Photo taken in 1997

HARRY B. GRAY
(b. 1935)

INTERVIEWED BY

SHIRLEY K. COHEN
SEPTEMBER 2000 – MARCH 2001

AND

HEIDI ASPATURIAN
JANUARY – MAY 2016

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Chemistry

Abstract

Two interviews in seven and six sessions respectively, with Harry Gray, the Arnold O. Beckman Professor of Chemistry. The first series of interviews, conducted in 2000-01 with Shirley Cohen, deals with Gray's life and career up to that time. The second series**,** conducted in 2016 with Heidi Aspaturian, covers the period 2001–2016, expands on a number of topics discussed in the first interview series, and adds to the account of Gray's earlier decades. Discussion topics common to the two interviews are cross-referenced in both texts.

2000–01 Interview

Gray opens this interview series with a description of his family roots and formative years in Kentucky's tobacco-farming country, including his youthful career with the local newspaper and early interest in chemistry. He then provides an account of his undergraduate studies at Western Kentucky State College (BS 1957), graduate work with F. Basolo and R. Pearson at Northwestern University

(PhD 1960), and postdoctoral work with C. Ballhausen at the University of Copenhagen, where he pioneered the development of ligand field theory. As a professor at Columbia University, he continued work at the frontiers of inorganic chemistry, published several books and, through an affiliation with Rockefeller University, was drawn to interdisciplinary research, which led him to accept a faculty position at Caltech in 1966. He talks about his approach to teaching and his research in inorganic chemistry and electron transfer at Caltech, his interactions with numerous Caltech personalities, including A. Beckman, G. Hammond, A. Kuppermann, J. Labinger, R. Marcus, L. Pauling, and J. Roberts, his efforts to revamp the undergraduate curriculum, and his tenure as chair of the chemistry and chemical engineering division. He discusses the vision for and construction of the Beckman Institute, its multidisciplinary programs, and his tenure as the facility's founding director (1986–2001). The interview concludes with Gray's assessment of chemistry's key advances over the previous thirty years and predictions for the future.

2016 Interview

In this follow-up to his 2000–01 interview, Gray elaborates on his family history and youthful interests, including his early fascination with the chemistry of color, his first patent at age eighteen, and his rapid rise through the ranks of his hometown newspaper in Bowling Green, Kentucky, capped by a front-page interview with a very young Elvis Presley. He describes his experiences growing up in the segregated South and his father's controversial stance in support of school integration in the 1950s. He talks at length about his years as chairman of Caltech's chemistry and chemical engineering division (CChE), particularly his experiences recruiting future Nobel laureates R. Grubbs, R. Marcus, and A. Zewail onto the faculty and his interactions with Caltech administrators, trustees, and donors. There is extensive discussion of his somewhat unorthodox but highly successful approach to teaching and mentoring undergraduates, as well as recollections of his involvement with the Caltech theater arts program and student pranks. He discusses his solar and alternative energy research and his work over the last decade with "Gray's Solar Army," a worldwide network of students engaged in testing potential catalysts for solar cells. He shares his perspectives on chemistry as "*the* 21st century science," details his current research into electron transfer and redox reactions, and comments on his relationship with a succession of Caltech administrators. A look back at his professional awards, including a 1986 White House visit to receive the National Medal of Science, and his thoughts on how chemistry and Caltech have evolved in the last fifty years round out this interview. This interview is partially restricted. Per agreement between Professor Gray and the Caltech Archives, dated April 2017, portions of the manuscript are closed for ten years. Closed portions are clearly marked in the transcript.

Administrative information

Access

The interview is unrestricted.

Copyright

Preferred citation

Gray, Harry B. Interview by Shirley K. Cohen and Heidi Aspaturian. Pasadena, California, September 2000 to March 2001 and January to May 2016. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Gray_H

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEWS WITH HARRY BARKUS GRAY

2000 – 2001

BY SHIRLEY K. COHEN

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2016

BY HEIDI ASPATURIAN

PASADENA, CALIFORNIA
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Harry B. Gray, 1997

NOTE TO READERS

Per agreement between Professor Gray and the Caltech Archives dated April 23, 2017, portions of this interview are closed for ten years, to April 1, 2027. Closed portions are clearly marked in the transcript.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

Interview with Harry Barkus Gray **by Shirley K. Cohen**

Pasadena, California

Session 1	**September 22, 2000**
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SESSION 1

September 22, 2000

COHEN: I'm delighted that you have agreed to participate in this project. Now, tell us a little bit about your mother and father.

GRAY: My mother's name was Ruby Hopper, and she came from a family of farmers in south–central Kentucky in a little town called Woodburn, which is about fifty miles north of Nashville, Tennessee. It's very close to the Kentucky–Tennessee border. The closest large city is Nashville. She had a couple of brothers, and I mention them because I think they had more influence on me than other people in my family. My uncles were both tinkerers and scientists in their own way. One actually went on into physics and engineering after the war. They were both in the Pacific in World War II and really had no education. My mother's side of the family did not do formal education.

COHEN: So your grandfather was a farmer?

GRAY: Yes, both of my grandfathers and grandmothers were farmers in that part of Kentucky—tobacco farmers and other things. Everybody in that region was a tobacco farmer. They didn't have much money. They always borrowed money in the spring and planted tobacco and then sold the tobacco in the fall. And those sales paid back the bank. They paid their bills and went on to the next year. That's the way life was in that part of the country. On my mother's side, I had a wonderful grandmother and grandfather who got me to the farm every summer to work—get up 2:30 in the morning and go on the milk route. We'd take the milk cans into town, pick them up off milk stands, take them into the closest large town, which was Bowling Green, to the milk place; and then head back to the farm on the truck and work there all day.

My mother grew up in this type of community. She went to the little Woodburn High School, where she met my father, who was the principal and, more importantly, the women's basketball coach. Of course in this little tiny school, I think there were maybe eight women. I mean, we're talking about a farming community of maybe 250 people total, with a post office, a school, and a railroad track going to Nashville, cutting the town in two. You lived on one side of the tracks or the other. But it mainly had a general store and a bunch of farmers.

The school burned down and wasn't rebuilt. The post office burned down and wasn't rebuilt. It was that kind of small town. It's still there, but there are probably only fifty people in it now. But my family on my mother's side is still there, running a very large farm now. We had small farms then.

COHEN: Is this still a tobacco farm?

GRAY: No, no, no! Tobacco is no longer fashionable. Also, many people just oppose tobacco. My mother's family was always very health-conscious. And so now we don't grow tobacco. It's my cousins now who are still running the farm. I'm still attached to them. It's the only family I have left. My father's family has been gone for many years. My father died when I was rather young, and my grandfather on my father's side was already dead when I was born. I knew my paternal grandmother, but she died fairly early. So my father's side has really not been in my life the way my mother and her family have been.

At any rate, she grew up in this environment. I was born in this little town—no hospital. I was born in a doctor's office—Dr. London. [Laughter] Not Fritz London the physicist, but Dr. London.

My father had grown up close by in the same county, probably some twenty-five miles away, also in a very, very small farming community, on a little tiny tobacco farm. My grandfather on my father's side, whom I never met, was a blacksmith—very much like Arnold Beckman's father was a blacksmith. My father was quite interested in education, so he went to the University of Kentucky and got into English and education and things like that, and went into teaching. Nobody else in my family had ever gone to school. My father and then this one uncle —my mother's brother—who came back from the war were the only two, really, who had gone on to any kind of college education. Then they went on to more advanced work—some graduate education. Both got master's degrees. My uncle—his name was Claude Hopper—got a master's degree from Vanderbilt in engineering and physics. We're talking about people who usually dropped out after eighth grade. But he was so fired up when he came back from World War II that he realized that he really wanted an education.

COHEN: Of course, there was the GI Bill, too?

GRAY: He went on the GI Bill. My other uncle, who'd only finished the eighth grade, didn't go back to school. But you could tell that both of them were very good scientists. They built all kinds of things. They built ham radios; they communicated with people all over the world. They built instruments. This other uncle of mine, whose name was James Freeman Hopper, was a wonderful man. When he came back from the war, he was a fantastic scientist. He was always interested in chemistry and agriculture and crops. All through his life, he discussed chemistry with me. He'd never taken chemistry, but it was quite clear that he had read so many books—it was incredible. He was self-taught. And he could talk to me about chemistry just like Jack [John D.] Roberts [Institute Professor of Chemistry, emeritus, d. 2016] could.

COHEN: So you spent your summers with them?

GRAY: My summers I'd spend out in Woodburn, where I was born. Then what happened is that my father was offered a job as a high school principal in Bowling Green, which is about twelve miles north of Woodburn. I was born in 1935 and he was offered this job in '37, or '38. So my mother and father and little baby me moved to Bowling Green. He became principal of the school, which he was until he died.

COHEN: That must have been a prestigious position in the 1930s.

GRAY: Very prestigious. So he was quite well thought of. I mean, here's a guy who's in this little dinky town of Woodburn, that nobody ever heard of, and suddenly he's principal of a big high school in Bowling Green, which is one of the larger cities in Kentucky. Well, he had earned this interesting reputation and become well known when he coached women's basketball at the tiny school with eight or ten women in it. Every woman in the school played on his team. [Laughter] He took this team and won the state tournament in Kentucky. They played against these big schools in Louisville and Lexington with hundreds and hundreds of students, and he beat them all. The team won a state championship in the early 1930s, maybe two times in a row. So he was a big hero, because basketball is everything in Kentucky. If you're a basketball fan, you know that basketball and Kentucky are synonymous. So here's this young guy, in this little town of Woodburn, who's winning basketball tournaments with five women from a school that has barely enough women to actually have a team. [Laughter] And they win the state championship. So this guy is known. That's my guess—that they were talking about him all over the state and saying, "Who is this guy?"

COHEN: Was he a big man?

GRAY: Yes, he and I look a lot alike. So, we moved—this was the first time outside of a farming community—into a town of 20,000 people or so. And that's where I grew up.

I was just thinking, while talking to you, what is the first thing I can really remember? And the first thing—it's etched in my mind—is Pearl Harbor. When I think back, I know exactly where I was—with my dad and my mom in this little house in Bowling Green, Kentucky. I'd just turned six. And I'll never forget, standing there sort of mid-day, on that December 7, 1941, listening to FDR and the radio. I know exactly where the radio was. I remember my father and my mother were glued to the radio, listening. That's my first really vivid memory—FDR telling us that this terrible thing was happening. I can't remember much in my first ten years except that. But I remember that vividly. Anyway, so much for that.

COHEN: So, you went on to school in Bowling Green?

GRAY: Yes, sure. I walked to a school called the Eleventh Street School for first grade. I guess there were kindergartens, but I was taught at home. My father was a teacher, of course. He taught English and math at Bowling Green High School, as well as being principal. My mother was also quite good in mathematics—although she was self-taught and didn't have a degree. And her brothers were extremely good in mathematics and physical science types of things. If they had been in Boston or out here, my uncles were the kind of people who would have been at MIT or Caltech. They were extremely good in mathematical things and so was my father, for that matter. Quite intuitive.

I was into reading and math very early because of my father and mother and my uncles. When the war ended in '45, I was nine and now in the middle of this school. When my uncles came back from the war—fortunately they came back, although they were in some pretty heavy stuff in the Pacific—they had a tremendous influence on me. My one uncle—the one who ended up in Vanderbilt in physics—was now talking physics and stuff all the time. This was the first time that somebody in my family was really going to grad school. I mean, this was a whole new concept. My family didn't have much money. In these farming communities, this was a big deal. At least all these things affected my life. [For more on early life, including impressions of life in the segregated south, interests in reading and science, and other activities, see *Gray 2016, Session One*]

I went to junior high school and then to the high school where my father was principal. This, of course, had an interesting effect on me because I was an extremely good student. I was driven. I loved school and it came very easily to me. I could handle everything in math, and was always looking for something more to do. But there was also all of this pressure from my classmates. From first to sixth grade, this was no problem. But when I got to junior high school and to the high school where my father was principal, I think a lot of my classmates felt that I was making these great grades because I was connected. And I started sort of turning off academically, in the sense that I was worried about being popular and being accepted. I was sort of unusual anyway, because I was making so much better grades than everybody else and kind of running the classes and so forth. And starting to chase women seriously. [Laughter] I had my first date when I was in sixth grade. I was a little bit more advanced there too. I was the first boy in my class to ask a girl out and have a date—Barbara Scott in sixth grade. I asked her to go with me to see Elizabeth Taylor in *National Velvet*. We took the bus down and went to see this movie. [Laughter] So I was dating in sixth grade.

But during seventh, eighth, and ninth grade, I could tell that my popularity was sort of strange because people considered me a little weird because I was so academic. Up to that time, I had never made anything but A's and A+'s in my life, and I decided this was hurting me. I wanted to see if I could make a C, so I could be more popular. [Laughter] So I sort of blew a couple of exams in eighth grade English just for the hell of it. When the teacher realized what was going on, she called me in and said this was absolutely outrageous and that I should be ashamed of myself. Her name was Mrs. Craig, and she really read me the riot act and said that "you should have much more pride than this, and you should never ever do this again." I remember that very clearly, and I never did do it again. That incident made a big impact on me. I thought about it, and I decided mentally that "trying to get along with these characters here isn't worth it. They're either going to accept me for who I am, or they can go shove it." I made a decision at that time, in the eighth grade, that I wasn't going to slough off just to be accepted socially. It wasn't worth it, it wasn't me, and to hell with them. Mrs. Craig really straightened me out on that. I went on, and I graduated first in my class. I never made a B.

I never had a real chemistry teacher in high school. I had a chemistry teacher, but she was more a social studies teacher. At that time in these high schools in Kentucky—and in many other places—they didn't have real science teachers. Not many people were taking that much science. So typically, people in the PE department—coaches—were assigned science classes as part of their thing. And people pooh-poohed this a lot and laughed at it, but as a matter of fact, it's not bad for people who really are driven and want to do things, because it gives them a chance to develop. My teachers didn't put up any barriers. They were quite happy with me. And by this time, in junior high school and high school—because of my uncles' influence and other influences—I had already read a tremendous amount of science. I knew more chemistry than anybody in the school—any teacher—by far. I had my own lab in the basement. I was a tinkerer like my uncles. I sort of followed their example. They had stuff in the barns—labs and ham radios. They were doing all this stuff. And I worshipped these guys.

COHEN: And your parents encouraged you.

GRAY: Oh, my parents encouraged me, of course. They loved academics. My father was in love with teaching, and my mother was very driven this way too. She was clearly somebody who would have done something incredible if she'd had formal training. She was extremely smart. You could tell that. She could do anything in math. And I sort of followed along. Here's this kid coming along, an only child. You understand this. I basically had a "Jewish mother"—in the sense of her really being proud and pushing me and supporting me.

COHEN: Now they just say "a Chinese mother." [Laughter]

GRAY: [Laughter] I had a very supportive family environment.

COHEN: Now, the idea of going away to college would have been financially hard. You stayed in your own community.

GRAY: I stayed in Kentucky to go to school because we didn't have any money. I had applied to Vanderbilt and various places, and I had scholarships. But even with that, I couldn't afford to leave home. My father never made any money as principal of this high school. I think his last salary, before he died, was less than $4,000 a year. My mother didn't work.

Of course, I'm from a very hard-working family of farmers, where everybody pitches in and works. So when I was ten years old, I got my first job. I went down to the newspaper in Bowling Green—*The Park City Daily News*. Bowling Green is called "the park city," because it has a lot of parks. That was one way for a kid to make money—to carry newspapers. But they said, "You can't get a job to carry newspapers until you're twelve." And so I said, to hell with that. I hooked up with a guy named C. C. Tatum and I said, "I'll work for you on your paper route. I'll carry your papers and you can sort of collect." That's how I started carrying newspapers in a very tough district of Bowling Green when I was ten years old. I had a bicycle, so I bicycled down there every day after school. It got to the point where I worked all night Saturday putting out the Sunday paper. I did something known as inserting: you know, fitting the comic section and the little magazine inside the paper. And I became an expert inserter. I could insert faster than anybody because I have great hands. I was a good athlete. I was playing tennis and all sorts of things at that time. So they hired me to insert all of the newspapers on Saturday night. I would carry papers during the week, work all night Saturday inserting papers, and then carry the Sunday morning paper in this district. I got my own paper route at age twelve. Then, I worked my way up into the circulation department, keeping books because I was very good with mathematics and numbers. So they made me the accountant at age fifteen or sixteen. They made me the circulation guy—assistant manager or something—so I could go up and work in the office. Along with carrying newspapers, I was also keeping books for this newspaper. By the time I graduated from high school, I was making a lot of money. I was already independent, carrying my own weight. But I couldn't afford to leave home. The whole family income was less than $4,000 a year. I don't know how much I was making, but I was supporting myself. [For more on newspaper "career," including an encounter with Elvis Presley, see *Gray 2016, Session One*]

So I went to college. I got a scholarship to a college in Bowling Green called Western Kentucky State College. It's part of the state system. It was a small college, though it just happened by an enormous stroke of luck to have a fabulous chemistry department. It had three excellent chemists—all with the PhD—who had reasonable reputations and were excellent teachers.

COHEN: How do you account for this?

GRAY: Oh, gee!

COHEN: Were they people from Kentucky?

GRAY: Yes—Ward Sumpter was a Kentuckian from this neighborhood. There was another very well-known organic chemist from this neighborhood, but he was now a professor at Northwestern. His name was Bob [Robert] Baker, and he and Sumpter knew each other well. Sumpter had grown up in this neck of the woods, and so he had just settled here. He was quite a well-known organic chemist, actually. He had a PhD from Yale, and he had done some very nice research. So suddenly this little school had a chemistry department—mainly because of this guy Sumpter, who wanted to live where he had grown up. He was quite a good teacher, and I did research with him as an undergraduate. The other two professors at Western Kentucky were a man by the name of Glenn Dooley from Iowa, and a guy named C. P. McNally, who was a physical chemist. They were all quite good chemists. It's quite remarkable. It was a little like going to a place like Oberlin or Swarthmore in the sense of being at a small school, with something like 1500 or 1600 students total. The chemistry department was kind of a family. All the chemistry majors knew each other; they were all doing some sort of research with some of these people. You got a lot of individual attention—I got a tremendous amount. And I was self-taught because in high school, as I said, they pretty much let me go my own way. Basically I had taught the chemistry course myself. There was an official teacher, but they basically farmed it out to me because I knew all the experiments. I ran the experiments, and I ran the class. And they supported me. Everybody said, "Fine, how are you doing?" They encouraged me, and I learned a tremendous amount on my own.

Western Kentucky, which is called Western, is on a hill in a very beautiful location. It is now a big state school [Western Kentucky University]. But at that time it was a little college; it had evolved from a small private school called Ogden College, which was a little, kind of science-oriented college. Then it became this state college—training teachers mainly—and then part of the state university system. But at that time, it had a very small-college atmosphere—a little campus, a few students, and a few dedicated teachers. And I got a very good education there.

I was able to do research in chemistry. This guy Sumpter was connected to everybody around, and he encouraged me to go to Northwestern for graduate school. He was well-connected there, and he knew some of the things they were doing. Even though I could have gone pretty much anywhere, I think, to grad school—I had offers from MIT and a lot of other places because I was quite a good student, and I had very good scores on all kinds of exams—Sumpter directed me to Northwestern because of various people he knew there. That was a tremendous stroke of luck for me because of my going into inorganic chemistry—an area that was just really starting. I think Sumpter knew that there were people at Northwestern who were in on the beginning of this field. Some of the more prestigious schools really were more traditional in organic and physical chemistry and didn't have this new area. That really made my life, that decision to go to Northwestern. Or, it was made for me. Sumpter basically told me I had to go. [See also *Gray 2016, Session One*, for more on undergraduate and graduate years]

Anyway, that's a lot of early stuff. Now I really wanted to emphasize a few things that influenced me enormously. One was getting my own job at ten and working really all my life. I think that's very important. A lot of kids miss that. I grew up really fast because I was running my own business—buying newspapers from the newspaper and selling them to customers. I had become an independent operator. Then throughout high school, I learned chemistry myself because my uncles encouraged me to and they were into science. Then, when I went through college, I learned, and these three professors I mentioned helped me, and they were very supportive. But I was way ahead of them in a lot of areas.

So I stayed at home to go to college because I couldn't afford to go elsewhere. I think I was a lot like the Kentucky version of the kids in New York who went to City College and got a perfectly decent education. Some of them became extremely famous. In college I worked full time. I worked over sixty hours a week.

COHEN: By this time you had a family already, isn't that so? Or was that when you were in graduate school?

GRAY: No, no. When I was at Western Kentucky, I didn't have a family. I met Shirley there in a physics class. She was from a little town called Beaver Dam. And we started going out. We got married after graduating. When we went to Northwestern, we were married. And we had Vicky very quickly. So we had a child in grad school.

But when I was an undergrad, I continued working at the newspaper—sixty hours a week. Now I was in the circulation department, doing the books. I wasn't carrying papers anymore. I'd work all afternoon, and most of the evening. I'd work all day and all night Saturday. The only time I had to study was a little time on Sunday—all through four years of college.

COHEN: When did you play tennis?

GRAY: During tennis season, I practiced in the afternoons. I was one of the best players in Kentucky. I played on the tennis team at Western every year and won several tournaments. I went down to the newspaper at five o'clock, after tennis practice.

COHEN: So sleep isn't something you need.

GRAY: I didn't need much of it. I worked all day Saturday. I continued to work sixty hours a week all through college, play tennis, take a big load of classes. And there were all kinds of activities. I studied a couple of hours on Sunday.

It just turned out that courses came easy to me. I could do the chemistry problems in the back of my head. I could do all the mathematics and the physics. I'm not saying I was a goof-off. As a matter of fact, when I was doing my newspaper job—when I did the books and so forth at night—I could do all the studying I needed to do. These courses I had in college weren't that demanding. I could usually study for the finals and so forth.

I did learn one thing that was very, very important. I learned how to manage my time. I was with my mom during this period because my dad had died suddenly of a heart attack. I was still young, so I had to learn to deal with that and to really grow up at that point. It was sudden. My dad was there one day, and the next day he wasn't. And that really had a tremendous effect on me—a tremendous driving effect.

There were two things that had this impact on driving me, to make me say, “You know, I’m really going to make something out of myself.” One was my grandmother—whom I loved dearly—who died of cancer. She was the one on the farm in Woodburn whom I would visit every summer, the one who cooked for me. She was just fantastic. And she got stomach cancer, and it was very painful. I was ten or eleven when she died, and I was so devastated. I wrote some little notes, saying, “I’m going to do something about this.” That probably drove me into science more than anything else. I said, “I’m going to go into science; I’m going to do something about this.” I haven’t. [Laughter] I haven’t in a direct way, but in an indirect way, maybe I have. But I decided I was going to make good. I was going to do this for my grandmother. I loved her so much.

The second thing was my dad dying on me suddenly. Both those events really, really, focused me—my grandmother and my father both dying when I thought it was too early. This wasn’t right. Somebody should have been able to do something. My grandmother had stomach cancer. She was in Vanderbilt Hospital. And they couldn’t seem to do anything. And then my dad dying affected me in another way.

My dad was a real liberal guy in a real wonderful sense of the word. He fought for integration. This was the time when schools were being integrated, and my dad was in middle of this. [See also *Gray 2016, Session One*] Kentucky is a very Southern state. My dad fought the good fight and helped with these things. But he took a lot of flack from conservatives. Some of these people were trying to preserve all that crap, and my dad fought against that. I don’t want to overstate it, but he was under a lot of stress. I knew it, and my mom knew it. Shirley knew it—I knew Shirley by this time. I was sure this enormous stress he was under killed him. Also, he was a chain smoker. He set himself up. He was a good man; he was a very good man. He loved people; he had no biases. People were people. There was not a drop of bad blood in his body. I was sure that killed him—the system, the stress he was under. And I said, “I’m really going to do something.”

HARRY B. GRAY

SESSION 2

September 29, 2000

COHEN: We left you on the steps of graduate school.

GRAY: Yes. When I really got to the point of getting into the big time—getting into grad school at Northwestern—I was really ready to learn a lot of stuff that I had explored myself but didn't have a good theoretical understanding of. I had done a lot of experiments myself in the lab. I'd done a lot of reactions. I'd seen a lot of colors [see also *Gray 2016, Session One*]. I knew how to do things, and so on. I had a very good hands-on feeling for an enormous amount of chemistry. But my theoretical understanding of chemistry was essentially zero. I was ready to go to a place where people really understood some of the theoretical stuff that was going on. I wasn't burnt out. I have seen kids at Caltech and MIT, you know, who come in and do undergraduate stuff. They get into an enormous amount of stuff so early, which is wonderful in one way but in another way can absolutely burn them out. They know too much, as it were. There's a lot to be said for timing. I was really ready—just like, okay, hit me with the fire hose now. When I went to grad school, I just sucked it up like you wouldn't believe. I was learning all this stuff on the theoretical side that I'd explored experimentally. It was just fantastic. I was willing to literally study night and day, seven days a week. And so I got out of graduate school in three years and went to Copenhagen on an NSF [National Science Foundation] postdoc, doing literally theoretical work. I'd gone from nothing to something.

COHEN: What was your thesis about?

GRAY: My thesis was on platinum. I was into metals. You see, when I went to Northwestern, analytical chemistry, physical chemistry, and organic chemistry were the three fields I knew. I didn't even know there was something like inorganic chemistry. To me—and this background was very old-fashioned—inorganic chemistry meant sulfuric acid production. When I got to Northwestern, I realized there were two guys I worked for—Fred Basolo and Ralph Pearson—who were really right on the edge of this hot new stuff called modern inorganic chemistry. This new theory called crystal field theory—now called ligand field theory, for which I've become famous—was just starting. And I was there. I looked at it, and I said, "Gee, this is where I want to go. I want to go into this stuff. It's brand new." So I decided to work on a problem in platinum mechanistic chemistry—the mechanisms and reactions of platinum complexes. Very little had been done on it. This was brand new. My thesis was sort of the definitive thesis on the mechanisms and reactions of platinum complexes. And in a way, it's a famous thesis, because a lot of the fundamental work there has played out later into this platinum cancer drug called cisplatin—the world's largest anticancer drug.

Those were the kinds of compounds I worked on as a graduate student. But I did not discover that they were anticancer. Nobody was looking at that then. I was looking at their mechanisms from a fundamental point of view. But a lot of that work later played out very nicely in understanding the mechanism of the platinum anticancer drug. That platinum drug, by the way, is the drug that cured Lance Armstrong, the great cyclist who won the Tour de France. He's a success story. You know, it's a hundred percent effective against testicular cancer, this drug. And it was discovered accidentally by a guy named Barney [Barnett] Rosenberg at Michigan State in the late 1960s—over ten years after I did my thesis. He had been working with some rats with tumors and suddenly discovered that this was a very powerful anticancer drug. Barney Rosenberg discovered it; of course, I didn't. But then people looked back at my thesis work to understand how it might be working. Because the reactions involved are very much the ones that I studied and laid out many, many years ago. I actually went to one cancer conference during that period.

COHEN: This was when?

GRAY: My thesis was done between 1957 and 1960. My first publications in platinum work from my thesis were '60, '61, and '62. I literally wrote the book on the subject with Cooper Langford when I went to Columbia [See *Gray 2000–01, Session Three*]. It basically summarized my work in platinum chemistry and some other work as well. It came out in 1966. And then I went on to other things.

But I was at a cancer conference in the early 1970s, when this drug was real red-hot. I was just sitting there in the back, and I asked some question. They told me, "No, no, you can't possibly be right because Gray's work shows that—da-da, da-da, da-da." [Laughter] I didn't tell them I was Gray; I just sort of let them go on. I forget what I asked, but they used my work to answer me. And I said, okay, and I left. [Laughter] I've had a number of experiences like that.

So I was in graduate school, working on platinum reactions with my advisors, Fred Basolo and Ralph Pearson. Working on platinum chemistry, I'd become very interested in inorganic compounds and their spectra and colors. This was a very exciting time in inorganic chemistry because there was a revolution in the field's theory. Inorganic chemistry previously had been old-fashioned, devoted to the manufacture of sulfuric acid and things like that. Now there was this new crystal field theory.

It has an interesting Caltech connection, because Hans Bethe, who still visits Caltech regularly and is in his nineties [Bethe died in 2004. *–Ed.*] and a wonderful man—I've interacted with him many times at Caltech—really wrote the first paper in crystal field theory—in 1929, in his "Splitting of Terms in Crystals." This paper, which was written in German, of course, and was rather obscure, was more or less ignored by chemists for years. It was rediscovered after the war by [Hermann] Hartmann and H. L. Schläfer in Germany, who wrote papers on it that chemists could understand, explaining the spectrum of the titanium ion in solution. And shortly thereafter, a group of young people in Denmark—primarily Carl Ballhausen and C. K. Jørgensen—started working in this area. At the same time, Leslie Orgel, in Cambridge and his group started on it too. So it was these three groups who had revived Bethe's crystal field theory for chemists, since he didn't really try to apply it to chemistry problems so much. And then through the 1950s, they really started showing how Bethe's theory could be applied to understanding the magnetic properties and the colors of inorganic compounds. Many gemstones—ruby, sapphire—these wonderful gemstones owe their beautiful colors to electronic transitions in inorganic and transition metal ions, like chromium and vanadium and iron and manganese, and so on. So there was tremendous excitement.

To put it into context, Linus Pauling at Caltech had explained the bonding in these compounds in the early 1930s. His theory—the valence bond theory—could account for the structures and the magnetic properties of these compounds, but it simply could not account for their beautiful colors. So there was a tremendous revolution in the 1950s coming out of these applications of the so-called crystal field theory, which just swept the inorganic community. And Linus's theory took a backseat in the '50s. These young people in Germany and Cambridge and Copenhagen were showing that they could now explain all of these colors very simply with the crystal field theory. And it was extremely exciting.

At this time I was in graduate school at Northwestern, working on compounds that had some of these colors. I was very taken by this and really wanted to go into it deeply myself and help develop it. My supervisors—Basolo and Pearson—were attuned to this. They were part of this revolution—but they were more interpreters and PR people for it. They weren't really directly involved themselves in the development of the theory, but they were very excited about it and talking about it and promoting it all over the place. They very much helped to popularize it in the United States. So it was really catching on like wildfire.

So I said, that's what I want to do. I want to go to Cambridge or Copenhagen. I didn't really consider Germany at all.

COHEN: Why didn't you consider Germany?

GRAY: Because Hartmann and Schläfer were very technical and limited to very simple systems. They clearly were not pushing it forward into chemistry the way Ballhausen, Jørgensen, or Orgel were. It was clear that Hartmann and Schläfer were much more connected to the old-time stuff of Hans Bethe, with just slight changes. They were looking very narrowly at a few little things. And so I wanted either to join up with the group in Copenhagen, or go to Cambridge and join up with Orgel.

Well, beer decided it. I had a little conversation with Fred Basolo and Ralph Pearson. Basolo had spent time in the lab in Copenhagen, and he knew all the people there well. And he said, "You really should go to Copenhagen. I know them. It's a great city. It's a wonderful laboratory. It has an enormous tradition in inorganic chemistry. And they've got these two bright young men"—only one bright young one by that time, actually. Jørgensen had left the country, because Ballhausen had beaten him out for the chair. There was only one chair. So Jørgensen took off and Ballhausen was left. He was in his early thirties at the time. So I picked Ballhausen in Copenhagen over Orgel at Cambridge, which was a good choice for me, because Orgel shortly thereafter went into evolution—and has been in that the rest of his life. He's at Salk Institute now. So he exited the field, but Ballhausen stayed in it.

There was also Ralph Pearson's statement, "Danish beer is a lot better than English beer, Harry." So between that and Fred's direct connections with the lab, I wrote to Carl Ballhausen and said I'd like to come to his lab. He wrote me back a two-line letter saying, "Well, gee, I hope you get some money to come. I'll be very happy to have you."

So I applied for a National Science Foundation postdoctoral fellowship. At the time, they had fellowships that you could take to any place in world that you wanted to go. They don't have those anymore. At that time it was quite unusual for an American to go to Europe. After the war, the action in chemistry had shifted to the U.S., and the Europeans and Asians were coming here to do postdocs. But here was a brand new field where the action was in Europe.

So I applied for an NSF fellowship. I also applied for a Fulbright. I applied for everything I could, because I really wanted to nail something down so I could go. I knew I could count on my advisors—Basolo and Pearson—because I was doing well with them, publishing, and they were very high on me, so I knew I had a couple of great letters. I had also taken various kinds of exams for this NSF deal, and there was a chemistry graduate exam, or something, on which I think I got a perfect score. So I knew I had a great record. But I needed one more reference. There was a great inorganic chemist at Northwestern named Pierce Selwood, who had a tremendous reputation. But he dealt with people in a very formal way, so I didn't really know him like I knew my advisors. But I'd had two courses with him, in which I had made the top grades. So I figured he was good for a reference.

I walked into his office, and I said, "Professor Selwood, I'm applying to go to Copenhagen to work in crystal field theory. And I'd appreciate it if you would write me a reference." And I'll never forget this as long as I live. He looked at me, and he sort of recognized my face. He said, "Well, I'll certainly write a reference for you. If anybody deserves a fellowship to go to Europe, you do." Then he said, "By the way, what's your name?" [Laughter] I had this feeling that I should pull this reference back. But he did write a Fulbright reference for me.

I did get the Fulbright; I did get the NSF; I got everything I applied for. I took the NSF because it was more money than the Fulbright. But I did find out later from Carl Ballhausen that Selwood had written this tremendous recommendation for me in which he said I was the brightest young guy in the country and I deserved a fellowship, but it was very sad that I was going into this new field [laughter] because this really fly-by-night theoretical stuff was really not going anywhere. [Laughter] He was a very old-fashioned guy, a very Pauling-like guy at this point. This new field was taking on Linus, and Selwood was very upset with it.

At any rate, I got these fellowships. Shirley got pregnant again. [Laughter] And it was wonderful. We had Vicky, our first child, and now we were pregnant with our second, expecting our second one. The baby was due in October, and we were going to go over to Copenhagen in September. So there was considerable discussion about whether we should do this, and whether I should maybe change my mind and go to Harvard, because I could take this NSF fellowship anywhere. They weren't really doing this field at Harvard, but there was a fellow there in theoretical chemistry, which was close enough that I could have gone there.

But then some doctor Shirley was seeing pointed out that they did have doctors in Copenhagen, and that it was possible to have babies there. So we lined up a physician in Copenhagen with Carl Ballhausen's help, and then with all of this ado, we left for Copenhagen on the *Gripsholm* from New York. Shirley had about a month-and-a-half to go before the baby, and we danced all the way over—nine or ten days on the boat to Copenhagen.

We showed up. We had a friend there from Northwestern—Arne Jensen. He picked us up at the boat and took us to a *pensione* that he had arranged for the three of us, and another one coming.

So I started this postdoc to learn and really get involved in this new theory. And I did; I took hold right away. And I could immediately see that this crystal field theory was too simple, although Carl Ballhausen was very much on the side of protecting this extremely simple version, in which all the atoms were treated as ions. That's why it was called crystal field. I knew that there had to be some compromise here and that covalent bonding had to be included. By this time, which was the fall of 1960, you could see the initial tremendous success of the crystal field theory in calculating the colors of metal complexes. But when you went into the details, the calculations of the energies and so forth, you could see that the theory was clearly off. It was off by enough that there had to be something more going on. It was too simple, and I knew it had to be changed.

So I started working on the version that we now call molecular orbital theory, or ligand field theory. I first took the nickel ion. The nickel ion in aqueous solution is a beautiful green. There are three absorption bands in its spectrum, and you can explain them in crystal field theory. Of course, the Pauling model can't explain any of it. Crystal field theory can explain the three bands, but not the energies of the three bands exactly. It always misses one band by 1500 to 2000 reciprocal centimeters, something like that. So I decided I would calculate that spectrum exactly within the crystal field model, including the configuration, interaction, excited states, and so forth. And there was no way I could account for the spectrum within the crystal field model. I knew, within three months of having joined Ballhausen's group, in Copenhagen that this theory had to be changed. And so I started on a version that included covalent bonding, and which is now called ligand field theory or molecular orbital theory.

It related to the theory of Robert Mulliken. Robert Mulliken had been, if you like, the great competitor of Linus Pauling. There were two big theories in the 1930s. There was Linus's valence bond theory, and there was Mulliken's molecular orbital theory. But Linus won out easily in the 1930s and '40s because he was much more articulate than Mulliken. He wrote clearly and his theory was fine; it was just limited.

Mulliken's theory was more general and applicable, certainly, to excited states in colors and things like that. He had shown how that was true, but he hadn't really shown how it could be applied to transition metal chemistry.

So I set about to really apply Mulliken's molecular orbital theory to inorganic compounds. I made the first real calculation of a transition metal ion, including covalent bonding, but keeping the crystal field part of it to calculate state energies. And I calculated the vanadium spectrum—the so-called vanadyl ion, vanadium + 4, with an oxygen on it—a VO + 2 complex. It's a beautiful blue color, a very distinct absorption spectrum. I calculated that. I explained that. I did the experiments on it as well—what's known as polarized crystal spectroscopy on a crystal—to show that this model could completely account for the spectrum of this ion. And I developed a new method of including covalent bonding into inorganic ions. The so-called Hückel theory in organic chemistry was very successful in explaining the spectra of organic molecules, like benzene and ethylene, and things like this, and the conjugated double bonds, and so on. But if you just applied it to inorganic compounds, it failed miserably because of the difference in electronegativity of, say, a vanadium bound to an oxygen.

So I worked out a way to include this difference, so you could apply this kind of molecular orbital model to an inorganic system. That's my main contribution from Copenhagen—to develop that new model, which was subsequently applied by everybody to inorganic spectroscopy. So this new molecular orbital model—now called ligand field theory—has been the standard in inorganic theory for forty years now. It's widely applied. And by no means was I the only person doing this. But I played a part at this time.

So now we've got a model that explains the structures, the magnetic properties, and the colors of all these compounds. And it's also capable of calculating the energies very closely. And it's more realistic in the sense that it's not just ionic bonding; it's covalent bonding, which we all know has got to be there. It was a tremendously successful year.

COHEN: Did you work by yourself mostly?

GRAY: Mostly by myself, because Ballhausen was the type of person who would sort of suggest things and then work on his own problems. There were other Americans there. This was now a lab where American postdocs were coming because of this theory. It was a very exciting lab. Tom [Thomas G.] Spiro was there; he's now a professor at Princeton. Curt Hare, who's now a professor at Miami, was there. We were all working on our own under Ballhausen. But I really did this pretty much all myself and wrote the papers myself. In fact, I'll never forget the paper I wrote on the nickel ion—I had shown that you couldn't really account for its spectrum using the crystal field model. I knew that Ballhausen wouldn't like that because he very much wanted to stick with this simple model. He didn't want to give up the simple model.

So I did this calculation, and I left this report on his desk. I didn't really talk to him that much. We had tea—in Europe, you have coffee and tea in the morning, coffee and tea in the afternoon—and Ballhausen always sat in the professor's spot, and nobody would dare sit in his seat. Everybody else was sitting around him, but I really didn't talk to him that much. And I had dropped on his desk this report that basically was the last straw for the crystal field theory. He didn't talk to me for a month. I didn't think he was happy, but I wasn't sure; I couldn't quite read him. It turns out, he wasn't unhappy. Because finally I said, "What did you think of my report?" He said, "Oh, it's about to come out in the journal *Chemica Skandinavica*." [Laughter] I said, "What? It was just a report. Besides, the results are all negative. Who would accept that?" He said, "I've accepted it. I'm the editor of the journal." [Laughter] So it came out in the journal.

After that, Ballhausen himself was very supportive in pushing into this new area. Much to his credit, he was extremely critical of everything, including his own work; he did not hang on to stuff when he knew it was wrong. It was very clear he knew it himself, and he encouraged me to push into this new area. He did not try to hang on, like many people do. I must say, Linus was not very much like that. Linus would hang on. Linus would never admit anything he ever did was wrong, as far as I could tell. Ballhausen was just the opposite. When he figured out that he was wrong, he'd say, "This is ridiculous. How could I have thought this?" He was an interesting guy, a completely different character from Pauling. He liked to explore things, but when he found that he'd made some mistake, he would viciously attack himself.

So he encouraged me to move on. And I moved on with him. I did this myself, but in consultation with him. He and I published a subsequent article together, although I wrote it. When I wrote the paper, he came down, and—I'll never forget this. He said, "Harry, this is a very good paper. It's such a good paper that I'm putting my name first." [Laughter] So it's "Ballhausen and Gray" who published this new model. A few years later, we wrote the book on molecular orbital theory applied to inorganic compounds, which was very popular and helped establish this whole business. Later when I was at Columbia, we rewrote it. It was first written in Danish, and I helped translate it.

It's important, because this affected me tremendously. When I took my first job at Columbia, a lot of my early work there was developing this new model and explaining all kinds of inorganic compounds that the crystal field theory simply could not treat because it was limited to an ionic model. The more covalent the compound was, the worse the crystal field theory was. So I started working on compounds that were very covalent, like metal carbonyls, carbon monoxide complexes of metals, nitric oxide complexes of metal, and cyanide complexes of metal. These are things that the crystal field theory couldn't treat at all, because they are very covalent complexes. You know, it was like a toy store when I got to Columbia, because all of these inorganic compounds hadn't been treated. And I had a new model, I could treat them. So rapidly, I published a treatment of all of these things that the crystal field theory couldn't handle. I published paper after paper, showing how this new model could accommodate these complexes.

COHEN: How did you get to Columbia?

GRAY: How did I get to Columbia? That's a very interesting story. And once again, it's a very important story in my life. It's one of those fortuitous things. I've had various lucky breaks all along.

When I was at Northwestern, before I went to Copenhagen, I decided I would nail down a job for when I came back to the States. I applied for jobs, and I got several interviews. One was at Case [now Case Western Reserve University] in Cleveland—it might have been just Case Institute of Technology then. I got an interview at Vanderbilt in Nashville, which was close, of course, to my home—forty miles from our farm. And I got an interview at Louisiana State University in Baton Rouge. I got offers from all three places. But I didn't get interviews at the big places—Stanford, and I forget where else.

I decided to accept the job at LSU. I'm not sure why now. You would think I would have accepted the Vanderbilt offer, to get back home. But for some reason, I accepted the LSU job. I was just more impressed, I guess, with their department. I accepted the job before I left for Copenhagen, so I had a place to come back to after my one-year NSF postdoc.

So, I was developing all of this stuff and ready to go join LSU's department in September of 1961. But I was really plowing new ground now. And Ballhausen had a lot of connections around the world, and he clearly thought that I should go for the big time. I said, "Well, I've accepted this job at LSU, and that's where I'm going." But he figured I had the potential to go to the big-time at this point. Not that my Northwestern advisors, Basolo and Pearson, didn't think this. But there was a fellow named Robin Fraser working with Henry Taube at Chicago who was competing with me for jobs at the time I was interviewing in the States. He was getting the big-time offers, and I was getting the middle-level ones. I was clearly finishing second to him in these places. His adviser, Taube, was one of the world's great inorganic chemists. He won the Nobel Prize in 1983. And his top student, Robin Fraser, had published many more papers than I had as a graduate student, with Taube. His record looked better than mine, and he was supported by Taube. Well, that's the way it goes.

But at Copenhagen, though, I was plowing new ground and clearly emerging as the top guy. Every time a visiting person came through, Ballhausen would immediately bring this person to me, and he would say, "Harry, tell them what you're doing." I never got any of their names. I mean, what happens in Europe—you've undoubtedly seen this yourself—is that in these crowds, they sort of mumble and they never introduce anybody. You have no idea who you're talking to. And I went through this over and over. Ballhausen would bring everybody to me and drop them off. I didn't realize that probably in the back of his head was this scheme to get me a job at some big place. He would bring somebody in and say, "Harry, tell them what you're doing," like he thought I knew all these people.

So I did this I don't know how many times. I would tell them about my new model, my new theory, my experiments. [Laughter] Most of them would go to sleep while I was doing this. But this one guy he brought to me—it must have been in June or July of '61—seemed extremely interested, but I didn't know who he was either. He was asking really good questions, and he was obviously very sharp. He was all over me on this new theoretical model and the experiments I'd done and so on. We talked for about three hours. It was a very exciting discussion.

At the end of the three hours, he looked up at me and he said, "Harry, this is really terrific stuff. Would you consider coming to Columbia?" And I said, "I sure would. Do you know anybody there?" [Laughter] He said, "I'm Martin Karplus." I think I said, "Well, do you know anybody at Columbia?" He said, "I am at Columbia." Karplus was a Caltech graduate; he worked for Linus Pauling. He's one of our great Distinguished Alumni. He's a great chemist. He was a tremendous theoretician—everybody knew that at the time he was the leading guy at Columbia. I didn't know that. So he plugged me in. He said, "Yes, we'll make you an offer right away." I said, "Well, you know, I'm signed up to go to LSU." He said, "Well, you take it when you want to." I said, "Oh, really? Is that the way it works?" [Laughter] You know, no letter, no nothing. One week later, I'm offered an appointment—a letter from the chemistry department chairman, Charlie [Charles] Beckman, not Martin Karplus. But obviously, he'd sold the department. They hadn't interviewed me. Karplus must have gotten the department together and said, "There's this kid in Copenhagen. And we'd better do something." I'd never met Beckman. But he wrote me this letter, "Dear Dr. Gray, we'd like to make you an offer." It was an instructorship, at Columbia, for the princely sum of $5500 a year. [Laughter] But it was an offer at a top place one week after a three-hour discussion with a guy I didn't know. That's the way my offer came about.

So Ballhausen came to me and said, "You've got to take this. This is the big time. You're hot. If you go to LSU, you won't be able to do any of this kind of stuff. If you get into an environment like Columbia, you will go up. You've got to go there, Harry, I don't care what." He's leaning on me heavily. And I said, "Well, I have this obligation. I'm not a weasel."

So I wrote to LSU. And I said, "I have this offer from Columbia. I've got to take it at some point. I'm committed to you for a year of teaching or whatever you want. And I'll do that. But Columbia has already told me that they want me so much, that they're going to keep this offer open. And I'm going to take it."

They wrote me back very nicely, saying, "Go to Columbia now. We understand. We didn't think we could hang on to you anyway, after we interviewed you. We'd lose you anyway. So we wish you well." That was in August of 1961. One month later I started at Columbia.

COHEN: You didn't know New York City particularly?

GRAY: Not at all. And now we've got two kids. We've got to get to New York. I booked a flight on Icelandic Airlines. Shirley was going to go to Kentucky while I started this job. She was going to go visit her family and stay there while I tried to find an apartment and get started teaching. It's a long story, but we arrived. We were a day late in getting into New York on this flight, which was scheduled to take sixteen hours from Copenhagen to New York, stopping in Iceland. Instead, it had to turn back, and we stayed overnight in Norway. So we arrived in New York with two small children after forty-four hours. I've never forgotten this, because when we arrived, Shirley had to catch her flight to Louisville. And she was exactly on time, but exactly one day late. [Laughter] Because our flight had taken an extra twenty-four hours to get to New York. So I saw her off, and I took a taxi to Columbia, to start teaching literally the next day. [Laughter] I got a big room at 96th and West End at the Marcy Hotel. And now I'm in New York. And I'm literally teaching a course, and I don't know what it is, the next day. That's how fast all this happened.

So that's how I got to Columbia.

**Some material in this session was originally recorded during Session One.*

HARRY B. GRAY

SESSION 3

October 6, 2000

GRAY: So now I've just arrived at Columbia in 1961. I had no idea what I was going to teach or what I was going to do. I walked into the chemistry chairman's office there, and he assigned me a course. These things had come about so fast, as we discussed last time. By the time I got to Columbia, they had promoted me to assistant professor. [Laughter] From instructor at $5500, they said, "Congratulations, you're now assistant professor at $7000." [Laughter] So that was a great start, getting a promotion on the first day. This was the time when universities were converting from an instructor system to an assistant-professor system. I think in the old days, everybody started as instructor, and then went to assistant professor and associate professor and professor.

Anyway, they were converting, and they converted me on the first day. And Charlie Beckman said, "Welcome. How would you like to teach our special advanced freshman chemistry course"—a Columbia College course that was only for Columbia College students, only for men. Columbia College was all male. Most of the courses at Columbia could be taken by Barnard students, except for the classes that were old-time Columbia College courses. And this was one of them. They didn't let the women in. They said, "We're phasing this course out, and you're just the guy to do it. You've just arrived. You have no idea what you're doing. [Laughter] You won't hurt anybody, so we thought you'd be a good guy to phase it out. It's the last time we'll ever teach it."

So this was kind of an advanced placement freshman course, and I plunged right in. I mention this because I then decided I would do something completely different with the course and teach my new ideas about chemical bonding. Molecular orbital theory—I said, "I think I understand this well enough to teach it to these enthusiastic Columbia freshmen who are high-level. They already know enough chemistry by definition, because they're in this special course. So I will give them something that's not in any textbooks. I'll give them what I've been thinking about in this field, and I'll try to develop it at a level for freshmen."

Well, it was an experiment. I thought it might go over, and it did. The kids loved it. I got standing ovations after some lectures. It was incredible!

COHEN: Did any of the faculty come?

GRAY: Yes, I think so, from time to time. If you look at my first book, which is called *Electrons and Chemical Bonding*, which was written during this time, it's based on this course. It *is* that course. So I was just basically saying, well, this stuff is good enough to get into chemistry courses everywhere, because what was in the chemistry courses in bonding was, of course, the Pauling theory. Which was fine. But I felt that the molecular orbital theory and the ligand field theory offered lots of exciting things, like explaining colors of compounds and other things that people could literally see around them and relate to very nicely. And I said, "Gee, this will be fun. I'll give teaching this a try. I don't know whether this will work or not."

And, of course, I had no choice, because I was starting the next day. [Laughter] And what else did I know? [Laughter] They said, "Well, here's this big book on thermodynamics that we've used in this course. You don't have to use it." I started with a little bit of it, but it was clear that the kids in this course already knew a lot of chemistry.

So I went all out, threw everything away, and developed this new stuff in chemical bonding. And the kids just loved it! They were there, and they didn't miss a class. It was incredibly electric. Columbia students are just terrific, and they're interested in everything. They're very enthusiastic. There wasn't an empty seat in that classroom all year long. I've never had an experience like that. It was just fantastic! I guess that's why I've loved teaching so much, because my first real experience was so terrific—the response I got from the students, and everybody was digging in. It was like—"Okay, we're doing something really new with this young guy who's twenty-five years old." I wasn't much older than these kids. I was blasting ahead, and they just loved it! We developed all this stuff.

During that first year, I met a guy who really influenced my life enormously—Bill [William A.] Benjamin. He had been a book editor at McGraw-Hill. In fact, his uncle, Curtis Benjamin, was CEO and head of McGraw-Hill. Bill was a young guy at the time. He'd started his own publishing company, called W. A. Benjamin. He's had a very close connection with Caltech over the years and published many Caltech authors, including Dick [Richard P.] Feynman [Tolman Professor of Theoretical Physics; 1965 Nobel laureate in physics; d. 1988] and Murray Gell-Mann [Millikan Professor of Theoretical Physics, emeritus; 1969 Nobel laureate in physics]. When I met him, he already had this connection with Caltech people, especially with Jack Roberts. He and Jack Roberts really sort of started the company—I think Jack backed him in some way. I think he'd been Jack's editor at McGraw-Hill. Of course, I didn't know Jack Roberts at all at the time. Of course I'd heard of him—he was a great physical organic chemist. I had heard his name in my organic courses at Northwestern, so I knew he was a great guy.

Bill Benjamin dropped by my office during my first year of teaching this course with this new material. Bill was a good friend of Carl Ballhausen's, my Copenhagen advisor, whom we talked about last time. Carl probably told him, "You know, this kid is starting at Columbia. You should go by and see him. He may have a book for you."

So I was very impressed to have this very confident guy, Bill Benjamin, dropping by my office. He said, "I'm W. A. Benjamin. I understand you just arrived from Copenhagen. You're developing a lot of exciting new material in chemical bonding." [Laughter] I was blown away. I thought, how does this guy know this? He said, "I'm president of W. A. Benjamin." And he gave me his card. I didn't realize he was both the president and the only employee. [Laughter] It's one of those things. He'd broken off from McGraw-Hill. He and his wife, Orly, were really the whole company. He had a little office on Broadway, above a bowling alley, that he was operating out of. He was a very exciting guy because he knew sort of what everybody was doing.

COHEN: How was Jack Roberts part of this company?

GRAY: I think he was involved directly in helping to start it. Certainly his book *Nuclear Magnetic Resonance* and his little *Notes on Molecular Orbital Calculations* were the first couple of books, I think, in the company.

So Bill and I had lunch. Pretty soon, he'd signed me up to do a book based on my course. And sure enough, I did it. In fact, all of my early books were signed up by Bill Benjamin. At one point, he had signed me up for I don't know how many books I hadn't written. [Laughter]

My first book, which was also my first book for Bill, was *Electrons and Chemical Bonding*, based on my course. That became a bestseller in the field. And I must say, it influenced all of the introductory chemistry teaching in the country. I can say now that every freshman course—and certainly the first term of our Chem 1A course here at Caltech, is really based on what I taught at Columbia in 1961–62. It's all derived from that course and that initial book, which was published in 1964, made its way rather quickly into freshman books—the first one being *Basic Principles of Chemistry*, which I wrote at Columbia, with Gilbert Haight.

COHEN: Was Haight at Columbia?

GRAY: Haight was at Swarthmore. I had met him in Copenhagen, where he was on sabbatical in Jannik Bjerrum's lab. I was learning Danish in a high school in Copenhagen, and I met Gil and his wife, Shirley, in this class. Gil's quite a bit older than I am, and we became close friends. His work is close to mine in some ways. He's a tennis player—we play a lot of tennis together. When I was at Columbia, I would literally take the train from New York to Philadelphia on the weekends and stay with Gil in Swarthmore and write. Or he would get on the train and come up to New York and stay with us. We did this book together sort of on weekends, in between playing tennis and having fun together. And it was inspired, really, by my first book—that is, this new approach to chemical bonding.

COHEN: Now, where did you start making connections with Caltech people?

GRAY: It started with Jack Roberts. The first draft of *Electrons and Chemical Bonding* was sent to him to review. I'll never forget [laughter] the manuscript coming back, covered with Jack Roberts's typical red ink, saying, "This has potential but it's awful and it has to be redone." I've gotten used to that over the years. He's an incredibly good reviewer. But at the time, I looked at that and I was shocked. But it really energized me. I completely rewrote it. I added another chapter or two, which he thought would be a good idea, and shaped up the book. He was really instrumental in reviewing it and helping me with it. That was my first real connection with Caltech. I still had never met him, although I certainly "met" him through this red ink. To this day, he has helped me with things like this. He's a fantastic reviewer.

Electrons and Chemical Bonding was published in 1964 while I was still at Columbia. I dedicated it to my students in Chemistry 9–10. And it sold like hotcakes. It was one of a series. Bill Benjamin was a pioneer in putting out little books in paperback. His idea, actually, was to have a collection of paperback books that would make freshman courses interesting—rather than some standard text that they had to go through. So he did publish—I don't know how many—little paperbacks on chemistry. Mine on chemical bonding, one from Berkeley in chemical thermodynamics, and one on kinetics. He must have put out ten or twelve paperbacks. His idea was that teachers could put together the course they wanted by using these paperbacks, but that really never caught on. I think people still like to have a single book, to this day, in these courses. It was a very imaginative, interesting idea, but it never really caught on, although some of these individual paperbacks—and mine was certainly one of them—really caught on and sold a lot of copies.

COHEN: That was nice extra income for you.

GRAY: Yes, that was very nice at the time, because I wasn't making that much money. So that's how that came about.

I followed that with a book with Carl Ballhausen—a much higher-level book, based more on research, called *Molecular Orbital Theory*.

Benjamin had this interesting idea of publishing books with notes in them—sort of informal notes, with reprints of papers in the back. He did several of those with famous people at Caltech at the time, including, I think, Gell-Mann and Feynman. So I got to know people sort of through Benjamin, because I was working with him all the time.

I also wrote a book with Cooper Langford, who was a graduate student with me at Northwestern. He had gone on to London as a postdoc and then became a professor at Amherst. We were good friends. He came to Columbia for six months or so to work with me on ideas we had about reaction mechanisms of inorganic compounds, based on our work at Northwestern. I still was pursuing this at Columbia, and we had new ideas about how to formulate inorganic mechanisms. Organic mechanisms and nomenclature, like SN1, SN2, and stuff like that, had been formulated by Sir Christopher Ingold, a physical organic chemist.

But the inorganic reactions required some new concepts, something that wasn't SN1 or SN2. This was a concept that we called an interchange mechanism, where a ligand just interchanges rapidly with a ligand that's bonded to a metal. We reformulated mechanistic reactions for inorganic compounds and suggested nomenclature. Instead of SN1 and SN2, we suggested that there had to be three categories, which we called associative, dissociative, and interchange, and we defined those very clearly in this work. This was really an original work. It had stuff that had never been published. It was a little book—I liked to say it was the first book ever published that was cheaper to photocopy than to buy. [Laughter] It was a little, bitty book, but it was a book of original ideas of Cooper's and mine, which we'd been talking about, back and forth, for two or three years. He called me up one day from Amherst and said, "Maybe I should come for six months and we'll write this book."

We'd had these ideas, and we'd been expressing them a little bit in the literature. But we needed all of this to gel. So he came—I've forgotten what year, it's probably 1964 or something like that. The book was published in 1966. It's called *Ligand Substitution Processes* by Langford and Gray. I was quite productive in writing books in this Columbia period. There's four books: *Electrons and Chemical Bonding,* based on my course; *Molecular Orbital Theory*, with Carl Ballhausen, based on our new ideas; *Ligand Substitution Processes,* with Langford, based on our ideas on reaction mechanisms; and then this book with Gilbert Haight on *Basic Principles of Chemistry*, which really set a new tone for freshman chemistry and just completely changed things.

Basic Principles wasn't well written. We threw it together. Bill Benjamin, of course, had put a lot of pressure on us to get this thing out. He knew we were on to something. So we really published it before it was ready, in a sense. It was too original, had too much new stuff in it, and it wasn't that teachable initially. It was too original, and people couldn't handle it. They tried it for a year, and most people dropped it.

That's where Dick [Richard E.] Dickerson came in. Haight and I had introduced all kinds of new ideas into this book—maybe too many new ideas. Dickerson, who was here at Caltech, was a very good writer, and Bill Benjamin knew that. So he got him to really rewrite our book. It was a lot of our ideas, but he really put it together—smoothed it out, stitched it together quite beautifully. It came out in 1970 as *Chemical Principles* by Dickerson, Gray, and Haight, and it was very successful, for fifteen years at least, at the upper level of chemistry courses around the country. It's been through several editions.

COHEN: Were you running a lab at this time?

GRAY: Oh, yes. As soon as I arrived at Columbia, I, of course, wanted to start my lab right away.

COHEN: Now where did one get money in those days?

GRAY: Well, that's very interesting. It's not at all like nowadays. I mean, you had no money then. Nowadays, you have a big start-up package—which typically could be two or three- hundred thousand dollars, in some cases a half a million, or even more than that. There was no start-up in those days. [Laughter] Zero! I mean, I went into the chairman's office, and he said, "Well, Harry, we'll put you in this office up here and we'll give you this one lab over here. There's no start-up, no money, but we'll let you use the stockroom. There's a guy here named Marty [Martin] Friedlander, who builds things. You may want to build some instrument with him to get started. Here's an old spectrometer down the hall and you can use that." And I could check stuff out of the stockroom. If you took on a graduate student, they'd give you a small research allowance for that student—I believe it was $500 for supplies. So if you took on a student or two, you'd have a little account. And you'd start hustling for money pretty fast.

At that time, it was much easier than it is now. As a matter of fact, within a few days or a few weeks after my arrival at Columbia, I got a call from Oren Williams, who was director of the inorganic chemistry program at NSF. He called me and said, "Dr. Gray, I understand you just arrived from Copenhagen. I'd very much like you to send me a proposal because I'm very interested in funding your work."

Now, that doesn't happen these days. [Laughter] I wrote him a proposal that was really very derivative of stuff I'd already done in my graduate work. I sent it to him, and he funded it right away. I think I also applied for other small grants. And I raised some money pretty fast. But I was invited to; it was a completely new world. The field of inorganic chemistry—this modern kind—was brand new. They were looking for bright young people in the field because there were only a handful of us. The NSF was very supportive, and I owe a tremendous amount to them.

Of course they knew about me, because I'd had this NSF postdoctoral fellowship. So I already had this nice connection with NSF. And I had an NSF fellowship when I was a graduate student as well—one of these so-called "Sputnik fellowships." When Sputnik went up, Congress appropriated more money for NSF. [Laughter] And I think my NSF graduate fellowship came about because of Sputnik.

Again, Carl Ballhausen and various people had told Oren Williams about me. There weren't that many young people in the States starting up in new areas of inorganic chemistry. So Oren knew about me, and it was pretty clear that he was anxious to fund my proposal. It looked to me like if I sent in anything halfway decent, I was going to get funded. And I did.

So I was off to the races in my lab. I talked to the first-year grad students—just as we do here—and I told them some of my ideas and so forth. And three people signed up with me right away: Nancy Beach, P. T. Manoharan, and Raymond Williams. And then shortly thereafter, I got a postdoc: Ernie Billig, who was working at a company and just wanted to get away from it and do some research again. He came over to my lab and said he'd like to do a postdoc with me. All sorts of people were interested in working with me. This field was brand new, and people could sense it was exciting.

I assembled a group of five or six, or eight or so, within the first year or two, working away in the lab and doing both theoretical and experimental work. So I was really furiously trying to raise money so I could support all of this. I developed a very active group within the first year and we were doing all sorts of things and publishing in both this new ligand field- theory business and in inorganic synthesis.

We got into sulfur-containing compounds—a ligand system that's now called dithiolenes—a system with two sulfur atoms in a kind of a chelating system. We got into that, started making new compounds, and discovered that they stabilized all sorts of strange oxidation states and structures. We just sort of happened upon that, and we began making new compounds right and left that had new properties and publishing a lot on it. So on the theory side, we were developing all kinds of new ways to make the ligand field theory approach apply to real chemistry, to real chemical compounds that people were interested in. And on the other side, we developed a lot of new inorganic syntheses—a lot of new compounds with unusual structures, unusual bonding.

So I had two interesting things going, both of which were really paying off very successfully. We published quite a few papers in my first year-and-a-half. My first group was great, and everyone in it was very active.

Interestingly, most of them were older than I was. [Laughter] I was twenty-five when I started. Ray Williams had been some place, and P. T. Manoharan had come from India. It was always an interesting group of folks who were all about the same age, working together. And soon I picked up several other students—Ed [Edward I.] Stiefel, Richie [Richard] Eisenberg, and Bob [Robert C.] Rosenberg, and Zvi Dori from Israel—and developed quite an exciting group. Most of these people have gone on to very impressive careers. Rich Eisenberg's a big shot and professor at the University of Rochester, and editor of the journal *Inorganic Chemistry*. Ed Stiefel [d. 2006] is a research director at Exxon and has won national awards. And Zvi Dori became a famous professor at the Technion in Israel. All of these people went on to big-time careers. My first student, P. T. Manoharan, became vice chancellor of the University of Madras. So it was an exciting time when we were developing a lot of new areas of inorganic chemistry.

It was going so well that after a year-and-a-half, I got a letter from the University of Chicago, offering me a tenured position. I was twenty-six, twenty-seven, something like that. I was pretty young. I had been invited to Chicago to give a talk. Of course, I had no thought of tenure right away at Columbia. I mean, I felt I would be in there for a few years and fighting: It was tough to get tenure in the Ivy League at that time. It wasn't even clear you could get tenure in chemistry at Columbia. There were eight assistant professors at the same time I was there. We knew there wasn't more than maybe one permanent job—or maybe two.

So I was invited to the University of Chicago to give a talk. I was invited to several other places to give talks too. And I didn't realize that people were really looking me over. I was a bit naïve, I guess. I was invited to Chicago by a very famous guy there by the name of Jack Halpern. He had been at McGill in Montreal with Rudy [Rudolf] Marcus and Sam [Samuel] Epstein. Jack was one of the leaders in the mechanistic field and a very tough guy, actually.

I upset Jack during this Chicago talk. I'll never forget this. I gave the value of a rate constant for a reaction as zero. And Jack knew—and I knew and everybody knew—that a rate constant had to have some value. It could be very small, but it couldn't be zero. [Laughter] So Jack challenged me on this. He said, "Well, Harry, you know very well that it can't be zero." And I looked at Jack—he was always on the attack. So I decided I would just put him on a little bit. If I'd known they were looking me over for a job, I would have bowed and scraped. [Laughter] But instead, what the hell, I didn't care.

So I said to him, "Jack, I measured that very, very carefully. It's zero." [Laughter] And he got very flustered. He got red in the face. I think he knew I was putting him on. But I could sense the chemical physics faculty at Chicago pulling for me—because Jack always attacked and people usually just sort of melted when he did and just gave in completely. Here's this kid who's not giving in.

COHEN: And you knew you were wrong.

GRAY: Yes, I knew I was wrong. He knew I was wrong. Everybody knew I was wrong. But I wasn't giving in. So I could sense this support coming from the chemical physics faculty, saying, "Okay Harry, stick to your ground." [Laughter]

Anyway, I did. After the seminar was over, Jack said, "Harry, you know you can't…" and I said, "Yes, I know, Jack, but I thought we'd have some fun with that."

[Laughter] And then he looked at me and said, "How do you like Chicago?" I said, "Why?" He said, "Well, if you like it, what do you think about living in Chicago?" I should have known something was up.

I think Jack liked to be taken on, too. He didn't like people just crumpling. He liked somebody who was going to stand up to him—and I was certainly going to do that. [Laughter] There was no way I was going to be intimidated.

So nobody said anything about a job or anything. And I went back to Columbia. Then literally two weeks later—it was sort of like the job offer I'd gotten from Karplus in Copenhagen—a little letter comes—no phone call. The faculty must have gotten together. The letter came from the chairman of the department, Norman Nachtrieb, a wonderful, gentle, kind man.

Now, I'm in the middle of only my second year when his thing comes in, saying, "We enjoyed your visit very much. We'd like to offer you an associate professorship with tenure at Chicago at a salary…" As I recall, it was $13,000 a year, and I was making $8000. And now Chicago was offering me tenure and $13,000. I fell over!

I went home and said, "Shirley, pack your bags. We're going! $13,000 at the University of Chicago, with tenure." [Laughter] I had enormous respect for the University of Chicago and for Halpern. He was and still is really a fabulous inorganic chemist. He was somebody I knew I could really talk to and who could teach me a lot.

And, of course, I was the only inorganic chemist at Columbia. So I thought, "Here's a colleague I could have at Chicago, who's very sharp and about ten years older than myself, and it would be kind of a perfect interaction. This is a wonderful opportunity. I'll have a colleague. I'll have tenure. I'll have a salary. [Laughter] We can get out of debt. We're out of here!" Because I figured I didn't have a good shot at tenure at Columbia—for all the reasons I mentioned. I hadn't really thought about tenure there. When I took the job, I was just happy to have it, and I was having a lot of fun. Tenure really wasn't in my head until this letter came, and I realized that here's a *real* job.

I'll never forget dropping into the chairman's office—Charlie Beckman again—and saying, "Charlie, guess what? I got a tenured offer on a job at Chicago." [Laughter] I thought he'd congratulate me or something. Instead he turned absolutely beet red. I thought he was going to pass out. He said, "Let me see that." [Laughter] Then said, "I'll get back to you." So in a week, Columbia offered me tenure.

COHEN: How about the $13,000?

GRAY: No, they didn't match that. They gave me a little raise—I forget what—but they did promote me to tenure right then, by calling around and getting three or four people to write some letters or something. They said, "We can't let you go." The faculty were all very supportive. It was a little embarrassing because I had colleagues—assistant professors that I was with—many of whom were as good or better than I was. And I got this tenured job because I'd had this other offer. You know, it's the way it goes. It's the name of the game. It did set me a little bit apart from the others. You know, it's a strange world. Of course, it changed everything. We had been living in this little apartment on the corner of Riverside and 116th. We'd been on the list for a Columbia apartment, which was bigger and more affordable, since the day I arrived. We had never moved passed number fifty-five, or something, on the list. And then I was moved to number one on the list—a wonderful apartment.

So they offered me this 29 Claremont Avenue apartment and tenure and a little bit of a raise, to stay. And that was it. I loved my colleagues at Columbia. We loved New York. I felt—and I still feel—enormously loyal to Columbia because they gave me my first chance to do something. You really have to have a pretty good reason to abandon a group of people who have given you your shot. And, of course, by this time, we were very close to most all the faculty.

HARRY B. GRAY

SESSION 4

October 11, 2000

GRAY: After I decided to stay at Columbia I kept working on the ligand field theory and on synthesis of novel compounds. And things are going great. I've written these first books that are about to come out—*Electrons and Chemical Bonding*, *Ligand Substitution Processes*, *Molecular Orbital* Theory, and *Chemical Principles*. They're all in the works. I'm also going to a lot of meetings and giving talks. Things are going really well.

Then, I guess, what happens is that many other universities start trying to recruit me. I hear from Caltech—Jack Roberts writes me a letter.

COHEN: This was before you met him, even?

GRAY: This was before I met him. But he had reviewed the first draft of my first book, as I told you [*Gray 2000–01, Session Three*], and had written all over it. So he certainly knew about me. And Caltech clearly was interested in getting someone in this new field.

COHEN: What year is this?

GRAY: This is 1964. Actually, I think Jack Roberts wrote me a letter in late 1963, right after I'd gotten tenure. He was chair of the chemistry division here and he wrote, inviting me out to give a couple of lectures. Caltech really didn't have any modern inorganic chemists—and neither did most places. Mostly they had sort of old-fashioned, inorganic chemists. Very few universities had inorganic chemists who were working in these new areas—the so-called renaissance of inorganic chemistry.

So Caltech was clearly interested in looking over a few people. There weren't that many people to look over. I know that they invited Dick [Richard H.] Holm, who was at Harvard, out for a couple of talks, and I'm not sure who else. And in my usual modest way, when Jack Roberts invited me to give two lectures, I said, "Well, I can't possibly tell you all that I'm doing in two lectures. I insist on three." [Laughter] I'll never forget that. Roberts already had an impression that I wasn't going to be an easy guy. You know, "What are we getting into? This kid wants to come and give three lectures. I've invited him for two, and he insists on three. He can't even begin to touch the stuff he's working on in a mere two lectures." [Laughter] So I'm already coming on strong.

At the same time, Stanford started to express considerable interest in me. And there were probably several others. But Stanford and Caltech were the two places that I really was starting to consider. So I accepted this invitation to come out and talk. I'd never been to the west coast.

I came out in February of '64 to give these three talks and spend three days here. And I'll never forget the time, because the day I came out was the day of the famous Cassius Clay [Muhammad Ali]–Sonny Liston heavyweight championship fight. I had quite a bit of money on Cassius Clay. He was from Kentucky, and I knew of him. But Sonny Liston was the big favorite. I was very upset that I wasn't going to be able to listen to the fight because it was during the airplane ride. And when I got out at LAX for the first time, I just heard that Clay beat Liston. I think I had Clay at about 10 to 1. So I was extremely happy. [Laughter]

I got in in the evening, and I was picked up at the airport by the famous Huibert Barleeus, who's a Caltech legend. I was very impressed because Huiberts Barleeus looks like—and is—a very classy guy. He drove me out to the Athenaeum to stay. I had a nice chat with him. It was all very pleasant. I have several wonderful recollections from that trip. One was meeting Norman Davidson [Chandler Professor of Chemical Biology, emeritus, d. 2002]. Of course, I met Jack Roberts and George Hammond for the first time, and Wilse Robinson, who recently passed away. Wilse made a tremendous impression on me because he was so gruff. He wanted to know why in the world anybody would work on the stuff I was working on. What was it good for?

I gave three talks. Clearly, George Hammond was extremely taken with me, I could tell, and was very excited. I was meeting all these people really for the first time. I knew them by reputation only. I could tell Hammond was grabbing Roberts and shaking him, saying, "We've got to get this guy." [Laughter] George Hammond was really the guy who pushed very, very hard to get me here, and kept after me, even when I eventually turned Caltech down.

I had a wonderful meeting with Jack Roberts. He and Edith had me up to their house. And I went back with a very good feeling about Caltech. I'm not sure whether Jack Roberts propositioned me during that trip or whether it was shortly thereafter. But the upshot of it was that they invited me to come out for a quarter and basically teach a course in this new field, which they didn't have.

The "inorganic chemist" at Caltech was Don [Donald] Yost. Yost was a great man, but he hadn't done this kind of modern inorganic chemistry. In fact he had long since sort of given up inorganic chemistry and was teaching a very strange course—some kind of mathematics or something. He was doing his own thing, but he certainly wasn't paying attention to this new field. He was retiring anyway, or had retired. So there was a big gap here in this area, and Jack Roberts wanted me to come out and build it up.

Shirley and I and the kids came out in March of '65, and I taught a course here in the spring quarter. We got a nice house way up in Linda Vista. I had a cool car—maybe a Mustang. I think Lea Sterrett—Jack's assistant in the chairman's office—rented it for me. They got the house for us, and so forth. I would buzz in every day, down Linda Vista and down Orange Grove a bit, maybe down California, into Crellin-Gates. This was before any of these other buildings had been built. Noyes was being planned at the time. And Holmes Sturdivant was holding forth in chemistry as sort of Jack's assistant with buildings and laboratories and so forth.

Linus [Pauling], of course, had departed—he left in '62. Harden McConnell was still here. He had accepted an offer to go to Stanford and was about to depart. Sunney Chan [Hoag Professor of Biophysical Chemistry, emeritus] was coming to sort of replace him. But Linus had left.

But I did meet Linda [Pauling] Kamb and Barclay Kamb [Rawn Professor of Geology and Geophysics, emeritus; Caltech provost, 1987– 89; d. 2011] on this first trip because they were very close to Norman Davidson. The old chemistry gang were very, very nice people. We took an instant liking to everybody here.

I enjoyed the students and did some experimental work myself while I was here. I got along famously with George Hammond—and with everybody, for that matter—Jack

Roberts, Wilse Robertson, and Aron Kuppermann especially. The Kuppermanns were always extremely good to us.

During this visit, Jack made me a permanent offer. He said, "We really want you here. We've talked about it." And I said, "Well, I'll think it over; I'm very loyal to Columbia." I remember everybody recruiting us, and it was extremely pleasant.

I went back to Columbia in June of '65, thought it over and turned down the offer. I wrote a letter to Jack saying I thought it was a wonderful offer. "I really love you guys and would love to be there, but I really can't see myself leaving Columbia. My friends are here." I was very loyal to them. So I turned it down.

But George Hammond kept after me. He was flirting with MIT—they'd offered him the chemistry chairmanship there. He came to visit us in New York in the fall. We had become really close friends—we still are—and we were working together on a number of projects. One of his projects was a new chemistry curriculum, and I was very intrigued by it.

In New York that fall, suddenly he said, "Well, if you don't come to Caltech, I'm going to go to MIT and be chairman." I said, "Well, we really need to work together." So somehow, in the midst of all of this, we decided we'd call up Jack Roberts, and I said, "Jack, if you'll make me that offer again, I'll take it, because there are a lot of things I want to do with George."

There were some things as well that I had realized I couldn't get done at Columbia. I needed more lab space. I needed to interact with biologists and physicists because I was starting to think about going in biological directions. There was some physics work going on at Caltech—building a quantum magnetometer that would make magnetic measurements on very dilute magnetic samples, like in proteins—that I wanted to get involved in. So there were a number of reasons that led me to change my mind. But the main reason was George and I saying it would be nice to be together, working on some of these things.

So Jack reissued the offer, and I took it—sort of on the spur of the moment. Then I had to walk into Columbia, after turning Caltech down and tell them, well, I'd really decided to go anyway. That was very hard.

COHEN: Did you have anything to do with Lee DuBridge [president of Caltech, 1947–1969; d. 1994] during all this?

GRAY: Yes I had a lot to do with Lee DuBridge and a lot to do with [Robert] Bacher [professor of physics; Caltech provost, 1962–70; d. 2004]. I was very impressed with them. You see, I'd had very little to do with the Columbia administration. The president I barely knew. I did know David Truman, the dean of the college, who was a very close friend and very supportive of me on several occasions. But the Columbia administration overall was much more distant from the chemistry department faculty than at Caltech. I obviously loved the Caltech style—a small place where you really got to know everybody. I had talked to DuBridge several times during this visit in '65.

COHEN: But you were coming here as the big shot. You went to Columbia as wet-behind-the-ears, starting out.

GRAY: Yes, that's right. But I was very impressed with DuBridge, and I was very impressed with Bacher. It turned out that one of the pieces of work I did in theory in Copenhagen actually took off from one of Bacher's papers. Parts of the ligand field theory take off from atomic physics. So I'd actually redone one of Bacher's papers and reassigned some atomic lines that he had on the nickel spectrum. So he was absolutely bowled over by this chemist who walked into his office and who started talking to him in great technical detail about one of his papers. I think he said, "Gee, maybe we better get this guy. He actually knows some physics. And he knows my paper." [Laughter] This was just an accident, but I took a great liking to Bacher and DuBridge, and I talked to them a lot during this trip.

As a matter of fact, at one point during all this, I was in this clash with Columbia about the fact that they had planned to build a physics building on their tennis courts. I was upset with this, and I'd like to note in the record that I left Columbia *the day* that they broke ground on the Barnard tennis courts. I'm a man of principle. The day they broke ground is the day we left. I'm a man of my word. [Laughter] So at Caltech I had DuBridge promise me that while he was around, he would never allow anyone to build a physics building on the Athenaeum tennis courts. I mentioned that, I think, in my tribute to DuBridge at his memorial at the Beckman Auditorium. I did get to know Lee very well, and I interacted with him a lot. A lot of those interactions are recorded in a piece that *Engineering & Science* published on the memorial, including the tennis court story. [Laughter] I have a bunch of tennis court stories—like, a good set of tennis is worth at least six physicists, and so on. [Laughter] But the answer is yes, I really got to know Lee. That had a tremendous effect on me: that people here really work together and like each other. There seemed to be a spirit here with the faculty and administration that I didn't sense at Columbia. [Laughter] Of course, a little bit later Columbia fell apart because the students felt so far from the administration that they decided to take it over and stage a sit-in. This was during the riotous Sixties.

COHEN: But you were gone by then.

GRAY: I was gone by then, but even before that, you could see this big gap between the administration and the working folks—the students. That was the first place the students really took over. Those protests led to the downfall of Grayson Kirk, and also, unfortunately, David Truman, who was a great man who should have been president of Columbia, but this whole uprising basically finished him off there as well.

But the thing that impressed me most when I visited Caltech was the staff—the secretaries, the stockroom people, the janitors. The regular people were extremely helpful. I mean, they went out of their way. The Columbia gang, it was more like a job at Columbia. There was a certain type of interaction that one had with secretaries here. There was a spirit of sort of everybody pulling together. I was very taken with this family spirit, particularly with the staff. I still feel that way. Every morning I come in, the first thing I do is go see Yolanda, who takes care of the fourth floor. [Laughter] And we talk.

I think in the end, these factors sort of came together, and we decided to come out.

COHEN: Was the weather a factor?

GRAY: No. To tell you the truth, we liked New York better than Southern California. We love New York. We loved to live there. Of course, the weather was a bit of a factor in that I love to play tennis. But on the other hand, in the 1960s, the smog here was really terrible. Things are much better now, but it was really awful then, as you may remember.

But it was really the people—the opportunity for a lot of interactions with some physicists and some biologists and George Hammond on some things. I was going much more multidisciplinary even then. The record shows that I have published with half the faculty here in terms of collaborations. When I came, I published with several people in low-temperature physics. It was Jim [James] Mercereau's group back then. And there was a guy named Massimo Cerdonio and Run Han Wang. There was a bunch of physics kids here that I interacted very strongly with and published with and did some experiments with iron proteins, which I think were very important. Even before coming, I could see that I'd have much more opportunity to do that here than at Columbia, where the chemistry department was fairly well isolated from other things. It's a wonderful family on its own, but here, I knew I'd have more opportunities to interact. So that played a part in coming out here.

So we came out in June of '66. The transition to Caltech was very important for scientific reasons, because I felt I could get into some new areas, like the biological aspects of inorganic chemistry. And inorganic photochemistry because George Hammond was a big photochemist. I'd done spectroscopy, and I felt that with George Hammond's influence, I could get into the applications of spectroscopy to photochemistry. I really started two new areas when I came to Caltech. One was inorganic photochemistry and the other was getting into the biological aspects of inorganic chemistry and the metalloenzymes and electron transfer, which is what I've been working on for a long time.

At the time, very few people were really doing quantitative work in these areas. But for me, they were logical developments from ligand field theory and spectroscopy. I had already had a big push on at Columbia because of my interaction with Rockefeller Institute, which was in the process of changing its name to Rockefeller University. There was a famous president there by the name of Detlev Bronk, who really ran the place with an iron hand. He made all the decisions personally. He even picked the graduate students. It was a beautiful place and a very interesting operation, the Rockefeller Institute. It had a big impact on me, because its students were biologists. There are a lot of very famous biologists now who were at Rockefeller at the time. I was invited there to direct a thesis when I was at Columbia. It turned out that a young man from Harvey Mudd, by the name of Alan Latham, had been accepted there for graduate work. He didn't realize that the place was all biology. He actually thought they had a chemistry department, because Bronk told him they had. Bronk would call around to all of his buddies—presidents of classy universities—and say, "Do you have anybody there who's good enough for the Rockefeller? I'll take him." The president of Harvey Mudd was an old buddy of Bronk's, and he said, "Yes, we've got this really great young chemist by the name of Alan Latham. How's your chemistry department?" Bronk says, "Oh, we've got a great chemistry department." [Laughter] He had exactly one guy in the field—Ted Shedlovsky—and he was retired. So Bronk recruited Alan Latham, who wanted to do chemistry. The Rockefeller grad students were like royalty—they had full scholarships; they had their own budgets for research. It was a very classy operation. Sort of a super version of Caltech—even smaller, with something like ten or fifteen graduate students total.

Well, Latham shows up at Rockefeller with a giant piece of platinum that he had gotten from his family, because he was married to a gal whose father was big in platinum mining in Alaska. His father-in-law had literally called him in before he went to Rockefeller and said, "Here, my son, go do platinum chemistry. Here's some platinum. Go do good work with it." [Laughter]

So Latham showed up with this enormous sample of platinum and told Bronk that he wanted to do platinum chemistry. And Bronk had no idea that platinum was an element; he had no idea what he was talking about. [Laughter] He frantically called Shedlovsky—a very prominent electrochemist who worked with platinum electrodes—and said, "Ted, what are we going to do? A grad student shows up and wants to do chemistry." [Laughter] Ted freaked out, called his buddy Jack Miller at Columbia and said, "Jack, what are we going to do? Some kid has just shown up and wants to do platinum chemistry. Is there anybody at Columbia we can hook him up with?" Jack said, "Yes, there's a kid who just arrived here from Copenhagen, and about half of his work up to now has been on platinum complexes." [Laughter] So Shedlovsky called me from Rockefeller and said, "Professor Gray, I'm desperate. I understand you know a lot about platinum. Would you come over here and direct the thesis? By the way, I'm also inviting you to lunch." [Laughter]

So I went over to Rockefeller; it was wonderful. Everybody ate together at lunch—the students, postdocs, faculty. It was a wonderful place, a wonderful spirit. They offered me the adjunct professorship on the spot. [Laughter] And I met this guy and directed his work.

I'm telling you this story because it influenced my whole career. I directed Latham's thesis in platinum chemistry, and Rockefeller asked me to give my modern inorganic chemistry course again. I gave it there for several years. Even when I got to Caltech, I was still going back and spending a month at Rockefeller, giving this course.

What happened was that I ran into all of these bright biologists who were taking this course to learn about metal ions. They started bugging me, saying, "You know, Harry, you really should work in this field. You have all this to offer—you know all about ligand field theory and spectroscopy and all of this stuff. And there are all these metals in biology—hemoglobin has iron; myoglobin has iron; cytochrome oxidase has iron and copper—and all of these enzymes are working with metal. Why don't you get in here and figure out what's going on?" And I said, "Gee, maybe I will." [Laughter]

I started to think about it. I started a few little things here at Columbia. But having this sort of push from the Rockefeller students, I could see that there was an enormous area that an inorganic chemist could work in if the inorganic chemist learned a little biology. Many of these Rockefeller students have become very, very famous now—Jerry [Gerald] Edelman, Phil [Phillip] Sharp, Chris [Christopher] Walsh—a great bunch of great scientists. I was learning from these people, and they were learning from me. I decided that when I got to Caltech I would learn some more from biologists here and go full blast in this business. So I started really setting up serious work in biological inorganic chemistry.

COHEN: Now you were in the same building with the biologists, weren't you—in Church?

GRAY: Well, I was when I visited for the spring quarter. But when I came out permanently, I had an office on the first floor of Crellin and a lab in Spalding. They were building Noyes. As a matter of fact, when they were recruiting me on the three-month visit in '65, Holmes Sturdivant came in one day and said, "Here's some space. Why don't you design your labs. We're going to give you the third floor of Noyes." And they did. I designed a bunch of labs and so on, but when I came out, they weren't ready yet. So I started out in Spalding and Crellin. That worked fine until we moved into Noyes in '67, '68. So here we are.

Now, at the time, I'm still doing some of my old stuff. But I'm launching off into new areas, mainly because of this push I had from the Rockefeller people and just my own thinking about where to go. I'm also starting to think seriously about going into photochemistry because of George Hammond. That was settled for good when a very brilliant young fellow by the name of Mark Wrighton showed up from Florida State and decided he would be the one who would put George Hammond and Harry Gray together in photochemistry to do inorganometallic and organometallic photochemistry. So it was Mark Wrighton, George Hammond, and I who really launched that effort. But it was Mark's initiative.

COHEN: Was he a professor here?

GRAY: He was a first-year grad student—a brilliant grad student. He's now a distinguished alumnus. But it was Mark, with his collaboration with George Hammond, who really got me into inorganic photochemistry work for a long time. He didn't even do a postdoc—he went immediately from Caltech to a faculty position at MIT. He later became department chairman at MIT, then he became provost of MIT; and now, he's chancellor of Washington University, St. Louis. He did okay—a tremendous success story, a brilliant young scientist. Even today, after all of this and his enormous career, he's about fifty years old. He's one of the most impressive young chancellors in the country. But he has left science to pursue this sort of career.

What do you want to talk about now?

COHEN: Well, it's 1968. DuBridge is leaving. We've got a new president. Is this affecting you in any way?

GRAY: Yes. I'll never forget DuBridge leaving to go to Washington in '68 to become the science advisor to Nixon. And I'll never forget the goodbye talk he gave in Beckman Auditorium. Then, of course, Harold Brown [Caltech president, 1969–1977] came. I had a strong interaction with Harold Brown, mainly through tennis. He's a tennis player and so when he came, he instantly latched on to me for tennis games. I played him regularly all the time he was here—I think basically once a week.

But he was the opposite of Lee DuBridge, who was always cracking jokes and having fun. Harold was quite formal. Harold would always have his secretary call me to arrange tennis games. And Harold would barely talk to me. I'm not saying he wasn't friendly—he was quite friendly. But he was shy or something—I'm not sure what. He didn't shoot the breeze the way Lee DuBridge did. So it was quite a different administration. I do think he was a wonderful president. He did a lot of great things for Caltech.

COHEN: He started the social science department.

GRAY: Yes. And he's an incredibly smart guy. My impression of Harold Brown was as an unbelievably brilliant man who had everything in his head—all the facts in his head. I'll never forget faculty meetings where, you know, somebody would try to say something or other that had some fact in it, and Harold would correct them. I mean, you couldn't catch him on anything that involved numbers or facts. I think the faculty had great respect for him. I certainly did. Because he really knew his stuff. He didn't give you a lot of BS. What he said, he could back up. I think he was a great president, although he wasn't a warm and fuzzy kind of guy.

I would always beat him at tennis. He was very upset with that. The only time he got me was in '69 when Shirley and I were in India for two months and I came back in terrible shape. I'd lost about twenty pounds, I couldn't eat anything, and I was sick as a dog. He made sure he arranged a tennis game through his secretary for me the minute I got back from India. I'd just gotten off this terrible thirty-hour plane flight from New

Delhi to L.A. through—I don't know, everything—Hong Kong, Tokyo. It took forever, and I was sick as a dog. And here was a note for me to meet Harold Brown immediately the next morning for a tennis game. [Laughter] I don't think he won the match, but I think he took a set from me. [Laughter] Because I could barely hold the racket.

COHEN: Coming back to the chemistry department, who was running the shop then?

GRAY: Well, when I came out, Jack [Roberts] was running the show. And soon, George Hammond became chairman. He and I were engaged in lots of activities together, including a very major curriculum-revision activity. That's one of the reasons I came out. I was interested in George's ideas for a new chemistry curriculum. His point was that the old disciplines were way out of date—physical, organic, inorganic, analytical, and so forth. We were still teaching courses in these areas. That was thirty-five years ago, and, of course, we're still doing it today. [Laughter] It hasn't changed.

But George was way ahead of his time. He had this love-hate relationship with Jack Roberts. They were competitors in a way—they were good friends certainly, but they were highly competitive. Jack's a conservative chemistry kind of guy who wants to stay with the old-time religion—for very good reasons. George is a real go-go guy, who wanted to just drop everything we were doing and change to a curriculum that was based on chemical structure, chemical dynamics, and chemical synthesis. He said, "Look, organic and inorganic chemists are all sort of blended together; the field's become really multidisciplinary. It's about time we recognized it." I was very taken with this. So he basically recruited this young, brash guy to go out there and make it happen, and be his right-hand guy.

So we initiated the Hammond curriculum at Caltech [See also *Gray 2016, Session Two*]. He became the chairman of the division somewhere in there.

COHEN: Were the rest of the faculty happy with this?

GRAY: The faculty were mixed on it. Jack was dead set against it, and some others were as well. But George and I, and some others, were for it. Several places around the country liked it, and they decided to try it. So it wasn't only Caltech.

So Jack Roberts allowed us to try it—you might put it that way—if we would teach the courses ourselves on a trial basis, with some students who would volunteer, and keep our regular courses going at the same time, as much as possible.

It was quite an exciting experience. But, to tell you the truth, it failed because George's structure and dynamics and so forth, are also arbitrary divisions, and it turned out that they really weren't any better than the standard ones. I will say this for the regular chemistry curriculum—the courses have been developed so much that they're technically superb. They're extremely good courses. There are lots of good textbooks and so forth. It was just too much to ask for two people to develop an entirely new curriculum without all of these materials and people not understanding, really, how to teach them elsewhere.

But this experiment was interesting, and it led to a number of changes, particularly in laboratory courses. Laboratory courses at Caltech in synthesis and things like that became multidisciplinary, and they still are. There were some very positive things that happened in this curriculum-revision experiment. But the experiment overall failed; it just didn't last. It didn't catch on, and still hasn't. To this day, we have pretty much the same curriculum that we had when Arthur Amos Noyes and Ernest Swift [professor of chemistry, emeritus, d. 1987] devised it. [Laughter] And there's nothing wrong with it.

We have instituted a course called Chem 10, which allows students to listen to seminars on research in their first year. These days, students get into labs and do research right away, and that's the way they get into the multidisciplinary stuff. The courses are probably just fine—they're technically fine, and they're good at training. I don't really see a strong need now, frankly, to change the curriculum so much. But what we do need to do is to let high-school students and college students know about some of the exciting things out there in chemistry, and let them get involved with it themselves. I think that by getting students into research very early we've sort of solved the problem that George was worried about in the 1960s. I feel that today the chemistry people, and the courses themselves, have become a lot more multidisciplinary, without actually changing the curriculum.

GRAY: I think we had a big influence, now that I look back at it—although at the time, it looked like it was a big flop. After three or four years, we just couldn't go on. George was pretty frustrated himself, and that was one of the reasons he left Caltech. He was chairman of the division, wanting to do these things, and he felt like the conservatives were sort of holding him back. He reacted a little in a Lee Hoodish-type sense—Lee also felt that Caltech was kind of holding him back when he wanted to do bigger and bigger things. George wanted to do bigger and bigger educational experiments, and to experiment outside of his standard field of organic photochemistry. He felt Caltech was just too conservative for him. That's why he stepped down as chairman and went to UC Santa Cruz in '72 as the vice chancellor of natural science. He wanted to play in a bigger ballgame and have a bigger impact. When it came to his curriculum experiment, he felt that Jack and others here had sort of put him down. I didn't feel that way, to tell you the truth. I felt like it was an interesting experiment, and I had a lot of other things going as well. But George took it personally. I think he felt that it didn't succeed mainly because nobody was pulling with him at Caltech; they were kind of pulling against him. And so for a variety of reasons, he went to Santa Cruz. I was sorry to see George leave. In my opinion he overreacted to all of this stuff. But that's the reason he left. He felt that he had to play in a bigger ball game.

Then, he went from Santa Cruz to Allied Signal. He became a big research director in industry before he retired. He stayed involved in research and playing in ball games, consulting with various universities and people, and still having a big impact. But it was sad to see him go.

That's when John Baldeschwieler [Johnson Professor and Professor of Chemistry, emeritus] came in as chairman, from Stanford. That was sort of negotiated. The year that George left was the year that Shirley and I went back to Copenhagen on sabbatical. That was also the year we spent several weeks in South Africa by invitation of the students there. And that was the year Noah was born—we went back for another kid in Copenhagen.

Copenhagen was kind of a transitional year when I was thinking about getting much deeper into inorganic photochemistry and biological inorganic chemistry and to really build on what I'd been doing. I'd sort of gotten things going at Caltech, but I needed a year off just to really think about which way I wanted to go. It was a very productive year. When I came back, I was really hitting on all cylinders in these new areas.

Of course, Jimmy Carter was elected president in '76, and Harold Brown left. And Murph [Marvin "Murph" Goldberger, professor of theoretical physics; president of Caltech, 1978–1987; d. 2014]] came as president.

COHEN: Did you have anything to do with that, with Murph's selection?

GRAY: I'm trying to remember how much I had to do with it. I was certainly very much for Murph. I just love Murph. Murph is my kind of guy. He's fun to talk to. I'd gone through Lee DuBridge, who was fun, and Harold, who was more formal—but I liked them both equally well. And Murph I could really talk to. He and I both have the same kind of sense of humor. He came in 1978, shortly after I became chairman of the division [of chemistry and chemical engineering]. When I became division chairman, [Robert] Christy [Institute Professor of Theoretical Physics, emeritus; Caltech provost, 1970–1980; acting president, 1977–1978, d. 2012] was acting president because Harold had left. So, at first, I was really reporting to him. Then when Murph came, I really hit it off with him. I think Murph was great, and I still do. He had a tremendous impact on chemistry. When we wanted to do a lot of things, Murph was extremely supportive. You could say, "Murph, here's what we'd really like to do," and you could take him down to the Burger Continental Restaurant and get him together with a bunch of chemists. He was fun to work with. You would never in a million years get to first base with Harold Brown with this kind of approach. You had to approach him in a very formal way and explain it all. But with Murph, you could just talk ideas. Murph was very sharp. He would catch on instantly, and it was fun talking to him. So we started a lot of things with Murph.

I followed Baldeschwieler as chairman of the division. I was chair from '78 to '84, when I stepped down, because I felt that it was starting to cut into my research and I had better not take a second term. I started one, and then I had second thoughts, saying, "If I do another term, I'm really going to have a hard time keeping my research going."

I've made a number of decisions like this during my career—about whether to keep doing more administration or my science. I've always chosen science.

COHEN: Were there any major upheavals or successes during your tenure as chairman?

GRAY: Well, of course, I've had some fights and struggles and conflicts. Murph chose Robbie [Rochus] Vogt [Avery Distinguished Service Professor and Professor of Physics, emeritus; Caltech provost, 1983–87] to be the provost. And Murph and I and Robbie had an interesting relationship. Robbie was a great guy and only wanted to do great things for Caltech. But he and Murph had totally different styles. That sort of blew up in the end with Murph and Robbie, and led to whatever.

I had worked with Robbie on several committees, so I knew him quite well. I had been on a number of key committees around the Institute and with JPL and so forth—with people like Lee Hood and Robbie and others, who were really leader-types and also very outspoken, very opinionated. We had lots of disagreements, and so forth, but I got along with them fine. However, I found working with Robbie as provost very difficult. I worked with Murph really well. We could chat and joke and talk about the future. But Robbie was too tough for me. He wanted it done his way. I think he resented the fact that I got along with Murph so well. Robbie wanted to play it like the army, up the line, and felt that I shouldn't really do anything with Murph. Robbie was my boss—he was the provost—so I should work it out with him. The fact that I had already had this relationship with Murph before Robbie became provost bothered him.

As a matter of fact, Murph told me later that he had two choices for provost—me and Robbie. He picked Robbie. [Laughter] I'm not sure why.

COHEN: Maybe he knew you wouldn't take it?

GRAY: Yes, I might not have. I was never offered the job—I think I've been considered several times, but I've never been offered it. I'm not sure I would have taken it. [Laughter] But Murph picked Robbie for whatever reasons. He probably needed Robbie. My guess is he felt that I was so much like him, Murph, that we'd be a disaster together. I think he worried that I would give the store away, and he was probably right.

I tend to be expansive and do things that I love to do, and I encourage people to do things. I think he felt I might be fiscally irresponsible or something like that, and that he'd be better off with Robbie because Robbie would toe the line. Murph could be the good, up-front guy, and Robbie could really run the ship.

But it didn't work out. They really became very mad at each other at the end. I think they parted company under pretty bad terms. But I've continued, of course, to interact with Murph, and I interact with Robbie pretty well. We talk. We're on speaking terms, although I couldn't continue to be chairman with Robbie as provost.

COHEN: Did he try to tell you what to do?

GRAY: Well, he really kept me down. He embarrassed me a couple of times when he felt that I had overstepped my bounds in things that I had done for chemistry and had not followed his directives in certain ways. He embarrassed me by calling me in with Murph and David Morrisroe [vice president for business and finance, 1969–1994, d. 2002] and pointing out that he had some notes that I'd said this and I'd done that, and things like that. I felt pretty bad about that.

But that wasn't the real reason that I stepped down. The real reason was because I felt I had done my job. I'd started all the things I wanted to. I'd recruited a whole bunch of great people and really made this place what it is in chemistry. And I'm very proud of that. Frankly, I was sort of idling at that point anyway. I'd done my thing for five years, going on six, and I wanted to get back to my work and research much more. So that's the reason. But it was tough, I have to admit, working with Robbie, because he and I are so different. I'm sure a lot of it was my fault. Trying to make jokes with Robbie when he didn't think it was funny. [Laughter] But, you know, Robbie wanted the right things. He's a tremendous supporter of Caltech. And we were all trying to pull together to make Caltech great. It's just that we had different ideas on exactly how to do it. But this was at least interesting times. They were turbulent times.

COHEN: Who were the new people that you brought in?

GRAY: John Hopfield, Rudy [Rudolph A.] Marcus [Kirkwood and Noyes Professor of Chemistry; 1992 Nobel laureate in chemistry], Ahmed Zewail [Pauling Professor of Chemistry and professor of physics; 1999 Nobel laureate in chemistry], Bob [Robert] Grubbs [Atkins Professor of Chemistry; 2005 Nobel laureate in chemistry], Dennis Dougherty [Hoag Professor of Chemistry]. You know, I recruited two Nobel laureates—Rudy and Ahmed—that's not bad! [See also *Gray 2016, Session Two*, for a detailed account of how Zewail, Marcus, Grubbs, and other faculty were recruited to Caltech.] Also, Murph and I conspired together to get John Hopfield from Princeton. I'm very proud of that, because John brought in a different view. He was a guy in physics who wanted to get into biology and chemistry. The Princeton physics department didn't really want him to do that; they wanted to keep him in physics. So Murph and I said, "Come out here and you can be a professor of biology and chemistry and do the new stuff that you want to do." John came out here, and he did great things for us. He's back at Princeton now, but he did great work here. I'd say Murph and I recruited him. We went together to chemistry and biology and said, "Here's a guy that ought to be in your departments."

We did a number of nontraditional things, Murph and I together. I did a number of them myself. I recruited Rudy Marcus. I'll never forget the lunch at the NAS for Rudy and Laura Marcus. Rudy was at Illinois at the time. I was thinking—gee, it would be nice to have Rudy Marcus here; he's such a great guy. So many people would interact with him because of the theoretical work he'd done—I would, and Fred Anson would; there's a ton of us who would, and it would be just great to have him here. And as we were just having lunch together at the Academy, Rudy was talking about how wonderful his trip to Oxford had been. He loved Oxford and the close interaction there, with faculty getting together at lunches. I was thinking, "Gee, that sounds just like Caltech and the Athenaeum [the Caltech faculty club]," when he said something like, "Oxford made me an offer. I would have gone, but the salary was so low." [Laughter] Then Laura said something about loving Pasadena. And I immediately said, "Well, you know, Rudy, I can bring you guys to Pasadena. I can actually give you a decent salary. You can have lunch every day at the Athenaeum, just like at Oxford—at that round table, where all those guys have lunch every day." [Laughter] And it came true. Today Rudy's the mainstay at those Ath lunches, you know. There's Francis Clauser [Millikan Professor of Engineering, emeritus, d. 2013] and Jack Roberts and Ahmed—it's the old gang there, at that one table, every day. So back then, I said, "Rudy, I'm going to go back, and we're going to make you an offer." I didn't have any of my colleagues to consult or anything. But I said, "We're going to get you and Laura to Pasadena." And we did.

Ahmed we recruited from Berkeley [See *Gray 2016, Session Three*, for a full account]. I had to bring him in twice. The first time he came people weren't quite ready for him, I think. I brought him back the next year, and we got him as an assistant professor to start with. If you look at a great fraction of the division now, and the stars and so on in it, I'm very proud that I recruited many of these people. I started developing some of these appointments when John Baldeschwieler was chairman, and I was chairman of staffing. So essentially all the folks in chemistry who are carrying the ball now, I recruited. That's my legacy. I think that's my legacy. And then, of course, Fred Anson [Gilloon Professor of Chemistry, emeritus] became chairman, and Dave Tirrell [McCollum-Corcoran Professor of Chemistry and Chemical Engineering] has just been recruited as chairman. These are just wonderful people. So it carries on.

COHEN: At this time, you were also doing things on the international scene. You were starting to win many honors. [See also *Gray 2016, Session Six*]

GRAY: Yes. Of course, I was elected to the NAS at a very young age. I was elected in '71—five years after I came to Caltech. I was thirty-five years old—the youngest member for a while after that. I've been in the Academy now for about thirty years. I was winning a bunch of other honors. I won the ACS Award in Pure Chemistry. A lot of prizes came in big bunches there in the early 1970s. I was elected to the Academy the same year that Hans Liepmann [von Kármán Professor of Aeronautics, emeritus; d. 2009] was elected, and when I won the National Medal of Science in 1986, I won that with Hans Liepmann too. But that was a lot later. I remember that Shirley and I went to the Academy for me to be inducted in 1971, and I was wearing this tuxedo. And essentially everybody there thought I was a waiter. [Laughter] Several people asked me for a drink.

And I said, "I'm a member." And somebody would say, "Sure. And I'm Abraham Lincoln." Nobody believed me. [Laughter]

Yes, it was a very exciting period. Awards were coming. My work was going well. I was having lots of fun doing research.

Then, of course, when I became chairman, I seriously started interacting with Arnold Beckman. The Beckman story deserves maybe a session. It is a story that starts in 1966, when I first met him, and goes on to the present day.

COHEN: And we have another story, which is the Pauling story.

GRAY: That is another interesting story. We have to do the Beckman story and the Pauling story.

HARRY B. GRAY

SESSION 5

November 17, 2000

GRAY: My interactions with Linus—I'm ready to talk about Linus. I should have made some notes, but everything I've been doing is right off the top of my head. It's probably better that way, because you only remember the bigger things.

You know, Linus was a great hero of mine when I started chemistry.

COHEN: Had you met him at this time?

GRAY: No. I'd never met him. Of course, when I was in school, everybody had heard of Linus Pauling. When I was fifteen, I guess, the American Chemical Society had its 75th anniversary, and the government issued a commemorative chemistry stamp. I was a stamp collector, and I wrote and got a first-day cover—which I still have. And then I started learning about Linus Pauling. He was really *the* chemist of the century. Everybody knew about him. All my professors at Western Kentucky talked about him all the time and how great he was and how important.

COHEN: Did you use his book?

GRAY: No, I never used his book. But my teachers in Kentucky always talked about him. He won the Nobel Prize while I was an undergraduate. We knew about this and about his contributions.

But when I got to Northwestern, I sort of ran into an anti-Pauling group. My appearance there coincided with this renaissance of inorganic chemistry I've been telling you about and the crystal field theory, and all the theories that were replacing Linus's valence-bond theory—about which his book *The Nature of Chemical Bond* was written. This new model—crystal field theory—had come on very strongly in the 1950s—at least among inorganic chemists. Chemists like my teachers Basolo and Pearson were talking about how much better the crystal field theory was. Linus could never explain the colors of transition metal compounds—gemstones, all of these wonderfully colored materials. But crystal field theory had enormous success in explaining the colors of these compounds. Ever since I was a kid, in my basement, I'd been very intrigued with the colors of lots of chemical compounds. Here was this theory that explained it. So I went way over to that side of the fence.

Linus, even at that time, was resisting the crystal field theory, although he wasn't paying much attention to it. So there was sort of an anti-Pauling, anti-valence-bond group in inorganic chemistry, and that's what I was subjected to.

COHEN: Was that just at Northwestern, or other places?

GRAY: No, it was all over the world. Linus, of course, had a wonderful characteristic all his life. He would never admit he was wrong about anything—or that anything could possibly be better than what he'd done. He never lost that. He never gave up on valence-bond theory; he never gave up on vitamin C. You know, he never gave up on anything. [Laughter] His nuclear-shell model in physics. Linus was so great because he was focused, and he harped on things. If something came along that looked a little better, he didn't think it was better. He would argue to the end. He would try to patch up his theory and make it work.

In this case, he couldn't do it. It wasn't that his theory was wrong. It was just good for certain parts of the problem—things that we call the ground states of these systems—and explained the magnetic properties perfectly well. But it couldn't explain the colors. There's a wonderful article about me that talks about this, called "Harry Gray, the Color Chemist." [Laughter] It's in a big, popular, glitzy magazine that wrote about me. I've got it somewhere.

Then, as I told you earlier, I got interested in the crystal field theory and how it explained the colors. When I wrote my thesis at Northwestern, I started—as I recall—by presenting Linus's theory of these compounds, and showed what it was good for [See *Gray 2000–01, Session Two*]. Then I went on into crystal field theory and showed what that was good for. I do believe I should go back and look at it, but I think I gave an even-handed treatment of both, rather than taking some adversarial sort of position. A lot of people who were developing crystal field theory were really dumping on Linus big time, and he certainly didn't deserve that. His model really was the only bonding model that worked for inorganic chemistry for twenty years, and it stimulated an enormous amount of work. He's justly called the greatest chemist of the twentieth century, for that and for other things he did. Even his critics, I think, give him that. I certainly do.

Well, as you know, I went off to Copenhagen, then, to pursue this other theory that Linus didn't like at all.

COHEN: He was already very aware of this, but was ignoring it?

GRAY: He was ignoring it. And I'll tell you a little story on that, on how he was ignoring it. One of his great supporters was a professor at Northwestern, Pierce Selwood. I think in one of our earlier sessions [*Gray 2000–01, Session Two*], I told you that he wrote me a recommendation to go to Copenhagen on a Fulbright. In the recommendation he pointed out what a tragedy it was that I was going to Copenhagen to work on this peculiar theory that was sort of ridiculous and wouldn't get me anywhere. He was a great fan of Linus's theory, and he didn't really want me to do this. He thought it was a waste of time. Of course, he didn't show it to me, but I heard about it later from the people in Copenhagen who'd read it. He went on and on, about how promising I was and how tragic it was that I was going to go down the tubes because I wasn't following the old-time religion.

Well, when I got to Copenhagen, I really found that there were people in this community of crystal field theory and ligand field theory who were extremely anti-Pauling, particularly my boss, Carl Ballhausen. It was not because they didn't like Pauling's theory. It was because Pauling was paying no attention to them at all. They'd been working for ten years, developing this model for explaining colors, and he wasn't giving them the time of day. In 1960, while I was there, Linus published the third edition of *The Nature of the Chemical Bond*. I will never ever forget Carl Ballhausen, having just gotten his copy, coming into my little office, throwing the book on the table, and saying, "Look at that piece of crap," or something like that. [Laughter] "Look at that! Look at how, Pauling's ignored us completely!"

In the middle of this giant, third edition, Linus had devoted one page to all the work that had gone on in the 1950s about how crystal field theory explained the colors of all these compounds. Clearly, he didn't want to have any part of it. He wasn't interested. He devoted this one page to it, saying something like "Well, there's an interesting sort of theory going on, but I don't think it's going anywhere." [Laughter] "And there's some people pursuing it." Then he went back to his model. He never really accepted it or admitted it did anything useful, although it's completely replaced his model among inorganic chemists. Nobody in my business ever uses his model now, because it doesn't explain anything that anybody's interested in anymore.

So before I met Linus, this is sort of where I was coming from. This organometallic compound ferrocene had just been discovered. It was kind of the prototypical organometallic compound. John Bercaw [Centennial Professor of Chemistry, emeritus] has made his career investigating these kinds of compounds—the so-called sandwich compounds—and many others. So, ferrocene's discovery led to this enormous explosion in so-called organometallic chemistry, for which Geoffrey Wilkinson and E. O. [Ernst Otto] Fischer shared the Nobel Prize in the 1970s. It's now an enormous field, which we have many people working on here at Caltech—Bob Grubbs and John Bercaw, to name two very prominent investigators. And this ferrocene molecule had just sort of started it—it's a molecule with two cyclopentadienyl groups and an iron atom right in the middle. It has a very complicated bonding scheme that you really can't account for with Linus's theory. You need the ligand field theory, really, to account for it, and it accounts for it quite simply. Ballhausen and others originally showed this. Later I showed how simply it does so. But Linus, in his book, went to great lengths to show how he could account for it, with 1800 resonant structures or something—some enormously complicated explanation. He had the molecule on the cover of the third edition of *The Chemical Bond*.

So Ballhausen was just beside himself because Pauling had completely ignored him and everybody else in the field and had focused on his very complicated explanation. [Laughter]

When I got to Columbia, I continued working on this. And here is my childhood hero, Linus Pauling, whom I'd heard so much about, dismissing it as unimportant. These other people I'd worked with were really down on him, because he was just plain and simply ignoring them. He couldn't have cared less. I found out later that that was Linus's personality. He had his own way of thinking about things, and he really didn't have time for anything else that was competing; he didn't think it was any good.

My first real interaction with Linus came when I published my first little book, *Electrons and Chemical Bonding*. It explained bonding in ways that were really quite different from Linus's model and explained how new approaches could replace it in courses that were now being taught. [Laughter] He never forgave me for introducing ligand field theory and molecular orbital theory in such a way that you could actually teach it in place of his theory.

COHEN: How did you know that he was so displeased?

GRAY: He wrote to me, and he told me several times. He first wrote me while I was at Columbia. He wasn't angry, and at least he paid attention to me. He wrote me a very nice letter, saying that he'd seen *Electrons and Chemical Bonding*, and that "frankly, my model explains it better." Later, we exchanged some letters on benzene structure and things like that. It was perfectly nice. He didn't attack me; he just pointed out that his model was better, and that I really didn't have to go to all this trouble to introduce this new model, because his own model could take care of everything. I didn't agree with him. But our letters weren't hostile, and it was clear that he wasn't really mad at me. He sort of treated me like a son, working in the field. I think he respected my work. But he was trying to set me straight. He kept that up for a long time.

COHEN: But he didn't stand in the way of your coming to Caltech?

GRAY: No, he didn't. As a matter of fact, his daughter, Linda [Kamb], was extremely supportive, and I believe Linus was very supportive. Of course, he'd already left Caltech—he had nothing to say. He left in '62, if you remember, right after he won the Nobel Peace Prize, and nobody at Caltech paid any attention to him, which insulted him. I think he was very hurt by the treatment—or lack of attention—he got after he won the Peace Prize. And he said, "I'm out of here." But that's another story, and I'm sure it's in all sorts of other archives. I do know the story, but it's not for me to tell. The trustees and others thought he was too left wing. Well, I never thought that. I greatly admired his stance, and I think his contribution to world peace was one of his great contributions, and I always will. But I never had any discussions with him about this. Our discussions were always on science.

When I got to Caltech, as I told you earlier, I finished my big freshman introductory chemistry book that was originally Gray and Haight, and became Dickerson, Gray, and Haight. In that book, I wrote quite a lot on Pauling's electronegativity model. I wrote a lot about the valence bond model, and I gave even treatment to various bonding models. Of course, he didn't like ligand field theory, but he had sort of gotten over that. What he didn't like now was the way I treated electronegativity. What I had written was that Robert Mulliken had showed that electronegativity could be the average of ionization energies and electron infinities. I basically wrote a paragraph on Mulliken, but I put it first, because it was easy to explain. Then I gave five pages to Linus, who'd done most of the work in electronegativity. He was slightly upset with this, and he called me. I certainly talked to him on the phone several times about it. He said, "Harry, you know very well that I invented the electronegativity model. Two years later Robert Mulliken showed that it could be equated to this." I said, "You give me some lines of text, and I'll straighten it out." He gave me some lines, and I put them verbatim into the next edition. Historically he was sort of correct. But after I'd given him five pages and Robert Mulliken a paragraph, I thought it was a little petty and picky of him to call and say, "I really want to be first." They'd always been in competition. But I changed it because it was historically correct to put it the way he did. I thought it flowed rather well, and I published the next edition and everybody was happy.

Ten years later, Linus called me again, and he said, "Harry, I just pulled your book off the shelf just the other day. I seem to remember talking to you about this. Now you know very well that I invented the electronegativity model. Two years later Robert Mulliken showed that it could do this." And I said, "Yes. I talked to you about that ten years ago, Linus, and I adopted what you told me verbatim. It's in the next edition that I sent you." He said, "Oh, I'm very sorry. I must be slipping." [Laughter]

This business glued us together much more closely. From there on, we had a really close, very friendly relationship. Not that we hadn't had it before, but I think that our exchange over the electronegativity business really put us together as good friends. I said he was right, and he was. I've always said he was right when I've believed he was right, and I've always argued with him a bit when I thought he was a little off base. But I never won any of those arguments with him. [Laughter]

Later, after this, I had several scientific interactions with him, although none were at Caltech. In all the times he visited Caltech, we met at lunches and talked socially. We talked science at other places.

For example, when Henry Taube won the Pauling Medal, they asked me to come up and give a talk, which I did. Linus was there. When I won the Pauling Medal a few years later, Linus came and was very pleasant to me, and I gave a talk about my work on electron transfer in proteins. This was 1986—he was eighty-five years or so—and we had become really buddy-buddy. He'd really warmed up to me now—my childhood hero and I now are really good friends. My Pauling Medal talk was in Seattle, and Linus was in the front row, and he asked a number of good questions. After it was over, he finally said, "You know, Harry, you've really done something pretty important. I have to admit it." He had never ever said that before; he had never ever congratulated me on any science. Any time I had previously talked to him about my science, he had always said something like, "Well, I knew that was the way it was going to come out." Why did you bother doing the experiment?" [Laughter] But finally, when I got the Pauling Medal and presented my talk on the work we'd been doing here on electron transfer in proteins, this was something he hadn't thought of. He'd never thought of these distant, long-range reactions. It was a complete surprise to him. I finally talked about something that he thought was important but that was completely out of his sphere and that he hadn't thought of himself. Everything else I'd done, he sort of told me he'd thought of it long before. [Laughter]

And this was a wonderful experience we had during those couple of days in the mid-1980s, when I actually talked about some stuff that he was really intrigued with, and he questioned me, and we talked about it. He talked about how one might do theory on it, and so forth. From then until he died, we had a very nice relationship, and I had a lot of exchanges with the family. When he was really going downhill, the family called me, and I got a chance to talk to him.

Overall, I'd say he affected my life quite a bit. I enormously admire his science. I wish he'd been a little more receptive to other ideas. I think he would have been an even greater scientist if he had been a little more receptive. With Linus, it was almost always one way. If you agreed with him, it was good. If you didn't agree with him, he ignored you. [Laughter]

COHEN: So he wasn't argumentative.

GRAY: With me it was always pleasant. I never had a bad exchange with him. I don't think he was ever mad at me—he just thought I was on the wrong track. To the end of his life he continued opposing molecular orbital theory and ligand field theory as ways of teaching bonding to students. He just felt this was the wrong thing to do, and he said so on many occasions. He stayed with that line to the end. So he never, ever gave in to me or acknowledged, say, my contributions to teaching chemistry in this way. I think he always thought that it was a bad thing that I introduced—although it took over completely and is the only real thing around now in courses, and in all levels of chemistry. He always felt that it was better to teach his model and do other things. But we didn't argue about that in the last twenty years. He had his ideas, and I had mine, so we talked about other things. It wasn't a big deal.

COHEN: But there's still no question in your mind that he was the greatest chemist of the twentieth century?

GRAY: I believe he was the greatest chemist of the twentieth century. He did so many wonderful things, and had so many great ideas. He stimulated so much work. Recently there was a big, worldwide vote on the seventy-five greatest chemists of the twentieth century. I'm happy I made the list. Jack Roberts and Rudy Marcus made it too—three people at Caltech. Linus, of course, was on it. The reason I want to mention this is because Linus, Robert Woodward of Harvard, Glenn Seaborg, and Wallace Carothers were far and away ahead of everyone else as the top four chemists of the twentieth century. My own feeling is that, of those four, Linus was the greatest. I think most chemists would agree with that statement. I certainly believe it.

I have a couple of other stories about Linus and his legacy at Caltech, which I think is very important to talk about, along with why he got so mad at Caltech and left. It took a long time to get him back. Ahmed Zewail played a wonderful role in bringing him back.

It's a strange business, his legacy at Caltech, because he did all of his important work at the Institute. He was here as a student and a professor, and, as chairman of the division, he built everything. It should be a wonderful legacy. It's sort of tragic that he left in '62 and went to UC San Diego. He was also at Santa Barbara and Stanford, and now, he's sort of associated with these places and not Caltech. When the newspaper lists Nobel laureates at the University of California, they list Linus Pauling. [Laughter] That's really crazy. For most of the twentieth century, he *is* Caltech chemistry. When you think of Caltech chemistry, you think first of him. That's certainly true now. Whether it will be true in fifty years I don't know, because of his affiliations with these other places. When they give affiliations, his last affiliation now is Stanford. Well, he never did anything at Stanford. [Laughter]

COHEN: And of course, his shrine is at the University of Oregon.

GRAY: His shrine is actually at Oregon State. We've let a lot of things get away from us, and it's really too bad. We may lose out a lot, although all of his work that's worth mentioning was done here. His legacy—all the things that he built up as chairman of the division, and everything else—is a Caltech story.

That's why these oral histories are important—to have that on the record, that Caltech chemistry and Linus Pauling are absolutely synonymous.

COHEN: But, you know, in some sense, sometimes one comes to the end of something and it is time for people to leave and move on.

GRAY: Yes. I've got a few other stories to tell on that. We could wrap it up next time.

HARRY B. GRAY

SESSION 6

December 1, 2000

GRAY: I think we should talk about Arnold Beckman. I guess you'd like to know when I first met him.

COHEN: That's right.

GRAY: Shirley and I had just come out from Columbia. In late June of 1966, we drove out to California to take up our appointment here at Caltech as of July 1.

We came west and settled in. I had, of course, heard of Arnold Beckman when I visited here earlier, in the spring of '65, but I hadn't met him. He was at the time chairman of Caltech's board of trustees and had already built the Beckman Auditorium, which was his first major gift to any university. Beckman Behavioral Biology Lab wasn't built until 1974.

I'd heard a lot about Dr. Beckman, and of course I had used Beckman instruments for my thesis. After I came out permanently, I heard a lot about him and his interactions and the fact that he had been a graduate student here in photochemistry and had become a faculty member and had started a company.

I met him for the first time after an Alumni Seminar Day talk that I gave during my first or second year. I looked up when I started giving this talk and saw Dr. Beckman and Mabel Beckman in, I think, the second or third row. [Laughter] I recognized them—I'd seen his picture, and I recognized Mrs. Beckman. As I recall, I gave some talk about the role of iron in living systems—the role of iron in our biological functions—because I was doing a lot of work in that area at the time. I noted that Dr. Beckman and Mrs. Beckman seemed to be quite interested in this talk. I found that hard to believe.

Afterward, they both came up front right away and introduced themselves to me. That was my first meeting. "Arnold Beckman. Dr. Gray, I'm very pleased to meet you." And Mabel chimed in immediately, saying, "Dr. Gray, I really didn't understand much of what you said, but you were so enthusiastic, I was so taken with it, I really enjoyed it."

Then Arnold sort of nodded, and I knew instantly that we were going to hit it off. Mabel had given me the okay. And I knew—"I'm going to develop a good friendship with these people. This is going to be nice." I just have to say that I was very flattered that the chairman of the board of trustees and his wife would actually come to my talk, listen to it, and come up and chat with me afterward. This was totally foreign to me. At Columbia, I'm not sure I'd even met any of the members of the board of trustees. The university administration and board were way out in some place where the regular faculty members never really mingled. It really wasn't done. And here at Caltech, I dealt every day with DuBridge and Bacher. Now here were the board chairman and his wife. [Laughter] I thought, "My God! This is a completely different operation." It just cemented my feeling that this was a real family operation, and that the trustees really cared about the place. So this was part of the education of Harry Gray about what Caltech was really like. I really felt good about that.

COHEN: Did the Beckmans really come to everything?

GRAY: Oh, they always came to Alumni Seminar Day and to other various things. I ran into them a few times. But we really didn't develop a close relationship, I would say, at this time. That happened about the time that I became division chairman, roughly ten years later in 1978. In the 1970s chemistry had a visiting committee, which was made up of Caltech trustees and distinguished chemists from other places. Sometimes there was overlap because some of our trustees were distinguished chemists. I'm fairly sure that Dr. Beckman was part of this. Certainly I know that Jim [James] Glanville was very prominent on it. Tory [Victor] Atkins and a number of famous chemists, like Henry Taube, were also involved. But the mainstays among our trustees at that time were Dr. Beckman, Jim Glanville, Stan [Stanley] Rawn, and Tory Atkins. I really got to know them very, very well, and Shirley and I became very close friends with all of them.

COHEN: Did they all live in the area?

GRAY: Stan Rawn's in New York. Jim Glanville was in New York, Texas, and Connecticut. Tory Atkins was in the Bay Area—he's now living around Santa Barbara.

Dr. Beckman was, of course, at his place in Corona del Mar. So these people would come in for their big meeting once a year or so. But I would see them a lot, because they also came to regular trustee meetings, and they would pop in and talk to me about how things were going in chemistry and chemical engineering. They took a tremendous, deep interest in the welfare of the division.

I got to know Arnold Beckman during this time very, very well, through our joint love of chemistry and chemical engineering and the division. Arnold had been a little standoffish because of some dealings or other that I'm not completely sure of, and I think he felt a little separated from the division. So we started bringing him back in for advice, really—we weren't hitting him up for money. Slowly but surely, our relationship grew stronger and stronger as we got to know each other better and better.

Beckman congratulating Gray on becoming the first Arnold O. Beckman Professor of Chemistry, 1981.

We saw eye-to-eye on many things and had some of the same values, I think. So Arnold and I became very close—just as I became very close to Jim Glanville and Stan and Tory. All four of them have really done so much for the division.

COHEN: Now, you also had a social relationship with these people?

GRAY: I had a strong social relationship with Jim Glanville, to a lesser extent with Tory and Stan, and a little bit with Arnold. I've never had a really super-strong social relationship with Arnold Beckman. But Nancy and Jim Glanville became very close to Shirley and me. We went out all the time and got together. Since Jim died, we still see Nancy all the time. We've maintained that contact with her and with their son John, especially.

Anyway, this is the Beckman story, but it is related to Jim Glanville in certain ways, because they reinforced each other during this time and supported the chemists and the division.

While I was chairman, it became clear to Dr. Beckman that Crellin Laboratory badly needed renovation, and so my first major project with him was the Beckman Laboratory of Chemical Synthesis. We had lost David Evans to Harvard, and we thought that one reason was the terrible state of our facilities for synthetic chemistry in Crellin. They were run-down. The visiting committee went through and looked at them, and all of them and they could tell right away that these laboratories badly needed renovation.

And so after Arnold had gotten to know all the key people in the division and so forth, he said, "I'd really like to do something about this."

COHEN: So actually he came to you?

GRAY: I was talking to him all the time as chairman of the division. I wouldn't say he actually "came" to me, because this was almost a continuous discussion with the visiting committee. But, somehow, during this process, Arnold let it be known to me and probably to several others that he would entertain a serious proposal to do something about this. And, of course, at that time, David Morrisroe was very much in the Arnold Beckman sphere. He was very, very close with Arnold Beckman, who was, I think, really responsible for Morrisroe coming to Caltech with Harold Brown. David Morrisroe was in constant touch with Arnold Beckman and with Jim Glanville. And, of course, I was in daily contact with David Morrisroe. I think it's very likely that Arnold Beckman mentioned to Dave Morrisroe that he was now ready to do something about this, and the message got through to me, and then the appropriate group put in the proposal. The proposal was to redo Crellin and Church as a combination and put the Calder arches that we saved from the demolition of Throop back up in this adjoining sort of mezzanine between Crellin and Church. That area with the arches would be called the Arnold and Mabel Beckman Laboratory of Chemical Synthesis.

Arnold put in about $15 million to do that, of which a large amount went into a matching fund for equipment. We had proposed that to him, saying, "We'll raise money for equipment and your fund will match that. We'll be able to leverage it and double our impact." He liked the idea that we would go out and hustle money and expose our work to peer review. If it passed muster, then the Beckman Fund would match it. Dr. Beckman has always had this feeling that matching is a very, very good thing. It means you're not just throwing money at something, but supporting something that somebody really works for and hustles money for. He feels that his money is a good investment then because it's obviously being taken seriously [See also *Gray 2016, Session Three*].

We did that, and it was a roaring success. We managed to hang on to Peter Dervan, which was one of the things we hoped we would be able to do. We were able to get Bob Grubbs, and fix up things for him, and to get Dennis Dougherty, and so on. That was quite a successful project, which I know Dr. Beckman and Mrs. Beckman were very proud of. They loved the Calder arches and the way it all looked, and the fact that we had revived synthetic chemistry at Caltech.

COHEN: Now were there any real big givers on the matching fund, or was that all sort of bits and pieces?

GRAY: The matching money was all stuff we raised from the federal government—NIH and NSF—for equipment. Nobody really put in any more money than the Beckmans, as far as I know, on that particular project. There probably was some from Jim Glanville, because Jim gave regularly to the division when we had some needs. It was not uncommon for Jim to meet with us. I'd show him some stuff—let's say the Mead Lab for Undergraduate Research. He'd go back to his office in New York and send me a check for $25,000 or $50,000 with a little note saying, “Why don’t you take care of that. I know you need to do that.” That was Jim Glanville.

COHEN: Now, did you have a real in with all these people that other departments didn’t have? Or did you think they had their “fairy godmothers” too?

GRAY: I think from time to time, other divisions have had their big supporters, because the trustees, I think, divide into interest groups. This particular group was really interested in chemistry and chemical engineering. Stan Rawn and Jim Glanville were graduates of Caltech in chemical engineering. Arnold Beckman was a Caltech chemistry graduate and had been a faculty member here. These are people who are really alums of the division and very involved in its health and prosperity. So I think it was a natural thing. But I never ever hit them up directly for money. I was really more interested in their advice and counsel. When Arnold Beckman would step aside from this advisory role, he would usually come in through Morrisroe to indicate an interest in fundraising. He really dealt mainly with Morrisroe on this. It’s no secret that he didn’t really care for Murph [Marvin Goldberger]—I think mainly because Mabel Beckman for some reason didn’t care for Mildred. [Laughter] It’s one of those crazy things. I’m just guessing, but Mabel probably felt Mildred was a little too left wing for her. I think she was uncomfortable around Mildred, and they didn’t really hit it off. I don’t think there was anything that bad, but it’s just that Arnold Beckman didn’t really feel comfortable talking to Murph, whereas he felt very comfortable talking to Dave Morrisroe, because he had a long relationship with him. So when he wanted Caltech to know that he would like to help Harry Gray out on something, he would tend to go to Morrisroe. And then Morrisroe would give me the word. [Laughter]

I think we did rather well with our trustees at that time. We didn’t do anything out of line; we didn’t hit them up directly. I didn’t go around any of the proper channels at this time—we would go through whatever the proper channels were. Our visiting committee worked very, very well for us because these people not only had a lot of wherewithal themselves to help us, but they also had tremendous contacts with other rich people who helped the division enormously over the years. For example, the Mead Lab came about because the trustees knew about our undergraduate laboratory needs after the Sylmar earthquake in 1971, and they knew that the Mead family had left some money to do something. The trustees located this for us and made it happen. I could give you many examples of where having these very caring trustees looking at the operation regularly has made a big difference.

That having been said, I think that after the success of the chemical synthesis laboratory, Arnold really took to our approach. He was now involved again in the division, talking to chemists whom he liked to talk to. He had really warmed up to the division, which he'd lost touch with a bit. Now we're up to the early 1980s. I'm still chairman and he's now quite warm and friendly and spending a lot of time with us, and I'm spending a lot of time with him. Our friendship has really now been going for some time, and we confide in each other all the time about things.

Then he sends a clear signal to me and to Dave Morrisroe that he'd like to do something much bigger in chemistry. "I kind of like the synthesis lab. But now I'm thinking much bigger." He sent some clear signals out that he'd like us to come up with some ideas to make a real impact on the campus. He had been talking to the University of Illinois about the Beckman Institute there, and it was pretty clear he wanted two Beckman Institutes. He was already in the process of making the one at Illinois happen. They were ahead of us on that kind of solicitation. That's when we got our little group together—mainly the people who had been involved with the chemical synthesis laboratory and others on campus in biology who had interacted a lot with Dr. Beckman. John Abelson [Beadle Professor of Biology, emeritus] and Mel Simon [Biaggini Professor of Biological Sciences, emeritus] were involved a little bit. It was mainly Lee Hood, Eric Davidson [Chandler Professor of Cell Biology, d. 2015], myself, Bob Grubbs, and Peter Dervan [Bren Professor of Chemistry]. We had a little group for working up ideas to propose to Dr. Beckman. We had all kinds of variations, but we finally came up with this idea of building a big Beckman Institute that would be devoted to completely revising the infrastructure for chemistry and biology research at Caltech. It would be devoted to developing instruments, methods, and technologies that would support basic research in chemistry and biology, help bring them together, make the research more interdisciplinary, and make a place where people could come in and work around high-tech things. In other words, ramp chemistry and biology up from single laboratory test tube-like sciences to much bigger science-type models that had been around in astronomy and physics for a long time. I proposed to Dr. Beckman, "Arnold, it's time to make this move. It's time to get chemists and biologists out of their little labs, working on these little things, and get them into the high-tech world of big instruments and big methods, and getting engineers and physicists together with them to work on bigger problems. Now's the time. You started it Arnold. You made all these instruments. And now we have to find a place to put them all." [Laughter]

COHEN: Now was he doing this sort of thing at the Beckman Institute in Illinois, or was this really a new idea here?

GRAY: This was a new idea. At Illinois, he just glued together a bunch of disciplines, particularly a lot of engineering. Their Beckman Institute is quite different from ours. Ours is unique. Ours is really built to support the new chemistry and biology that's sort of merging together for the 21st century. I think it's positioned us well to do that. So, at the time, that was our idea, and Arnold liked it. I cannot tell you how many times Murph Goldberger and I drove down to Orange County, to his various locations. Very often we met him in a kind of temporary location that a pharmaceutical company had loaned out to him. The Beckman Instruments' big office was in Irvine. But he had a temporary office in this other spot.

COHEN: Was he still actively involved with his own business?

GRAY: Yes, he was still actively involved, although he was sort of moving out. It was during this period that Beckman Instruments merged with SmithKline & French to become SmithKline Beckman. He was quite involved in that and would fly to Philadelphia for this and that. At the same time, he was beginning to retreat from all that and to really put his time into the Arnold and Mabel Beckman Foundation. They had more and more money and more and more responsibility for giving it away.

The thing is, Arnold didn't really want to give the money to Murph at this point. We had this great proposal, and it was clear he wanted to fund it. But he waited until his

Illinois man, Tom [Thomas E.] Everhart [professor of electrical engineering and applied physics, emeritus; president of Caltech, 1987–1997], came to be president of Caltech before he really released any money—although he did pledge it, and Murph announced at his last commencement as president that Arnold Beckman had pledged basically $50 million to start the Beckman Institute at Caltech. But Arnold did not let go of any money until Tom Everhart got here. [Laughter]

It was an interesting deal. Arnold pledged $40 million if the trustees—namely under the leadership of Jim Glanville—would raise $10 million to match that, for $50 million. And Arnold agreed to put in another $10 million if the trustees would raise another $10 million for the general use of the campus. Arnold Beckman always had in mind that if he was going to build something, it would be very important to support other things on campus as well. He was very sensitive to that. And I think his formula, frankly, for support of the Beckman Institute, was brilliant, and has led to all kinds of support around campus. Not very many people appreciate what an impact it has had.

COHEN: Can you give me a specific?

GRAY: Well, the $10 million that went into the general fund.

COHEN: For whatever was needed?

GRAY: The trustees raised that $10 million, which went into non-Beckman Institute activities on campus. Where it actually went, I don't know. You could ask Steve Koonin [Caltech provost, 1995–2004] or possibly Robbie Vogt where it all went. But the point is that he did care about campus happiness as a whole. He didn't want us to set up a Beckman Institute with so much support that people looked at it and said, "Well, those guys are so special. They're getting all this support, and we're not getting anything." And, of course, we set up an endowment for the Beckman Institute, so that it would be self-supporting—so it could pay its light bills and all this sort of stuff. The building itself would not be a drain on campus.

He set all this into motion. Not only did he do that, but when we gave him the first plans for the building, which called for about a hundred thousand square feet of space, he looked at it and said, "Harry, that's ridiculous. Build more space. Build a separate floor and shelve it for future growth. I'll help pay for it."

When we originally designed this building, we only had a basement. We didn't have a sub-basement. Arnold Beckman came and said, "That's ridiculous! You're going to fill this up fast. You're going to need expansion space. Build a sub-basement *and* a basement." And that's one reason the Archives are now located here. He was visionary and knew that this building was going to be a hit and was going to grow, and pretty soon we would need the extra space. Now we've already filled everything, and it's only ten years later. He knew that would happen, and we didn't. We were thinking too small.

The point I'm making is, all through the years, Arnold Beckman has not only given money, but has also been visionary. He's given very, very good advice about things to do in the future and how to look ahead.

For the Beckman Institute's matching funds, Jim Glanville took charge. He made raising the $20 million from the trustees happen. He and Nancy gave a lot—the money for the Glanville Courtyard and the Beckman Room. It was supposed to be anonymous, but they gave all that money to build the Beckman Room. So this was really the operation that cemented Caltech's relationship with the Beckmans, which had been a little shaky, but now got really strong.

COHEN: So he really liked Everhart?

GRAY: He liked Everhart. He didn't dislike Murph, although I think he felt uncomfortable around him. Murph was more of an East Coast-intellectual kind of guy who told great jokes and had a lot of fun. And I think he and Arnold, just felt a little uncomfortable around each other. It's ironic, because I felt so comfortable around both of them. [Laughter] I mean, I loved to talk to Arnold. I had no problems with him. And Murph I just loved to death. I could just sit around and talk for days with him. I think Murph is fabulous. But these two guys together, I think, couldn't make small talk. There was no chemistry.

COHEN: Now where was Robbie Vogt during all of this?

GRAY: Well, of course, after Robbie became provost, sure, he was in the middle of all of this. In fact, he was in the middle of the Beckman Institute fundraising with Murph. In my opinion, he wasn't really directly involved. When I'd go talk to Robbie, he would talk like he was very critical about all this, and that he had talked to Dr. Beckman and made it all happen himself. You could see no evidence for this. But Robbie felt that he was in control of the Beckman Institute project. All I know is that I never went down to see Dr. Beckman with Robbie. Murph and I always made the pitch, and Arnold always wanted more details. Because Murph kept going down with what he thought was the matching money, and Arnold kept pointing out that it was really old money. [Laughter] So it took a long time for Arnold Beckman to approve of the first $10 million. The trustees really had to raise new money before they made that happen.

I never had any meetings with Robbie and Dr. Beckman. My meetings were always either with Morrisroe and Dr. Beckman, or with Murph and Dr. Beckman. Robbie was here running the Institute and disagreeing with Murph on most things. When I went with Murph, Robbie wouldn't go, of course—for the reasons that you know well. But when the time came to start taking credit for the Beckman Institute, of course, there was a long line of people who were willing to take credit for making it happen. Robbie may well have talked to Dr. Beckman several times over the phone. And when he came for trustee meetings, he told Dr. Beckman that he thought the project was a good thing. What his actual involvement was, I just don't know. My interaction with Dr. Beckman, in making the Beckman Institute happen, was through this little group that met regularly to generate ideas, which I then took to Dr. Beckman to try to explain. And I took Murph with me, or Murph took me, or I took David Morrisroe. That's the way it happened.

Then we started construction on a super fast-track basis, because we knew Mrs. Beckman was not well. We all wanted desperately to get this thing up and running while she could see it happen. We all wanted to restore the Goodhue architecture to the campus, especially Dr. and Mrs. Beckman. The old Goodhue campus, we love it. It had been sort of ignored in several newer buildings, which I call the aircraft carriers—Beckman Behavioral Biology and Baxter, to name two. And Millikan Library of course.

So we really wanted to make a statement by restoring the beautiful style of the Goodhue campus. And that's exactly what we told the architects. We had wonderful architects. Tim Vreeland was the designer-architect we worked with—Diana Vreeland's son. He's a great architect, and he really took care with the design. I love what you see in this building, the courtyard, and the arches, and I think that we succeeded in restoring the feeling of the old Caltech campus and in establishing it to the north of the old campus. And we succeeded in making a statement here, which was in fact then carried on in the Moore Building, in the Avery House, and in the rehab of the bookstore and Winnett Center. So I think we did have a good effect on the campus. That came about through a combination of myself, Shirley, the Beckmans, Murph, and a number of other people working together.

Mrs. Beckman died in early June of 1989, and we dedicated this building in late October. Of course, Dr. Beckman was distraught at the time. In that sense, the dedication was bittersweet, but it was a wonderful occasion for everybody. The governor and lots of other people spoke. It was a great dedication. But the missing link was Mrs. Beckman. For the first time Mabel was not here with Arnold at a big unveiling of this type. And that really affected him tremendously. It was very hard, but we went through it together, and of course that brought us even closer.

The other element of my relationship with Dr. Beckman is the close work I've done with him and his philanthropy at other places—especially the Arnold and Mabel Beckman Foundation, which I've worked on with him. I'm a member of the board of directors of the Foundation. I worked closely with Dr. Beckman in the 1980s when he really started revving up his philanthropy nationwide. I would literally be with him once a week. I'd be down there talking to him about one thing or another, and he'd be asking my advice about everything else.

By this time, he wanted to build the Beckman Center for the NAS. That was built in 1998 down in Irvine. During the 1980s and '90s, he would continuously call me in and say, "Harry, what am I going to do? I've got all these proposals." [Laughter] Now the foundation was going pretty well, and he had a lot of money to give away. But he was still doing it all himself.

COHEN: When did he break with David Morrisroe?

GRAY: That was during the Everhart administration. I would have to go back and look at the exact date. But I was already on the board of the foundation. David was not a member of the board. He was the treasurer of the board, I think it's fair to say. Of course, David was very influential on the board when Harold Brown was the chairman of the Arnold and Mabel Beckman Foundation. David and Harold Brown, of course, worked closely as a team with that group on the board.

Then the board really changed. George Argyros came on. He's the current chairman of the foundation board, and also a Caltech trustee. Other new members were added. It would have been in '94 or '95 that the break between Arnold Beckman and David Morrisroe occurred.

It was very traumatic, because they had been so close. David had been a trusted advisor of Arnold Beckman's. But basically what it amounted to is that David had been running the foundation. He was also involved as executor of the Beckman Family Trust and estate, and so forth. I think David just got a little too involved with too many aspects of Dr. Beckman's life and money. I don't think he did anything wrong, but there was some perception that he was too involved. I think the family—particularly the Beckmans' daughter, Pat Beckman—decided that Dave was really just too close. I think Pat felt that she was family and should have more to say about how things would go. One thing led to another, and Dr. Beckman agreed that Dave Morrisroe should step down from being executor and from having any role in the foundation.

About that same time, Harold Brown was kind of encouraged to step down as chairman of the board. So they both left.

At this point I think Arnold was taking more advice from the family and others who were close to him every day. I think that was when he started to let up on the controls a bit and to let others dictate some things that he would go along with. He's done that more and more in the last five years, and now the foundation is being pretty much run by the board rather than by Arnold himself. So the break with Morrisroe really represented a big change. But the foundation by this time was running okay.

COHEN: Did Lee Hood's leaving bother him to any great extent?

GRAY: I don't think it upset him that much. The relationship with Lee Hood was very complicated. Lee Hood had invented these instruments that I think he then offered to Beckman Instruments for development and for commercialization. As I understand it, Beckman Instruments passed on that; they decided not to take them. So Lee went elsewhere with them—to what is now Applied Biosystems, which has become incredibly successful. But I believe that Mrs. Beckman felt that Lee Hood had somehow double-crossed Dr. Beckman on this deal, although I do not think that's true. I think it's the management of Beckman Instruments that blundered and didn't see their potential and didn't take them. But the fact that the company did not get these great new instruments and get credit for it bothered Mrs. Beckman.

COHEN: She really had her finger in every pie.

GRAY: Well, she certainly knew about this. They talked at home about it, and I think she convinced Arnold that Lee Hood was not loyal to Dr. Beckman. So that was one of the problems we had in getting Arnold back on board at Caltech. But I think I overcame that. Lee was in the Beckman Institute, and Arnold approved of that—he really liked Lee a lot personally and loved to talk to him. I don't think there was anything wrong with Lee's relationship with Arnold. I just think it was complicated, and also included Mabel. I think that Arnold's relations with Murph were also clouded by various interactions with spouses.

But the bottom line is, that when Lee left, Arnold was sort of neutral on it. I think he still had this feeling that he should have gotten those instruments and hadn't. [Laughter] I don't think he considered Lee's departure a catastrophic loss at all for Caltech because he was sort of neutral on Lee. A lot of people were doing frontier things at the Beckman Institute. It wasn't like Lee's work was the only piece of action. I think he was much more interested in several other people—myself, Peter Dervan, Bob Grubbs, and Nate [Nathan] Lewis [Argyros Professor of Chemistry]. When Lee left, I think that it bothered the trustees and people here more than it bothered Dr. Beckman. But it didn't cause us to lose support or anything like that. It was just the way the cookie crumbled.

I will say that in the 1980s, he really asked me to step in at the Beckman Foundation and give him some advice on things that he should be doing. He had all these proposals, and he would tend to refer people who wrote them to me to sort out for him. I did put in a lot of work trying to evaluate proposals and advise him. Arnold did not like to say no to anybody. He would use mechanisms like, "Well, I'm going to have to discuss this with Harry Gray." I became sort of his right-hand man and science advisor on everything during the period from the late 1980s to about the mid-nineties. He would use me as a kind of way of deflecting people that I was pretty sure he didn't want to fund. He didn't want to be the bad guy, and I enjoyed talking with them. But it was a lot of work. I knew that we had to develop something a little more regular than this, or there was going to be a real disaster, because now we're talking about big money. So I and several others proposed that we start a young investigators program. We said, why don't we start some real programs and give the money to proposals?

In the early 1990s, I proposed the Beckman Young Investigator Program to him, and he accepted it. This was really the first big program of the Beckman Foundation, and I'm very proud of that because it was my idea. He loved the thought of supporting young investigators.

COHEN: Now how does that work?

GRAY: Well, it's been a tremendous success. We have about a hundred Beckman Young Investigators all over the country now. It's a program where young investigators, or beginning assistant professors, put proposals in to the Beckman Foundation for awards. We typically get about two hundred proposals a year—many assistant professors at Caltech have received these awards. They're given mainly in chemistry and biology, although there are occasionally one or two in physics. The proposals are reviewed by a national evaluation committee that makes recommendations of the winners to the Beckman board, which then always accepts those. There are about fifteen or sixteen a year—$200,000 each, with no strings attached. We've done it for something like ten years. Faculty are only eligible to apply in the first three years of an assistant professorship.

The program has given all sorts of wonderful young people around the country their start. Dr. Beckman is very proud of it. I'm very proud of it. It's been a tremendous success. It's well known now, and our young investigators are well known because some of the first ones have become big superstars. A couple of recent MacArthur Award winners were Beckman Young Investigators.

COHEN: And that program has its own board that makes the selections?

GRAY: It's got its own evaluation review panel, which is now made up entirely of ex-Beckman Young Investigators. That panel plays into something called the grants advisory council, which then plays into the Beckman board for final ratification. The whole process works very nicely now, and there are several other programs: The K-6 Orange County Hands-On Science Program; the Beckman Fellows Program, which we just started at Caltech; the Beckman Scholars Program, which funds undergraduate researchers all over the country. We started quite a large number of Beckman Foundation programs during the last ten years.

COHEN: So that's really getting away from the big grants to build buildings. I mean, this is an entirely new direction.

GRAY: Yes, but we're still thinking about making big grants to build buildings because the foundation now has a half-a-billion dollars. The five regular Beckman Centers get annual support, but there's still a lot of money to give away, and we're trying to give it away responsibly in the way we believe Dr. Beckman would want. We're trying to faithfully follow his philosophy statement, which he carefully put together, about what he wanted to support—high quality basic research. That's where it all stands now. There are now two family members on the board other than Dr. Beckman, who is still a member of the board, of course.

That's a long version of my thirty-plus years with Dr. Beckman.

COHEN: Does he really have any cognizance of what's going on now?

GRAY: His short-term memory is completely gone. He's okay in a one-on-one conversation. At any given time, he can talk to you, and he remembers a few things. He's come up every year to talk to Caltech students—he was up last February, I think it was. Our students talked to him; they asked questions; he answered them. I'd say that in short spans of time, he's still with it. But his short-term memory is completely gone, and, of course, he has faded a great, great deal in the last year or two. So he is pretty much out of it in terms of the board now. We can't really ask him to come through on anything now. He's going to take advice from others. It's up to George Argyros as chairman and the rest of us on the board to carry on with what we know he would have wanted to do.

COHEN: So you are a member of this board. And that takes up what part of your time?

GRAY: I'd say ten percent. I put a lot of effort into the board and related things. They count on me, since I've been with them so long. For example, next week, I'm going to meet Pat Beckman to discuss a proposal for Hoag Hospital in Orange County. We're going to look at their science, and that will require most of Monday. On Tuesday, a delegation from USC is going to meet me to discuss their proposal to work on macular degeneration. So next week I will devote a good part of a couple of days to Beckman Foundation activities. That's a little more than usual. On average I would say I put in a good half-day a week. There are four regular daylong meetings a year, and during the weeks before the meetings, I put in a good deal of time on foundation matters. So it's a major commitment. And it's worthwhile; it keeps me in the loop. Dr. Beckman, of course, has done so much for me, my work, and the work of Caltech, that I definitely owe him this. My job description here, in running the Beckman Institute, really includes putting in an equal amount of time to the foundation, and then doing my own research.

That's what I do now. I don't do as much teaching as I once did, but I'm doing as much research as I've ever done. Running the Beckman Institute is a much smaller job than being division chairman, but I would say that doing my combined jobs as director of the Beckman Institute and a board member of the Beckman Foundation, and trying to do them together would be about equal to the job of division chairman, roughly speaking. The rest of my time I work on research. I work seven days a week.

HARRY B. GRAY

SESSION 7

March 23, 2001

COHEN: We were going to talk about what has gone on these past ten years at Beckman Institute, and how you administer this.

GRAY: Maybe we should start with how we picked the resource centers to occupy space and how we got going. Space is always a big issue.

After I'd presented the Beckman Institute plan to Dr. Beckman, we had to find some way to pick the first groups who would come into the facility and develop new methods here. A very logical person from the beginning was Leroy Hood. He had been one of a number of us who met regularly to formulate plans for the Beckman Institute. Because we intended that the institute would develop new technologies, methods, and instruments that would support a lot of research, the idea also was that we wouldn't have many permanent faculty there. Mostly, it would have principal investigators who had space in other chemistry or biology buildings. They would come to Beckman to participate in something we call the resource center—a unit that had people in it developing these new methods, instruments, and technologies. They'd be doing research themselves while they developed these new technologies. At the same time, they would make these instruments and methods available to anybody at Caltech or elsewhere, for that matter, who could come in and use them.

COHEN: Was the money all from Beckman?

GRAY: Some came from the trustees. Arnold Beckman had a challenge gift that required a $10 million match—which the trustees raised—and another $10 million match to match some other money that he would put in. He eventually put in $50 million, which was matched by $20 million that the trustees—mainly Jim Glanville—raised. Dr. Beckman also gave $10 million as a Caltech-wide, separate gift. Of course, we also raised a lot of federal money. As a matter of fact, we analyze this every year. The amount of Beckman support now on an annual basis is only about six or seven percent of the total budget. All the rest is raised from other private donors and from the federal government—NSF, NIH [National Institutes of Health], DOD [Department of Defense], and so forth. So we leverage the Beckman money on an annual basis. We make a report annually to the Beckman Foundation on how much we've raised and what percentage of their money we're using versus other monies. The foundation very much likes to see this high degree of leveraging. To them it indicates that we're not soft, we're not just spending their money; we're hustling a lot of federal money for projects.

What we decided to do then was to entertain proposals for resource centers from Caltech faculty. After I was appointed director by Murph, we formed an executive committee to look at proposals while this thing was being planned and built. We were planning what we were going to put in it, exactly what sorts of resource centers. Lee Hood, Eric Davidson, Mel Simon, Bob Grubbs, and Peter Dervan were some of the key people. After a number of proposals came in, we decided on an imaging center. But we didn't have the expertise, and we decided that we would need an external appointment. We knew that there was an enormous need to support biology with a big imaging center that could do optical imaging and magnetic resonance imaging. That's how we made the appointment of Scott Fraser. We established Scott as the head of the biological imaging resource center—probably the largest single resource center in the Beckman Institute. And he brought in some people—Tom [Thomas] Meade and Russ [Russell] Jacobs—from outside as well, to join him in running this big center. They have been extremely successful imaging all sorts of things and in developing new imaging techniques. It's also been a very successful interdisciplinary effort. This is exactly what we had in mind—involving chemists, biologists, engineers, physicists, and so forth in interdisciplinary efforts to develop new technologies and make use of them to do things you wouldn't be able to do otherwise. They've developed new methods for imaging, and developed new hardware, software, and contrast agents. They've had several articles in *Science* and *Nature*.

COHEN: And they take up a good deal of space, physically, in this building.

GRAY: Yes, they take up a very large part of the basement of the Beckman Institute, and office space on other floors as well. Leroy Hood's biotechnology center, which was the initial center, was the largest by far until Lee left. He occupied the second floor of the building. He had an enormous resource center here, with a big NSF grant. So he was one of the big, initial thrusts.

COHEN: What's happened to Lee Hood's space?

GRAY: A lot of it has been turned into facilities for things he had started that we're still running—facilities for peptide analysis, protein analysis, photosynthesis, peptide and nucleotide synthesis. These were all things that he had more or less built up, and we've continued them as facilities to support chemistry and biology. They're very important. Jerry Solomon, who was in the biological imaging center, now has a computational biology center here.

Another one of the original centers was one that I set up for condensed matter laser spectroscopy. I brought Jay Winkler here from Brookhaven to run it as a Member of the Beckman Institute. We set up these special positions called MBI, or Member of the Beckman Institute, which are very high-level staff positions, sort of like senior research associates. These are people who are running and managing these centers and doing research of their own there. Russ Jacobs is the MBI in the imaging center for Fraser. Jay Winkler is the MBI in the laser center, which is right across the way from the Archives. He runs it for the benefit of the groups on campus who need to do condensed-matter laser spectroscopy. It's also been very successful in terms of usage by the campus and importance to the campus. Ahmed Zewail and other chemical physicists use it, largely to do gas-phase work, which is very specialized. Bill [William] Goddard [Charles and Mary Ferkel Professor of Chemistry, Materials Science, and Applied Physics] got in fairly early with a big computational center. It has turned out that he moved entirely into the Beckman Institute. So his permanent office space is here, as is Scott Fraser's. Their resource center space could turn over at some point, if we decided to change it into something else, but they would keep their permanent space here.

We also set up a materials center—an experimental center for the study of materials—with Nate Lewis as the principal investigator. Initially Seth Marder was the MBI, but he took a professorship at Arizona, and now Michael Freund is the MBI. That center does high-vacuum surface work. They have all kinds of instruments to measure the properties of materials. They do all kinds of polymer characterization and specialized syntheses of materials. Many people on campus interact with that center—Dave Tirrell, Bob Grubbs, Dennis Dougherty, John Bercaw, Julie [Julia] Kornfield [professor of chemistry], and Mark Davis [Schlinger Professor of Chemical Engineering]. A large group of people use that center because it has lots of methods and instruments that are very useful. So it is another very successful center.

COHEN: If somebody wants to use one of these centers, do they have to bring their own money?

GRAY: No. Different centers operate different ways, but we pretty much support them from Beckman funds. People on campus can pretty much come in and use them free of charge for the most part—if it's a research activity. If you need somebody in a facility to do something for you, there's a charge for that. But where you're involved in research in the resource centers, it's all supported by Beckman funds. So you really don't have to have any money. You need to make an appointment with an MBI or somebody else working in the center to talk over your proposal with them. If it's a good research problem, you'll get time on the instruments and help from the people there, and so forth, to make it happen.

I think it's worked pretty well. We have a list of contact people on the Web. And we have a big catalogue of available instruments, and so forth.

Today we have a whole number of resource centers and facilities across chemistry and biology to support them. We have a genome center. We have a transcription factor center. We have an X-ray diffraction facility. We have a biomolecular design center. We have a mass spectroscopy center. Our newest one, really, is the new materials center that we reconfigured with Michael Freund. But we're continually upgrading things.

Now, all the space is occupied. There are some things we'd like to do now that we don't have space for. So we're looking for opportunities around campus to make connections with people in other kinds of space. We're particularly doing that with our new Beckman Fellows program—a prestigious sort of senior postdoctoral program. We give Beckman Fellows a large stipend and a large amount of research support to come in and work more or less independently on projects associated with faculty members all over campus. So we will probably have some of these people in other buildings.

COHEN: Will you interact with the new Broad Center for biology?

GRAY: We think we'll interact very strongly with it because there'll be a large imaging activity in the new building. We also have a large one here, so we think there will be a lot of interaction in imaging and in structural biology. It's my understanding that Doug [Douglas] Rees, Pamela Bjorkman, and Steve [Stephen] Mayo are going to move to the new building and have a big structural biology effort, which we also have in the Beckman Institute. We see opportunities there to collaborate and to build up both imaging and structural biology and maybe some other things as well. That's also another opportunity for us, because we have support and an infrastructure, and this building has a lot of new space. So we will probably end up with activities in the new building.

COHEN: Now tell me about Jay Labinger.

GRAY: Jay Labinger has been the administrator of the Beckman Institute from the very beginning. We also have an executive committee that makes all the resource allocations. I don't make them myself; I make them in consultation with the executive committee. We get together every three months or so and go over where things stand and what the resource allocations to these various centers should be based on—basically on reports and proposals from the centers. We look at those carefully, and we then recommend allocations to the provost, and he either okays them or not. He's always okayed them up until now, because we go pretty carefully and the executive committee looks at them carefully. And Jay Labinger runs the show. He's extremely good. I hired him at the very beginning, and I really lucked out in getting him. He's incredibly smart and wise. I tend to be very softhearted and to always say yes to everything that comes along. He's much more realistic and sensible. He will say no when he needs to and will tell me when I have to say no. So he's been an enormous help to me. I couldn't have run the Beckman Institute without him. In fact, he runs it so well that it gives me plenty of time for research. I don't really do administration; I've never been very good at it. But I think I'm very good at delegating. I leave it to Jay really to run the show, and he does it magnificently. We have a great relationship, including a tennis relationship, that keeps us going as well. So it's been a lot of fun working with Jay. He is so good, and he knows where everything is. He is unflappable.

COHEN: So there's been a lot of success, at least in the machinery . . .

GRAY: Yes, I think we've got a very good thing going here that will continue to be of enormous value to Caltech as a whole. We pay our way; we're not a drain in any way. I think it's just the other way—we give back. We bring in a lot of money.

COHEN: Do any people come from outside?

GRAY: Yes, a lot of people come from outside to do experiments here. Practically every center we have has some specialized experiments that really can't be done anywhere else. That brings people here.

We have to be careful with that and not get overrun, because our first duty, if you like, is to the campus and the people on campus. If we have any extra time, people coming in from outside can do things. Many of the external people who come in are actually doing collaborations with campus people, and they end up publishing together. It's still a Caltech activity in that sense, and it gives us a little more outreach into the international community. And that's been very, very good.

Most of our centers, I think, are very successful. They've all had external reviews, where people come in and look at them and see how they're working.

COHEN: Have you ever had a case where you've had an unsuccessful center that you had to phase out?

GRAY: We have had centers that weren't doing what we had hoped as a resource center. Instead they were doing outstanding research and not reaching out to the rest of the community. Their research was really international class, but it was just the research of one group. [Ferkel Professor of Chemistry] Jack Beauchamp's mass spectroscopy center, I think, was an example of that for a while. So we decided to call those explicitly research centers and not phase them out. As long as they were doing world-class research, we felt we had a big investment in them. Jack's was recognized by the external review committee as a great center, but one that was doing Jack's work. It was and is great work.

It turns out that after we sort of changed that to a research center, Jack started reaching out to other people. [Laughter] Now it's become a real resource center. But we haven't really had to phase anything out. We've had differing levels of success, but I think I can say without exception that all of our centers have been enormously successful from just a world-class- research productivity point of view. They've been successful to different degrees in reaching out to the Caltech community and making the instruments available. Some of them have been enormously successful at that, and others have pretty much tended to stay as more of an extension of the research group that started them. But those groups have always been ones that are putting out tremendously important research results and winning big prizes, and so on. We've felt that that investment was really worth it, and we can live with the few that don't meet the original idea of a resource center.

We've also started some centers initially as research centers under one group, and then expanded them into resource centers. We did that with Mel Simon's genome center and with Eric Davidson's transcription factor center. They're now resource centers, reaching out to many more people, and they have been quite successful.

So we have a good mix of very distinguished chemists and biologists as principal investigators in the Beckman Institute. Most are NAS members. And so we have a very high-powered faculty group associated with the Beckman Institute, but only Bill Goddard and Scott Fraser have permanent homes here. The rest of us have our connections in other buildings through our divisions. If they phased out our centers, we would have a place to go.

COHEN: I see. How about the postdocs that you've brought in?

GRAY: Each center will have postdocs and students who work largely in the centers. They'll also have ones who work in the other buildings and maybe use the center a little bit. There are lots of postdocs and students whose home really is in the Beckman Institute. Take Nate Lewis, for example. Of course his office is over on the second floor of Noyes, and most of his group is there too. But he also has a group working in materials in the Beckman Institute. Their space is over here. That's true of essentially all the groups who are associated with the Beckman Institute.

Of course, my main office is over here. In fact, it would be difficult for me to really retreat to Noyes, having established a large operation over here. But I still have a research group in Noyes.

COHEN: How did the Archives come to be here?

GRAY: Lots of other things here aren't directly part of the Beckman Institute, but are in the Beckman Institute building. That's because, as I mentioned earlier [*Gray 2000–01, Session Six*], when Dr. Beckman looked at our original plans, he advised us to build a larger building. And so we added the sub-basement, and we divided the space between the Beckman Institute and the provost. The provost now has several activities here, including SURF and the Archives. A number of things in this building are in what we call provost-space. It was Dr. Beckman's vision, really, that led to a really nice place for the Archives. You were over in Millikan Library, as you remember, and it was kind of crowded. SURF was very crowded; and now SURF has a nice office here.

COHEN: Did Dr. Beckman have a say in the provost space?

GRAY: No. He didn't have a say in ours either. That's just not his style. He says, "I find good people and turn them loose." So I think he said, "I'm going to let the provost decide what to do." So it was really Dr. Beckman who urged us to build a larger building with the money and various resources. And boy, it's a good thing we did. We wound up with extra space that was assignable by the provost and that we figured we certainly didn't need for the first ten years of the Beckman Institute. I suppose now, looking at it, I could probably use that space. But frankly, it's very, very nice to have others here. It gives this place a better feel. It's very nice to have the Archives, the Beckman Institute auditorium, and the SURF office here. The interactions that we have with all of you make this a much more interesting institute. I think it came out very well.

COHEN: Now, here's an article that you published back in 1977 in the Caltech research magazine, *Engineering & Science*. It's called "Action in Chemistry," and in it you make some predictions about how chemistry's going to evolve in the next thirty years. Well, it's been twenty-four years. Shall we see how you're doing?

GRAY: Yes, let me have it. What did I say? Has it happened? Let's see, [reading from article] "I would like to leave you with a few predictions. I'm confident that within thirty years, we'll make hydrogen from water using solar energy." And that has happened. We're making it here. NREL [National Renewal Energy Laboratory] is making it. There's a twelve-and-a-half percent efficient solar cell splitting water at NREL.

COHEN: Was any of the original research for that done here?

GRAY: Certainly the original work, using red light and simple compounds to produce hydrogen from water, was done here by my group and Nate Lewis and Kent Mann in the 1970s. We've proceeded now from this and have gotten to the point where one is really designing systems that may be useful. So I would say that we are making hydrogen from water using solar energy. That prediction's okay.

"We will produce synthetic fuels by reduction of carbon monoxide and carbon dioxide." That's certainly true. We can do that now. "We will catalytically convert nitrogen under mild conditions to ammonia, hydrazine, and new materials." I would say that that's still on the drawing board. We can't do it under mild conditions, really, yet, catalytically. But thanks to work here by Doug Rees [Dickinson Professor of Chemistry]; and his group, we do now know the structure of the enzyme that converts nitrogen under mild conditions to ammonia. Professor Jonas Peters [Bren Professor of Chemistry] here is working on this problem of catalytically converting nitrogen to ammonia, hydrazine, and other materials. That's one of his big research projects; he's very excited about it, and Doug Rees and I are talking about it all the time. So I think I'm going to have to give that one more time. I made this prediction in 1977, so we still have six years to go. I think we may make it. I'll tell Jonas he's got six years to go. [Laughter] But we're getting very close to that one. There's been lots of advances, particularly in understanding the structure of the enzyme that does the conversion. No one has really been able to fix nitrogen in a simple compound. It's still one of those great mysteries.

Here's my main prediction now. In the last century the great advance for chemistry was that we understood covalent bonds—thanks to Linus Pauling and others. We understand the covalent bond, the strong bonds. This is the century in which we're going to understand the very weak bonds—what we call noncovalent assembly. These include the hydrogen bonds, the van der Waals interactions. These are the interactions that make life, that make the big biological machines that run life by making the enzymes that carry out these processes that we still cannot do. Thanks to the Genome Project, and the fact that we're going to know the structure of probably every protein in the human body before long, we're going to finally understand these weak interactions—these noncovalent interactions—and can put these big molecules sort of together so that they recognize each other. How to do this is to my mind *the* big problem in chemistry now—the biggest problem of all; one that will be solved in the next thirty years, I think, thanks to the genome project, and to all the other things that are going on.

So I'll leave you with that prediction [See also *Gray 2016, Session Four*]. Along with the indication that I've got to go see Jonas and tell him he's got six years to do that nitrogen job. [Laughter]

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Harry Barkus Gray **by Heidi Aspaturian**
Pasadena, California

Session 1	**January 28, 2016**
Session 2	**February 17, 2016**
Session 3	**February 25, 2016**
Session 4	**March 17, 2016**
Session 5	**April 7, 2016**
Session 6	**May 3, 2016**

SESSION 1
January 28, 2016

ASPATURIAN: This is January 28th, 2016, and this is the first in a series of interviews with Professor Harry Gray, which will amplify and expand upon a series created in 2000 and 2001 [hereafter referred to as *Gray 2000–01*] with Shirley Cohen and will also bring those interviews up to date, to this year. You know, here's something: Where does your middle name come from? Barkus.

GRAY: It's a family name. I really need to get one of these people who trace ancestors to look into who was originally named Barkus. But it is a name carried along on my father's side. His name was Barkus.

ASPATURIAN: That was his first name?

GRAY: No, that was also his middle name. But he was called Barkus—Barkus Gray—and I was called Harry. All I know, all I was told, is that it was a family name and somehow perhaps a Scotch-Irish name.

ASPATURIAN: Where else does your family originate?

GRAY: I'm not sure. My mother's family seemed to come originally from Germany, but I don't really know. I do know my wife's [Shirley Gray] family came from Germany. Her maiden name was Ernst. I think my father's background was mixed, but somebody in the family came from Scotland or Ireland. I haven't really looked into that, actually. I've never done a trace back to see where my family came from. I should do that.

ASPATURIAN: In the earlier oral history transcript, your mother's surname is spelled as Hubber with two 'b's and also as Hopper. How do we spell it?

GRAY: It was spelled Hopper with an "o." It could have been the other originally, but it certainly morphed into Hopper. [Laughter] Her first name was Ruby.

ASPATURIAN: That's a pretty name. It's very popular again now.

GRAY: It apparently is. [For more details on family history, family influences, and early life, see *Gray 2000–01, Session One*]

ASPATURIAN: You mentioned a few things when I saw you last week when you didn't want to go on record because you weren't feeling well.

GRAY: I didn't want to make a mistake.

ASPATURIAN: But now I'd like to bring these things back into the record.

GRAY: Okay.

ASPATURIAN: You talked about Bowling Green as being kind of the Confederate capital of Kentucky.

GRAY: It was the Confederate capital of Kentucky.

ASPATURIAN: I'd like to hear more about your experience growing up in the segregated South.

GRAY: You know, only now, looking back many years later, do I realize I sort of had two lives. One was in the schools I went to, and the other life was my work. I carried newspapers almost all the time I was a kid in a largely African American section of Bowling Green, and I made many friends there.

ASPATURIAN: That was from about the age of twelve, I believe?

GRAY: Well, I started before that. At age twelve you could become a newspaper carrier for the *Park City Daily News*, which was the local newspaper, but I wanted a job early so I started hanging around the newspaper probably when I was about eight years old. At eight or nine I was hanging around trying to latch onto somebody. There was a newspaper carrier, C.C. Tatum—I don't know what his actual name was—and he took me on as an apprentice. I followed him around in this rather tough section of Bowling Green.

ASPATURIAN: Largely African American, from what you're saying?

GRAY: Largely African American section. It was very, very close to what I guess was then called the colored high school—a high school for African American kids.

ASPATURIAN: So the schools were all segregated in those days?

GRAY: The schools were all segregated. I went to segregated schools, but I carried newspapers in the African American section very close to where the African American high school was. And I made many friends, and it never occurred to me that there was some problem. I never thought about race, actually; I was very much at home with my friends in this part of the town. I don't know why I didn't think more about it. I went to schools that were all white, and I carried newspapers where it was essentially all black.

And it didn't bother me. I had a great time. I carried the newspapers, talking to people, interacting with them. I knew everybody on that paper route.

ASPATURIAN: Were you the only white kid carrying papers in that part of town? I know it's a long time ago to remember.

GRAY: I think I was the only white kid. And I think there were only white kids carrying newspapers. I don't think there were any African American kids carrying newspapers for that paper.

ASPATURIAN: They didn't hire them.

GRAY: If they tried, I don't recall it.

ASPATURIAN: So they could subscribe to the newspapers, but—

GRAY: They all took them. I would hang out, talk to people, and segregation never occurred to me. Then later on, in high school, I worked my way up on the job. As I gathered experience, I started keeping books for the newspaper. I became the accountant for the newspaper, and I was there every night. I worked sixty hours a week at the newspaper, and I made quite a lot of money there. My father didn't make very much money. We got by; we weren't poor, but we certainly weren't well-to-do, and so I needed to make money when I was a kid. So I've always worked, all the time. I worked all day Saturday, all night Saturday night. My only time off was Sunday after I delivered the Sunday morning paper, having been up all night, getting the Sunday paper out, which was one of my responsibilities. But then I was trying to remember when I first started seeing these things with my dad. I think it was about 1954.

ASPATURIAN: You're referring to his—

GRAY: His interactions over segregation [See also *Gray 2000–01, Session One*]. He was principal of the Bowling Green High School, and so he had been hired by and reported to the school board. The school board as usual was a political body, made up of elected officials, and so he served, as did the superintendent, at the pleasure of the school board; and I think there were meetings and discussions starting about 1954 that had to do with segregation or integration and so forth.

ASPATURIAN: Would this have been after the Brown v. the Board of Education decision by the Supreme Court?

GRAY: When was that—

ASPATURIAN: 1954

GRAY: Yeah, it had to be '54. It had to be when that decision went down that people started talking about this. That's when he started to become stressed, and things really started to get to him because his position, I think, was quite different from anyone else on the school board. I think the school superintendent supported him a little bit, but not enough.

ASPATURIAN: What was his position exactly? Did he favor integrating the local schools?

GRAY: I'm sure my dad did. My dad was definitely for civil rights. He was an early proponent of civil rights. One thing sticks in my mind. This was the era of Eugene McCarthy—ah, no. The other McCarthy.

ASPATURIAN: Joe McCarthy [Joseph McCarthy, Republican senator from Wisconsin, was instrumental in fomenting and fueling the "Red Scare" environment, generally known today as McCarthyism, in the U.S. in the early 1950s. *–Ed.*]

GRAY: Joe McCarthy from Wisconsin, not Eugene McCarthy. It was totally different. I worked very hard for Eugene McCarthy and his campaign in 1968. I was dedicated to Eugene McCarthy; I thought he was a great guy. And of course he lost. That was the tragic year when Bobby Kennedy was killed so the whole thing was a nightmare in '68.

But that was the first year I worked very hard for any politician. I was very opposed to the Vietnam War.

ASPATURIAN: We'll go into that more later. But this was Joseph McCarthy.

GRAY: Joseph McCarthy. I'll never forget that Joseph McCarthy appeared on a special television program; maybe it was when somebody interviewed him, maybe that great interviewer.

ASPATURIAN: Edward R. Murrow. This would have been the early 1950s.

GRAY: Somebody interviewed him; it was a big television thing. I remember that my dad and mother watched that, and that after it was over my father looked up and said, "That man is really bad." I could just gather from these things that he must have been under stress because supporting civil rights wasn't, I suppose, the right thing to do with his school board. I don't know exactly what the discussions were, but I do know that he had a heart attack and died suddenly while I was in college.

ASPATURIAN: Which in your earlier oral history, you somewhat attribute to the strains and stresses he was under [See *Gray 2000–01, Session One*].

GRAY: I attribute it a lot to the stress and strain he was under. I may be wrong about that, but my mother and I both felt it. I know my mother did because she could see it more than I could, seeing him every day. She felt that the other factor was that he was a chain smoker. And that wasn't good either. So the fact that he was a chain smoker maybe had something to do with it, too, because he was very stressed. So it all—

ASPATURIAN: Came together.

GRAY: It all came together, and so I had to adjust very quickly to the loss of my father, being an only child and thinking about going off to graduate school and stuff like that. That's when my uncles stepped in and supported me. My younger uncle, who was a major player in the vacuum tube plant in Owensboro, Kentucky, took me on for the summer after my dad died so I could make some money.

ASPATURIAN: How old would you have been?

GRAY: I was eighteen or nineteen, something like that. My mom stayed at home, while I went to Owensboro and worked in the tube plant, where, by the way, I did some important work. My first patent ever came out of the work I did there: I developed a carbon coating for vacuum tube grids that was more reliable than any of the previous ones that General Electric had developed. I built eighty tubes and I tested them, and seventy-nine were perfect. When I showed the people there what I had done, I remember the panic that set in before they checked that I had signed the patent release. [Laughter] I think they knew that this was going to be a big deal for General Electric. It did turn out to be a big deal because later, when I was an assistant professor at Columbia, I got a letter from the CEO of General Electric saying they had tracked me down and saying, "You'll be happy to know that your invention"— basically, the carbon coating for grids for vacuum tubes—"is now in every high-voltage rectifier in the world." [Laughter] In fact, it's the only one of my patents—I've had lots and lots of patents—apparently that made a big difference. And it was done when I was just a kid and didn't know a lot about what I was doing, but I did a lot of Edisonian work [i.e., hands-on experimentation and fabrication à la Thomas Edison –*Ed.*] on that thing, and it worked out.

ASPATURIAN: Something else you mentioned when we talked the other day is that where you grew up, the Civil War was talked about like it had happened the day before yesterday. And that when you went to register to vote—

GRAY: Oh, yes. Sometime when I was a kid they passed a law that in Kentucky you could vote at eighteen, so the minute I was eighteen, I went into the Warren County Courthouse to register to vote. They all knew me; it was a small place and everybody knows everybody. And the clerk said, "Hello, Harry." I said, "I'm here to register to vote." "Okay, I'm registering you." I said, "Don't you want to know what party?" or something like that. He said, "Oh, no, I'm registering you as a Democrat because that's the only way to register in this county; otherwise you can't vote." Because no Republicans ran for office. There were no Republicans. Everybody's a Southern Democrat.

ASPATURIAN: At that point it was the Solid South [i.e., solidly for the Democratic Party].

GRAY: It was the Solid South. Everybody's a Southern Democrat and they ran everything—the Southern Democrats, very conservative. They didn't realize they were all Republicans until much later.

I've been a life-long Democrat. I was told to register as a Democrat, I followed orders, I didn't complain. I said, "Well, all right. I want to vote in local elections, so put me down."

ASPATURIAN: I want to look a little more into your early interest in science. One of the things I found in a couple of the interviews that you did online was that you talk a lot about your early fascination with color.

GRAY: Yes.

ASPATURIAN: When did that start? Do you recall?

GRAY: It started very early because by the time I was eleven, I wrote a note to my mother saying, "I'm going to devote my life to science."

ASPATURIAN: You wrote it in a note?

GRAY: I wrote it in a note, that I've decided to devote my life to science. I wrote her that note after my grandmother, her mother, died. She was a very special grandmother, living on a farm in Woodburn, Kentucky. I'd been on the farm many times. I went to it in the summers, and I stayed with her. I got up early, went on the milk route with the little cows, all kinds of stuff. She was a special grandmother, and she was taken from me by cancer. And I thought that was very bad. I was very upset about that. That's when I wrote my mother that note, feeling somebody's got to do something about this. I had been fascinated with color before that. Even when I was five or six, seven, I was fascinated with colors. But this grandmother's death galvanized my resolve to become a scientist. And I haven't wavered.

ASPATURIAN: No.

GRAY: So I started setting up my own lab.

ASPATURIAN: In your home?

GRAY: Yes. I started with a couple of chemistry sets, a Gilbert chemistry set first. I might have had a Chemcraft set too, but I know I had a Gilbert set. And I was frustrated that the Gilbert set didn't have enough chemicals to satisfy me, so I started writing to the supply house in Chicago.

ASPATURIAN: You were just a kid, right, writing to a chemical supply house? Did they know you were eleven years old?

GRAY: No. They thought I was a professor at Vanderbilt.

ASPATURIAN: Now why would that be?

GRAY: I don't know.

ASPATURIAN: I see.

GRAY: I must have put on a good show because they sent me big bottles of nitric acid, sulfuric acid. I had these big bottles of acid. I bought big retorts, bigger than my flask. I had a big lab.

ASPATURIAN: And what did you do with all this stuff?

GRAY: Well, I experimented with my mother's clothes. I would pour acid on her clothes to see how fast they would react. That's one thing that didn't go down too well.

ASPATURIAN: Did she know you were going to do this before you did it?

GRAY: I don't think so. But she was okay with it because I was experimenting, and she encouraged me.

ASPATURIAN: What were you trying to find out? What the colors broke down into or what happened to the fabrics—

GRAY: I don't recall, but I made a lot of colors. There were a lot of color compounds I was interested in making. One was Prussian blue, which I liked very much, and I made lots of it.

ASPATURIAN: Is that a bright blue?

GRAY: It's like blue ink. And in fact it is blue ink in lots of things. It's a very deep blue dye. So I was fascinated by Prussian blue, and I also made other things. I made colors that disappear when you shined a light. I played around with lots of colors. I played around with smoke bombs. I liked making very hot things.

ASPATURIAN: Were you reading science magazines to get ideas? We had no Internet in those days. Where did your inspiration come from?

GRAY: I got chemistry books from somewhere. I was reading stuff by the time I was a senior in high school. I knew a lot of chemistry, and that's when they let me teach the high school chemistry course. There was a teacher assigned to the course, Mrs. Ross, but she let me basically teach the course because I knew far more chemistry than anybody at the school. And so, you know, we had a nice class. [Laughter] As I like to say, I was inspired by my high school teacher.

ASPATURIAN: You make a reference in one of your interviews to emptying out the high school where your father was the principal with a smoke bomb. What happened?

GRAY: It was either sophomore or junior year in high school. I think I was in the eleventh grade when they asked me to do some kind of smoke bomb thing that they needed for a high school play.

ASPATURIAN: Special effects.

GRAY: Special effects, and I overdid it. I overdid it and set off a smoke bomb that emptied the place.

ASPATURIAN: How did your parents react to all of this? You dissolved your mother's clothes. You emptied your father's school with a smoke bomb.

GRAY: I should say that my parents were always totally supportive of me. I was an only child, and I was a good kid. If I got into something like this, it wasn't deliberate. I wasn't trying to hurt anybody, and I was doing my best. I made extremely good grades. I was always at the very top of every class I was ever in.

ASPATURIAN: And working sixty hours a week on top of that.

GRAY: In high school, yes. I had no trouble with any of the courses. I was way ahead of everybody in everything, and my parents just—you know, the fact was that I didn't goof off, I didn't get into trouble. I always made terrific grades and was very good to teachers. I respected all my teachers, and they cut me a lot of slack.

ASPATURIAN: It sounds like it.

GRAY: Thinking back on it, my parents just said "Look." I did well in everything, and they just supported me. I think they were very proud of me.

ASPATURIAN: Did you have other hobbies and interests when you were a kid and an adolescent? I'm always interested in what kinds of books people who go on to be influential scientists read. Were there books that particularly appealed to you as a youngster?

GRAY: I liked Sherlock Holmes.

ASPATURIAN: Okay, the great investigator. And he used a lot of chemistry.

GRAY: I liked murder mysteries. I've always liked murder mysteries and true-life narratives. In more recent years I've liked P. D. James. I've read almost all of P. D. James. That's the kind of thing I like: Agatha Christie, P. D. James, Dorothy Sayers.

ASPATURIAN: Any nonfiction that appealed to you as a kid?

GRAY: Well, I like nonfiction a lot. I love documentaries. I love Ken Burns, what he does on television.

ASPATURIAN: As a child or adolescent, did you enjoy history, for example?

GRAY: Very much. History's always been one of my favorites. I love history and the historical context of everything. One of my hobbies, almost, is history of science. I really love the history of how science developed; that's a big thing for me. I had a lot of other hobbies. I collected coins, I collected stamps. Mainly because my uncles all collected all kinds of stuff. One of my uncles collected everything, and I kind of followed him in my coin collection. I gave him my coin collection after a while because he loved it. I never gave him my stamp collection; I still have that. I collected baseball cards.

ASPATURIAN: How about rocks and minerals, with their beautiful colors?

GRAY: Well, I've loved crystals. I love to grow crystals, and my daughter loved to grow crystals and we shared that. I've never collected gemstones like George Rossman [professor of mineralogy]. I could have gotten into that because I love gemstones, and I love to talk about the colors, and I always devoted a whole class to the color of gemstones because it's big in my field and I think it's terrific. I could have gotten into that early, but you probably had to have a little money to get into that.

ASPATURIAN: Maybe so.

GRAY: And I could collect coins a little better, and stamps, without having too much money. I had lot of hobbies. I played golf. I played tennis. I was quite a good tennis player. I won many tournaments. In other words, I was an athlete. I was an athlete both in high school and college. I played Division 1 Tennis in college. NCAA Division 1. A lot of tennis, a lot of tournaments. I played a lot of golf.

ASPATURIAN: Were you ever tempted by a pro career?

GRAY: No, I was clearly not that good. And I never thought of that, actually. I clearly knew what I wanted to do: I wanted to do science. I knew that for sure, and these other things have stayed with me as hobbies. I like music a lot. I like dancing. I was a really good swing dancer, and I won a lot of swing dancing contests with partners. Rock and roll I loved because of the closeness to Nashville.

ASPATURIAN: That's right, you said that in Bowling Green you were just over the border from Tennessee.

GRAY: Yeah, I was in Nashville, always hanging out with rock and roll and all kinds of stuff. I interviewed Elvis Presley.

ASPATURIAN: Really? In what capacity? When?

GRAY: For the newspaper.

ASPATURIAN: Let's talk about that for a moment. The college newspaper?

GRAY: No, the *Park City Daily News*. The newspaper I worked for.

ASPATURIAN: Oh, my goodness. How old were you?

GRAY: I'm a year younger than Elvis.

ASPATURIAN: Elvis has left the building, however. I don't know how old he would be.

GRAY: Elvis has left the building. But as I told you, Heidi, I worked nights at the newspaper keeping books.

ASPATURIAN: I did not realize you also did journalism for the paper.

GRAY: This was before Elvis Presley became super-famous. I was probably eighteen. He was probably nineteen. He wasn't twenty because we were born in the same year, and I think he was born in January [January 8, 1935], and I in November [November 14, 1935]. He was not quite a year older than I was, so we were practically the same age. But even at that time, of course, his manager, Colonel Parker—

ASPATURIAN: Oh, yes, Tom Parker.

GRAY: He was in Louisville, and he had this fleet of Cadillacs that went with him. He had a whole group of people, and there must have been six or seven Cadillacs in this caravan that he took from Memphis through Nashville, and from Nashville on to Louisville. And on the Nashville to Louisville road, just north of Nashville, is where I'm from. There is a nightclub right outside Bowling Green called Manhattan Towers.

ASPATURIAN: The name. I love it.

GRAY: Manhattan Towers. And one night his entourage stopped at Manhattan Towers, and the word spread all over town. It might have been eight or nine o'clock at night—

Elvis was in town! Elvis was at Manhattan Towers! And the city editor calls me because I was the only guy at the newspaper at night, doing the books. I usually worked until about eleven at night, doing the books. It might have been around 8:30—I've forgotten—but I got a call from the city editor. He said, "Harry, Elvis Presley is at the Manhattan Towers. Drive out there and interview him. Take the big camera." It was one of these big four-by-five things, where you have to plug in the film and pull the thing out. I went out and barged into the Manhattan Towers. There must have been thirty women surrounding him.

ASPATURIAN: Elvis?

GRAY: Elvis. They were everywhere talking to him, with these other guys hanging on, of course; his group around him. So I barged in and I said, "Elvis, I'm from the newspaper. I'd like to interview you." He says, "Okay, come on out to my car." We sat in his car, in his Cadillac. He sat in the driver's seat, and I sat on the other side, and we talked for an hour. He was a nice guy. A very nice guy. The one thing I remember is that he said, "I feel that things are getting away from me. It's going to be too big for me; I'm not sure I can handle all this." And so I wrote it up. It was on the front page of the *Park City Daily News* the next day.

ASPATURIAN: Does a copy of that still exist somewhere?

GRAY: I don't know. It probably does in microfiche somewhere. Elvis Presley in the *Park City Daily News*. It was on the front page the next day, I can tell you that.

ASPATURIAN: This would have been around what? 1956, I guess.

GRAY: Well, it had to have been either '55 or '56. I don't think it was '57. I left Kentucky in '57. [Laughter]

ASPATURIAN: Let me get this straight: By the age of twenty you were basically the CFO of your local newspaper; you had a patent; and you'd interviewed Elvis Presley. Anything else?

GRAY: That's about it. [Laughter] That's not bad.

ASPATURIAN: No, it's not. I imagine that in high school you excelled in math and physics. What drew you to chemistry? It sounds like you could have gone in any direction you wanted.

GRAY: That's a very good question. I'm not sure why because I did a lot of math and physics. Those fields somehow just never struck me the way a chemical change—looking at how colors change and reactions—appealed to me.

ASPATURIAN: Was it something about the tactile interaction, too? I mean, you were working physically with things as opposed to thinking about them all the time?

GRAY: Yeah, probably, probably. Experimenting. I could experiment with things easier. Physics was a little more abstract and so forth, and math. I like to fiddle with compounds and stuff, so I guess you can get into hands-on science earlier with chemistry than you can with these others, and I was hooked by that. It's hard for me to say exactly why I completely stuck with chemistry.

ASPATURIAN: You knew when you started college that that's what you were going to do?

GRAY: Oh, absolutely.

ASPATURIAN: You were that sure that young.

GRAY: I knew I was going to be a chemist when I was eleven years old.

ASPATURIAN: At the age of eleven?

GRAY: Oh, yes. I was dedicated. I never wavered. I was going to do chemistry. [Laughter] No early teacher or anything like that convinced me. On my own, I figured I wanted to do chemistry. Period.

ASPATURIAN: Biographies of famous chemists, anything like that? As a kid.

GRAY: I knew about the American Chemical Society, and I started writing in to them about stamps. I've got some things I received back from them. But it's hard to understand why I was so dedicated to chemistry.

ASPATURIAN: Somehow—

GRAY: I never wavered. At Northwestern I very briefly thought about going to med school and getting an MD as well as a PhD.

ASPATURIAN: Was this partially because of what your experience had been with your grandmother?

GRAY: I don't think so. I think it was just that I thought maybe it would be a good thing to do. That thought lasted about one day. [Laughter] I think it was because two or three of my classmates thought, "Let's look into this," and I went with along them for a day—it certainly wasn't my initiative. [Laughter] I explored doing an MD at the University of Chicago while I was also doing a PhD at Northwestern. I spent a day looking into what I would need to do and decided I didn't want to do it. But other than that, I never wavered.

ASPATURIAN: I find this interesting because, as you know, I've done a number of these oral histories, and there are varying experiences; but several people have told me that the roots of what they have made their careers started very early in their childhoods. Something just grabbed them and wouldn't let them go.

GRAY: I just loved chemical reactions and looking at change—mixing things and looking at how they changed and thinking, "That's really fascinating; what's behind all that? And what do these colors have to do with it? Why are these things different colors? Why do the colors change when you pour hydrochloric acid over cobalt, for example; why does it turn deep blue? What is that?" I learned later what it was, and I wrote lots of papers on it. [Laughter] I've explained a lot of it myself.

ASPATURIAN: You found your answers.

GRAY: Yeah. And then when I went to Western Kentucky State College [now Western Kentucky University] in Bowling Green, I had three chemistry teachers there who were all very good and who basically gave me free rein of everything and let me do research. One was a man by the name of Ward Sumpter, who had done synthetic organic chemistry at Yale, and I hooked up with him for research, so my first research was really in synthetic organic chemistry, and he wanted me to go to grad school to do that. I did well in synthetic organic chemistry and did a lot of nice work with him as an undergraduate, and then he directed me to Northwestern University. There was a Kentucky chemistry network all around, and there was a Kentuckian at Northwestern named Robert Baker, who was a distinguished organic chemist, and Sumpter basically told me to go to Northwestern and hook up with him. [See *Gray 2000–01, Session One*, for a more detailed account of the undergraduate years at Western Kentucky.]

ASPATURIAN: Anything else you'd like to say about your formative years?

GRAY: We've got a lot of it already on tape don't we Heidi?

ASPATURIAN: We have some of it in your earlier interviews, but what we've got now is mostly new.

GRAY: We've added a few things to it.

ASPATURIAN: Elvis did not make it into the earlier interview.

GRAY: Elvis didn't make it into the early interview! I tend not to push that because it's really hard to believe, but it's true that I did interview Elvis Presley. [Laughter] Before he went to New York and became super-famous, I had that one-hour meeting with him, one evening.

ASPATURIAN: One enchanted evening.

GRAY: At Manhattan Towers. [Laughter] And that was it, and he went on to great fame and glory.

ASPATURIAN: So did you in your own way, I would say.

GRAY: Yeah, and apparently I lived longer.

ASPATURIAN: That's true, that's true.

GRAY: He got into all kinds—it did overtake him. Life was too much for him. He was right; it was too much for him.

ASPATURIAN: What was it he said to you, "It's getting away from me"?

GRAY: He sort of predicted that in a way when I talked to him. He was clearly a sweet guy, actually; I really enjoyed talking with him. I think he said, "I'm happy that you took me out because I'm tired of all these people clawing at me." He said, "I'd like a little time for myself." I remember that, and I remember that he was a nice guy.

ASPATURIAN: I'm sure he was in those days.

GRAY: He was a very nice guy, forthcoming, and I think he could see what was coming, that his career was going to be something that was going to be hard to manage.

ASPATURIAN: Was this the first piece of yours to appear on the front page?

GRAY: Not only was it the first one, I think it was the only one because I don't ever recall being asked to do another one. I think this was a one-off operation. I didn't mean to say the editor called me all the time. Normally there wouldn't be anything of enough interest to call me out and get me out to someplace. But Presley, he was in another world, so they had to cover that.

ASPATURIAN: Somebody had to.

GRAY: Somebody had to cover that. So I did it. But that was the only time. I was the accountant; they didn't want me to write stories.

ASPATURIAN: Somebody had to watch the finances. Did you continue working at the paper all through your years in college?

GRAY: Yes. Until I left for Northwestern I continued working all the time. As a matter of fact, they gave me more hours after my dad died, so I could make more money.

ASPATURIAN: Obviously, you had a head for figures and it sounds like also for management. Were you ever tempted by the thought of moving into the private sector and going into business?

GRAY: No. In fact, I think I made a pretty good decision after my summer at General Electric. I was successful there, but I didn't like it that much. I could tell that I don't really want to work in industry and that my thing with the newspaper is just a job: "I don't really want to do this; I want to do science." I will say that at this period of my life, I probably felt that I would never reach the level of being an outstanding researcher, and so my thoughts were probably about becoming a chemistry teacher.

ASPATURIAN: Really? That is where your ambition—

GRAY: I would say that when I went to Northwestern, one of the things I thought I could do was get a PhD and go back to Kentucky as a chemistry teacher. But as soon as I got into research at Northwestern, and it became clear I was quite good at it, it was the folks there—my teachers, my professors—who convinced me that, "really, you should be in research." [See also *Gray 2000–01, Sessions One and Two*]

ASPATURIAN: So this was in graduate school that this happened?

GRAY: Oh yeah, my, my. I would say that my goals evolved from being very modest when I was in high school and a college undergraduate into much greater goals through my time at Northwestern. By then I could see what was out there in chemistry and maybe how I could fit in and things I could do that hadn't been done yet, and with support from the people there who said, "You know, you're really good at this."

ASPATURIAN: And these were top-drawer people it sounds like.

GRAY: "You're really good at this; you can make a difference." And I remember one of the professors at Northwestern, Ralph Pearson, who just turned ninety-seven and is now at UC Santa Barbara, coming in and saying, "Look Harry, I know you thought about going back to Kentucky and giving back and teaching, but you need to play in a much bigger ball game. You should go abroad and get into this stuff; you can make a big difference. Later when you do all this great stuff, you can always go back to Kentucky and give back." I remember he said that. "You should go abroad."

ASPATURIAN: Did this come as a revelation to you? Were you stunned to hear him say this, or had it been germinating for a while?

GRAY: No, no. I had come around to the same thoughts myself—that I'm pretty good at this. I published my first independent paper—without my supervisors on it—when I was a graduate student. My lab mate and I did an independent study, which we published, and I was thinking about bigger things that I could do, so I had already sort of evolved into saying "My goals have changed. I think I can play in a bigger ballgame. I've seen what the big ballgame looks like and I'm comfortable with it."

ASPATURIAN: *You* didn't feel it was getting away from you. Quite the contrary.

GRAY: I thought I was always in pretty good control. And so I went to Copenhagen as a postdoc [University of Copenhagen, on a National Science Foundation Fellowship], and I really started a whole new field. Copenhagen is where I developed ligand field theory.

That was a great time because crystal field theory was a physics theory, and I needed to develop a version of it that was good for chemists. And that's what I set my sights on. When I got to Copenhagen, I started doing that and published a couple of papers that are really landmark papers in the field, and that got me the job at Columbia. [For a detailed account of the years at Copenhagen and Columbia, see *Gray 2000–01, Sessions Two, Three, and Four*.]

ASPATURIAN: You were still very young.

GRAY: I landed at Columbia at age twenty-five. By the time I was twenty-eight, I was a full professor there. Because I was really doing all this new work in this new area that I was on the ground floor of because I developed a lot of it in Copenhagen, and I carried it on at Columbia. So my star rose really fast. I was also very lucky because I was there at just the right time when the field needed something new, and there was almost no one in the U.S. in the field, and so only reason I was promoted so fast was because I was in great demand. I got offers from essentially every school in the country—from Stanford, from Caltech, from Chicago, from Ohio State, from North Carolina, you name it. Everybody was recruiting me. It wasn't so much the body of work I'd done as it was the fact that I was being recruited by everyone that got me promoted so fast. I wouldn't have been promoted so fast in a normal situation, but Columbia had to respond—

ASPATURIAN: All these offers.

GRAY: Essentially after just a year at Columbia, I got a tenure offer from the University of Chicago. I thought, "Columbia's never going to promote me; my bags are packed. I got a tenure job, I'm out of here, let's go to Chicago." But Columbia quickly got their act together and countered. They countered with a great apartment and promoted me, and I stayed because we loved Columbia. Shirley and I would have stayed at Columbia if Columbia had been big enough to support inorganic chemistry and allow me to recruit the folks to build what I wanted to build, what I built here. John Bercaw [Centennial Professor of Chemistry, emeritus], [Jacqueline] Barton [Hanisch Memorial Professor], Nate [Nathan] Lewis [Argyros Professor of Chemistry], Theo [Theodor] Agapie [professor of chemistry], Jonas Peters [Bren Professor of Chemistry], all these people I was able to recruit—

ASPATURIAN: Yes, I'd like to go into that more next time [See *Gray 2016, Session Two*].

GRAY: So we have a big operation here. I could never have done that at Columbia because Columbia's department is small and dedicated to organic and physical chemistry. It wasn't because of anything against Columbia that I left. I love Columbia. I love New York. I would loved to have stayed if I could have built the field there.

ASPATURIAN: But it was not possible.

GRAY: Yes.

ASPATURIAN: Do you remember what specifically drew you to Caltech?

GRAY: The opportunity to grow the field. It was J. D. [John D. "Jack"] Roberts [Institute Professor of Chemistry, emeritus, d. 2016], who said, "You could come here and build a field. We want you to do that. We want you to recruit people," and so forth. Caltech wanted to do what I wanted to do at Columbia but couldn't because it wasn't big enough to expand like that, didn't have the room. It's just a smaller department. But here, I could do it.

ASPATURIAN: Why don't we end on that note today?

GRAY: That's a good note to end on.

HARRY B. GRAY

SESSION 2

February 17, 2016

ASPATURIAN: I wanted to step back for a moment in the context of all this youthful enthusiasm for Bernie Sanders [during the 2016 Democratic primaries]. You talked last time very briefly about becoming involved in the Eugene McCarthy campaign.

GRAY: Yes, in ’68.

ASPATURIAN: It was your first political involvement.

GRAY: Well, my first real serious political involvement, I guess, was in the McCarthy campaign because there was such turmoil over the Vietnam War. I was very much against the war, as were lots of kids here at Caltech, and there were grad students and post-docs, and we had rallies and so forth. I remember meeting with McCarthy.

ASPATURIAN: Really?

GRAY: Yes. I know I met him at some place in Southern California, and I think it was on campus. There was a group supporting him here at Caltech. That was an interesting time because I think it was not much later that George Hammond, who was a close friend of mine, was fired by [Richard M.] Nixon. George was going to be the deputy director of the NSF [National Science Foundation], but Nixon nixed that because Hammond had given a talk against the Vietnam War during that period—but it was after McCarthy.

ASPATURIAN: It would have to have happened after the 1968 election.

GRAY: It was after the election. Nixon was president.

ASPATURIAN: Right, right.

GRAY: So it could have been in ’69 or ’70. I remember that because Hammond [at the time chair of the Caltech Division of Chemistry and Chemical Engineering] made a talk about his opposition. There was a big crowd out by Winnett Center. That got back to Nixon’s group, and they withdrew his appointment to the NSF. I remember that. But, yeah, I went around knocking on a few doors for McCarthy, trying to rev up, drum up, enthusiasm and support for McCarthy before the California primary, which he lost to Kennedy.

ASPATURIAN: I remember that aftermath very clearly.

GRAY: That was the end of Eugene McCarthy’s campaign, and it boiled over with Humphrey.

ASPATURIAN: Kennedy’s assassination.

GRAY: Kennedy was assassinated the night he won the primary, and then we had Humphrey and all the other people fighting in Chicago [at the 1968 Democratic National Convention] and this big turmoil in Chicago at the convention. It was a big mess, and Humphrey emerged as the candidate. We were all against all the other folks—we were for McCarthy. A lot of people sat out the general election, and that’s what allowed Nixon to win.

ASPATURIAN: That’s right. The country was so polarized.

GRAY: We were really very polarized. I realized my mistake toward the end and said, “I’ve got to vote.” I remember I was in West Virginia and called Shirley and said, “You gotta vote for Humphrey.” Toward the end there, Nixon was just—I don’t know. I just said, “We gotta beat this guy.”

ASPATURIAN: Humphrey very nearly pulled it out. The thinking was, two or three weeks more, and he would have won.

GRAY: I think he would have pulled it out. People were starting to see the downside of Nixon and so on. But Democrats were really split, and all of McCarthy's supporters were very upset. I remember that. Anyway, it is what it is. But with McCarthy I finally had a candidate I really liked. The next one was George McGovern [1972], and he got crushed. Then I really liked Jimmy Carter [1980]. He won, but he was pretty ineffective as president. Disappointing. I think he was really too smart to be president. And then I kind of rambled along, and now I'm for Bernie. But you know, various people who are scared to death of [Donald] Trump and [Senator Ted] Cruz are telling me it's not right to be for Bernie because he'll get crushed. They're telling me it'll be just like George McGovern all over again. Then we'll be in bad shape. I should be supporting someone more reasonable. I said, "Okay, I can be for Mayor [Michael] Bloomberg [three-term mayor of New York City, 2002–13]." I do like Mayor Bloomberg a lot because he's for gun control.

ASPATURIAN: You wouldn't support Hillary Clinton?

GRAY: Well, given the choice. [Laughter] I'm not supporting her now, but if the choice is Hillary Clinton or one of these lunatics, you've got to support somebody who's at least reasonable, although I think she's got a lot of baggage, and I think she'll say anything to get elected. And that's not true of Bernie. Bernie's a straight arrow, I think. So I'm really for Bernie. I don't know what to do now. I have very mixed feelings. I thought Hillary did a very good job as Secretary of State. I was very much for [Barack] Obama and I think he's done a great job as president.

ASPATURIAN: I agree.

GRAY: I don't understand these people who are dumping all over him. He's done more than anyone could have expected anyone to do in this type of world. I don't know. I guess my problem with Hillary goes back to early days when it was pretty clear she knew that Bill had lots of problems with other women and all of that stuff. She knew that and she just flat-out lied about it. Just because she desperately wanted to get ahead. I think her ambition has overrun her judgment in many cases, and I don't think people trust her.

That's the problem, a matter of trust and integrity. I don't think she has integrity and Bernie does, but she won't ruin the country the way those kooks will. So, given that, I know which way I'll go. I've learned my lesson a few times. You get so invested in some of these candidates you like, like Gene McCarthy. I said, "Okay, they've clobbered him, and I'm just not going to vote. I'm going to sit it out." Well, that's a big mistake. That's a big mistake. You get crazy things if you sit it out. You've got to stay in there and at least vote for the better candidate. And she's certainly a better candidate than those kooks. I mean [Donald] Trump's a joke, and Cruz is dangerous. I don't know what to make of [Senator Marco] Rubio. This guy from Ohio [Governor John Kasich] seems halfway reasonable. I don't think Jeb Bush would get us in big trouble, but I don't care for him. In fact, the Republicans and the "show," the reality TV they're putting on now, is embarrassing for the country.

ASPATURIAN: Well, of course, Donald Trump is at the center of it.

GRAY: Can you believe it? It's just crazy, Heidi; it's just totally crazy. Anyway, I worked hard for McCarthy. I liked him a lot. I thought he was a really great guy who would do great things for the country, but the Establishment got him. You know how the folks who are really liberal get branded as commies or something. There's a branding there. At least Bernie has stood up and said, "That's who I am. That's who I am."

ASPATURIAN: You have a whole new generation that doesn't remember the Cold War and has no auto-recoil from such words as "socialist."

GRAY: Bernie is telling it like it is. [Laughter] What am I going to do? Anyway, I'm voting for Bernie.

ASPATURIAN: It'll be interesting in a few months, won't it?

I would like to talk today I think about your years as division chair [of chemistry and chemical engineering] and the recruiting that you did.

GRAY: Oh, I did some recruiting that I'm very proud of.

ASPATURIAN: So you're up with that?

GRAY: I'm up with that.

ASPATURIAN: How did you come to be division chair?

GRAY: I think that when John Baldeschwieler [Johnson Professor and Professor of Chemistry, emeritus] became division chair, we had brought him in from Stanford specifically to be division chair. It was 1973, and he was chair for five years— a one-term chair. I became division chair in '78. Seventy-eight to '84, and John, I guess, had been doing a lot with George Hammond, who had been division chair before that. George Hammond and I were very close. We collaborated, and George had me doing a lot of things, helping out on committees and stuff, and so at some point—George might have done this, or it might have been Baldeschwieler—they made me chair, basically, of the tenure appointments, the staffing committee. I became chair of the staffing committee, a very powerful position. You then get involved in all of the recruiting and the tenure decisions.

ASPATURIAN: I'd like to talk about that also.

GRAY: I'm not sure when I became committee chair; maybe I was on it when Hammond was chair. But when Baldeschwieler became division chairman, I definitely became his lieutenant in the sense that I was, I think, chair of the staffing committee all during his tenure. So I was actually more involved in recruiting there and drumming up things than I think he was. I really became the leader at that point for recruiting.

ASPATURIAN: Whom did you recruit during this time?

GRAY: I recruited Ahmed Zewail. [Pauling Professor of Chemistry and professor of physics; 1999 Nobel laureate in chemistry. He died in August 2016, three months after the completion of these interviews.]

ASPATURIAN: Yes, I'd like to talk about that. He was at Berkeley at the time.

GRAY: He was at Berkeley, and I knew he was interested in a position here, and I knew he was going places. I had enormous faith in what he could do. But Ahmed is very aggressive and obviously very, very good, so when I started to recruit him, the first time, I think his aggressiveness—

ASPATURIAN: He came down here?

GRAY: It was a two-year deal. The first year, we didn't make him an offer. I think I wanted to, but I couldn't talk some of my colleagues into it. I think they were—well it's one of those things where you have a hard time being for people who are actually better than you are. [Laughter] There's a lot of that in college and university recruiting where you see a really bright young person who's going places and is maybe a little too aggressive in his or her style, which in an interview might rub a couple of more conservative people the wrong way. So they're hesitant to go all out, although they're not really opposed. You need everybody enthusiastic to make an appointment here. So the first year he was down, I couldn't get enough strong support to make him an offer. But he was still doing his postdoc in Berkeley, so he was perfectly okay. We saw him early. But I brought him back. I knew we wanted to recruit this guy.

ASPATURIAN: Why? What was it that impressed you so much?

GRAY: He was very smart, and I thought he had lots of good ideas, vigorous. I thought he was just what we needed. A young guy who would give the place a real shot in the arm. This was of course before [Rudolph A.] Rudy Marcus [Kirkwood and Noyes Professor of Chemistry; 1992 Nobel laureate in chemistry]. It was before Bob [Robert H.] Grubbs [Atkins Professor of Chemistry; 2005 Nobel laureate in chemistry]. I don't remember who was around, but we had some fairly conservative folks back then who looked at him as maybe the brash young gunslinger and were a little hesitant. It was kind of like, "Well, let's wait and see; we don't have to go so early; let's bring him back again." It wasn't that people were against him. They were just trying to sort it out, I think. I don’t remember the exact history of it, but I do remember that he came once and I started drumming up business and support, but I needed to get him back one more time a few months later to make sure we could get all the support we needed to make him an offer. And the second time was very effective. Everybody got enthusiastically behind it, and we recruited him and never looked back. And of course he’s a superstar. He started winning awards.

ASPATURIAN: What he says in his book [*Voyage Through Time: Walks of Life to the Nobel Prize*] is that he was juggling offers from about a half a dozen places. Caltech had not yet made him an offer. I think he said he called somebody and then heard from you, saying, “Ahmed, please don’t make a decision yet; we want you to come here.” Does that ring a bell?

GRAY: I think that’s right, that’s right. I knew he had a lot of offers. And I wanted to counter those offers. Down deep I thought he really wanted to be here because he had this enormous respect for Linus Pauling, who had been here. The tradition here meant a lot to him, and so I think he really wanted the offer here, and he was holding everybody off. He and I had a number of conversations on the phone, I’m sure, during this period, where I’m saying, “Ahmed, I really want you here. I’m sure I can do it, give me a little chance, I’ll get the offer.” I was really doing the recruiting on the staffing committee, so I was the guy who recruited him, and I was the guy who made it happen. If it hadn’t been for me, I don’t think it would have happened.

ASPATURIAN: That’s kind of what it sounds like when he writes about it in his memoir.

GRAY: Yeah, so he’s written about that?

ASPATURIAN: In his autobiography, *Voyage through Time*.

GRAY: So he does relate this.

ASPATURIAN: Oh, yes, he does. He does.

GRAY: He probably remembers it even better than I do, but I do remember being very close to him and interacting with him all the time, basically saying, "We're going to get this to you; hold off; we want you here." There was a second visit during this period in which everybody said, "Yeah, that's right, let's go for it." Maybe there were a last two sort of conservative folks who weren't quite sure we should go all out, but they came around big time on that second visit, as I recall. We got the offer to him in time, and he came, and the rest is history, as they say. He did get off to a little bit of a slow start when he came. I remember he was getting his lab going, and we thought he deserved awards fairly early on, but he hadn't won any. So we found an award in New York for a young investigator, I forgot the name of it. It was his first award. We found that, we nominated him for that, and he got that. And then, once he got that little medal, he started getting everything. [Laughter] We got a tiny little regional award for his work; they recognized what he was doing that was really good; and then of course he got hooked up with Dick [Richard B.] Bernstein at UCLA and together they made a fabulous team; they really grooved. I think Dick showed him a few things, but Ahmed made it happen. And Dick was a huge supporter of Ahmed's. So that's what transpired. Then he won the Wolf Prize and the Nobel Prize, and he's a huge star. But I think I justly deserve credit for recruiting him.

ASPATURIAN: He talks about it in his memoir. I'm working with him on his oral history also, and he talks about it there.

GRAY: I like to take credit for that. And then when I became division chair, I recruited Rudy Marcus and Bob Grubbs. I guess we have to look up when they came [1978; see *Gray 2016, Session Three*], but I recruited them both.

ASPATURIAN: Would you like to talk about how that came about?

GRAY: Yes. I take credit for recruiting all three of our Nobelists—I got three Nobel laureates. And I recruited another great scientist, Dennis Dougherty [Hoag Professor of Chemistry], who is a National Academy [of Sciences] member and a Nobel quality guy. So those four recruits—I did those, and I am so proud of those four. I think that was a major advance. It really made our division incredibly strong. They've all stayed, and they've all contributed, still all. Rudy Marcus is ninety-three, and he's still contributing. Dennis is fantastic, and Bob Grubbs, of course, is a superstar.

I do remember recruiting Rudy Marcus. I had my eye on him because of my interest in electron transfer chemistry, which I was doing, and of course Rudy was the leading theoretician in the world in electron transfer chemistry. I was pretty sure he was headed for the Nobel Prize, and I figured this place would be good for him and he would be good for us, but I didn't realize that we had a good shot at him. He was at the University of Illinois. He'd moved to Illinois from Brooklyn Polytechnic. That's where he started. I had known him when I was at Columbia and greatly admired him. In the early 1960s when I was still a professor at Columbia and he was a professor at Brooklyn Poly, I would go to their seminars, which he ran, and he was very dynamic. He would preside over the seminars, and I said, "This guy's really great." I didn't really get to know him then, but I knew of him, and I would see him in action. Kind of banked that. Then, sometime in that period when I moved out here, he moved to Illinois and he was doing very, very well there. But he did all his landmark work at Brooklyn.

ASPATURIAN: The theoretical predictive work on electron transfer.

GRAY: Yes, which won him the Nobel Prize. His famous paper was published in 1956. So, you know, I had him sort of in my mind, thinking it would be nice if we could recruit Rudy Marcus and how could I do this. I got to know him at the National Academy meetings. He was a regular attendee there, and I was chairman of the chemistry section at one time. I was actually chairman of the whole chemistry section, and I got to know all these folks really well during that period. That was when it happened. I had lunch at the National Academy with Rudy and his wife, Laura, and they had just come back from Oxford, where Rudy had been a visiting professor, and Oxford had tried to recruit him. I remember this, and Rudy will remember this luncheon. We were just talking about things, about his trip at Oxford, and he says, "Yes, I really loved Oxford; I loved the discussions we had, small groups and so forth; it was just terrific. And they made me an offer. They tried to recruit me. I loved the place, but the salaries are very low." And then the light bulb went on. I said, "Rudy, Caltech is very much like Oxford—it's a small place where you can interact with other people. And by the way, our salaries are much higher. How would you like to move to Pasadena?" At that moment, before he could say anything, Laura said, "Oh, Rudy, I just love Pasadena." [Laughter] I knew then, I had him. I had him. I came back, I got the gang together, got him out here.

ASPATURIAN: By "the gang," you mean the faculty?

GRAY: The faculty. I said, "We gotta invite Rudy here." We had him out for a talk, got everybody together and got an offer together, and I called him up. This all happened in a few months' time after he'd given me the signal at the NAS meeting in April. I don't know exactly when but a few months after that, we had him out, and I called up and made him the offer. He didn't mess around; he called me back and said, "I'm coming."

ASPATURIAN: No negotiating.

GRAY: No negotiating, it was a done deal. I do remember the recruitments very, very clearly. How it went with Ahmed, back and forth on the phone and all this stuff, and his offers; and me getting him back for the second visit and convincing all the folks that this is a guy we want, we gotta go for it. Rudy was pretty easy. He had a big reputation. The only slight problem I had with Rudy's recruitment were a few people who said, "Well maybe he's a little older than we would like; maybe we want somebody in their thirties or forties." He was already a little over fifty. But he proved that was a lot of BS—he was just getting started. So he's been here almost forty years too. He must have come right after Ahmed [1978; See *Gray 2016 Session Three*]. Bob Grubbs came around then too. And I recruited him.

ASPATURIAN: What led you to single out Bob Grubbs?

GRAY: I was really responsible for pushing directly both Ahmed and Rudy. They were really my ideas, which I had to sell. Bob Grubbs—that bubbled up. I knew him because he was at Columbia as a graduate student when I was on the faculty. So I had known him from way back and greatly admired him.

ASPATURIAN: And he was also a fellow Kentuckian.

GRAY: He's a fellow Kentuckian. There was a real gang of folks who wanted to recruit Bob. John Bercaw was one of them. I wasn't the total leader of that the way I was with Ahmed and Rudy. With Rudy, I was definitely the person who pushed it from Day One, because the chemical physicists didn't come up with that idea; I did. And then I convinced them that that was the way to go. It wasn't hard to convince them, but they didn't have the idea. I think they felt that Rudy's already well established, we can't move him, and he's over fifty. They were looking for younger people in general, which is what people always do. But I came up with this idea—and I wanted Rudy Marcus for selfish reasons, because he was the theoretician in the area that I was doing all these experiments in. So it was a little selfish on my part. Now with Bob, I was definitely the guy who took the leadership in calling him and recruiting him. I remember that.

ASPATURIAN: Where was he at that time?

GRAY: He was at Michigan State, and I had known him, of course; we were great friends at Columbia, we played basketball together.

ASPATURIAN: Yes, I can see how that would be.

GRAY: We played basketball together all the time. He was a little downhearted at Columbia because he'd come from Kentucky, then Florida, and the big city was a little much for Bob. I'd been there, and I sort of said, "You're going to be okay, Bob." We actually bonded and became very good friends. We weren't that different in age, and so I was very close to both Bob Grubbs and Bob [Robert G.] Bergman, who was also at Columbia at the time. Interestingly enough, it was Bob Bergman whom I helped recruit early on to come to Caltech [1967] but then he left for Berkeley [1977]. He and John Baldeschwieler, who was division chair, sort of fell out, so Bob decided he was going to leave. Then it fell to me as either staffing chair or real chair, of the division, to recruit somebody to replace Bob Bergman. That's what it was. So it's interesting: I knew these two people from Columbia; one left Caltech; now we gotta recruit the next one. A number of names actually came up, but we thought we needed to get a senior person to replace Bob Bergman in this field, and Bob Grubbs had done all kinds of really innovative and interesting stuff. But there was another guy—I think it came down to Bob Grubbs and Chuck [Charles] Casey, who was at Wisconsin. There was a group here who wanted to get Chuck Casey and another group who wanted to get Bob Grubbs. I was happy with either one. They were both terrific scientists. But Chuck Casey had a two-body problem. His wife was a big shot in the University of Wisconsin administration with a big salary, and she didn't want to leave. So we sort of gave up on Casey and focused on Bob Grubbs. It was really the organometallic chemists and some of the inorganic chemists, like Bercaw, who wanted to go after him. I was part of that, and I agreed with that, and I did the recruiting. I called Bob up and I told him this and that, and I negotiated the whole thing, and he remembers that very well because he always says, "Harry, you didn't offer me enough. I took your first offer. I should have held out." [Laughter] But after he got here, I was very good to him when I was chair. I remember buying a 400-megahertz NMR for him. He needed it and I had to really go do that when Robbie [Rochus] Vogt [Avery Distinguished Service Professor and Professor of Physics, emeritus; Caltech provost, 1983–87], who was provost, didn't want to do it.

ASPATURIAN: He didn't want to spend the money?

GRAY: He didn't want to spend the money and I told him, "This guy is really going places, we gotta get this right now," and so I made that happen. But Robbie and I got so crosswise over it that a few months later, I turned in my resignation.

ASPATURIAN: That was why you resigned?

GRAY: I was tired of fighting with Robbie. A lot of it was my fault. I'm not faulting Robbie Vogt. I was very aggressive as chair. I kept telling Robbie that he should be congratulating me because I was building the world's greatest chemistry department.

And that instead of yelling at me and not giving me what I wanted, he should be congratulating me. [See also *Gray 2000–01, Sessions Four and Five*; *Gray 2016, Session Three*]

ASPATURIAN: What was his response to that?

GRAY: He would yell at me. [Laughter]

ASPATURIAN: I see.

GRAY: So there was this interesting group. [Marvin L.] Murph Goldberger [professor of theoretical physics; president of Caltech, 1978–1987; d. 2014] was president. David Morrisroe [vice president for business and finance, 1969–1994; d. 2002] had all the money. Robbie Vogt was the provost, and I was the division chair in chemistry. And I was very aggressive with the administration in pushing our agenda forward, and I always overspent. I always was way in the red, overspending to do things I thought were important to build the division. I did all this recruiting—and look what's happened.

ASPATURIAN: Did you have a governing philosophy as division chair?

GRAY: Go after the best people [see also *Gray 2016, Session Three*]. Spot the best people and really go for it. Just bull ahead, don't listen to anybody who tells you that you can't do it. Don't listen to the administration. Bring 'em in. Before I became staffing chair and division chairman, the division operated sort of in the abstract by discussing appointments before there was a real body to discuss. Who's going to get the next appointment? Is it going to be organic chemistry, inorganic chemistry, physical chemistry, chemical engineering, or chemical biology? And they would argue forever about who needed it more. It drove me crazy. And it never went anywhere because people would stonewall it all around. There was no motion. That's why we weren't getting anybody because people had their little groups. My philosophy was, let's get some real people in here and then talk about it. Let's bring in the people like Ahmed and Rudy Marcus and Bob Grubbs; let's get young people in and look at them, and then we'll talk about who we should appoint rather than this whole business of what field is next. Let's bring the best people in; I don't care what they're working on. Get people fired up about them, and then if they're all fired up about it, I'll go over to the administration and say, "We gotta have this person [thumping desk for emphasis]. We gotta have them right now. This isn't just an appointment we're asking for and then we're going to look for it; this is a real person. You've got to decide right now."

ASPATURIAN: How did the division react to the change in the hiring culture?

GRAY: They went for it. I was very convincing, I think.

ASPATURIAN: Was there anyone you really wanted to recruit who did not come here?

GRAY: Well, let's see. During my time as head of the staffing committee and as division chair, I think I got everybody I wanted. I think I was pretty successful. There have been lots of people since whom we've wanted, who've turned us down.

ASPATURIAN: I was about to ask that question.

GRAY: [Laughter] Apparently I'm a better recruiter because I said, "Look, you're going to come here, you're going to do great work, we're going to support you, no doubt." I think people now are too timid: "Well, you know, we may not be able to do this or that." I didn't say that. I said, "We're going to do whatever it takes. We're going to spend whatever money it takes and do it." I didn't know whether I could do that or not, but I was going to force Robbie Vogt to come up with money. I will say this: Murph Goldberger was incredibly supportive.

ASPATURIAN: I hear good things about him from a lot of people.

GRAY: He was incredibly supportive. He doesn't get the credit he is due for being a great president of Caltech. He was one of the great presidents because he knew what the faculty wanted, and he went out of his way to get it. Robbie Vogt, of course, was the provost, and what he wanted was Murph's job. Robbie's history was knocking the next guy off in line and moving up. See, I was division chair when he was division chair of physics [Division of Physics, Mathematics and Astronomy]. Jack Roberts was the provost, and I knew he wanted to knock Jack off and get his job. But Jack Roberts was so straight arrow. Robbie, whatever he's good for—and he's good for a lot of things—was trying to undermine Jack and get Murph to fire Jack so he could get Jack's job. And then Murph didn't understand that the next guy Robbie's going to go after is Murph. And they cancelled each other out, actually, and they both got fired.

ASPATURIAN: Did you see these problems with Robbie and Murph coming before Robbie became provost?

GRAY: Oh, yeah. Oh, yeah.

ASPATURIAN: You did. Did you try to advise Murph that this was not the best idea?

GRAY: Well, you know, Murph had a decision to make when Jack Roberts stepped down as provost. The two candidates to be provost were me and Robbie.

ASPATURIAN: That's right, in your earlier oral history you talk about this with Shirley [*Gray 2000–01, Session Four*].

GRAY: Murph had a choice of picking me for provost or Robbie. And he picked Robbie. And he told me, "You know, Robbie really wants the job, Harry, and I don't think you really want it that much because you're doing so much with chemistry." And it's true. That's absolutely true. I didn't want the job.

ASPATURIAN: But it shouldn't be the key criterion of hiring somebody.

GRAY: I didn't want the job. I desperately wanted to go back to my lab, really, and I finally did. What drove me to resign was not really Robbie so much as his behavior gave me an excuse to go back to my lab. I could blame it on him, saying, "How dare Robbie

Vogt, yelling at me all the time?!" But that's just an excuse because I said, "I really want to get back to my lab. I really want to get back to my research now. This is cutting into my research." And I could use Robbie as an excuse: "I can't take it anymore so let's agree to disagree. I'll resign." And Robbie was happy with that. He didn't fire me. I could have kept on. In fact, the division overwhelmingly wanted me to continue for another term.

ASPATURIAN: I'll bet they did.

GRAY: They encouraged me, and I started my second term with their enthusiastic approval. I got through a first year of it, but things with Robbie and me got to the point where I could say, "I'm sure he'll be happy if I resign, and I do want to because what I really want in life is my science." I'd made a decision not to go up in administration. I turned down a couple of big president's jobs.

ASPATURIAN: Where?

GRAY: I shouldn't talk about it, but one of them is a real major private university.

ASPATURIAN: Not a liberal arts college, a university?

GRAY: No, no, a big-time research university, private. I turned that down. When I was division chair I turned it down.

ASPATURIAN: Must have been a temptation, Harry.

GRAY: Actually I took the job. I was courted by this place, and one of our trustees who was also a trustee at this university wanted me to put my name in the hat, and I did and made some visits. Their trustees came to visit, and then they called and offered me the job, and I accepted. Overnight I said, "This isn't right for me." I called them the next day and said, "I have to withdraw. I can't do it; my life is science." So that also was part of it, and then I said, "Well if I'm going to do that, I've got to get out of this division chairmanship, too. I've already recruited three Nobel laureates—" Well, I didn't realize they were going to be Nobel laureates, but I thought they were going to be pretty good. "I've done my job; I'm getting out of here on a high note." And I did.

ASPATURIAN: On that note, I have several other questions about this, but I feel I should tell you we've done forty-five minutes.

GRAY: Let's stop here and pick up here.

ASPATURIAN: Absolutely.

HARRY B. GRAY

SESSION 3

February 25, 2016

ASPATURIAN: Last time we talked about pinning down the hiring dates for Rudy Marcus and Bob Grubbs, and I did check into those. They were both hired in 1978, the same year you became division chair. So you must have juggled both appointments simultaneously.

GRAY: Okay, that's good. I don't know which one came first.

ASPATURIAN: I could not find that.

GRAY: I was clearly doing both of those appointments.

ASPATURIAN: You put them both through at the same time.

GRAY: Pretty good. [Laughter]

ASPATURIAN: I wanted to talk a little more about your years handling the division. You talked about how you revamped the hiring process, looking for the best people rather than saying, "Let's think about this or that niche."

GRAY: I was quite frustrated when I was staffing chairman, and even before, that all of the conversations about staffing had to do with which subgroup's going to get the next appointment, and there would be seemingly endless discussions of who had the best case. It went on and on and on, and by the time we figured out maybe what we should go for, all the best people were gone. I went through this as well when I was division chair, in that I would announce that we really ought to be looking at prospects all the time rather than trying to decide who's got the billet. I said, "Let's just start hiring the best people. Let's everybody keep your eyes open for great people all the time, whatever they're doing. Bring their names forward, and we'll invite them for seminars. If we get enthusiastic about them, whoever they are, then we present that as a specific case to the provost and president—the provost mainly—so we've got this *real*. Rather than going to the provost and saying that we need another appointment without a name attached to it, but just an area—because we're getting nowhere with that, since they come back with "Really, you've had enough appointments"— let's present the provost with an actual person we're excited about and say, "We've got to do something; this is a great person." So we expose the person to the provost and president—there's a real person involved and they can meet them. If they're that great, the provost and president will be excited about them too.

So we changed that culture, at least during my time, to that kind of recruiting operation. And of course, Rudy and Bob Grubbs and Ahmed were examples of that. I would say we were looking for a junior chemical physics appointment when Ahmed came up, but Rudy's case was just like that. He was a real case of a person I thought would be great here—and I just brought it forward that way, and we invited him for a seminar. People got excited about Rudy, and we went ahead. We didn't have a billet for that, but we had a person who was exciting. Rudy was probably the first appointment of that type. All the others were ones where people had fought for months or years to get a billet and then went and looked somebody to fit it. It's a tortuous process that means you just fall behind, and all of a sudden, you look up and everybody in the place is old and you haven't replaced them and your department's going in the wrong direction. I also made the speech that there's no such thing as staying even, being happy with yourself. You can congratulate yourself on where you are with certain appointments, but you can't stop looking. You can't stop trying to get better and better. There's no such thing as being great and just staying there, saying, "I'm great."

ASPATURIAN: No complacency.

GRAY: Yeah. You've got to keep looking for people who are better than you are. And that's another thing I preached. I said we should be looking all the time for people who are better than we are.

ASPATURIAN: Was that a hard sell with some of your colleagues?

GRAY: Not really. In a lesser place, that is a hard sell. In a university with a lot of folks who aren't doing that much, they're threatened by more active people and they resist hiring people better than they are. They're looking for people who look just like they are, who won't threaten them. That's another bit that I pushed. So if you put all these things together, you start recruiting terrific people, and during this period, we really built the division into a real powerhouse. And now we've got a lot of great, older world-famous people, but we're looking at it now like we've really got to find a lot of young people because we've got a whole bunch of people who are going to be retiring or dying or doing something else.

ASPATURIAN: What were some of the other major challenges you faced in your half-dozen years running the division?

GRAY: Well, there's always the challenge of raising enough money to do everything you'd like to do. And for that I established a visiting committee that was half trustees and half distinguished academics.

ASPATURIAN: Was that the first time this had been done in chemistry and chemical engineering?

GRAY: I think maybe this was the first time it was done at this very, very high level of trustees and academics, but I'm not sure of that. It's probably the first time it was a very, very active committee that I worked with all the time rather than your standard once-a-year visitors who come in and say "Hi" and write a little report and leave. I worked with this group continuously, showing them things, bringing Jim [James] Glanville here to look around. This was the period when I became very, very close to Arnold Beckman [chair of Caltech board of trustees, 1964–74; trustee chair emeritus, d. 2004]. I had been reasonably close to him before, but during my chairmanship I worked with him all the time. I talked to him every week. And I talked to Jim Glanville a lot, and there was Tory Atkins as well. I had all of the trustees on the committee really engaged, and I talked to them all the time. I'm sure this is the first time the chair of the division had such a strong interaction with a visiting committee, because I felt that if I could convince the visiting committee, particularly the trustees on the visiting committee, that we had a great case, they would help me raise money for things we needed. I knew we needed to redo the synthetic labs. I knew that we needed money for instruments. We needed matching money for things. The trustees had come through before when the earthquake hit to help us get money for Mead [Mead Memorial Undergraduate Chemistry Laboratory], and I'd been a little involved in that.

ASPATURIAN: Are we talking about the Sylmar Quake in '71?

GRAY: Yes, that's what destroyed the undergrad chem labs. That's when we built Mead. We had to have a big infusion of money to build Mead right away. Arnold Beckman, or maybe Jim Glanville, helped us find that money. It was hidden somewhere in the endowment that Mead or somebody had given some money that could be used for something like that. This was before I became chair, but I was already of course working with Arnold Beckman. But when I did become chair, I became very, very close to Arnold, and we talked all the time. I talked to Jim Glanville literally all the time, less so with Tory Atkins. But Jim and Arnold and I were in touch all the time. We needed things to happen, and so I got Arnold to meet with Peter Dervan [Bren Professor of Chemistry], and Peter really lit him up. And that's how we got the Beckman Synthesis Lab [Beckman Laboratory of Chemical Synthesis], from my sending Arnold to talk to Peter and Peter making the case that we really need to do this. And then Arnold came to me and said, "I want to do something." I knew exactly what to propose to him: $7.5 million for the building, and $7.5 million for instruments on a matching basis. Arnold liked that.

And we did that, and that worked great during my chairmanship. He liked what we did so much that that's when he said, "I want to do something bigger." That's what led to the Beckman Institute. He put me to work thinking about what to do there right after my time as chairman. So I spent the next two or three years working with Eric Davidson [Chandler Professor of Cell Biology, d. 2015], Bob Grubbs, Lee Hood, and Peter Dervan, coming up with plans that eventually became the Beckman Institute. So I had kind of a rump group there working on things. But clearly the challenges when you're chair are to recruit the very, very best people that you can find, get them in, get their labs going, and help them get going with resources. That takes a lot of money, and it also takes a lot of work on everybody's part to hire the best people. Fundraising in general requires a lot. And also taking chances and spending money that you really don't have to help people and being confident enough that you can find some way to raise it. I was known as the guy who spent more money than he had every year. I was always at least a million dollars in the red.

ASPATURIAN: How did that go over? [See also *Gray 2016, Session Two*]

GRAY: Not well. The administration, Robbie Vogt, was after me every year, yelling at me, and I kept telling him I was building the world's greatest chemistry department, and he should thank me for that, pat me on the back, and give me even more money. Of course when I did that—and my style—it made him even madder. [Laughter] That tension between myself and Robbie over the years finally led to my decision in the first year of my second term that "I've done enough now; I've been fighting the battles. Robbie and I really aren't quite on the same page, and I don't think it's his fault; it's probably my fault that I'm a little too aggressive for his taste, and it's also time to turn it over to somebody else who's a little more responsible in balancing the budget." Fred Anson [Gilloon Professor of Chemistry, emeritus] was certainly that.

ASPATURIAN: This is jumping ahead a bit, but I sometimes do that. How do you feel your successors have done building on your legacy?

GRAY: They've done well. They've all done well. Fred Anson was a very careful chair. He didn't really make any mistakes. He guided the ship. He was not nearly as aggressive as I was. He was more of a guy who I think would work more closely with the administration. I think they preferred him to me. He had more meetings. I had fewer meetings. I told the faculty that their job was to teach and do research and not go to meetings, so we weren't going to have that many meetings. Every month Fred, who was then chair of staffing, would send me a note or a memo saying we need another meeting or something. I was tired of getting those too, so I basically said, "Okay, Fred, you take over." But he did a fine job running the division, and then after that it was Peter Dervan. They all had their strengths and weaknesses. I had mine, and they had theirs.

ASPATURIAN: Peter was in only for five years, is that right?

GRAY: Yeah, Peter didn't do a second term. He was more like I am, I guess, in the sense that it wasn't his real cup of tea. He liked his research better. But he did a fine job, and then [David A.] Dave Tirrell [McCollum-Corcoran Professor of Chemistry and Chemical Engineering] took over and did two terms. And Dave Tirrell, I would say, was a model chair. He was a tremendous guy, dealing evenly with everybody, with leadership. I would say of all the chairs I have ever known, I would rank Dave Tirrell as the best chair. I think he was terrific at everything and worked with everybody. He had a very nice touch. He made everybody feel good about things, and he was very effective. Jackie [Jacqueline K.] Barton has been excellent too. She has really run a great ship and been very good at recruiting. She's very, very good at raising money for fellowships and so forth. She's very organized. You know exactly where you stand, and she's been very successful raising money for the division. Those are great strengths. So, you know, we've had a string of very good leaders. I don't have any complaints about leadership. I think they've continued to recruit great people and raise money and do things for the division. Our division is very strong.

ASPATURIAN: Yes. I don't know how much stock you put in these popular rankings of academic programs, but Caltech chemistry invariably comes out at the top of the list year after year after year.

GRAY: Yes. I think we turned the tide with that during my time when we added really fantastic people. We built on that, and momentum has continued. We invariably come out No. 1 or something close in all these rankings, and we're very, very strong. This is a very, very strong division, arguably the strongest division at Caltech in terms of just fantastic graduate students and postdocs, and world-famous faculty members. A huge number of our faculty are in the national academies. I suspect—I'm pretty sure—we have more than any other division. I'd be willing to bet on that because it's something like fifteen or sixteen, and if you count the National Academy of Engineering and the IOM, the Institute of Medicine, we probably have twenty-some odd or something, out of a faculty of forty. It's really incredible.

ASPATURIAN: It's a high ratio.

GRAY: Very high ratio. I think we have the highest ratio of any chemistry department in the country.

ASPATURIAN: Did you continue to be actively involved in recruiting after you stepped down as division chair?

GRAY: Oh, yes I've been actively involved all along. It's very important. One of the most important things is to recruit new faculty—young people and senior people and so forth. We just did a successful recruitment of a really world-famous theoretician from Princeton. His name is Garnet Chan [Bren Professor in Chemistry]. He's very young—thirty-six or thirty-seven—but he's won major awards already.

ASPATURIAN: Was it hard to get him away?

GRAY: It was tough. Princeton pays these huge salaries. It was tough to recruit him, but we did. He started at Cornell and made a big reputation there and won a big national award. And then Princeton recruited him. He hadn't been at Princeton all that long. But then we have Tom [Thomas] Miller [professor of chemistry] here, who's a very outstanding young theoretician. We need young theoreticians, so Tom really led the charge in recruiting Garnet Chan. And we got him. He's coming.

ASPATURIAN: How involved have you been in the chemical engineering side of things?

GRAY: Pretty involved. I worked with Dave Tirrell and John Brady [Chevron Professor of Chemical Engineering] and John Seinfeld [Nohl Professor and professor of chemical engineering] and Julie [Julia] Kornfield [professor of chemical engineering], Mark Davis

[Schlinger Professor of Chemical Engineering], [Richard C.] Rick [Richard] Flagan [McCollum-William H. Corcoran Professor of Chemical Engineering and Professor of Environmental Science & Engineering], and Frances Arnold [Dickinson Professor of Chemical Engineering, Bioengineering and Biochemistry]. I have written joint papers with Francis Arnold. So I'm very close to the chemical engineers. This is one of the great advantages at Caltech—that the chemical engineers and the chemists are one group here. There are only three schools in the country where that's true formally, and that's Berkeley, Illinois, and Caltech. And at Illinois and Berkeley, while they're formally together, they're still sort of separate. Here, it's one real group. It's seamless. A lot of the chemical engineers work with chemists, and a lot of chemists work with chemical engineers in terms of grad students. There's a lot of collaboration, a lot of exchange. This is the only school in the country where these two groups are really together. And I just found out this week at Berkeley that it looks like the chemists and the chemical engineers there are going to separate: The chemical engineers are going to join engineering, and the chemists will be by themselves. They were telling me about it when I was up there. They don't want that to happen. But because they have a huge deficit at Berkeley—a $150 million deficit—they're going to save money by maybe doing this. But they were all complaining about that, because they want to be together. But here we all have strong connections, and there are folks who are really in both camps. Rustem Ismagilov [Bowles Professor of Chemistry and Chemical Engineering] is in both chemistry and chemical engineering. It's really, really close here. It's nothing like other schools. At Columbia, a chemist never sees a chemical engineer. They're in different buildings, they have different things going on. I don't think there's any interaction. Essentially no interaction. Here, there's interaction all the time. This is a unique place for chemistry and chemical engineering.

ASPATURIAN: I'd like to move off the administrative side of your career to the teaching side and your relationship with your students, undergraduates in particular, because you did cover the graduate relationship quite a bit with Shirley. One of the things I noticed is that some of your students have come back to Caltech as professors. I found a photo of you with George Rossman as your teaching assistant, back in, I think, the 1960s.

GRAY: As a grad student.

ASPATURIAN: He was a grad student. Nate Lewis, I think, was an undergraduate.

GRAY: I've got three major people. There is also Steve [Stephen L.] Mayo [Bren Professor of Biology and Chemistry]. He was my graduate student, and now he's chairman of biology [Division of Biology and Bioengineering].

ASPATURIAN: Of course.

GRAY: Nate Lewis was my undergrad, and he came back as a professor. George Rossman was my first Caltech grad student, and he transitioned right from my group into geology. They made him an offer right after he graduated because he knew more geology than the geologists. They immediately hired him.

ASPATURIAN: I wrote a feature about Nate Lewis some years ago when he won some award [Fresenius Award], and one of the things he said was that he came in and was kind of floundering and then he discovered this love for chemistry—and he talked about you as a mentor.

GRAY: He was floundering around, and he found my group and he found a home there. He basically had a home away from home. He was a pretty protected young man who'd never been outside much; a very family-oriented Jewish kid whose father was always here on Friday afternoon to get him home before dark.

ASPATURIAN: Really. [Laughter] Somehow that did not come up in the interviews I had.

GRAY: We had meetings on Friday afternoons in what was then a seminar room on the third floor of Noyes, and Nate very often would make a presentation to the group because he was doing a lot of great stuff—he published, I don't know, eight or ten papers as an undergrad—and his dad would always show up. His dad was a little guy, looked a lot like Nate. His dad just adored him; his family adored him; and his dad would show up on

Friday afternoon for the seminar to make sure that right after seminar, he would get him home before sundown. And I knew his dad well—I saw a lot of his father. We became great friends; we really hit it off. There's a really nice of photo of the three of us—me, Nate, and dad—from one time when we all went to the Athenaeum**.** But he made sure he got Nate out of here for the weekend, for Shabbat. Nate was very, very protected. And he was floundering because he'd come in from this protected family, and here he is at Caltech, wandering around, wondering what's going on, and he found our group and we adopted him. We were really his second family, and he was very comfortable in our group. He just took off and just did wonderful stuff.

ASPATURIAN: That's pretty much what he said. He said that in his first year he flunked so many courses, or so many exams, he didn't know what to do; and then he discovered this bent for chemistry in this very supportive environment.

GRAY: Then he just started getting all A-pluses. And he was incredible. He's incredibly smart. I don't believe that story about flunking things.

ASPATURIAN: Well, it sounded good.

GRAY: I don't think he ever flunked anything. I don't believe it.

ASPATURIAN: Maybe his first sets of exams?

GRAY: He might have flunked an exam along the way, and he's exaggerating. He was a fantastic student, believe me, an A-plus guy in everything. Go back and look at his record. Let's go back and check it. I'd be willing to bet you there are no Fs on his record.

ASPATURIAN: I'm hoping to do an oral history with him sometime in the upcoming year. I'll pursue it.

GRAY: Do that. Tell him I don't believe it.

ASPATURIAN: Were you involved in getting him back here? I think he went to MIT for his PhD.

GRAY: And then to Stanford for his first job. Oh, yeah, I was very involved because he felt like he wanted to come back to Caltech. He didn't feel that comfortable at Stanford.

ASPATURIAN: Too big?

GRAY: The atmosphere—there were some folks there that I think he had some trouble with. He was an associate professor there, so he got tenure, and he won a major award. We saw this opportunity to get him back, and John Bercaw and I really worked together to do that. John and I figured that we'd better move on it. We'd had a couple of inorganic chemists who didn't make tenure and we were getting sort of desperate, so we said, "Let's get somebody with tenure who we can add to our operation," and that was Nate. So, yeah, I had a lot to do with recruiting him.

ASPATURIAN: One of the things I saw in the past week or two on YouTube was that lengthy ACS [American Chemical Society] interview that you had with one of your former students.

GRAY: Rich [Richard] Eisenberg. "Voices of Inorganic Chemistry" [https://www.youtube.com/watch?v=Si7bjzkD-Fs]. It's a very good series. I liked my interview. I very often don't like stuff I have on YouTube.

ASPATURIAN: It had 7,000 views—well, at least 7001 now.

GRAY: But this one I like. Rich was a very good interviewer, and I thought both of us did a good job on that. He was one of my first Columbia students.

ASPATURIAN: That's what you say in the interview.

GRAY: And he's a terrific guy. We just had fun together so when we're talking together, that usually comes out.

ASPATURIAN: One of the things you talk about there is your philosophy that students—graduates and undergraduates—should be enjoying themselves in their research.

GRAY: Yeah. If they're not, they should do something else.

ASPATURIAN: I was struck by the fact that you say they must go through these very nuts-and-bolts chemistry classes but if they don't have the opportunity to approach those with an initial vision of how it fits into a bigger picture—

GRAY: Yes. You have to get people really excited about chemistry through doing research and so forth in order to make the case to them that going through these very technical, previously very boring courses is really worth it. That's why I'm so in favor of freshmen here getting involved in research right away. Once again, I've got about five freshmen who are going to do research with me in the summer quarter.

ASPATURIAN: As SURF [Summer Undergraduate Research Fellowships] students?

GRAY: As SURF students. So I always get a bunch of freshmen, and get them in labs and get them going. That way they can see the excitement of chemistry, and then when they get these courses that are very good technically but could be pretty boring, being already involved in research helps them realize why they're taking these classes now and why you need to know some of these things. If you have a need to know, the course becomes not boring but in fact necessary, and you realize that and so you work on it.

ASPATURIAN: A bit of a top down approach.

GRAY: Yes. If you're in these courses and you don't feel you have any reason to be, then you just get bored and tend not to do so well in them. So that's my philosophy: Get kids doing things in research early and then they'll take physical chemistry and be able to see why they need to know physical chemistry. They can see they need it in their work. It's true of organic chemistry and everything else too. Because I went through a period where I thought the problem with chemistry was the curriculum.

ASPATURIAN: At Caltech, this was?

GRAY: Everywhere. The chemistry curriculum hasn't changed in seventy years. It's introductory chemistry, organic, physical, a little bit of inorganic, and other things—analytical—but the structure of the curriculum itself hasn't changed in seventy or eighty years, really. In the late 1960s, George Hammond and I thought that this was what was wrong with chemistry—that the curriculum was the reason that kids weren't getting excited about chemistry. So we did this huge curriculum revision from inorganic, organic, physical and analytical, to structure, synthesis, and dynamics. And we formulated a curriculum that we tried here at Caltech. A few other schools tried it too, but it failed because—it just failed. It wasn't a curriculum problem. The courses that we have are technically very, very good; it's just that they don't show what's going on now in chemistry.

ASPATURIAN: Students can't see the relevance.

GRAY: You can't see the relevance. So it's really Jackie Barton and Nate Lewis who figured this out twenty years ago at Caltech when they said, "Let's have a course in the freshman year that introduces our kids to research in chemistry." It's called Chem 10.

ASPATURIAN: Oh, I remember Nate talking about this.

GRAY: Chem 10. The idea is, let's get the kids into Chem 10, bring in research groups every week to talk about what they're doing, let the kids visit labs and see what's happening and let them join a lab in the third quarter and apprentice in one of the labs that they pick because we've got forty research groups here in chemistry and chemical engineering, and we don't have that many kids. Every research group can adopt a kid, just like I adopted Nate Lewis.

ASPATURIAN: Nate credits his experience in your group with shaping his thinking on this.

GRAY: Yes. Before Chem 10, in the mid 1990s, we were down to three or four undergraduate majors in the division. We weren't competing with physics and biology or geology.

ASPATURIAN: Really. I did not know that. So you must have sensed that something needed to be done.

GRAY: Everyone was wringing their hands. George Hammond and I had thought it was a curriculum problem. It wasn't a curriculum problem. We needed to get kids into research immediately, so they understood the need to know. Nate and Jackie figured that out, and I bought into it immediately. Once I saw that, I could say, "Oh, my goodness, that's exactly what we need." And we started this, and our undergraduate majors went from nothing to a huge number overnight. Overnight. Now in the division we've got, I don't know, sixty or seventy, maybe a hundred majors.

ASPATURIAN: Well, one of the things you say in this ACS interview is that as far as you're concerned, in the 21st century, chemistry is *the* science.

GRAY: It is *the* science. There are three major problems facing our planet: Human health, energy, and the environment. In all three of these areas, chemistry has a huge role to play. And it's the only one that really is deep in all three areas. That's why chemistry right now is so critical for the survival of the planet and the quality of life, which is a human health issue. Chemistry of course is called the central science because it stretches out into every area, into physics, into biology, into geology, and geosciences.

ASPATURIAN: That's for sure.

GRAY: Chemistry has this problem: It's so into everything that people sort of get lost in seeing how important it is. People talk about how great biology is—well, half of that is chemistry. And then they talk about how great physics is, and about half of that is chemistry. But chemistry's in the middle of all these other things, and people are starting to realize this and that chemistry is making a huge impact now on the planet.

ASPATURIAN: Has the educational model you developed at Caltech twenty years ago been emulated elsewhere since it was so successful here?

GRAY: Yes, oh, yes. It's all over the place. We did SURFs—we invented SURF and all of that [The originator of SURF, Fred Shair, was a professor of chemical engineering at Caltech. *–Ed.*]. SURF was invented before we figured out we had to do this early with our kids all over the place. Every school in the country now is getting their kids into research earlier and earlier. Huge schools can't really do that because there are too many freshmen, but the small schools are all doing it. It's now the thing, the rage. At national ACS meetings, there are probably now 3,000 undergraduates who are giving papers and posters. It's far greater participation than in any other field. Thousands of undergrads go to ACS national meetings; it's incredible to see these things. They're so excited about what they're doing. Chemistry as a discipline has led the way in research for undergraduates. Mathematics has just discovered this in the last year or two. Other disciplines are doing it, but chemistry has this fantastic tradition. And the ACS realized this at about the same time we did and started having these big undergrad symposia at the national meetings, and the huge poster sessions. It's really fantastic to see that. No other discipline has this enormous concentration of undergrads doing research. A lot of it can be connected to stuff we did here. It actually goes all the way back to Ernest Swift [professor of chemistry, emeritus, d. 1987] around 1930 when he decided that undergrads at Caltech should be doing independent work. I would say that we had the first undergrad chemistry research anywhere.

ASPATURIAN: I wanted to ask you about some other aspects of your teaching that surface anecdotally all through reports and publications about you. Lecturing in a horse's head in the 1980s, students trashing your office, reversing all the seats in your lecture hall, bringing in a Hare Krishna monk to chant all day, beginning class with a popular song about the scientific elements; would that have been Tom Lehrer ["The Elements"]?

GRAY: Yeah.

ASPATURIAN: You kind of enjoyed participating in these undergraduate hijinks.

GRAY: It's sort of my style. When I taught freshman chemistry at Columbia for five years, I did "crazy" things during demonstrations. When I came out here, Jürg Waser [professor of chemistry, d. 2002] taught freshman chemistry. He didn't do much research, so here was a person who was basically hired just to do freshman chemistry. When he sort of retired or stepped down, I had been teaching Chem 2, which George Hammond and I invented as part of our new curriculum. It was meant for kids who wanted to take a more advanced freshman course, and we had fifteen or twenty kids volunteer to take it. It was very successful. So I was already in this, and when Waser stepped down, I started teaching freshman chemistry here. And it was kind of in disarray—as I think math and physics courses probably are now—in the sense that nobody came.

ASPATURIAN: No one came to class?

GRAY: Very few. It was a required class because Caltech has this core curriculum, but attendance wasn't that great. Kids didn't really want to be there, so they probably just figured they could skip classes and do fine. They were so smart they could just do fine on the exams. Back then, I think freshman physics and math were very well attended. It's flip-flopped now. Introductory chemistry with Nate teaching it is still well attended, and physics and math are struggling for people. Math, particularly: I've heard that in some of these required math classes, two hundred people are supposed to be there, and they have less than ten attending class. I don't know if this is true or not, but I've heard this. So, getting back to my early teaching here, in my usual style I went from this conservative style that Jürg Waser had to a really hip-hop style. The class was held on Mondays and Thursdays at eleven and I told them, "Look, I'll entertain all you guys on Mondays. I'll have demonstrations, I'll have stuff, it'll just be fun, and you'll love it. It'll be chemistry, but it'll just be a lot of fun. I guarantee you, you'll love it. And then I'll make a pact with you that you'll be here on Thursday. Thursday we'll really work hard. We'll explain some of the stuff we did on Monday, and we'll do some teaching on Thursday, and that's the deal. I want to see this place packed." And they bought it. And so on Mondays, it was incredible. We had all kinds of stuff. I had a very good-looking secretary called Georgene Hill. I had George Rossman as my TA. I had me. And we were a team of three. Georgene would help us do demonstrations, and the kids liked her. They loved George, and we'd do all kinds of stuff. We dressed up. It wasn't just a horse's outfit. I had all kinds of outfits, and George had some too.

ASPATURIAN: Do you remember any others?

GRAY: Yeah, I had a leopard outfit. I had a King Faisal outfit. I had a Nixon outfit.

ASPATURIAN: So the kids never knew what they were going to see next.

GRAY: I had started on Halloween when the kids redecorated my office, like Lavoisier's office [Antoine Lavoisier, widely regarded as a founder of modern chemistry, discovered the element oxygen and was guillotined during the French Revolution, in 1794 *–Ed.*]. I decided, "Well, what the hell, I may as well get a horse costume and give the lecture as a horse that day." Which I did. And they loved it. I got the reputation of being "Harry the Horse." Which got me—

ASPATURIAN: I was going to get to that.

GRAY: Which got me the part in *Guys and Dolls* [Caltech Theater Arts, TACIT, musical]. I was a natural for "Harry the Horse" because I was already known as "Harry the Horse." So that led me to become really good friends with Dick [Richard P.] Feynman [Tolman Professor of Theoretical Physics; 1965 Nobel laureate in physics; d. 1988] because he was also in the thing. So I'd sit up there every night with Dick Feynman, and Shirley Marneus [TACIT founding director, 1974–2008] would yell at us.

ASPATURIAN: Why was she yelling at you?

GRAY: Because we couldn't dance, we couldn't sing, and we couldn't act. She'd say, "Harry and Dick, you may be great scientists, but you can't act, you can't sing, you can't dance." [Laughter]

ASPATURIAN: You're terrible starlets, in other words.

GRAY: Yeah. [Laughter] "Don't get too cocky." But we loved her. And I get notes from Shirley to this day.

ASPATURIAN: Yes, she's great.

GRAY: I got a really nice note the other day from her.

ASPATURIAN: You enjoyed that experience?

GRAY: It was wonderful.

ASPATURIAN: Was that your one TACIT performance?

GRAY: I did three, all with Shirley. Rehearsed for two months every night. Lost my voice. I had a big part in *Guys and Dolls* [1977]. Smaller parts in *Fiorello!* [1978] and *How to Succeed in Business Without Really Trying* [1987].

ASPATURIAN: I remember *How to Succeed in Business*.

GRAY: Anyway, the current guy who's running the thing—

ASPATURIAN: Brian Brophy.

GRAY: Brian Brophy emails me all the time, trying to get me to do things, and I'm too busy now.

ASPATURIAN: Not tempted?

GRAY: I'm not tempted. I can't do it. I put in an enormous amount of time on those shows. I gave it my all. He should go with younger people. But in my chemistry classes, it was a combination of stuff I brought in with demos and stuff the students did. They rigged up the Tom Lehrer audio. They invited the Hare Krishna chanter. I put up with all of it, and we just had a great time together. The great thing was that if you weren't in 22 Gates at 10:30 a.m. for an eleven o'clock class, you didn't get a seat.

ASPATURIAN: It was that popular?

GRAY: "Be at 22 Gates early because it's going to be incredible this week." Stuff like that. I had the *Tech* people coming; the *Tech* ran a front-page story on it every week. I packed the place.

ASPATURIAN: You did teaching as performance art, basically.

GRAY: I packed the place. And the conservative faculty were on my case.

ASPATURIAN: Within the division?

GRAY: Yes. They didn't like this: I was a showman; this wasn't real teaching. I kept telling them that if the kids don't come, there's no teaching.

ASPATURIAN: Who were your critics, do you recall?

GRAY: Jack Roberts was the main one. He would stand outside of 22 Gates on Mondays, and I would come out of class, and he would lecture me that this was not a good thing to do. I would say, "Jack, I'm doing it; you can't do anything about it because I'm a full professor. You can't do anything to me. I've got this class, I've got kids coming; they're learning things. You can give a class and three kids come, what's the point?" But I took a lot of gas.

ASPATURIAN: You took some heat.

GRAY: But not for long. After a while, after I started getting these incredible ratings from kids on the teaching performance stuff. The kids loved these classes, and I got the best ratings in the division. So people let up on me, but when I first started this, Jack thought it was horrible. He thought, "This is going to really come apart." But it didn't come apart. I don't know how many years I went on with it. And then I taught with John Bercaw and other people. It was really a great time. I believe in making chemistry interesting to people and getting them in there and talking about it, and that's what I do now in this class I teach in advanced inorganic. I get all kinds of people talking about stuff and arguing. It's always been my style.

ASPATURIAN: From the sublime to the ridiculous: How did your colleagues react to, was it the Playgirl centerfold?

GRAY: [Laughter] Yeah, yeah. That goes to show you what my relationship was with students.

ASPATURIAN: Of course.

GRAY: They always let me know about these kinds of things in advance; it would be a violation of the honor code if they didn't have my permission. They broke into my office several times to do things. One time they put a miniature golf course into the conference room right here, next to my office. They broke in my office to restructure everything; they did that two or three times. Once, as you said, they turned the chairs in the lecture hall around so that when I came in their backs were facing me. It didn't stop me. I took a little portable blackboard to the back of the room and I lectured. I ended up at the top with the clock, looking down. They put all this effort into unscrewing all those chairs and turning them around, unbelievable. But it's because we had a great relationship.

ASPATURIAN: Right, they were having a good time.

GRAY: Yeah. We had a great relationship, and so when they did the centerfold, they called me.

ASPATURIAN: Was that a *California Tech* centerfold?

GRAY: No, no, no. That was, no, no, that was the catalog.

ASPATURIAN: The *Catalog*?

GRAY: It was the *Caltech Catalog* that the admissions office sent to prospective applicants. They sent that out to high school kids. They had me in the centerfold as the "Chem-mate of the Year." You've probably seen it.

ASPATURIAN: I have seen it. Do you remember what year that was?

GRAY: I've got the catalog somewhere.

ASPATURIAN: I bet you do. I think it might have been not long before I arrived here.

GRAY: I do remember one thing: The administration caught it. About 500 catalogs had gone to schools, before they caught it, and they hired three older women to manually cut out my photo from thousands of catalogs.

ASPATURIAN: I bet they hired three dowagers, right?

GRAY: They did. The interesting thing is that they worked all week to cut them out manually. At the end of the week whoever hired them called me, and said, "Harry these women have been working all week, cutting. It would be nice if you go and say hello to them." I said I'd be happy to. I walked in and they said, "Awww, we're tired of looking at you."

ASPATURIAN: [Laughter]

GRAY: It was funny, but you know, that's the kind of interaction I had with folks. One day when Jackie Barton was teaching Chem 1, I was supposed to dress up like Darth Vader and capture the head TA. They were going to capture Jackie, and we were going to take them out like in *Star Wars*; we were going to take them up to the mountains or something. I forgot about my part. I forgot, and they "captured" Jackie and took her out and dumped her in the mountains someplace.

ASPATURIAN: Did she know this was happening?

GRAY: No. But she was a good sport. She was a good sport then. They dumped her somewhere, and she got back. But I forgot to show up and play Darth Vader. And so the next day there were these huge banners all over campus with Darth Vader's picture and the caption, "Have you seen His Imperial Highness Lord Gray?" And then I knew something terrible was going to happen to me. [Laughter] And two weeks later, that's when they broke into the conference room next door and turned it into a high-tech miniature golf course. High-tech, with funny tees and stuff. The course went all through campus; the 18^{th} hole ended up at Blacker House. It was the Blacker House kids who did it. They thought I'd be mad. I was happy. I played the golf course for three months. I didn't take it out of here. The [*Pasadena*] *Star News* guy came and photographed.

ASPATURIAN: Of course.

GRAY: It was a fun golf course. Rich Eisenberg and I played it, actually, when Eisenberg interviewed me. He and I played it all the way to the 18^{th} hole at Blacker House, and the photographers followed us around. It was fantastic. I've always had this kind of relationship with the students.

ASPATURIAN: Do you have students who have emulated that teaching style? I know Nate Lewis talked about it.

GRAY: Oh, yeah, Nate has. They threw him into Millikan Pond.

ASPATURIAN: I remember writing about that.

GRAY: He absolutely does. I'm not sure anybody else does, but Nate definitely does. Nate has followed in my footsteps in this and they love him. The kids in Chem 1 love Nate.

ASPATURIAN: He was always up for fun things from public relations [Caltech Office of Public Relations] too. He was great to deal with on that score.

GRAY: He's got all kinds of great ideas on that. I'll tell you, he got me into the class this year. I helped him teach one of the last classes of Chem 1 last term, and the place was packed. I'm impressed with the fact that he's kept these kids coming, and that hasn't happened in physics and math. Just because he's fun. He's a fun guy. Takes 'em to lunch. Takes somebody to lunch, I think, every time he teaches. He takes five or six kids to the Athenaeum.

ASPATURIAN: I remember that when I talked to him he said, "My philosophy is that teaching chemistry should be fun."

GRAY: He may have learned that from me.

ASPATURIAN: I think he probably did, and from your group. I have a note here that you and Richard Feynman had a bet between you about who taught the harder class. You started to teach Chem 213 and won the bet. Does this ring a bell?

GRAY: That doesn't ring the bell, but we might have had a discussion about that where I said, "I've got a harder course than you have—Chem 213, Advanced Ligand Field Theory." I don't remember making a bet, but I might have told him, "I think I have a harder course than you." That might have happened. I did talk to him a lot during those long nights with Shirley Marneus. Because there was nothing to do. We would sit up there in the balcony in rehearsal, or we'd sit up there in the stands in some room, wherever it was. And you know how the rehearsals go; it's hurry up and wait; finally we get to get down there and do some work; then wait for another hour or two while they're doing all sorts of other things. So Dick and I had a lot of time to talk. He was a great guy. I think I told you that I asked him to give a talk?

ASPATURIAN: No.

GRAY: Okay, we were organizing an AAAS [American Association for the Advancement of Science] meeting here in Southern California, and the planning committee said, "We want to get some speaker who can talk to eight- and nine-year-olds. We're going to bus in 3,000 of them from around LA to the California Science Center downtown by USC [University of Southern California]."

ASPATURIAN: When was this, do you recall?

GRAY: Well, it was during this period when I was in tight with Dick.

ASPATURIAN: Around the time of *Guys and Dolls*?

GRAY: Yes, it was during that period. So I said, "Well, Dick's the guy. I'll ask Dick." And I said, "Dick, will you do this?" And he said, "Sure I will." [Laughter] He showed up in his informal outfit, and I remember that 3,000 kids sat outside on this big grassy knoll around the science center. They're packed around there, and he started talking to them. He talked for about forty minutes, and they were fascinated. They didn't move. They were all looking at him. Gee, whiz. He had a way. He had a way with all ages of people. Here are nine-year-olds, and you figure—in three minutes, they're going to be running around, getting into trouble, doing all kinds of stuff. They didn't move.

ASPATURIAN: They were transfixed.

GRAY: Absolutely. Just absolutely. Just loved the guy. I couldn't believe it. He did such a job. But that was Dick Feynman. He did stuff like that. He was a good guy. He was a good guy. So, where are we?

ASPATURIAN: It's been an hour.

GRAY: We've done an hour. Let's do another one some time.

ASPATURIAN: Yes, let's.

GRAY: You're getting lots of good stuff!

ASPATURIAN: Yes.

GRAY: I'm just talking.

ASPATURIAN: That's the idea.

GRAY: But I've had lots of experiences in my life.

HARRY B. GRAY

SESSION 4

March 17, 2016

ASPATURIAN: Today is March 17th, St. Patrick's Day, and this is interview session No. 4 with Professor Harry Gray, who, for the record, is not wearing any green whatsoever.

GRAY: Sorry about that. I realized that when I went into the gym this morning, and the gal at the desk who always greets me, said she didn't have any green either and she was upset about it. But then it turned out that she had a little thing in her hair that was green, so we let her off the hook. She was acceptable. But I had nothing so I've got to find something.

ASPATURIAN: Well, there's a small green rim around this voice recorder we're using, so maybe that will—

GRAY: I've got the Scotch tape dispenser.

ASPATURIAN: You've got the Scotch tape, and I've got the voice recorder. Today, we're going to pick up where your earlier oral history [*Gray 2000–01*] left off, with the year 2000. But, I do want to go back to something else first. You gave a talk several years ago at Northwestern ["Powering the Planet with Solar Fuel": https://www.youtube.com/watch?v=fwqVsRLHq24], which is online, which I watched, and there were a few things you brought up that I wanted to ask you about. One was the sculpture—the water fountain here in the Beckman Institute courtyard. You talked about how you designed that.

GRAY: Well, at the time we were talking with the architectural designer, Tim Vreeland, about what would be a nice sculpture fountain for the Beckman Institute. AC Martin was the architectural firm we worked with. The main design person there was Tim Vreeland, whose mother was very famous.

ASPATURIAN: Diana Vreeland?

GRAY: Tim is Diana's son. Very nice guy. Shirley and I worked with him on various design aspects of the courtyard, including the areas of the courtyard that have geometrical figures in them, squares and circles and octagons and so on. When we talked about a sculpture fountain, I said that I wanted something that would try to tie biology and chemistry together, particularly inorganic chemistry. That is not normally thought of as being part of biology, but it is a field I worked in a lot, so I had a particular interest in doing something with inorganic chemistry in a biological context. I came up with the idea of the protein ferritin, which stores iron in our livers and spleens and has a big, almost spherical, cavity—it's kind of a nano-container—in the middle of it, where about 5,000 iron atoms are stored for use in making hemoglobin and myoglobin and things like that, mainly for red blood cells. It's a beautifully structured protein with faces and so forth. It's a big spherical-looking thing—well, it looks like the thing in the courtyard.

The structure appealed to me, and I suggested this to Bill [William] Schafer [senior research associate in chemistry, emeritus], who was working with me. Bill is an X-ray crystallographer here and a close friend of mine and had worked with me when I was chair of the division. I brought him over to the Beckman Institute with all the other X-ray people. Bill and I were very close. Bill was working on a lot of nuts and bolts of the building as well. He was one of the main contacts with AC Martin and all the lab designs and so forth. I suggested to Bill that this ferritin molecule would be a good sculpture fountain. Bill did the early drawings. We wanted it to be a sculpture fountain, and we wanted water to flow across it, so we built a big model of it in cardboard to see if water would flow evenly across the face. We took the model over to Murph Goldberger and showed it to him and started pouring water in his office. [Laughter]

ASPATURIAN: In his office?

GRAY: In his office. I don't know exactly—

ASPATURIAN: Did he have a sink?

GRAY: Well, I don't remember the details, but we didn't get very far before he said, "Stop, Harry, I'll give you the money to get it done properly in granite." There's a discussion of it on the web: you can google "Caltech snub cube" [http://www.publicartinla.com/sculptures/caltech/polyhedron_fount.html]. Because the structure that we put together is called a snub cube. It's not exactly the structure of ferritin, but it's close enough, and it's very symmetrical and beautiful. In that sense, Bill and I designed it. We won the art award from the Pasadena Beautiful Foundation that year. I have it right next door, the plaque and the citation.

Harry Gray and the "Polyhedron with 432 Symmetry" located in the Glanville Courtyard of the Beckman Institute. October 1989

ASPATURIAN: Do you want to go get it now and read the citation into the record?

GRAY: Yeah, sure.

ASPATURIAN: Let me put this on pause. [Pause] Okay, we're back on, and it says—

GRAY: "The Pasadena Beautiful Foundation awarded the Beckman Institute, California Institute of Technology, the award of merit, president's award, honoring your special enhancement of our city, 1991," signed by the foundation's president and the award's chairman, whose names I cannot read. Dated February 27, 1992.

ASPATURIAN: It's a nice thing to have.

GRAY: Isn't that nice?

ASPATURIAN: Yes, very.

GRAY: So that's the story of the sculpture fountain.

ASPATURIAN: The ferritin sculpture.

GRAY: The ferritin sculpture. Bill and I really were the two designers, the creators, of the sculpture. That's recounted in various things. There's a link somewhere that goes to a place where it was written up, either by Romy Wiley, who does things like this—

ASPATURIAN: The architectural aspects of campus.

GRAY: Or something else. Somewhere there's a two- or three-page piece on the sculpture fountain, which I think tells the story accurately of how Bill and I worked together to make it happen. But it was my idea. I was the one who went to Bill and said, "Let's do a sculpture fountain where the sculpture looks like ferritin because the outside is the protein and the inside is this big cavity where all the irons are." That's a huge thing in biology, ferritin, and it's mainly iron.

ASPATURIAN: It's an interdisciplinary molecule.

GRAY: That's the whole story on the sculpture fountain.

ASPATURIAN: Something else you said when you gave that Northwestern talk, and I wrote this down, is, "All my good ideas come when I'm teaching. A tremendous amount of great research comes from teaching when you're thinking about how you're going to present this." Did you have some specific examples in mind when you said that?

GRAY: Well, you know, when you're trying to explain things to students in courses, you can explain at a certain level, and if they don't get it, you try another level, and you go to a deeper level, so you have about five or six levels that you can use to explain some concept to them. At a certain level where you're really thinking about how something works, very often you come up with an idea for an experiment or something else that you didn't have before. I've had lots of good research ideas when I was thinking about how I would actually teach a topic, not so much when I was actually teaching it right in the classroom.

ASPATURIAN: When you were preparing the lectures?

GRAY: When I was thinking about how I would teach this topic and thinking about what it meant at a deeper level than I would normally teach it, and go down deeper and deeper. My view is that if you really understand something at a deep level, you can teach it almost any way that you need to. If you don't understand it, you can only parrot something that you read in a textbook, and that's about the end of it. You only have a superficial knowledge of it. But if you have a deep knowledge of the subject, you can explain it at six different levels. I've always felt that I wanted to teach things that I understood deeply, so I could go as far as a student wanted to go, and when I think about this myself—when I think about how I'm going to teach something—I think about the different ways one could teach it, and very often during that process, I come up with ideas to do something that's led to some research. For example, in electron transfer work, I've thought long and hard about how to teach electron-transfer concepts and theory versus experiment, and it was probably during this time of thinking about it that I came up with the idea of binding two metals together at distances to see if electrons would flow between them. My guess is I came with that idea, which resulted in a really important experiment in 1982, in the course of teaching electron-transfer chemistry.

In the solar energy conversion field, I've had many occasions while I was trying to figure out how to teach mechanisms of reactions that split water and so forth, where I've thought, "Wait a minute, I can think about another way of doing this experiment." So, yeah, that's what's happened, and I've talked about it a lot. I gave a Priestley Medal address, which I called the "Joy of Research and Teaching," where I pointed out that the professors who didn't want to teach, who thought it was a great thing that they didn't, quote, "have to teach," unquote, were missing out on an opportunity to really think about things deeply so they could teach them, and in that process come up with ideas about whole new research areas to go into. So I was basically saying that these professors who think it's really cool to get rid of their teaching responsibilities are really missing the boat. Teaching and research to me is a continuum of scholarly work. I've made that point all my life, and I believe in it. For me, it's made a big difference in new areas I've gone into, things I've thought. Every chance I get to comment on that, I do.

ASPATURIAN: One other thing I wanted to ask you about, going back to last time. We talked about Nate Lewis as a student, and also George Rossman, who of course went into geology. And then Steve Mayo, who went into bio and is now chair of that division. I wondered if this was common with you that your students jump disciplines?

GRAY: Yes, because I stress independence in my group. I let people go in the directions that they really want to go. Steve did more things that are closer to biology, and Nate did stuff closer to solar conversion, inorganic chemistry. George did things that were closer to mineralogy, which is what he does now. But this is also a reflection, actually, of how vast the field of inorganic chemistry is now. Inorganic chemistry has gone from nothing, really, as part of introductory chemistry—not a real field—to coming into its own in the late 1950s and early 1960s. When you look at the elements in the periodic table, all of them except carbon are inorganic chemistry, and so the vast number of disciplines that depend on inorganic chemistry are going to include biology, geology, solar energy conversion. As we said last time, chemistry is called the central science, and the main reason for this is inorganic chemistry, which is really central to all of these things.

ASPATURIAN: It has tentacles everywhere.

GRAY: For instance, the role of inorganic chemistry in biology is recognized now as enormous, because the metal ions like calcium and zinc and iron and on and on and on are key to life. And then of course mineralogy is all inorganic chemistry. In solar energy conversion, all the main solar converters are now inorganic materials. Inorganic chemistry has its tentacles everywhere, and since I've worked in almost all the fields of inorganic chemistry over the years, my students are in all of these areas. Steve went into biology, George went into mineralogy, and so on. These are all pieces of inorganic chemistry.

ASPATURIAN: When you talked to Shirley back in 2000, she alluded to a series of predictions you made in *Engineering & Science* back in 1977 [*Gray 2000–01, Session Seven*].

GRAY: You're right.

ASPATURIAN: Let's see, "making hydrogen from water using solar energy."

GRAY: We've done that. That's a done deal.

ASPATURIAN: That's right. "Producing synthetic fuels by reduction of carbon monoxide and carbon dioxide."

GRAY: That's a done deal.

ASPATURIAN: "Catalytically converting nitrogen under mild conditions to ammonia hydrazine and new materials." You concluded by saying, "I'm going to have to go talk to Jonas Peters and tell him to get it done in the next six years."

GRAY: Yeah, that's right. Jonas is working on it and making great progress. I guess of the people around the world who are making progress on it, Jonas is doing as well or better than anyone else right now because he is converting nitrogen to ammonia under what I'd call mild conditions, certainly of temperature—low temperature conditions. So a lot of progress is being made that way, but to this day there's nothing out there yet that looks like it's going to replace the Haber process or is that competitive with biological nitrogen fixation. [The Haber process is an industrial process that uses an iron catalyst at high pressure and temperature to produce ammonia from nitrogen and hydrogen. *–Ed.*] But signs are on the horizon that this is coming. And progress has been dramatic in just the last few years.

ASPATURIAN: I found a recent article describing a 2013 breakthrough in the Peters' lab using an iron-based catalyst.

GRAY: Yeah, yeah, he's got a beautiful iron-based catalyst doing better and better and better now. The first real simple iron-based catalyst is Jonas's. There's a guy at MIT, Dick [Richard] Schrock, who has a molybdenum catalyst that's pretty good, but I think Jonas's is the best thing going right now. And he's making great progress. So my prediction is going to come true. We're going to have a really good nitrogen-ammonia catalyst and maybe make other things as well. I could say it's going to happen in the next ten years; I'm hoping it will happen in the next five.

ASPATURIAN: The other thing you said was that you were going to make what you called a main prediction. You probably remember: "This is the century in which we're going to understand the very weak bonds—what we call noncovalent assembly."

GRAY: Yeah, and there's enormous progress there. I would say that we've reached that point where we have a very, very good understanding now of noncovalent bonds. So I think that's already come true. I would say of my predictions that they've all come in, except the nitrogen fixation reduction one, which is still a work in progress. So I did pretty well.

ASPATURIAN: Yes, you did. Turning now to what you've been doing since you and Shirley talked, in 2005, apparently you returned to your longstanding interest in solar energy research. Why then?

GRAY: Well, I never really left it. I worked in solar energy conversion in the 1970s and early 1980s and published some key papers, some of them with Nate Lewis, others with Andy [Andrew] Maverick and others. After a flurry of work in the field, there was a real lull in solar energy work, starting in the 1980s.

ASPATURIAN: Why was that, do you think?

GRAY: The price of oil went way down. Its availability went way up. The funding for solar research went way down, and a lot of things that were going on during the 1970s and very early '80s dropped off the cliff. I never lost interest in it, though, and I kept a little something going all during this period. A lot of my electron transfer work and photochemistry work related to it, but it wasn't in the mainstream of solar conversion. That is, I wasn't working specifically on splitting water. I was doing more fundamental things like excited-state electron transfer, fundamental things that would relate to it at some point. But then I came back to it toward the end of the 1990s. I'm not sure of the exact dates, but it was when BP [formerly British Petroleum] started a big program with John Bercaw and Jay Labinger [faculty associate in chemistry] here at Caltech to convert methane to methanol. There was a big methane to methanol program—you can check when they started it [the year 2000]. Basically BP came in, and said they wanted to convert methane gas to liquid fuel and transport it. It's called gas-to-liquid conversion, and all the oil companies have been big in that, BP in particular. So they started a big methane to methanol program, in which Caltech and Berkeley were the main actors, and they put a lot of money into Caltech and Berkeley researchers—here it was mainly Bercaw and Labinger. But BP also had a long-term interest in solar. They were thinking

"Well, in the long term, though, we're going to end up with solar conversion," and it just turned out that the chief scientist at BP who was in charge of this, Bernie [Bernard Joseph] Bulkin, was an old friend of mine from New York.

ASPATURIAN: Columbia?

GRAY: When I was at Columbia, he was at Hunter College. And I'd known Bernie forever. Then he went to SOHIO [Standard Oil of Ohio], and then he got into BP. I hadn't reconnected to him for years, but I knew him really well from New York. And Bernie of course knew about me. He wanted me in the program and some solar in the program, probably just to keep something going—to say that BP is interested in long-term solar. So they put a little element of the program in for Jay Winkler and me and said they wanted us to restart our solar energy work. That's when our solar energy thing restarted—with the BP thing, sort of a lower level for a while. Nate Lewis was always doing work in the area, but he wasn't in the BP program. My former student Dan [Daniel] Nocera was getting into the field at MIT. So I had a little BP program going, Nate had his program going, and Dan had something at MIT. One thing led to another, and we got together and decided to try to get one of these NSF center grants to support an MIT–Caltech collaboration. We put together a proposal, and we got that funded in 2005, so that was a Caltech–MIT thing. I was still doing things with BP but gradually transitioned to this NSF small center. Then after three years, after we did very well in that initial collaboration, we competed for the big money, the $20 million NSF grant to expand our effort to include all these other institutions. In 2005 just MIT and Caltech had the small grant from NSF. In 2008 we got the big one [Center for Chemical Innovation–Solar—CCI–Solar]. Our program was really one of the first big solar programs in the world. The first one was a Swedish consortium. The second one was ours, our NSF program. And out of that, after one or two years, Nate decided to compete for the Department of Energy grant to support JCAP [Joint Center for Artificial Photosynthesis]. But basically he used our program as a platform to go get really big money from DOE.

ASPATURIAN: At the time BP funded you, was overall interest in solar ramping back up, or was this kind of a one-off from BP?

GRAY: Interest was ramping back up in renewable energy about that time. Oil companies were talking about the long term—what they were going to do fifty years from now. Many of them said, "We're energy companies, and fifty, sixty years from now, we're probably going to end up being solar companies, so we'd better start supporting some stuff." And several oil companies have since started supporting quite a bit of solar work.

ASPATURIAN: As I understand.

GRAY: And so that's the way we transitioned from that BP grant into NSF and then bigger NSF support. When we started the big NSF program, in 2008, that's when we started all of this work with kids and high schools, searching for new catalytic materials, which after a couple of years led to my now famous Watson lecture, in which I announced that I had a solar army.

ASPATURIAN: Was that your idea, mobilizing all these kids throughout the country? Where did that come from?

GRAY: The initial idea came from Bruce Parkinson, who's a PhD from Caltech, and who was at Colorado State at the time. There are many accounts of it, in various publications, because it's now in the record as a great program, and we won a national award one year for it as the citizens' project of the year. It was my idea to expand it widely.

ASPATURIAN: Was Parkinson your student?

GRAY: No, he was Fred Anson's. He was at Colorado State when he developed a certain kind of kit, the SHArK [Solar Hydrogen Activity Research Kit] that allowed kids in certain colleges like Grinnell and Reed to search for new catalytic materials. I was the person who said, "High school students can also do this. Let's expand it dramatically and create a huge operation worldwide with mentors." It was my idea to convert the project to something that high school kids could work on after school to get them fired up about science and about chemistry. But it started with Parkinson, and I picked up the idea and ran with the ball because I think bigger, frankly, than some of these people. [Laughter] I figured that this really was going to catch on, so let's go for it. We tried it here with a bunch of high school kids at Poly [Polytechnic Institute, private K-12 school in Pasadena]. We probably started there because those kids are right here [i.e., just across the street from the Caltech campus], and it really went right, and so we just started expanding it all over Southern California and then all over the country. Parkinson still has his little group going, but our program, which is SEAL [Solar Energy Activity Laboratory], is all over the world. There's one particular document that's really good on this and that really documents the history. I'm one of the authors, but it was really written by all the mentors who were here in the beginning of the program and who went to high schools around here; they've all written sections on their experiences.

ASPATURIAN: I know it's been written about, but for the oral history record, will you describe briefly what it is these kids are doing in the solar army.

GRAY: They're searching for new oxide materials that will oxidize water to oxygen. They start with simple chemicals, and they put them on conducting glass and heat them in a furnace to pyrolize them, which makes amorphous metal-oxide catalysts. Then they have an electrochemical rig that they can wire them up to, to see if when they shine light on them, they get currents. If they get lots of current, then it's likely they're splitting water in these systems. So they experiment with different formulations. They test for different metals to see if you add, say, a little titanium to iron, you get a better catalyst. The high schools pick what they want to do, and they all do different things. They have a certain amount of independence in deciding which materials they want to look at and whether they want to test stability–temperature effects, pH effect, ion-strength effects, or whatever. Each little brigade of the solar army has a slightly different view of what's important, and then they all come together at our meetings and give talks and posters about what they've found and they interact, of course, over social media. They're talking to each other all the time.

ASPATURIAN: I assume this is organized through high school science classes, largely chemistry and physics.

GRAY: Yeah, it's usually in the chemistry or physics class. Some are in class, but it's normally a volunteer, after-school activity in which a teacher participates and a mentor, for example, from Caltech. Either a postdoc or grad student visits there once a week and works with the kids.

ASPATURIAN: Was it tricky getting such a comprehensive network organized? How did you get something like this off the ground?

GRAY: We had lots of help from grad students and postdocs who wanted to interact with high school kids. It's amazing. We didn't know what the response would be at first, but a few leaders, like Jillian Dempsey, who's now an assistant professor at [University of North Carolina] Chapel Hill and who was one of my students, were really turned on about this. A lot of the students were already involved in outreach activities of various kinds, and what we found over the years is that there are a lot of kids at Caltech who want to get involved in a mentoring kind of activity to further whatever they want to do in life. We've had all kinds of volunteers come in. Not just the ones doing research in our program but kids all over campus who've come in and said, "We want to participate in a mentoring activity." The response has been fabulous, so it was easy. Then of course we had to hire real top-notch coordinators. We have two. One is Michelle DeBoever, here at Caltech, and the other one is Jennifer Schuttlefield [Christus], who's our national coordinator at [University of] Wisconsin–Oshkosh.

ASPATURIAN: Are these students preferentially clustered in particular states?

GRAY: They're clustered in California.

ASPATURIAN: But there is a national presence, as I understand it.

GRAY: Oh, yes, we have kids all over the country. Let me show you. We have a big solar army website. We have a huge website with maps on it. You can check all this on the web. Our big normal website is here [http://thesolararmy.org]. Here you are, solar army across the world. You can see the states [http://thesolararmy.org/wp-content/uploads/2014/11/Screen-Shot-2015-11-25-at-11.43.46-AM.png].

ASPATURIAN: Yes, West Coast, Northeast, upper Midwest.

GRAY: There's a lot in the Northeast, and there's activity around the world. [Laughter] We're big. This is very big. It's a big deal.

ASPATURIAN: You're not interplanetary yet, but you're global. Have any good leads emerged from the research?

GRAY: Yes. Some very good leads have emerged and have been followed up. Some of the best catalysts now have elements in them that people in the solar army identified. There are three or four documented examples of where they went on to research labs and resulted in papers and so on. There's one from Wisconsin. Our latest catalyst has some lanthanum in it, and you could argue that some of the lanthanide elements were found to be good additives by kids in the solar army. Yes, there are connections to their research activities. They've had some impact.

ASPATURIAN: Do you travel statewide and sometimes various other places across the country visiting some of these schools and interacting with the students?

GRAY: Oh, yeah, I've visited lots of them.

ASPATURIAN: Does this program help to get a lot of these kids more interested, for example, in chemistry?

GRAY: That's an understatement. I mean, these kids are so hungry to do research and hands-on work, and their high school courses really don't motivate them that much, but real hands-on research activities, working on a problem that is perceived to be one of the great problems of this century—renewable energy—really motivates these kids and so we have example after example of kids who've gone on to do chemistry or physics in college. We've been tremendous recruiters for STEM [Science, Technology, Engineering, and Mathematics]. As far as I know, essentially everyone who's in our program has gone on to college to do STEM type things. We've been a main source of recruiting bright kids for STEM subjects so that's one of the great outcomes of this. And also our mentors now occupy top academic jobs all over the country.

ASPATURIAN: You mentioned during an interview or lecture that you've gotten letters from kids as young as nine wanting to become part of the solar army.

GRAY: I sure have.

ASPATURIAN: So does this reach down into the middle schools as well?

GRAY: Not this program, but we do have several programs for middle schools. One's called "Juice from Juice," in which students make their own solar cells. We have workshops for the middle school teachers, and then they run those things with their kids. At our annual meeting, we counted up; we've estimated now that we have reached 10,000 kids.

ASPATURIAN: Has this model that you've developed, which in a sense is crowd-sourcing science among high school students, been imitated in other areas?

GRAY: I think it's starting to be imitated. Lots of people have contacted me to say, "We want to do this in biology." I think it's bubbling up now, and I think if I really looked into it, we'd find three or four examples now of how this is being implemented in other areas because we've certainly had lots of inquiries.

ASPATURIAN: I think this was the first endeavor of its kind?

GRAY: I think it was; it was the first big one of this type. I decided we're going to search for new materials in the transition metals part of the table. I'm not sure why I picked that, but I did. We're going to cross out all the expensive, rare elements. We can't do those. And we're going to make metal oxides; we're going to make them into rocks, robust catalyst rocks. If we want to use two or three metals, there are already millions of combinations, and there are billions of combinations if we want to do three or four metals. My view is that the best catalysts are going to be made up of several metals.

ASPATURIAN: You figure there'd be multiple elements.

GRAY: Multiple elements, yes. So I can either search with robots or students, and I decided students would be a lot more fun than robots, so I started the solar army with this sort of thing. Grinnell College was the first big hit, where a gal by the name of Stephanie Chung found that adding yttrium to a combination works well. And in the summer of 2010, we gathered the first solar army brigade here.

ASPATURIAN: Yes, I remember, I saw that. I came and wrote about it to some degree.

GRAY: And then we decided we needed to build a new kit, which was built by Jay Winkler's son, Gates Winkler, and Jay. This is now called the SEAL kit. It replaced our old stuff and allows you to search very quickly in real time with your workstation and in real time after school and to see the activity of whatever catalyst you have. These photos I'm showing you are of annual meetings of the high school kids. This is one from 2013, outside the Beckman Institute, where all the kids came and made presentations. An all-women group from San Marino High School made this presentation. We have an enormous number of women in our program.

Harry Gray poses with members of his Solar Army in front of Beckman Institute in the summer of 2015.

ASPATURIAN: And I think you've said that the student mentors in the program have gone on to very prestigious careers.

GRAY: Look at that list of schools where they are now faculty: UC Irvine, UC Davis, Harvard, Oregon, St. Louis.

ASPATURIAN: Washington U at St. Louis.

GRAY: Chapel Hill. MIT, UC Riverside, Portland State, Stanford, Denison, Cornell, Michigan, USC, Shanghai Tech, Texas. And industry and government.

ASPATURIAN: Very impressive.

GRAY: These are the alums who worked in our program mentoring kids in high schools.

ASPATURIAN: These are undergraduates as well as graduate students?

GRAY: Undergrads, grad students, and postdocs. This list was two years ago, so we now have two more years added to this. So our legacy is here. And the 10,000 kids we've mentored. The kids we've impacted. I think it's pretty impressive.

ASPATURIAN: When you go out and you talk to people in science, government, industry about this, what kind of reaction do you get?

GRAY: They think it's fantastic. This just lights them up. They come up and say this is the greatest thing they've ever seen. We get such incredible compliments on this from everybody. They say, "Your impact here is going to be so much greater than your research discoveries because this is going to multiply all over the place."

ASPATURIAN: The exponential effect. As for the science itself, how far along do you think things are?

GRAY: They're quite far along. They're far along enough now that people are going to be able to start building devices and troubleshooting them. It's time to start building things and working on them. We have working—

ASPATURIAN: When you say "we," you mean here at Caltech or the field in general?

GRAY: Well, all over the world. I'm talking about the big "we"—everybody working in this field. We now have things that actually work, and work for a long time. We've got to put them together into devices and start troubleshooting these devices and getting them out there. We're almost there. The only component we don't have now is an acid-stable water oxidation catalyst. We've got lots of basic ones that operate in a basic solution, and they're fabulous. We need one more component there and we need a few other things, but we've got plenty of things to split water.

ASPATURIAN: And scalability?

GRAY: We've got to start on that—some engineers have got to start building things now. JCAP ought to start building things.

ASPATURIAN: Are you involved with JCAP?

GRAY: No, just NSF. NSF and DOE are separate. NSF is very fundamental science; JCAP is supposed to be transitioning these discoveries to something that actually works.

ASPATURIAN: Are they doing that, in your opinion?

GRAY: Yeah, I think they are. I think JCAP is really working on that now, but they should push it hard now, even harder than they are. And other DOE labs should really now start pushing. DOE's mission is to do this engineering; NSF's is to do the basic, fundamental work that will be the foundational work for all the engineering. The DOE should be pushing this out there now. That's what their mission is.

ASPATURIAN: So Harry Atwater [Hughes Professor of Applied Physics and Materials Science] and Nate Lewis essentially are overseeing JCAP?

GRAY: Harry Atwater is the director, so he's overseeing everything at Berkeley, Caltech, and Stanford. And Nate is very involved in it, so it's up to them to come through.

ASPATURIAN: If you had to make a prediction now of the sort that you made in *E&S* back in '77, what would you say?

GRAY: Well, solar is coming on fast. Phototechnologies are improving every day. Cost is coming down, efficiencies going up, solar fuels will be coming in soon. We just have to get it going. It'll start catching on. It'll really catch on when we can make fuel cheaper than you can make it any other way, and that time will come.

ASPATURIAN: Is there resistance from some of the energy industry? One always thinks of the Koch brothers and their enormous influence.

GRAY: Sure, changing the infrastructure is very, very hard to do.

ASPATURIAN: That's a good way to put it.

GRAY: It will only come when we're economically very competitive for energy, and then people will get on board because it'll be the best way to go from a financial point of view.

ASPATURIAN: It's always money, isn't it?

GRAY: The environmental driver is not as powerful as the economic factor. But the fact is, we will get to the point in solar where this will be the best way to go economically. That's when it will really take off. Right now the price of natural gas and oil is so low that there's resistance. There's natural resistance because energy is really cheap now. But it's not going to stay like that. It's not going to stay like that forever, and when Florida is underwater, some folks will wake up.

ASPATURIAN: It would be nice if they woke up before.

GRAY: It would be nice, but they won't.

ASPATURIAN: Have you ever testified before Congress?

GRAY: I have.

ASPATURIAN: When, and what has that experience been like for you?

GRAY: I testified twenty-five years ago, around 1990 or so, about the NSF chemistry budget. I testified in the Senate and the House, making the case for chemistry as an essential science that we really need to invest in, and I did pretty well. I got a 13 percent increase for it in the NSF budget. And that wasn't bad. I did that for a couple of years when I was very involved with the NAS and a big National Academy report called the

Pimentel Report. I was sort of the second in command on that with George Pimentel, and I did the testifying in the Senate and the House. And I found people pretty receptive.

ASPATURIAN: They were responsive to what you had to say?

GRAY: They were then. I'm not sure they would be so receptive now.

ASPATURIAN: Well, it's a different climate.

GRAY: I think it's a very different climate now. The Republicans are so far right now, and a lot of them deny climate change. I think it would be a very hostile atmosphere, which I really wouldn't enjoy taking on. It would be for a younger, tougher person to take on now. Bernie could take 'em on.

ASPATURIAN: Even Bernie is going to get exhausted, I'm afraid.

GRAY: He's going to run out of steam, but he has moved Hillary quite a way. So in that sense, he's been very valuable. He's going to keep doing it, which is very helpful because then if she gets in a reasonable place and she beats Trump, we'll be okay, but I'm not so sure she'll beat Trump. The pollsters now are saying that he'll lose big time to Hillary, but of course they had Hillary beating the daylights out of Bernie too, and she hasn't. She's barely beating him.

ASPATURIAN: I have a quote from you that I encountered online, maybe from the talk you gave at Northwestern. "Photosynthesis from nature has run our planet since the beginning of time. It is now time to pay back. We have the potential to have a completely renewable planet." Do you recall saying this? I thought that was a very powerful vision and a powerful statement.

GRAY: What I tried to say was that photosynthesis has been running our planet for 2.4 billion years. It's been running well, and depositing oil and gas—and coal, which we've been living on since the beginning of real civilization as we know it—and we've been getting that really for free. It's time for us to pay back now. And the vision is that we would take the simplest renewable components and create energy. That's sun, seawater, carbon dioxide nitrogen, and oxygen—these are the basic components we'll have forever. And the idea is we'd not only make fuel out of these components, but we'll make everything else. We'll make all the polymers, we'll make all the pharmaceuticals, we'll make everything that we make now.

ASPATURIAN: You get clean water, too.

GRAY: We'll make clean water. We'll make everything out of these simple things, and it will be a totally renewable system. I articulated that vision in my 2009 *Nature* Chemistry paper ["Powering the Planet with Solar Fuel," March 2009].

ASPATURIAN: I think I heard this also in a talk you gave.

GRAY: I said basically that someday, maybe a hundred or two hundred years from now, we will be completely renewable; everything inputs in, out, and we won't be diluting anything, and it'll be a wonderful time.

ASPATURIAN: If we're still here.

GRAY: If the people are still here.

ASPATURIAN: Something I hear occasionally is that if the United States would only invest in a Manhattan Project– or Apollo–scale undertaking for renewable energy, we could solve most of the key problems within a decade. Do you agree with that?

GRAY: I think that's true. I believe that. We've got all the pieces now. If we put everybody together and really worked at it, we could have all of these solar systems working within five years. We could be running the planet on solar five years from now if we did a project like that. But a project like that would require a threat comparable to the Nazi threat of wiping us out, and that's what happened in the Manhattan Project.

Something comparable now would be massive flooding of Florida and New York—all of a sudden New York is underwater and people are dying and there's water everywhere. It would take a huge crisis.

ASPATURIAN: To galvanize this kind of—

GRAY: To make a Manhattan Project. Without that, it just won't happen.

ASPATURIAN: Is there anything else you'd like to say about your last decade or so of research before we close out this session?

GRAY: I'd like to save the rest for another meeting since I have a lot to say. [Laughter] I don't want to give that short shrift. Let's start there. I guess we need a next time.

HARRY B. GRAY

SESSION 5

April 7, 2016

ASPATURIAN: Let's see. I think we pretty much wrapped your solar energy and Solar Army involvement last time.

GRAY: We wrapped that up. We need to talk about other things.

ASPATURIAN: You did want to talk more about your current research. You are the PI for the Laser Resource Center?

GRAY: Yes.

ASPATURIAN: You and—

GRAY: Jay Winkler.

ASPATURIAN: And Jay Winkler. You seem to be doing a lot of work there.

GRAY: Yes, we do a lot of work there. Of course a lot of the catalyst work for solar is done in the laser center.

ASPATURIAN: That's the fundamental research end of things, I assume.

GRAY: Yes. We do things there for folks all over campus—people who have problems that they want to investigate with various kinds of spectroscopies but who don't have their own equipment, although many do. Many investigators have their own lasers, but the ones who don't and who want to investigate something with laser excitation or laser spectroscopy come to us.

ASPATURIAN: How long has it been in existence?

GRAY: Since the founding of the Beckman Institute, so it's been going for twenty-five years. I think it was maybe the first center in it. We serve lots of people. Jackie Barton's group uses it a lot. Nate Lewis's group uses it. Several other groups around campus have uses it for, and we have a system where if a research group is going to use it a lot, we train a grad student in that group to actually work in it. If they're not going to use it a lot, we tend to have our people make the measurements for them. We just finished a big project with researchers from UCLA who came over with some materials that do some organic reactions photochemically. They wanted to look at some of the fundamental photochemistry of these materials, and they can do that here so they brought their samples over. Bryan Hunter and Oliver Shafaat, who are in my group and work in the laser center, did the measurements. The UCLA group has just submitted a paper on this joint work. So that's an example.

The project that's really been going on for a long time is a protein electron transfer project. We've been working on electron transfer through proteins and what controls it, particularly for respiration in photosynthesis. That's how it connects into our solar energy program because natural photosynthesis is a model for us. In our laser center we've done lots of electron transfer measurements that relate to photosynthesis, and this work is funded by NIH [National Institutes of Health]. We're trying to learn lessons about how natural photosynthesis works.

ASPATURIAN: Why is NIH funding this?

GRAY: Why is it funding this? Well, I'll make that connection. There are many respiratory diseases that involve electron transfer. In fact, most diseases involve some kind of redox reaction, some kind of electron transfer reaction.

ASPATURIAN: Really, body-wide?

GRAY: Yes. Aging—the generation of what's called reactive oxygen species [ROS], which is behind lots and lots of diseases—is an electron transfer process. And so the connection between electron transfer and disease is very direct.

ASPATURIAN: Is it that the transfer begins to become less efficient?

GRAY: Well, yes, it becomes less efficient in our respiratory system. We start making peroxides: Instead of reducing the oxygen from the air to water, we start making peroxides that oxidize membranes. This is aging. Aging is electron transfer gone bad in these protein systems where it's not working quite right and you start making reactive oxygen species. It all has to do with oxygen. When oxygen came in 2.4 billion years ago, we had a powerful oxidant that could oxidize foods and glucose from photosynthesis, providing a lot of energy for life, which allowed evolution to start evolving increasingly complex organisms that use lots of energy. Couldn't do it before oxygen. At the same time, of course, oxygen came in as a Jekyll and Hyde, because oxygen's also extremely toxic. If it's not controlled in the body and cells, it will just destroy cells, and that's aging and diseases. Almost every disease has some component of some electron transfer process that went wrong.

ASPATURIAN: Is this discovery—the relationship of electron transfer fails to disease—a direct outgrowth of your work?

GRAY: No, it's been known for a long time. It's been known forever. Redox reactions, when they work—that's great. Electron transfer is a fundamental step in an oxidation-reduction reaction. And then people talk about redox reactions. Electrons are moving around all the time and so on, but redox reactions are what drive life. Each of us is a big redox reaction machine, and photosynthesis is all redox reactions, but if the redox reactions go bad—if they produce the wrong things or generate stuff that's highly toxic from oxygen—then we're in bad shape. One of our main projects right now is trying to make that connection between oxygen in protein electron transfer and how proteins are protected from oxidative degradation. We have a new hypothesis of how this occurs. We think there are chains of amino acids—what we call tyrosine tryptophan chains—in proteins that use oxygen, and that these chains are able to take the highly oxidizing species that are made out to the edge of the protein, where they're reduced in the cell by glutathione or some reductant so that they don't cause damage. If they're left there, they would completely destroy the protein and generate all kinds of reactive oxygen species, and that would be very, very harmful. So our main focus right now is looking at the connection between oxygen and enzymes and electron transfer and at how proteins are protected from doing the wrong thing and making too many reactive oxygen species.

Many protection mechanisms have been suggested. Jay and I have suggested a new one in a big paper ["Hole hopping through tyrosine/tryptophan chains protects proteins from oxidative damage," *PNAS*, Sept. 1, 2015] that *PNAS* [*Proceedings of the National Academy of Sciences*] has just published. Basically our hypothesis is based on analysis of all the protein structures now known, and what we found is that the ones that use oxygen directly have these so-called protective chains. We're analyzing this now and developing experiments to test our hypothesis to see if when you do make these highly oxidizing things in the middle of the proteins, the chains will defuse the bombs. When David Beratan at Duke [University] wrote a perspective or a commentary on our paper, he called it "Defusing Redox Bombs." So he actually described it better than we did. Our paper's got a technical title, but he presented it in a way that everybody can get around. So he's helping us get this word out. But we've got a whole new area that we're exploring now that we think is very critical to reducing the amount of oxidative damage, which in turn would greatly reduce all kinds of diseases. Many diseases are really pretty directly connected with damage that's done by oxygen.

ASPATURIAN: You mentioned respiratory ailments—

GRAY: There are respiratory ailments. There are mitochondrial diseases. Electrons don't get to the right place; they go to wrong places and make toxic things they shouldn't, leading to many mitochondrial diseases. But that's why NIH is funding this work. It has a direct relationship to all sorts of diseases.

ASPATURIAN: Is anyone—or is this too early—starting to look at the clinical implications—treatments, therapies?

GRAY: Not with this most recent thing, because this is literally just a few months old. It's just settling in, and people are trying to figure out the implications of what we're saying. I mean this is really out there, out front. So far out front that most people don't get it yet. Some very smart people have read our paper and say it's a great paper but they didn't really get the message, as far as we can tell. [Laughter] A few people have: People who are more into biology and medicine understand what we're saying, but most chemists haven't made the connection yet. There is a long way to go, but I've talked about this a lot, and people have come up and said, "Can we now rebuild these protective systems if they go bad? Can we help people out?" Probably there's some way to do this, but this is early stages, and we'll see how this develops. This could be a big, or it could be wrong. These chains may have some other function, but we think it's protective; they're protecting cells.

ASPATURIAN: Do you think this is a phenomenon that extends across the entire animal kingdom, or are you looking basically at mammals?

GRAY: We think this pertains to all aerobic life. We think every creature that uses oxygen for life will be protected this way.

ASPATURIAN: I mean, for example, tortoises and turtles live virtually forever. Maybe their repair mechanisms are better? They may have something going on that's really interesting.

GRAY: The turtles may have really good protective mechanisms. [Laughter] Some of these big ones live—

ASPATURIAN: Two hundred years, I think.

GRAY: Two hundred years, yes. They've probably got systems that are really protective, that don't go that wrong. Anyway, that's one of the big things we've been working on recently, and I wanted to get that on the record. In case we're right about this, I want this to be in the oral history.

ASPATURIAN: Absolutely.

GRAY: Somebody will look at this later. It'll either be right, and it'll be a big deal—a really big deal—or we're wrong about this and it'll disappear.

ASPATURIAN: How will it be tested? Are you now following up? Are other labs starting to work on it?

GRAY: It'll be tested by making mutants that interrupt these protected chains. For example, living organisms like yeast would be a first place to test, to mutate, these chains so that they don't work, and our prediction is that the yeast will die.

ASPATURIAN: When do you anticipate starting these?

GRAY: Well, we're starting some experiments now on just simple, single proteins. To see if when we interrupt these chains the proteins themselves start dying or are destroyed and generate something. We've got some experiments going now.

ASPATURIAN: Here at Caltech?

GRAY: Yes, at that level. And if they look like we're on the right track, we would probably talk to some people about doing experiments in something like yeast, or maybe rats or mice—some living animal model in which we can make these mutations and then see what happens. We're predicting that some mutations in these things will cause horrible disease or death.

ASPATURIAN: Do you know what's needed to disrupt the chains?

GRAY: Yeah, yeah, we do. We need to interrupt them with things that won't allow electron transfer through the chains—or what we call holes that come the other way—and we can interrupt them with a simple amino acid that won't—

ASPATURIAN: —Oh, I see, a targeted amino acid.

GRAY: Yes. We know exactly what to put in.

ASPATURIAN: Are you working with biologists on campus?

GRAY: Not right now. Because biologists get it, if we get these initial results, we'll probably hook up with some biologists and some animal systems and get to work on this because it could be a big deal. If this is behind something like Alzheimer's, it could be. Oxidative damage is the cause of lots and lots of diseases.

ASPATURIAN: Autoimmune disorders as well?

GRAY: All sorts of thing. There are all kinds of diseases where oxidative damage is the starting point for the pathology.

ASPATURIAN: I imagine things like chemotherapy that damage—

GRAY: Well, chemotherapy damages things, yes, absolutely. All sorts of things like that make you more likely to have oxidative damage and so forth. Anyway, our electron transfer work, which for years has been very fundamental with only sort of weak connections to disease—although NIH has funded it because of these potential connections—has now moved to the point where we're making a really direct connection.

ASPATURIAN: I see, yes.

GRAY: A much more direct connection where people can now see if we're right about this. This is really a big deal for a connection between electron flow and disease. Then somebody would be able to do something about it. That's what I'm working on now. So I'm still working on new stuff.

ASPATURIAN: You have graduate students still?

GRAY: Yeah, yeah.

ASPATURIAN: Are they working with you in this area?

GRAY: No, the grad students are pretty much working on solar energy problems. We have a postdoc who has just come from the University of Georgia to start working experimentally on this hypothesis that Jay and I did together.

ASPATURIAN: Do you have a name for the hypothesis? What do you call it?

GRAY: We call it Hole Hopping Protective Chains. Hole hopping tyrosine and tryptophan. The two amino acids involved are tyrosine and tryptophan, which came in late in evolution and probably in association with oxygen.

ASPATURIAN: I was going to ask, do you think they evolved in tandem with oxygen?

GRAY: Well, we've got folks now, people working on evolution, who are very, very interested in our ideas and are going to start looking into when these chains may have come in. We're sort of predicting they might have been there a little bit before, but that when oxygen came in, these chains started appearing all over the place. So we've got lots of predictions from this hypothesis. The first place I published it was in a meeting in London at the Royal Society. And then there are two major papers have appeared after that. This *PNAS* one, and one in the *Quarterly Reviews of Biophysics*, all analyzing protein structures in these chains that nobody had really found before. So it's based on analysis of all the structures in the protein databank, over a hundred thousand structures. We wrote a big code to search for tyrosine and tryptophan chains where they're very close together so they could drain out a hole or an electron very quickly.

ASPATURIAN: So in other words, this is research that you probably couldn't have done twenty years ago because you didn't have access to these large databases of information.

GRAY: We couldn't have done it twenty years ago, no way. You need powerful computers; this is really bioinformatics kind of stuff. And so the code we wrote searches the database for certain kinds of chains that have particular properties, and suddenly they popped up and started appearing in all these oxygen-using enzymes, called oxygenases and oxidases. They use oxygen directly to do things. The big one that we were interested in is something called cytochrome P450, which is a big deal. It's all over our bodies.

ASPATURIAN: It metabolizes drugs.

GRAY: It metabolizes drugs.

ASPATURIAN: I know a little about this enzyme.

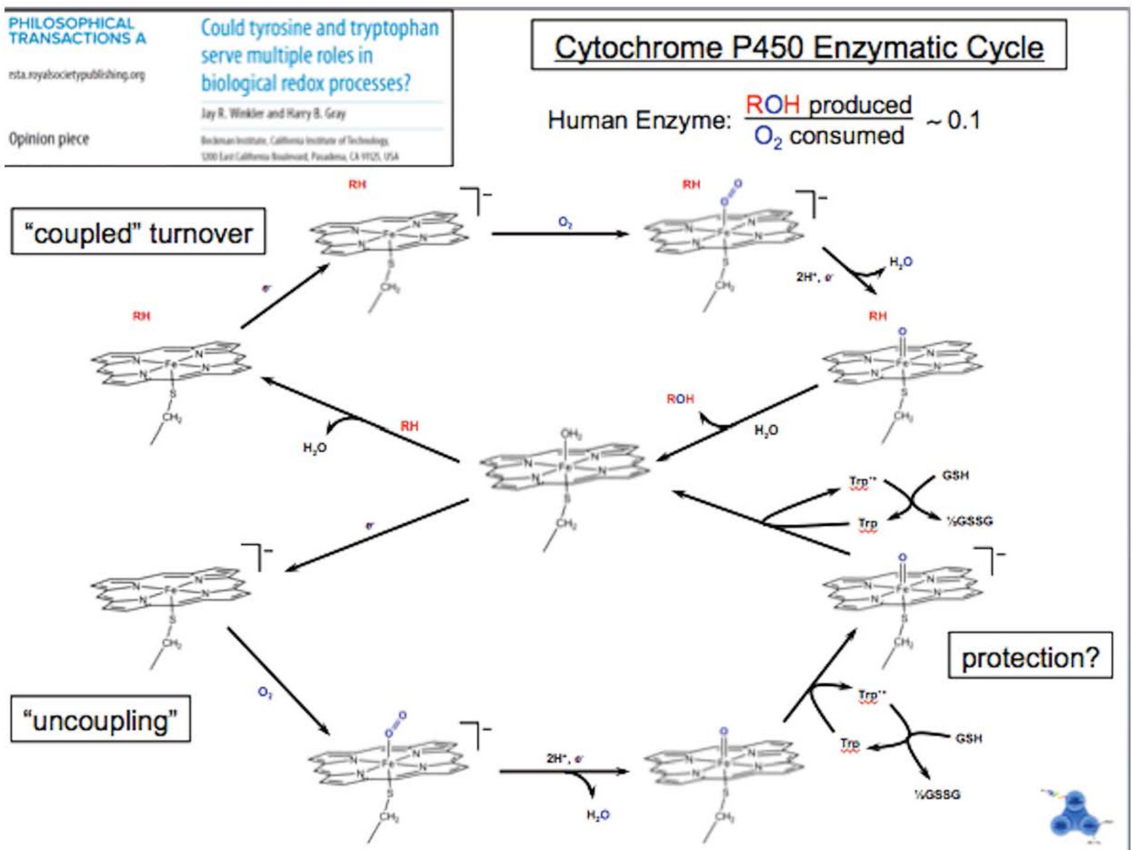
GRAY: The biggest human P450 of all does cholesterol and steroid biosynthesis. We found a fantastic protective chain in that one. And so the ones in our bodies, these P450s, have these big protective chains. So that's where we started and we found them, and then we started to find these chains everywhere in enzymes that use oxygen. But the P450s are big time. And this paper of ours has a big human P450 in it. But the P450s metabolize all kinds of drugs. They're associated with all kinds of disease. They're the ones where if you're a smoker, they end up making toxic radicals that lead to cancer. Lung cancer is directly related to P450 and smoking. And these polyaromatic hydrocarbons that get crushed by P450s get converted to stuff that's incredibly carcinogenic. So the P450s are big-time, medically related things, and we've been working on them for years. NIH is going to support work on those anyway. We'd been looking at fundamental electron transfer processes in them until we realized after some experiments that these P450s, which should have been destroyed by some of our experiments, were being protected by something. So we started searching for it, and we found these chains that we think, logically, are protecting these P450s from getting destroyed.

ASPATURIAN: These are literal chains. It's not a figurative protective chain, they're literally chains of amino acids?

GRAY: Yes.

ASPATURIAN: Because the concept of a chain providing protection is one you see everywhere on a larger scale. We have chains around property, around parks, whatever. Now you are suggesting biological chains serving a security purpose. I just find that interesting.

GRAY: Here, I'm going to show you one of our maps.



I'm talking about this all the time now. I talked about it at Georgia Tech, and I'm going to talk about it next week. Okay, here's the talk I'm going to give. The chain is going to give the enzyme 500 nanoseconds or less than a millionth of a second to do its regular reaction with cholesterol. And if the cholesterol is not there—if it's in the wrong place—and it doesn't do this reaction in a fraction of a millionth of a second, this chain is going to protect the enzyme. It's going to take the anoxidizing equivalent out to the surface and knock it out. So it's timed. These chains are built to put a timing mechanism in so they have time to do its job. If this chain were packed right into here, it would never have time because this would be picoseconds out so it wouldn't have time to do anything. So the first member of the chain is a gap in here that's a timing mechanism. Then the thing goes *zip, zip* out to the surface, instantly, because the chain is back together. We can calculate all these things from all of our fundamental electron transfer work. We know exactly what the timing is and we can calculate it. This map is the result of thirty years of work on fundamental electron transfer. We can tell you exactly how this is timed—that the survival time is going to be half a microsecond or 500 nanoseconds. It gives it that long. There's an enzyme that does methane oxidation, and it has a huge chain. This is the chain, the amino acid chain. And so that's what they look like. And so that's what we found. These are the things we found from the code. We found these things packed together in what we call a chain. It's like a chain around your neck.

ASPATURIAN: Yes, or like a chain link fence. It literally safeguards something. It'll be interesting if that's how nature works.

GRAY: Yeah, that's right. That's it exactly. We should have been a little pushier on this and what the implications are. This is all pretty technical. We didn't want to get too overboard here until we get real experimental evidence. We're working on that now, and we'll see whether this has anything to it. These chains could be doing something else. They could be there for another reason. All we have is a hypothesis right now. And we have structures that look like we're likely to be right, but it's not for sure we're right.

ASPATURIAN: How long do you anticipate the tests will take?

GRAY: It's probably going to take a couple of years before we really get enough evidence to say whether we're really on the right track. Anyway, that's where we are. We're up to date now.

ASPATURIAN: I wanted to ask also about the Resnick Institute [the Resnick Sustainability Institute for Science, Energy, and Sustainability]. You are a part of that.

GRAY: Well, my work in the Resnick is mainly through postdocs and students who have Resnick Fellowships, although I've been involved in the Resnick Institute from the beginning when Harry Atwater was named the director.

ASPATURIAN: Do you know, my sister went to high school with Harry Atwater. I think they were in the same class.

GRAY: He's a nice man, a good guy. He's no longer the Resnick director because Jonas Peters has taken over, and I've worked with Jonas all the time, too. I've been involved in a lot of committees and things with the Resnick, helping them make grants, helping them pick the award-winners for their award program. My research and CCI Solar has in effect been a part of Resnick from the beginning. But my research that's directly related comes mainly through postdocs and students who have Resnick fellowships. Our work on water oxidation catalysts is going on with Resnick support because Bryan Hunter, who's a grad student, has a Resnick fellowship. James Blakemore, a postdoc who just left for Kansas, had a Resnick postdoc. Sonja Francis and Brad [Bradley] Brennan, who are currently working with us, have Resnick postdocs. Samantha Johnson also is working with us; she has a Resnick grad fellowship. So there are a lot of Resnick fellows working with me on solar energy projects. I think Resnick has made a big difference in funding people who are doing great work, so the impact of Resnick support already has been great. It's going to grow as well.

I just had dinner, along with Jonas Peters and some others, with Stuart Resnick at the Athenaeum. We had a nice meeting, and he really likes what we're doing. I think he'll continue supporting the Resnick Institute, maybe even more than he has up to now, because I think he views it as a big success. I view it as a big success, and I think Jonas has lots of good ideas about carrying it forward and will really take it to another level. So it's a great thing for Caltech. It really is. We need a central place where we can really do all these energy projects and put them together properly and make sure people are talking to each other and so forth. I think the Resnick will increasingly play that role as time goes on. I've had only good interactions with the Resnick Institute, and I think our work is better for it. I know it is because we wouldn't have these terrific people otherwise. We wouldn't have as many of them because the Resnick has allowed us to leverage our money. We have put some CCI Solar money into matching Resnick money, so we can amplify our impact this way. We couldn't have as many postdocs working in this area if it weren't for our matching Resnick support.

ASPATURIAN: Kind of an exponential impact.

GRAY: Yes, it really is. It's really been great.

ASPATURIAN: Are there other things you'd like to see the Resnick Institute doing?

GRAY: Well, I think the support of people is the most important thing by far. I don't think we need the Resnick, for example, to have a whole instrumentation area. We probably have enough instrumentation in other places. If Resnick could do anything there, they could have some kind of website that tells people the locations of all the instruments in different programs. I think Caltech in general needs that because there are lots and lots of great instruments on campus, and people don't know where they are. Just today I learned that Jim [James] Heath [Gilloon Professor of Chemistry] has a magnetometer, a very fancy one that everybody would really want to use if they knew Jim had it. [Laughter] We have one at the Beckman Institute, but he's got a newer, fancier model that works better. So stuff like that ought to be on some standard website: Here's the instrumentation you can utilize and here's how to do it. I think Resnick might be a good source of that analysis for people on campus who are doing energy things. But otherwise I'd really like to see them just do more to support postdocs and grad students because they're going to have a huge impact if they do that. The program is good as it is, but I think a doubling of the Resnick postdoc and grad student program would really be a great thing, which of course would take more money. And maybe present that proposal to Stuart Resnick and say we're really doing great, and if we could double this program, it would have a five-fold impact.

ASPATURIAN: Well, you'll have to see how that works out.

GRAY: Something like that. And I think Jonas is thinking in those directions already. What about the other things?

ASPATURIAN: We mentioned administration. Campus culture and the evolution of the Caltech environment. I'm sure you have some thoughts on that.

GRAY: Well, you know, it's interesting that Stuart Resnick and the other folks at this dinner I went to were very curious about these same issues. They were particularly interested in how the student body has changed over fifty years—whether they were any different, any smarter. Students are generally better rounded now than I think they used to be; they were really super nerds fifty years ago. Now they're just regular nerds who have lots of other interests and so forth. But the administration has generally been very, very good over the years, and supportive of things that we wanted to do. I think Tom [Thomas] Rosenbaum [professor of physics; Caltech president, 2014–present] is a great new addition, as far as I can tell from dealing with him. He seems like a genuine person who's also a scholar. We do better when we have real scholars in the administration and ones who are actually active now like Tom is in physics and Ed [Edward M.] Stolper [Leonhard Professor of Geology, Caltech provost, 2007–present; interim president, 2013–14] is in geology. You tend to listen more to leaders at Caltech who are actually doing scholarly work than to people who would be in law or business or something, which is what most schools now have gone to for presidents.

ASPATURIAN: Who was president when you came?

GRAY: [Lee A.] DuBridge [president of Caltech, 1947–1969; d. 1994].

ASPATURIAN: Did you have much interaction with him?

GRAY: Oh, I had a lot of interaction with DuBridge. He was a great leader. When I came, DuBridge was president, and Bob [Robert] Bacher [professor of physics; Caltech provost, 1962–70; d. 2004] was provost.

ASPATURIAN: Yes.

GRAY: They were great leaders and visionaries and put Caltech in a great spot. And then Harold Brown came in. I had a lot of interaction with Harold Brown [Caltech president, 1969–1977], in large measure because he was connected with Arnold Beckman directly and so was I, and so we had the common Beckman connection. I saw a lot of him. Then there was Bob [Robert] Christy [Institute Professor of Theoretical Physics, emeritus; Caltech provost, 1970–1980; acting president, 1977–1978, d. 2012] briefly.

ASPATURIAN: Who was in the acting capacity.

GRAY: The acting thing. And then we had Murph. For my money, Murph was the most supportive president for chemistry. We'd of course only had physicists up to this time, including Murph; and the physicists in general, I think, had a hard time understanding what chemistry was all about, whether it was a real field or just kind of an applied physics operation.

ASPATURIAN: Did you find that a bit of an annoyance, that attitude?

GRAY: Oh, yeah—but these people were nice people, so I wasn't that annoyed. I was more annoyed talking to physics people at Columbia than to the ones here. But Murph made the difference. Murph valued chemistry for its scholarly activities directly, and he understood it better than DuBridge or Harold Brown.

ASPATURIAN: Why do you think that was?

GRAY: I think Murph was a people person. He was willing to sit down and talk to people and try to understand what they were doing. He would come over and hang out with the chemistry people, talk to them, go to lunch with us, and just chat. He learned much more about what was going on in chemistry, and I think he knew more than most physicists even, from his time at Princeton. But he appreciated chemistry much more than any other president we have ever had. I think Tom Rosenbaum will also be in that category.

But I think overall the physicists felt that chemistry was not at the level of physics in terms of importance.

And David Baltimore, I think when he came here, didn't understand what role chemistry could play. He was deep into biology, and I think he grew when he came in as president. He would keep asking what chemistry is really all about, and I think he learned during his time as president that there was something in chemistry that was really important for biology and that biologists had better really get a lot of chemistry because it was going to drive a lot of biology. I think he had a much better appreciation for chemistry at the end of his term than he did at the beginning. That's my feeling.

ASPATURIAN: I want to step back a minute. What was it like working with Murph on the one hand and Robbie [Vogt] on the other? One was president and one was provost, and they were two very different personalities.

GRAY: Yeah, well, I worked with Murph and I got pushback from Robbie. But I could work with Robbie. Robbie and I talked a lot and fought a lot, but we're good friends. It was one of those family disputes [See also *Session Three*]. He just wanted to slow me down. I was a little too out there for Robbie, and I wanted to move faster than he wanted to move on a lot of things. I was always hitting him up for more appointments, and saying we need this and we need that. Whereas Murph and I hit it off completely. I worked very closely with him all the time, and he played an enormous role in getting funding from Arnold Beckman for the Beckman Institute. I've said many times, and I'll say again, he didn't get the credit for all he did, and I will put that on the record over and over and over again. Tom [Thomas E.] Everhart [professor of electrical engineering and applied physics, emeritus; president of Caltech, 1987–1997] came in and got most of the credit. Tom Everhart was a president who I think appreciated chemistry fairly well. He was an engineer, and so was Jean-Lou Chameau [president of Caltech, 2006–2013]. They were okay. I've worked with all the presidents we've had, and I've been very good friends with all of them. I see Tom Everhart all the time. But for my money, Murph Goldberger was the best president for chemistry at Caltech.

ASPATURIAN: How about [Steven E.] Koonin? He was kind of an inside person who became provost and was in that position for quite a while [1995–2004].

GRAY: Koonin did a good job I think as provost. As director of the Beckman Institute, I had some discussions with him that were hard-hitting about money, and this and that and the other; and he thought I was wrong about a lot of things. But when we asked him to look into some of these things, and when he found one particular thing that he was actually wrong about and I was right, he made good. He was a straight-shooter guy. When he found what was correct, he wouldn't try to stonewall you. He would say, "Yeah, you're right, and I'm going to make that right." And so I felt working with Steve Koonin was fine. Barclay Kamb [Rawn Professor of Geology and Geophysics, emeritus; Caltech provost, 1987– 89; d. 2011] as provost was fine. Paul Jennings [professor of civil engineering and applied mechanics, emeritus; Caltech provost, 1989–95; 2004–07] was probably the most supportive provost. Jack Roberts was very supportive.

[PORTION TEMPORARILY CLOSED, pages 198-200]

[RESUMES, REMAINDER OF PAGE 200]

ASPATURIAN: What were your feelings about Chameau?

GRAY: I liked him personally, and I thought he was a very decent guy and very good for students. I guess a lot of faculty thought he was more of a lightweight or something; I don't know. I think he made a huge mistake going to Saudi Arabia. I wouldn't do that. They couldn't pay me enough to go to Saudi Arabia.

ASPATURIAN: Did you have any inkling that he was going, or why he decided to do this?

GRAY: No, I don't know what the dynamics, the backstory, was on this. There probably was a backstory of trustees or faculty members or something; maybe people were complaining to him or something. I don't know. And he just got tired of it and said, "I'm not happy here, and I'm going to leave." I don't know. I really don't know. Something must have happened here, though, that he got upset or just tired of dealing with folks or something, and I can understand that. It can wear you down.

ASPATURIAN: It's true.

GRAY: People can really wear you down, and some people may have just been coming to him complaining all the time about "you should change this and that," and complaining and complaining about whatever. And he just said, "Well, okay, I've had it; I'm tired of listening to these complaints." It could have been that the trustees were unhappy with him for some reason. I don't think so, but maybe they were. I really don't know why he decided to step down and go to Saudi Arabia. I'd like to know. I wish he would tell me.

ASPATURIAN: It's certainly an interesting decision. When you say with such emotion that people can really wear you down, do you have a couple of specific incidents in mind from your career here, or is this just a general observation?

GRAY: It's a general observation. I think when you're in a leadership position such as chairman of the division, there are going to be people who pull together and are good colleagues, but there are going to be a few people who take all your time complaining that they need more money or space, and they just keep complaining. You'll always have these people, no matter what, if you're in a leadership position. And it does drain you because they keep complaining, and they keep not pulling their weight or not doing the teaching they should be doing, or they're doing it badly—there's all kinds of stuff like this, and it really does wear you down. There'll always be a small fraction of folks who will just take up all your time.

ASPATURIAN: They suck up all the oxygen.

GRAY: They suck up all the oxygen, and after a while you just say, "I've had it. I've had it." [Laughter] "I'm tired of propping this person up and raising money because they're not raising any themselves." And they keep coming to me and begging for this, and they can't go on; or they're complaining about some colleague, complaining about something. It just takes all your time. I can imagine as president of Caltech, as accessible as it is here, that people could just be at his door all the time complaining about whatever, wanting this or that, and he finally said, "Okay, I've had it." Particularly if you're a very decent guy and are willing to talk to people. If you're a tough cookie and just shut 'em out, you can manage it. I'm pretty sure Baltimore was tough enough. He wouldn't even talk to people, probably. That's my guess, but Chameau was kind of a people person. He liked to talk to people, so he probably opened himself up to all of these people coming in and complaining about this or that and they thought he's a good guy, and he talked to them. My guess is they wore him down.

ASPATURIAN: You mention Baltimore.

[PORTION TEMPORARILY CLOSED]

GRAY: We had a connection going way back to his Swarthmore days.

ASPATURIAN: You and Baltimore did?

GRAY: Yes, one of his teachers at Swarthmore was Gil [Gilbert] Haight, who wrote books with me. I worked a lot at Swarthmore with Gil, and Baltimore knew that when he came here, and he liked to talk to me about that because Gil had a big influence on his life. And so I had a kind of connection with David, and I found him a really good guy to talk to. But I can understand other people's opinions because I've seen him giving talks and so on, and you get the impression that he could come across to a lot of people as arrogant.

ASPATURIAN: He struck me as a very ethical guy, someone who wanted to do the right thing.

GRAY: I think you're exactly right. I think he wanted to do the right thing. But you know, he is so smart, so incredibly smart, that he probably didn't realize how he was coming across to some people. Baltimore was probably always an elitist. I don't know. I'm not sure. But David and I get along fine. We love to talk about things, and I find him incredibly smart. Anyway. Well, we'll get together again.

HARRY B. GRAY

SESSION 6

May 3, 2016

ASPATURIAN: I would like to start by noting for the record that in fifty years you've won at least as many honors and awards. I wanted to ask which of these have been the most meaningful to you, and why?

GRAY: Well, that's a tough question. I would say some of the honorary degrees would be at the top of my list because I was able to see old friends and celebrate with them. I think that I have by now nineteen honorary degrees, and many of those were memorable. Certainly the one from Columbia was very special because I was on the faculty there, and I thought it was very nice of them to later give me an honorary degree. The one at the University of Pennsylvania is very, very special because I've known all the people there for a long time. The University of Copenhagen was very special. The Weizmann Institute was very, very special because I have so many friends in Israel. Probably my first [ACS] Award in Pure Chemistry was special.

ASPATURIAN: That was 1970. You were very young.

GRAY: That put me on the cover of *Chemical and Engineering News*. I'm on the cover of that, playing the guitar. So that's a famous incident and quite special. And many years later, after a bunch of other ones, getting the Priestley Medal was quite special. In Israel, the Wolf Prize, which is awarded in the Knesset, is of course is a very big deal, and that was very, very special. The Welch Award in Texas was a big affair, and lots of people came, and that was very, very nice. So those are some of the highlights. Of course the National Medal of Science.

ASPATURIAN: Yes, I remember writing about that for *On Campus* [Caltech's campus community newspaper, 1985–2000] after I interviewed you and Hans Liepmann [von

Kármán Professor of Aeronautics, emeritus; d. 2009. He received the medal in the same year, 1986].

GRAY: Being in the White House with President Reagan.

ASPATURIAN: Do you remember what you said when I talked to you about that?

GRAY: No, I don't.

ASPATURIAN: First of all you said—I think it was you, but maybe it was Hans Liepmann—that "the medal was so heavy they had to warn us not to drop it." And then you said, "Reagan isn't very tall. I'm taller than he is."

GRAY: That one's me. I'm a lot taller than he is.

ASPATURIAN: That piece got so much feedback because you both had entertaining things to say.

GRAY: Yeah. I think Hans must have said the thing was heavy and don't drop it. I can remember saying Reagan wasn't as tall as I thought. [Laughter] I thought he was taller. You see these images of people, and you think, "Well he's pretty tall."

ASPATURIAN: The magic of movies.

GRAY: He wasn't very tall, actually. I've got photographs to prove it.

ASPATURIAN: Did you spend much time talking to him?

GRAY: No. I chatted with him just briefly. It was special for the family to be in the White House. All day, we were roaming around.

ASPATURIAN: Really?

GRAY: Yeah, we were in there quite a while. It was a lot of fun looking around the White House and seeing things. So that was special. Those are some of the highlights, I would say, of my awards, although I've enjoyed every one. When people introduce me, they tend to mention the National Medal of Science, the Wolf Prize, and the Priestley Medal—because they're all chemists and they know that the Priestley Medal is the highest U.S. honor in chemistry. So those are the three that are always mentioned. I've had lots of them. I really piled up a lot of medals; I don't know how many there are.

ASPATURIAN: I didn't count them all, but, as I say, I think there were at least fifty awards from five decades.

GRAY: There are more than fifty.

ASPATURIAN: Another one that came to my mind was when you were voted a foreign member of the Royal Society because I remember interviewing you about that as well; and you said, memorably, that nothing would stop you from being there personally to sign the book in which Isaac Newton wrote his own name.

GRAY: Right. That was special, and I did look up Isaac Newton's signature. Sure enough, it's in the book. [Laughter]

ASPATURIAN: "I. Newton," I think.

GRAY: "I. Newton." So, yes, that was very, very special because they don't elect very many foreign members.

ASPATURIAN: No, they do not.

GRAY: It's a pretty special category. Of course when I was elected to the National Academy of Sciences, that was a highlight.

ASPATURIAN: You were very young.

GRAY: I was quite young. Thirty-five. So I've been a member forty-five years. You know, people are just getting elected this year some of whom are my age. [Laughter] There was one moment when I was the youngest member of the National Academy. Right after I was elected, they announced that I was the youngest member. That didn't last long, of course. [Laughter]

ASPATURIAN: There's always somebody nipping at your heels.

GRAY: There's always somebody. Oh, after a couple of years, they elect younger people. But at least I had a day in the sun. So those are some of the highlights. It's really been very special to get all of these honors. I don't know what to say about it except it's fun. I had fun doing the science, but it's nice to be recognized.

ASPATURIAN: Absolutely. I also wondered, what other institutions do you think of as major powerhouses in chemistry? Obviously Caltech is very high. You mentioned having a lot of colleagues in Israel. Tiny country. They have, I believe, six Nobel lureates, all in chemistry; I looked this up at one point.

GRAY: Yes, they do. Well, the Weizmann Institute and the Technion are real powerhouses. You mean worldwide?

ASPATURIAN: Worldwide.

GRAY: Well, Oxford and Cambridge certainly are. The ETH in Switzerland. Some of the Japanese universities—Kyoto, Osaka. I would say Osaka University is a powerhouse. And of course in America, MIT, Stanford, and Berkeley are all really right up at the top.

ASPATURIAN: How would you say the Chinese are coming along in their efforts? I mean, they're putting a lot of energy and resources into science.

GRAY: They're coming up really fast. I just had a conversation with Peter Stang, who's the editor of *JACS* [*Journal of the American Chemical Society*], and he says the Chinese publishing in *JACS* is now really going up fast. They're getting top papers into it. I think they've passed the Japanese now and maybe the UK, I'm not sure. But he was pointing out that they are very soon going to be second to the United States in terms of top papers. So I think it's going really well in China. They're investing a lot of money in science, and we have to watch out or they'll pass us. Although most of the great universities are in the U.S. still. Maybe our greatest resources are our research universities.

ASPATURIAN: Let's hope that can continue. How do you think overall that the climate for chemistry has changed in the fifty-plus years you've been active in the field?

GRAY: It's changed in the sense that when I started, very, very fundamental work was treasured more in the academy than applied work. There was, I would say, a bit of a disdain in chemistry departments for applied work. You needed to be really fundamental to be recognized, I think, as a top person. That's certainly changed over the last fifty years when we've recognized all the major problems in energy, environment, and health. People have transitioned more to looking at and working on real-world problems, and as that has happened in chemistry, I'd say chemistry has been taken much more seriously than it was in the 1960s. Back then I'd say physics was considered the "real" science. And chemistry and biology were sort of down there. But now I think that chemistry is viewed as maybe the most important science for the 21st century. Because the fundamental work in chemistry addresses all of the real-world problems now.

ASPATURIAN: Well, solutions to our energy issues, if they're going to come, will come through chemistry.

GRAY: Environmental issues as well. And there's the overlap of chemistry and biology in human health. So chemistry is in the middle of all the big time problems in this century. I think that after World War II when of course physics came through with the bomb and radar and all kinds of things, people were looking more to it. But now the spotlight is on chemistry much more.

ASPATURIAN: It's the peacetime science.

GRAY: The peacetime science is much more in chemistry, I think. So I've seen it go up and down over the years, but I think right now, it's way up.

ASPATURIAN: How do you think the funding climate has affected the ability of very talented researchers to do original work?

GRAY: Well, it's a mixed bag. I think it's become harder and harder for young people to get a good start because the funding climate is so competitive. And with this move toward more applied work, it's much harder now to get funding for really fundamental work. To get funding you have to have a spin on everything and connect your work to something that's important down the road. Whereas back in the 1960s you could get funding without sort of saying this is going to solve this, that, or the other. You could get it for just fundamental work, just for curiosity-driven research. You can't really do that anymore. The curiosity-driven research has to come as part of a package in which you get money for some project that's going someplace and people can see it's going someplace. Also there's been much greater emphasis on what NSF calls broader impact. That means outreach things, interacting with the pubic, which of course I do a lot. But in order to get an NSF grant now, you have to have a plan as to how you're going to disseminate your work or how you're going to make a difference with helping people to understand science better and things like that. That has changed the landscape quite a bit. So people who are just trying to get funding for pure curiosity-driven research are having a real hard time. The folks who understand how to deal with funding agencies and knowing how to basically spin their program toward things that are perceived to be really important are getting all the money. So, I would say, in research, the rich are getting richer, and the poor are getting poorer. I'd say the funding is being concentrated in a few places and groups who are doing things that the agencies perceive to be really important work for the future. There are some centers whose programs have expanded with big grants, while the individual investigator just in a curiosity-driven thing is having a very hard time.

ASPATURIAN: What advice would you give to such a person, based on all your years of experience?

GRAY: Well, there are many awards for young investigators, and I think my advice would be to write as many proposals as you can. My advice would be, "Don't give up." Seize an opportunity to collaborate with a research center or program that would work for you and allow you to do what you want, because there are a lot of centers around that people are attaching themselves to, and there are a lot of collaborations that work. I would say, look for those opportunities. And don't give up. If you think you've got something important, just keep sending your proposal in. At some point, somebody's going to recognize it. But it's a tough problem, and I see these people all the time who've been shut out. A lot of older people who are really quite distinguished are being shut out now because they're still living in a past where they could get funding just for having a good proposal with a good idea for fundamental work and are failing to connect their current research to problems that need to be solved.

ASPATURIAN: These are distinguished academics at universities?

GRAY: Oh, yes. For example, I know several very distinguished, professors at Berkeley whose NSF grants have expired, and they haven't been able to renew them.

ASPATURIAN: Do you think that Caltech is to some extent insulated from this because of its traditional focus of pioneering fundamental research, or have things changed here, too?

GRAY: I think Caltech has been very successful in acquiring funds from agencies, if you look at my colleagues in chemistry here. Jackie [Jacqueline] Barton would of course know this better than I since she's division chair, and she knows who's raising money and who isn't. When I was chair I certainly knew some people were having trouble getting funding while many others were just getting lots of funding. If you look at our division now, I would say we're raising a lot of money. For several years, we've been No. 1 in fundraising. And even though we have a smaller faculty than some, we're still No. 1 last time I checked, raising more money than any other university.

ASPATURIAN: What do you attribute that to?

GRAY: Well, we've got some big time players here. I mean I've raised over $40 million in my program. Ahmed Zewail has probably raised as much in his big center. Nate Lewis was the PI on the big JCAP thing, and that was $25 million for one year: Berkeley got some, and we got some. These are huge grants. I guess the reason we've raised so much money is that we've been working in areas and developing big centers—big collaborative programs—where the grants are very, very big. And we have some very aggressive fundraising people like Nate and Ahmed and me. [Laughter] Some folks over in chemical engineering as well I think are very aggressive, very successful in fundraising. I'm sure Frances Arnold has got plenty of money. Dave Tirrell, I'm sure. And the other thing is that almost everybody in our faculty is very research-active. Whereas you go to other places and you'll find a reasonable percentage of people who are out of research now and are teaching and not doing research. Here in this division everyone is research-active. There are two or three folks who are not that active, but everyone is doing research, and most—95 percent of the faculty members—are raising substantial amounts of money, I'm pretty sure. I know of only two faculty members in our whole division who are probably not raising much money, and I'm not sure of that. The person who would know what every person has raised is Jackie Barton because she keeps track, and I report to her all the time. You know, when I was chair, the financial people sort of kept sheets on how everybody was doing, and whether they were in good or poor financial shape in terms of being able to support their group.

ASPATURIAN: A report card.

GRAY: A report card. I bet Jackie has something like that now for people who aren't raising enough money to support their research operation so she that will have to supplement it somehow. She would know that. But my guess, based on all the money I know that we've raised and the fact that we're No. 1 in the country for raising money in chemistry, is that there are probably very, very few faculty members who aren't actively raising a lot of money. Almost everybody is.

ASPATURIAN: Do you think the fact that so much time and energy these days goes into raising money is detracting from the research that you and your colleagues have been able to do in the last decade or two? I hear this frequently from people.

GRAY: Most people would say that's true. And complain about it.

ASPATURIAN: That sums it up.

GRAY: I hear that a lot. I have a different take on it. I find that it's very good to look at what you're doing and think about it and discuss it with your group and write proposals. The national labs, for example, don't have that pressure on them all the time. They write shorter things, and I think that from having money that's easier to get and not having to think about where they're going, they get soft and they don't do leading edge work. I think the people who are doing real leading edge work don't mind thinking about where they're going and writing new proposals. It's been a good exercise in my group to do that.

When I started at Columbia in the 1960s, it was rather easy to raise enough money to support your group. One grant, or maybe two, would support everybody. Then it got more and more expensive, and I went through a painful period where I was still writing the proposals myself and sort of resentful of all the time it took to do that. Then I realized that you could make this a group activity, which would also be an educational activity—an actual learning experience—of getting your group involved in writing a proposal and trying out ideas, and that this would do several things. One, it made talking about research and drafting proposals and getting them done much easier than when the burden was just on me alone. And secondly, it was a great opportunity for the students and postdocs in my group to get experience with writing proposals, and they went out much better prepared to raise money. So we went into this mode of working on proposals together, and then it became fun again. Real fun to do this and talk about ideas and kick them around.

ASPATURIAN: It probably improved the overall quality of the project, too.

GRAY: It improved the work, and it improved the proposals. In a way these Caltech centers are an outgrowth of that. Here we get a lot of people together to work on these proposals, kicking ideas around and putting the proposals together. You'll hear a lot of people complain about the time it takes, but I have a different view of it. I think if you manage it properly, you can turn it into something that's a very good use of your time and help your group as well and be fun. I contrast this with everything I've been hearing recently about all the time that senators and congress people have to spend raising money for themselves, where they spend four or five hours a day asking people for money.

ASPATURIAN: They're certainly spending more time doing that then they are benefiting the country and their constituents.

GRAY: That's true, and there's been a lot of play on that recently. I've seen several interviews with congress people about how much time they're spending doing this, and several of them of course resent it. Others just say that's what they have to do: They've got five hours a day when they have to go to this little place they have outside Congress and sit and call fifty people or something until they're raised their quota for the day. I mean if somebody told me that, I'd say, "That can't be true; it's completely ridiculous." How in the world is the country operating that way? I think they should blow the whistle completely on that.

ASPATURIAN: I was just reading that it's been estimated that they spend maybe 120 days a year actually doing something related to legislation. All the rest of it is—

GRAY: Raising money.

ASPATURIAN: Well, it's not in Congress. Let's put it like that.

GRAY: It's not in Congress. I think the citizens of the country should say, "Stop this. Help us run the country. This is ridiculous." Well, the campaigns go on too long. The funding for them is out of control with these super pacs. It's a real disaster.

ASPATURIAN: Although the super pacs haven't done anything this year, have they?

GRAY: They haven't done anything.

ASPATURIAN: It's really interesting.

GRAY: Trump clobbered them. And Bernie. Bernie and Trump. Bernie got tons of money just in small contributions.

ASPATURIAN: Yeah, unfortunately, I guess his fundraising is falling off because it's pretty clear he's not going to get the nomination.

GRAY: Hillary's got it.

ASPATURIAN: Hillary's got it but I think he's pushed her to the limit.

GRAY: I'm still for Bernie.

ASPATURIAN: I'm a Hillary supporter.

GRAY: Are you? Yeah, that's right, we talked about that.

ASPATURIAN: Yes, we did. We can come back after the election and just see.

GRAY: Well, Bernie's got Indiana today. If he loses in Indiana, then he's finished.

ASPATURIAN: It doesn't matter; he's finished anyway.

GRAY: He's finished anyway; there's no way he can win where he is. I don't think there's any way Cruz can win.

ASPATURIAN: Not even with Carly Fiorina by his side.

GRAY: Yeah, that was a real desperation move, and Cruz has turned really sour. He's turned mean and sour and crazy.

ASPATURIAN: I think he's always been like that, Harry.

GRAY: I agree, he has been. You heard what John Boehner said?

ASPATURIAN: He just stopped short of calling him the anti-Christ.

GRAY: Well, he said Lucifer in the flesh. [Laughter] Wow! The Republicans are really having a set of problems.

ASPATURIAN: They're devouring themselves. Okay, too much editorializing. I wanted to wrap up by asking for your retrospective thoughts on Caltech. Five decades. This year, I believe, will mark your fiftieth year here.

GRAY: That's right. This is my fiftieth year here.

ASPATURIAN: So—

GRAY: What's it gone through? It was really fun to be here in the 1960s and '70s when it was smaller and you could talk to everybody and get to know everybody, really, in the faculty. It's grown. It's grown. Many more buildings, which means you don't see people as often, because for that proximity is everything. Now there are so many different buildings—although the faculty is about the same size overall. We don't really get together that much because life is much more complicated now in terms of all the things one needs to do to keep up with everything. There are many more seminars; paperwork has gotten sort of out of control. Just everything is more complicated, and so we have tended to go from a campus where you would hang out with everybody—the historians and physicists, biologists, geologists, and see them all the time, really—to now when it's much more likely that you just see your colleagues in your division. Your life runs around that more than it does around getting out and seeing a lot of other people, though you do see them from time to time. So that's definitely changed. I'd say that the Caltech interactions have gone from being throughout the whole Institute to pretty localized within people's subgroups or divisions and so forth. I don't really see physicists at all now, whereas in the 1960s, I saw them all the time. Because physics is physics, and chemistry is chemistry now. There are many buildings in chemistry. When I came, it was really just Gates and Crellin—Noyes had just been built. The campus was much, much smaller so you could walk around and see almost everybody all the time, run into physicists all the time. You know I saw Murray Gell-Mann [Millikan Professor of Theoretical Physics, emeritus; 1969 Nobel laureate in physics] all the time—of course I worked with him. I saw Dick Feynman a lot, and lots of other physicists—[William Alfred] Willy [William A.] Fowler [Institute Professor of Physics, emeritus; 1983 Nobel laureate in physics; d. 1995], and so on. And now I occasionally see Kip Thorne [Feynman Professor of Theoretical Physics]. [Laughter]

ASPATURIAN: Very occasionally, I'd imagine.

GRAY: At events of various kinds I see him occasionally. And I don't see that many geologists. I used to see Sam [Samuel] Epstein [Leonhard Professor of Geochemistry, emeritus, d. 2001] and Lee [Leon] Silver [Keck Foundation Professor for Resource Geology, emeritus] and Jerry [Gerald] Wasserburg [MacArthur Professor of Geology and Geophysics, emeritus; d. 2016] and all those people, all the time, just sort of wandering around campus. I got to the Athenaeum much more often than I do now. You'd go to the Athenaeum almost every day and talk to people. Now, I rarely go there, just because life is more complicated and there's more to do. But it's still fun. And I would say Caltech has gone the way of all places, getting more complicated, more bureaucratic, and so forth, but it's done so more slowly than anybody else. Berkeley, for example, in terms of bureaucracy, is completely out of control, as far as I can tell, with a zillion administrators and so forth. We've done this as well—look around—but not as fast as Berkeley and the overall University of California. So relatively speaking, it's still more fun to be here than at these other places. And we still have fun within our groups, I would say. I'm having as much fun overall as I did in the 1960s because I have a great group, but I would say people now are spending more time within their research groups than they are interacting all over campus.

ASPATURIAN: Do you think that's true with pretty much everybody?

GRAY: Yes. So we've changed a lot. Caltech is very different than it was fifty years ago. A lot of things are a lot better. The air is a lot better. [Laughter] In the 1960s and '70s, the air around here in Pasadena was really horrible. Research in some ways is better in the sense that we have bigger, more powerful instruments. You can investigate problems now that you couldn't think about fifty years ago because you can get answers to complicated questions like structural problems. When I started over fifty years ago, it would take four or five years—a whole PhD thesis—to solve the structure of a small molecule or protein. Now PhD theses sometimes have a hundred structures in them. You can solve a structure in one day. You can get a crystal and bring it over here and have a picture of the structure in less than a day. And so the advances in technology have made research a lot more fun in a way because you can get answers faster and you can work on problems that are much, much harder. These are problems that you wouldn't even have attempted fifty years ago, so that's good.

ASPATURIAN: That's exciting.

GRAY: So with all the expansion have come some things that bother you, but also some things that are really very good. Overall, I think everybody's pretty happy about the current state of Caltech. I think Caltech is still a real jewel, unlike any other place in the world, really, where people can interact a lot and know most of the folks, whereas at these huge places, you get lost. You don't have to get lost here.

ASPATURIAN: That's very true.

GRAY: So, it's changed a lot because of advances in technology and the need to have more and more supporting staff and more administrative folks to support things as they develop. So it is what it is. And I'm still very happy doing research and having fun.