we had that responsibility.

We had a number of assigned responsibilities that were recognized. We specified the materials that the Western Electric Company bought. They might negotiate with several chemical companies, but it was to our specifications.

When I was vice president of research, for example, I decided that the computer sciences were the one thing we really had to start pushing. Even when they put on a hiring freeze, I told the computer scientists, "You hire anybody who's really good that comes along, and I'll find some way or other to cover it." [laughter] It was a big enough organization so nobody knew exactly what we were doing.

There was still plenty of freedom to do things. But you couldn't take the whole organization and say, "Well, we don't need any more metallurgists, just let's make them all computer scientists." If you're only hiring at a modest rate, it takes years to change the emphasis of the organization—but I could push it in certain directions by controlling the hiring.

I'd get complaints from the people whom I didn't give slots to, but that's the way it would work. It was my judgment that really was a reflection of the whole environment of what the Bell Labs engineering organizations were doing.

I'm sure you know that it's an R&D organization, Bell Labs—it was. Research—I had close to fifteen hundred people and most of the scientific staff were Ph.D.s, but it was only fifteen percent of Bell Labs. The rest of it was all development. They were designing and specifying the equipment that Western Electric would manufacture and deliver to the telephone companies.

The only people who had the freedoms that we had were ourselves. We had, not total freedom, but pretty close to it. Compared to what I saw in other industrial research laboratories, we were in clover. [laughter]

But again, it was the same thing as I said earlier. They gave you the freedom, and you could produce. The attitude of the AT&T Company and the Bell Labs management was, "Okay, you had a great year this year, so we'll give you all the money that you had for this before. See if you can do it again next year." What you really needed to do was keep producing, year after year, great new things. As long as you're producing, fine.

BOHNING: You took an early retirement in 1982, I think.

HANNAY: Yes. Actually it was 1981; it's the end of 1981 I went out of the job. The rest of the time I was cleaning out my desk or having retirement parties or something. I left actually in early 1982. There was accumulated vacation and so forth.

I'd always thought that I might have burned myself out at that point. I was very close to the antitrust action because I had the patent organization. I had eighty-some lawyers in my patent organization.

They were patent attorneys. It turned out that, for the various legal actions, antitrust and so forth, many of the questions about things that we were doing and the complaints about us revolved around rather esoteric technical questions. Most lawyers didn't know anything about these things. You could take a patent attorney—well, he couldn't go to court and argue antitrust. He was the guy who could explain the technical reasons why you did certain things.

We were heavily involved. In the last couple years there, I spent an awful lot of my time on antitrust with the patent attorneys whom I had assigned to this thing.

Then the consent decree came along, and [the] Bell System was going to be broken up on January 1, 1984. I said, "I'm not going to be here then, because I've worked too hard to build"—to maintain, I didn't start from scratch—but let's say, "to build on the ideas of my predecessors, and to do everything to insure the quality of this organization."

I said, "There's no way that it isn't going to be broken up in time, it's going to be only an empty shell of its former self. I'm not going to be here." I decided then and there, when the consent decree was announced, that I was going to retire early, and I did, a couple years before then.

When you've been deciding how you're going to build this research organization—and the development people may have been less frightened by this, because they're still going to be developing equipment and so forth, and they might have had more pressures on schedules or to listen to the salesmen. But our freedoms, as we'd had them—shoot, they weren't going to survive in the long run. They were going to have to be scaled back some.

So I said, "Well, I only live once, I'm going to retire early." So I did retire early. Found myself for some years working harder. [laughter]

The first two or three years, I didn't think I was going to consult or anything. But I had a lot of friends, partly because of this freedom of interaction with other organizations and other companies, partly because I'd been active in the Industrial Research Institute—I'd been president of it, the IRI. Various people I knew were typically the vice president of research at other companies.

They approached me. I hadn't gone around looking for any consulting, but I started getting consulting, and then I was invited to join boards, and so forth. The first three or

four years, I remember, I counted up one time, and my wife said, "You're getting an awful lot of traveling." I looked at my calendar, and I had made thirty-six round trips from the West Coast to the East Coast in the course of one calendar year.

I couldn't imagine what would take this much time, but it seems to me that you're clever at keeping me talking. I've been spouting off about a whole lot of things.

BOHNING: Oh, it's been great.

HANNAY: I don't know whether we've even begun to cover all the topics that you might have wanted to ask.

I consider I've been very fortunate. I've worked at a great place. Now I look back on it, I can't imagine having worked at any institution that I had a greater affection for than Bell Labs. It was a great place to be—exciting people. They were smart, and they were doing exciting things.

I enjoyed the management thing. It wasn't that I disliked science, far from it—but they were paying me to do the managerial work, which I thought was something I could do better than just being an individual scientist in the lab. But I had my contacts with all these scientists.

Then, after I retired from Bell Labs—and I left with only the warmest of feelings, but I didn't have a warm feeling toward the U.S. government! [laughter] I still don't. Then I developed all kinds of interesting contacts with other companies. Before I knew it, I was working with the vice president of research at ARCO [Chemical], and at Eastman Kodak, and I was on the board of Rohm & Haas Company, and a whole lot of things.

BOHNING: I understand you and Howie [Howard E.] Simmons knew each other.

HANNAY: Yes, I knew Howie Simmons.

BOHNING: Did you become acquainted during this post-retirement period?

HANNAY: No, I'd known him before. I was never a consultant for him, but I just happened to know him. A lot of them I knew from the Industrial Research Institute.

In the chemical industry, the place that I had the most contacts with was the Rohm & Haas Company. I joined their board before I retired.

Then they asked me to be a consultant, in addition. I helped lure Bob [Robert E.] Naylor away from DuPont and hired him as vice president of research. He was a terrific success, and also Vince [Vincent] Gregory. You may know these people.

BOHNING: I met Vince Gregory.

HANNAY: Vince was the CEO. He was a very forward-looking fellow, and he said, "We've got to worry about environmental things—health, safety, and environment. The board should do this." I was the only outsider who was a chemist by profession, so I was the chairman of the board committee on Health, Safety, and Environment, and I got some others in. I brought in a couple people from outside, Gil [Gilbert S.] Omenn from the University of Washington, Dan [Daniel] Okun from [the University of] North Carolina on water quality and contamination.

I spent an awful lot of time with the Rohm & Haas people, and I got to know them very well and had great admiration for them. I was a regular commuter to Philadelphia on one or another of these board meetings—maybe there were nine or ten a year. We'd have to meet all day. We couldn't just do it in an hour in the morning. Consulting with Bob Naylor I'd go out, and he and I would review the work of all of his research people. This was just fun. Pure fun.

They paid me to do it! [laughter] But there weren't conflicts—I mean, there was not any overlap of interests.

Just to take an example, the things I did for Jack Thomas at Eastman Kodak, they weren't really different. I often got to know the CEOs of these companies. I was consulting with Jack Thomas.

[Later,] Walter A. Fallon was the CEO and Colby [H.] Chandler was the president. They said, "We want you to come and consult with us." Their problem was, "How are we going to move from an era in which film is things like razor blades and razors and Gillette—film and cameras?" They were giving away cameras to sell film. They said, "When it becomes electronic, and people start storing the images on tape, what are we going to do at Eastman Kodak? It's going to be hard on the film business." They wanted consulting activity, with my electronics background.

There were different areas to focus on. It was all fun; I learned so many different things from so many different people.

Rohm & Haas and Atlantic Richfield were the two that I spent the most time with. Atlantic Richfield had been created by Bob [Robert O.] Anderson. He just merged other companies. He first got the Atlantic Refining and Richfield Oil Companies. Later on he brought in the Sinclair Oil Company, and they were all merged.

He was a nut on technology, so he was buying biotechnology companies and solar cells and everything. When I was asked to chair an outside group that would advise them on science and technology, I think they had seventeen different laboratories. They wanted to rationalize this. [laughter]

So I got to know the petroleum business. I had some geologists and other kinds of people in this committee, a very distinguished group of people.

I'm trying to convey a sense of having worked a lot of hours in my retirement years, but having a lot of fun. To me, it was just a pleasure.

BOHNING: You've also done some economics work with Ralph Landau.

HANNAY: Oh yes. In fact, I just got a letter from Ralph; he was asking me why I had disappeared. [laughter]

See, I was a member of the National Academy of Sciences and the National Academy of Engineering, and he was in the National Academy of Engineering. I had agreed. Cort [Courtland D.] Perkins, who was a nautical engineer, a professor at Princeton, was the president of it. I wasn't an engineer, but I sort of regarded myself as an applied scientist. Cort said, "How'd you like to be the foreign secretary of this?" They hadn't worked themselves up to having real elections.

I said, "Well, that's all right. I'm a foreign secretary." I thought that I'd meet a lot of people from around the world; maybe I'd even get some foreign travel. It turned out that was a big thing.

What the dickens was Ralph? At one point, he became the vice president of the NAE. I set up a committee for the National Research Council on industrial innovation and what the U.S. Government was doing to suppress it, to kill it. Ralph and I were kindred spirits on this. We wrote some reports. We had a committee; I was chairman of it, and Ralph was the noisiest participant. We wrote some reports which did affect U.S. policy and which called attention to the inhibiting effects of regulations and antitrust and so forth (15).

Also, I decided we ought to analyze industries, so I picked a number of industries and we got experts on those—just the usual NRC things. We produced a series of volumes (16). I saw an awful lot of Ralph because he was right in the thick of all of these things, although they were really my projects.

I liked Ralph. I wouldn't want to see him every day, because he'd wear me out! [laughter] He's right in the thick of things still. He regards himself as an economist now, and he got a professorship at Stanford. I could say kind of an unkind thing—by giving them

enough money, you could be a professor of economics there, or I could, if we had the money to give them! [laughter]

Ralph made a smashing success as a chemical engineer. He had started off with an idea that really paid off. He was a terrific innovator, and he talks about innovation all the time. He'd had this one great big success which produced a company that made a pile of money. It was his company, it was his pile. I like Ralph. He's got an enormous amount of energy.

He's still active in the NAE and all these things in Washington. I got sick of it after about seven or eight years. I didn't want anything more to do with them—too discouraging. [laughter]

BOHNING: Our time is running to the end. I had sent you an agenda; I believe we've covered virtually everything on it.

HANNAY: I hope we have.

BOHNING: Let me just ask you about one thing. We've covered scientific innovation pretty well, although we haven't discussed it in those terms. However, what would you say is important for the future vitality of chemical R&D? That's number fifteen on the agenda.

[END OF TAPE, SIDE SIX]

HANNAY: I feel that in too many industrial research and development organizations, everything flows down from the top. It depends on the guy at the top—does he give the right kind of marching orders, and does he watch for this all very closely? He's not going to get very many bright ideas, except when he happens to have the bright ideas himself.

I've described Bell Labs. It was much more of a bottoms-up approach. You hired the best people you could. You gave them a working environment that brought them in, and they chose to come to you instead of going to MIT or someplace. You bring them in and turn them loose and see who can really make it, and tell them, "Your job is to change things. Change the world." You sort people out on the quality of their ideas.

I think that many of the companies—and this is an overstatement—in the chemical industry, they're too sure that it ought to work top down. Now admittedly, the top has to be aware of what it's going to support and know why it's supporting it. But I think that they'd be surprised at how much they'd get out of it if they gave people more scope in what they did.

You can afford this if you're spending a lot of money on this. Bell Labs wasn't spending all of its money on research. As I told you, it was fifteen percent, and that included a bunch of engineering things. We could have pointed to all kinds of things that were ongoing projects with definite ends. We called them development. Many companies will call that research. We called it development and when we said research, we meant research. It was competitive with universities.

Now I think that one thing that needs to be done is, more people in the industry need to develop an open mind about this kind of approach to things. They claim that they do it, but they don't. I hate to think how many good ideas that somebody might have are going to waste, because they're not being encouraged to see how far they can go with them.

That's one thing. Certainly, the spirit of inquiry into things is another. I don't like the idea of just saying, "Here's—". Our worry was more, we got something like a transistor or a laser, and everybody in the place wanted to go to work on it. We had to decide, "How do we keep them out of it?" [laughter]

My successor has a way with words—Arno Penzias. He put it very nicely. "Hire the very best people you can, point them in the right direction, and get out of the way." [laughter] Which is a pretty neat description of it.

BOHNING: Absolutely.

HANNAY: To point them in the right direction. People very quickly sense, "What's going to make an impression around this place?" They can make a discovery that is completely foreign there and has nothing to do with the business of the company, and they know that it's not going to make them. So they tend to say, "What does this place really need?" You know, "I want to make myself a success here or a hero here, what would do that?"

Well, I think I'd better go.

BOHNING: Well, I appreciate your spending the time with me this afternoon.

HANNAY: What's going to happen?

BOHNING: I'm going to send you a transcript of this.

HANNAY: Yes. If I want to add to it, or copy it, or change it, or subtract from it, I can do that.

BOHNING: Right. Exactly.

HANNAY: You're not going to get a lot out of this, but I will try to use some sense about the balance of things.

BOHNING: All right.

[END OF TAPE, SIDE SEVEN]

[END OF INTERVIEW]

NOTES

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9. M. Tanenbaum, L. B. Valdes, E. Buchler, and N. B. Hannay, "Silicon *n-p-n* Grown Junction Transistors," *Journal of Applied Physics*, 26 (1955)L 686-92.
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13. N. Bruce Hannay, "Semiconductor Principles" in N. Bruce Hannay, ed., *Semiconductors*, American Chemical Society Monograph Series, 140 (New York: Einhold, 1959): 1-49.
14. Ralph Landau and N. Bruce Hannay, "Taxation, Technology and the U.S. Economy: Introduction and Overview - The Guest Editors Perspective," *Technology in Society*, 3 (1981): 1-19.
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