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RUDOLPH A. MARCUS
(1923–)

INTERVIEWED BY
SHIRLEY K. COHEN

December 1, 7, and 14, 1993

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Abstract
An interview in three sessions in 1993 with Rudolph A. Marcus, Arthur Amos Noyes Professor of Chemistry at Caltech and Nobel laureate in chemistry, 1992, conducted by Shirley K. Cohen. Marcus recalls growing up in Montreal and Detroit, his undergraduate and graduate student days in chemistry at McGill University (BSc 1943, PhD 1946); Canadian anti-Semitism and quota on Jewish students; recollections of advisor Carl Winkler and other teachers Raymond Boyer, Otto Maass, and Bob McIntosh; fellow students Louis Nirenberg, Lazar Novak, Sam Epstein; research on chemical reaction rates. He then went to the National Research Council of Canada to do postdoctoral work under Edgar Steacie and Basil Darwent. Marcus discusses his interactions with Nathan Rosen and Wayne Bowers; the "Anomalies in Reaction Kinetics" 1951 symposium at the University of Minnesota where he first presented his work on the theory of unimolecular reactions (the RRKM theory); and his quest for a faculty appointment. In 1951 Marcus joined the Polytechnic Institute of Brooklyn faculty as assistant professor of chemistry. He recalls early experimental work there with gases, high-vacuum equipment, and rates of various chemical and photochemical reactions; his colleagues Herman Mark, Frank Collins, Paul Doty,

Ernest Loebl, Herbert Morawetz, Bruno Zimm, and Paul Ewald; and his key paper in 1956 in electron transfer theory. Sabbatical year (1960-1961) spent at Courant Institute of Mathematical Sciences, New York University; Dick Bernstein's role in Marcus's decision to wind down his experimental program around 1960; professor of chemistry at University of Illinois (1964-1978) and head, division of physical chemistry (1967-1968). Oxford and Munich sabbatical, 1975-1976. Consultant at Brookhaven National Laboratory and Norman Sutin's influence. Faculty years at Caltech (1978-present) and interactions with Harry Gray, Fred Anson, Jackie Barton, Ahmed Zewail, and other colleagues. Concludes the interview with his approach to theoretical research and getting students to focus on experimental phenomena; honors; post-Nobel life; opinions on "hype" and the role of chance in research.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

INTERVIEW WITH RUDOLPH A. MARCUS

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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COHEN: I wonder if you could tell us something about growing up in Montreal, and your childhood, and your schooling.

MARCUS: I was born in 1923, and lived in Montreal and in a nearby town, Ormstown, until I was three years old. We then went to Detroit to begin a new life (my mother had a miscarriage sometime before). We had many relatives, both in Montreal and in Detroit. We lived in Detroit until I was nine, and then returned to Montreal. I actually started school in Detroit. And in fact, there was one thing I didn't realize at the time: Of the Indians I learned about in history in school in Detroit, some were "good" and some were "bad" Indians. The same tribes I learned about later in Canada were given just the opposite reputations. [Laughter] And the difference, of course, was that the ones who were friendly to the British at the time (Iroquois, Mohawks) lived mainly in the US, and those who were friendly to the French (Algonquins, Hurons) lived in Canada.

As a child, I guess I always thought of my father in terms of sports. He was very good at a variety of sports. And my mother had the intellectual qualities. But neither of them went to high school, even. In my mother's case, she was born in Manchester, England. While she loved school, her family was simply too poor to pay the tuition for high school. In my father's case, there was no excuse, really, other than that he apparently wasn't crazy about school. His father was a small-time clothing manufacturer in the early 1900s in Montreal. Two of Dads younger brothers went to medical school at McGill and became doctors. But Dad was never interested in school. So he stopped when he finished grade school.

COHEN: What did he do for a living?

MARCUS: He originally wanted to be a cartoonist. He had talent in drawing, in fact, won a scholarship at the age of thirteen to the Beaux Arts School in Montreal. Then he got apprenticed on a newspaper—I think it was probably the *Montreal Star* or the *Montreal Gazette*. But there was some sort of an incident, somebody apparently spilled printer's ink on some plans, and Dad was blamed for it. Apparently, he didn't do it. Some anti-Semitic remarks were made, which he resented, and he left. And I think after that, he did various jobs. For a while he worked for his father. He also had odd jobs in Halifax, Nova Scotia and in Ontario, selling photograph frames, for example. Eventually, he worked as a clerk in various fruit stores, later became a manager, and eventually a clerk again. And that's really the way he spent his entire life. He wasn't a businessman at all. Just the opposite! For example, in the stores someone would make the signs to advertise the fruit. He enjoyed that (he also enjoyed sketching all his life), but I recall one instance while he was a manager. He was in the backyard of the store happily making signs while some clerk was at the cash register, apparently helping himself to some of the contents. (I think there turned out to be a cash shortage.) That was the kind of businessman Dad was! He was very good with people, a very friendly and very warm-hearted person. He was really just a natural athlete. He played baseball and hockey. He had figure skates that he could twirl around on. He ran. One time he ran in a race that Johnny Miles, an Olympic champion from Canada, was in. (Not the Olympics race!) My Dad had run in some marathon in Halifax before that—came in third. I remember so well his ice skating. And I'd watch him in a ball game, when he played baseball. Although I love sports, I never really excelled. Somehow I always sort of leaned towards books. But I loved sports, and still do.

My mother was a wonderful pianist and wonderful singer.

COHEN: Did she give lessons or anything like that?

MARCUS: No. She had received some training briefly at the Royal Conservatory of Music in Manchester, but mostly she was self-taught. I recall her mentioning that she did play an overture at some event—a debate that was taking place in a small town, Ormstown, in the Eastern

Townships of Quebec where we lived before going to the US. She gave me lessons, but after six months I didn't want to continue them. She told me I'd regret it. And, of course, I always have. She was really artistic and had so much warmth. So hers was, I'm sure, very much a frustrated life. If she had had opportunities, I'm sure she would have taken them and succeeded.

With this background, somehow from the very beginning, I always liked school. It was always my mother who would be asking me the spelling words, as part of the school lessons.

COHEN: Were you an only child?

MARCUS: Yes, I was. I guess early on I was very conscious, you know, of Dad, especially, not having gone to school. So I somehow got other idols. There were three of them—two of them were these uncles who were MDs. Another one was actually a great-uncle—an uncle of theirs and my father's—who lived in Sweden and who allegedly could speak thirteen languages. When I met his children many years later, it turned out he knew only nine. But it did turn out—this is something I learned when we went to Stockholm in '92—that he'd also written about forty books, some of which were actively used in Christian theology courses in gymnasia. I learned sometime before about his background from a 1939 Swedish "Who's Who." He had been converted and received his doctorate in theology from the University of Uppsala around 1915, and in the meanwhile had changed his name to Steen, Henrik Steen.

COHEN: Where did your family originally come from?

MARCUS: They originally came from the town of Wilkomir in what is now Lithuania, but at that time was Russia. They left there in—it must have been around 1890 or so, because my Dad was born in 1895 in New York City; and the next child was also born in New York. But all the rest of them (four more) were born in Canada. The family had moved up to Canada, where they had lots of relatives. For all I know, they had relatives in New York, too; I don't know. [Laughter]

COHEN: So you really grew up with a large family.

MARCUS: Yes, I'd say so. When we lived in Montreal, there were relatives there on my father's side and also a few on my mother's side. She had come over from England shortly after the First

World War with a sister and a brother-in-law, who had children born in Montreal. In Detroit there were also other members of my father's maternal family, cousins, uncles and aunts of his. In Montreal the Kirsches, my father's mother's family, were a well-known family.

COHEN: Were you French-speaking at all in Montreal?

MARCUS: My father could speak French, because of his work in stores. Of course, everybody who went to school in Quebec had many courses in French. You took French from grade 3 until the end of high school, grade 11 in Quebec province. When we moved back to Montreal from Detroit, I had missed a year and a half of French, and the school officials wanted to set me back a year as a result. But my mother went and spoke with them and persuaded them not to do so. I guess it probably wasn't difficult to pick up the French, because several months later I was given some award (a softball) for the child who made the most progress that term. (I even remember the teacher's name! Miss McKercher.) Perhaps it was making up the French that got me the award.

COHEN: These were state schools?

MARCUS: Yes, all were state schools. And actually, my recollections of most of the courses in school are that they were very good. In Detroit, at first we lived reasonably comfortably, even after the Depression started. And then Dad got an offer to manage a somewhat larger store—probably a little more money; and I suspect that he was a bit restless generally. (He certainly always walked quickly; anything he did, he did it quickly.) He went there, leaving a large chain store for an individually owned one and then that new store went belly-up with the Depression. For a year and a half, no new job came up. We then had to move to a much poorer neighborhood, living with one of Dad's maternal uncles who had a store there, and we probably lived over the store. I saw then that there were schools of a totally different type. I don't remember the education part, but the people were very different and, if I recall correctly, less pleasant. And then after that, we went back to Montreal, because in that Depression, without a job, our funds gone, and with no prospects, the outlook was bleak.

COHEN: Did the Depression hit Canada as hard as it did the United States?

MARCUS: I don't know. Anyway, Dad got a job when we went back to Montreal. And my mother worked, too, there, as a seamstress (she hadn't in Detroit).

My mother was the main one who no doubt pointed me towards school, and in fact she told me that when I was a baby and she used to wheel me in a carriage around McGill, she told me that I would go there. And, of course, since two of her brothers-in-law were going to McGill at the time, there was a precedent.

While I was growing up, we lived in a variety of neighborhoods. It depended on our financial state, which fluctuated. One time when Dad was managing one particular store, things were quite good and we lived in a very good neighborhood; the school was good. But the schools were good in Montreal wherever we lived, to the best of my knowledge.

COHEN: Were neighborhoods separate—Jewish, Italian?

MARCUS: Yes. Except there were some neighborhoods where there was more of a mixture. There was also at least one where Jews were excluded, the "Town of Mount Royal," also known as Model City, more or less a suburb of Montreal. That's all changed—after World War II, which also was the beginning of the end of many restrictions in universities, both in student admissions and in faculty.

In the region where I grew up, the high school that I went to was essentially 100-percent Jewish. Probably none of the teachers were Jewish, but they certainly were good teachers and good to the students. In what few memories I have, I have nothing but good memories of that school, Baron Byng, and of the grade schools. I remember, for example, one time the history teachers in the high school took those of us students who did best in their history classes on a day-long history trip, in their own cars, to the head of the Long Sault Rapids where Dollard des Ormeaux and his band fought valiantly. And, of course the teachers didn't have to do that. Baron Byng became known, I believe, as a school with many high achievers.

COHEN: It must not have been a very big school.

MARCUS: Well, to a child, the school was pretty big. [Laughter] It was really in two parts: There were girls on one side, and boys on the other.

COHEN: So the boys and girls were separated.

MARCUS: Oh, yes, they were separated in high school, but not in grade school. There were many able students there who went on to have good careers. Of course, at the time, going on to McGill was the thing to do, if you were living in Montreal.

COHEN: McGill is a Catholic school, isn't it?

MARCUS: No. It's actually nonsectarian. But there was a quota on Jews getting into McGill. It was our perception that you had to have a grade of 75% or higher for admission. A grade of 80 percent was first-class, the equivalent of an "A" here. A "B" was 65 to 80 percent and a "C" was 50 to 65 percent. So you had to have almost an "A" average in order to be admitted to McGill if you were Jewish.

COHEN: This was true only for Jewish students?

MARCUS: Yes, as far as I know. At that time in the early 1940s also, only a few faculty members may have been Jewish. Certainly none in chemistry. One example of the admissions' restrictions was a student who was denied admission to the medical school—there was presumably another type of quota for admission to medical school. There was a big hullabaloo on the campus about that; the students really protested because he was so good! He was then admitted and eventually, at graduation, he won the Governor General's Medal. A somewhat similar thing happened to a very close friend of mine, Herman Cohen, an excellent student (one year he led our entire high school in his grade) and also a relative by marriage. After receiving his BS at McGill he was required to take an MS before finally being admitted to the medical school. He later became a very successful surgeon in Montreal. Of course now, quite a few faculty members at McGill are Jewish (as well as from other ethnic groups). This prejudice was probably also true at Illinois; when somebody I know first went there, there wasn't a single Jewish faculty member in chemistry at Illinois or McGill. Eventually, Jews entered all sorts of higher echelons of the McGill administration.

So I was conscious of this difference. Schooling at McGill was fine, even though it was wartime. They didn't have enough people to teach every lab class. And I remember that there was a physics lab class that we chemists were supposed to also take, but we weren't able to do so because, presumably, there weren't enough instructors to teach it. Some of us complained to the head of chemistry, but without success. No doubt they would have offered the lab had there been the staff.

COHEN: Was there a draft in Canada?

MARCUS: There was conscription, but not for overseas service. The party in power in the government drew much of its support from the French Canadians as well as the English Canadians. The French Canadians did not support either Britain or France at that time, and so the government couldn't conscript anyone for overseas service, for fear of losing votes. There was conscription for home service, i.e., service in Canada. If you were a student—at least in the sciences—you received a deferment. You would go into one of the reserve training groups. For example, I was in an Army Reserve Group (McGill Reserve Training Battalion), and then an Air Force Reserve Group started, so I transferred into that. If you went on to work for your PhD in science, you were further deferred, because now you usually got involved (at least in chemistry) in some type of war research.

COHEN: So the war was not a big disruption?

MARCUS: Not the way it was in the US. In fact it expedited our college years: I arrived in McGill as a sophomore in September 1941 and received my BS in October 1943 after an accelerated summer program. As far as the school years at McGill, we had a lot of small courses. I believe the system is or was more like the British system. The people who taught the courses were dedicated.

COHEN: Were they mostly Canadians?

MARCUS: They were all or mostly Canadians, I think. Probably they taught subjects as well as they could. You know, when you compare the training with what people were probably getting at the very best places in the US, I think the people in graduate school in the US probably received much better training. It may well be that at the undergraduate level at that time, the

Canadian and US training may have been comparable. In fact, it could even be that with the concentration in many different courses at McGill it could even have been better, for all I know.

In graduate school in Canada, there were normally very few courses, much like the British system, and none were required. In that respect the US universities undoubtedly provided a more sophisticated level of training. Much of what I've used in my work, I've learned since I left the university; it was really on my own, because we didn't have that kind of detailed training.

COHEN: So you had an overall good education; no one teacher sticks out in your mind from this period.

MARCUS: I think that's correct. Well, one teacher in organic chemistry—it didn't affect me that much, because I didn't end up going into organic chemistry—but I remember him as being a very imaginative teacher. The students loved him—a chap by the name of Raymond Boyer. He was really imaginative. His course in second-year organic chemistry, for example, was much more challenging than a course that somebody else gave in organic chemistry for the third year. He gave us problems which involved designing of syntheses, for example. In other words, it wasn't just something you learned by rote; you had to use your imagination a bit.

I don't think there was much imagination required in answering exam questions in the courses that we had. But I do remember one graduate course in physical chemistry—a course we weren't required to take, but a number of us took—where a new young assistant professor, Bob McIntosh, asked questions that were really challenging. They were not only questions that he had explicitly covered in class. You know, so often in the classes we had, if you studied what you learned there and what was in the texts, that was enough to do well in the exams. In this case, it was a matter of putting together things that you had learned. And I remember how refreshing that was. I think it was either in first- or second-year graduate school. I remember being so surprised. It was one of the first times that we really had to think. [Laughter]

COHEN: Do you think that you did any of that yourself when you started to teach? An inspiration?

MARCUS: I don't know. I suspect I did, or at least I tried. Probably it was a mixture of things.

Designing challenging but fair exams isn't easy.

I would say that the training at McGill was probably good at the undergraduate level. The math training was probably very good. I took more math courses than any other chemist I knew of, and probably more than any chemist had taken there. It wasn't all that much, but it was more. [Laughter] But I really liked math. And I remember one course where I think we had to do every other problem. I don't remember if it was a course in high school or college. In any event I liked it so much that I did *every* problem.

COHEN: So you didn't think of majoring in mathematics?

MARCUS: Well, I wondered about it. At that time at McGill, I don't think you could major in math, but you could major in math and physics—it was a combination. And in the physics I felt less secure. In high school, I remember, we had a lot about statics—levers, pulleys, tension, and static friction, and somehow I had difficulty in grasping it. I was missing a key thing. Once I got to second year in college—my first year of college was taken as twelfth grade in high school to save money—I took a physics course, electricity and magnetism, and actually did well in it. In the midterm I still remember that I was third out of a class of 150. But then towards the end of the term I didn't study for the whole last part, so that ended up being my only "B" while I was at McGill, the rest being all "As". But it may have been partly not having felt comfortable with pulleys and the like. That was probably one reason I decided not to major in physics.

Another reason may have been the advice from my advisor when I went to McGill. Upon entering as an undergraduate, an advisor would ask you what you wanted to major in. Mine said that if you went into math and physics, you'd have less chance, being Jewish, of getting a job. Well, that certainly didn't discourage a number of people I know who went into math and physics there. Perhaps they had a different advisor! One of those students, in fact, later went to the Courant Institute [The Courant Institute of Mathematical Sciences, at New York University]. He became the first winner—he was a co-winner—of the Crafoord Prize that Gerry Wasserburg later won—the equivalent of the Nobel Prize in chemistry.

COHEN: What was his name?

MARCUS: Louis Nirenberg. I remember him well because I had taken two courses that he was in. And in one of them, anyway—I think it was the complex variables class, I'm not sure—he came in first, and I came in second in that class. But he was so far ahead in imagination of the rest of us that he'd even bring his own proofs of things and argue them with the professor. I'm sure I must have felt at the time that it was a good thing I went into chemistry instead. [Laughter] In other words, I knew enough math so that I could handle it as needed; but probably my strengths were in a combination of things, of which the math was just one part. So things worked out for the best, I imagine.

When I was an undergraduate and a graduate student, I used to do a lot of cycling. In fact, there was one trip that was supposed to be a thousand-mile round trip. A fellow undergraduate chemist, Lazar Novak, and I ended up doing 800 miles of it, for various reasons—75 miles turned out to be a gravel road, so we took a train instead, and for the final 125 miles we got a lift in a truck, either because of a time constraint or because Lazar was sideswiped by a car the night before. Another cycling trip, the following year, was for 300 miles, going over the mountains from Montreal and down to the coast of Maine. This time Lazar and I were joined by two others. I enjoyed cycling, and I belonged to some club that frequently went out on weekends cycling.

COHEN: Were you already a skier there?

MARCUS: Well, I had skis that I had bought at a pawnshop for two dollars when I was in high school. [Laughter] And some inexpensive new boots, which cost four dollars. So I did some skiing then. There was a small mountain in Montreal itself, Mount Royal, and lots of snow. And I skated—I lost a tooth—or, at least a tooth was killed—in hockey, when I was in grade school. But I didn't do much skiing. It wasn't until much later on, when our children were of skiing age, that we really took it up, and then I took it up seriously. But the cycling was something I enjoyed very much. Badminton and table tennis were other sports that I particularly enjoyed in high school. They were the major indoor sports there.

Then, three of us in graduate school at McGill made plans that when we received our PhDs we'd take a year off and go around the world. Not cycling. One of the chaps I graduated with, Johnny Devins, was the grandson of an old sea captain from Mahone Bay in Nova Scotia. So we were going to make plans to buy a boat. Another chap, Jim MacKenzie, had been a navigator in the Air Force and was going to be the navigator on this trip. We read a lot about the route we would take. But after two of us received our PhDs (the navigator was a few years behind us because of his time in the Air Force) somehow the idea had fallen through. But we did spend a lot of time making plans. [Laughter]

COHEN: So it sounds like you had a very nice, all-around, college experience.

MARCUS: Yes, I think so. Well, also, during the summer—and I think it may have been the last year of high school, and for a number of years afterwards—I used to go and work in a hotel in Ste. Agathe in the Laurentians. My aunt, one of my father's sisters, had married somebody who was one of four co-owners of a resort hotel there. So I went up every summer and worked as a busboy. That was great, getting away from the city. Just lovely countryside; it was on a long lake, and a beautiful place to be. I think there was a fair amount of interaction between the guests and the waiters, and even with a busboy. I remember one occasion when the waiters were trying to decide whether to go on strike. I had to go out of the room, because, since I was the nephew of one of the owners, they probably didn't feel they could trust me. [Laughter] Anyway, that period was very pleasant.

It was around then that I first learned about tennis. I remember one summer when I was working in a lab as an undergraduate—we could work in a lab as paid employees—a group of two faculty chemists (Boyer and McIntosh, whom I mentioned earlier) and some chemistry graduate students met and played tennis for an hour or an hour and a half every morning. I was part of that group. I enjoyed that. But I never really excelled at sports the way my father did.

I really had a lot of love for and from my parents. There was never any doubt in my mind, or in any of the relatives', about my place in our world. Even now, some of my father's brothers or sisters still alive will talk about some of the things I used to say when I was a child. [Note: Two of the three still alive during this interview have since died.]

COHEN: You got your degree in 1946.

MARCUS: Yes, my PhD. And then went to the National Research Council on a postdoctoral appointment.

COHEN: Let me back up just a moment. Did you have to choose a PhD thesis at McGill?

MARCUS: You had to choose a thesis advisor. That wasn't particularly difficult there, because at that time when I began graduate studies in physical chemistry, there was only one faculty member in physical chemistry who had a large research program. [Laughter] If you went into organic chemistry, I think there were two faculty members with research programs. Where there was a lot of research—where there was maybe more variety—was in the Pulp and Paper Institute. It had a very good reputation. But if you were interested in straight physical chemistry at that time, there was little doubt with whom you would work.

One faculty member was the physical chemist Otto Maass, but he was also Director of Chemical Warfare at that time and lived in Ottawa. He was a well-known Canadian chemist, who received his PhD at Harvard working with the first US Nobel laureate, T. W. Richards. I remember one incident in connection with him in graduate school. A friend, Walter Trost, and I sometimes played chess, but we didn't have a chess set at the lab. So we made a set out of corks. We had the board, and we had the chess pieces set up. I guess we must have left the set there overnight. Maass, who was normally in Ottawa and with whom Walter was studying for his PhD, must have seen it. The next day we found that Maass's ivory chess set had replaced ours.

COHEN: So your advisor was…?

MARCUS: Carl Winkler, the same one that Sam Epstein had. [Since the 1950s Sam has been a geology professor at Caltech.] In fact, Sam and I overlapped for one year in graduate school. There were only two universities at the time in Canada that gave doctorates in chemistry. Sam had received a master's degree at the University of Manitoba, Winnipeg. Then if you wanted to go on to graduate school in Canada, you would go either to Toronto or McGill, or a very few might go to the US (the inorganic chemist and Nobel laureate, Henry Taube, was one). Sam chose McGill. Actually, it turned out—Diane [Epstein] reminded me—that I had known her when I was still in high school. She's from Montreal. There was a group of us—I think it was around 10th grade, she was a year behind me—who would hang around after school. I guess I was friendly with one of the girls who happened to be Diane's lab partner. This I don't remember at all, but Diane says that I used to help this girl with her chemistry. [Laughter] And this girl wouldn't share the lab write-up with Diane. [Laughter]

Sam finished McGill in 1944, because he started a bit earlier, and I finished in 1946. But that gave us one year of overlap—our lab benches were just across the aisle from each other.

Graduate school was quite a change from undergraduate, for some of us anyway. In undergraduate school, we worked pretty hard; in fact, we probably worked pretty hard all the time. But in graduate school, although we worked, it was quite a different and new cultural environment for some of us. Many of the graduate students came from other parts of Canada, so there was a marked change in that way. In particular, the boys who came from the West were probably a hard-drinking group. Not Sam but, you see, he wasn't native to that part of Canada. So being exposed to that new group was different. A lot of time was spent going out and drinking beer and bowling. It was a different life. In fact, all my group of friends, in effect, had changed. When I was an undergraduate they were in various areas and not just in chemistry. But in graduate school, essentially, it became almost entirely the chemistry graduate students—except for one friend I had known in high school very well, Herman Cohen, who was a medical student and whom I mentioned earlier.

COHEN: Then you finished up and did your thesis.

MARCUS: That was very quick. The work really wasn't very challenging. I think I probably did the best I could with it, but the research wasn't very sophisticated. It was part of the RDX Program, and a very small part. Sam worked on another part of the RDX Program.

COHEN: RDX was?

MARCUS: That's "Research Department Unknown." That was the explosive that was used in the "blockbusters" that were dropped on Germany during the Second World War, and it is still used today. In fact, one of the processes that was used to manufacture RDX got its start in the early stages of its development at McGill, if I recall correctly. But there were various things to do research on in connection with RDX chemistry. Recovery of one of the active reagents, nitric acid, used in the production of RDX, for example, was one of the research projects in Winkler's laboratory. So part of the time I worked on the properties of nitric acid. Another part of the time, I worked on some gas-mask project, the use of silica-carbon mixtures instead of carbon, but I could never get an improvement using them. But most of the time was spent on a small area of the RDX chemistry—working on chemical reaction rates. These projects were all really war-related, but relatively unsophisticated and really on the periphery. As research problems, they were very far from working at research frontiers, at the cutting edge.

Begin Tape 1, Side 2

MARCUS: The best thing that we may have experienced there was the enthusiasm for research. The research director, Carl Winkler, really liked his students—used to go out to lunch with us, and we really liked him. He was a Canadian from Manitoba. He received his PhD from McGill, working with Maass, and then went to Oxford for a D. Phil., as a Rhodes Scholar. He worked there for somebody who later was awarded the Nobel Prize in chemistry—Cyril (later Sir Cyril) Hinshelwood. His research, and that of Hinshelwood, was in what's called the field of chemical kinetics—that is, chemical reaction rates. Most of us, in one way or another, continued in that general area. Most of my research since has been in that field, and indeed that type of research became one of Canada's strengths in chemistry.

COHEN: You knew there were quotas for getting into the university. But once you were there, did you get any sense of any anti-Semitism?

MARCUS: I didn't really see explicit, overt signs of anti-Semitism. As an undergraduate I always felt, though, that I was outside the general university community. In other words, the Jewish people formed one sort of undergraduate community there, and the majority group formed another. Of course, there was some interaction.

COHEN: Was this socially?

MARCUS: Yes, socially. Of course, in classes there was all free interaction. But the way you spent your free time outside of classes was, at least, for many of us, apart. I was very conscious of that. Nevertheless, a close friend as an undergraduate was Ronnie Glegg, a fellow chemist and a mulatto from Jamaica. Many excellent students came from the West Indies to study at McGill during the war and were well accepted. Graduate school—with the boys from Western Canada, and a much smaller number from the Atlantic provinces—made the social experience and extent of interaction very different for me.

Shortly before I completed my PhD Basil Darwent, a former PhD student of Winkler's with whom I had overlapped, came down from the National Research Council of Canada to look for people to go to Ottawa in a postdoctoral capacity. Edgar Steacie, head of its chemistry department, was forming a new postwar postdoctoral program to bring in a steady stream of new young faces. This was 1946. Steacie, who had received his PhD at McGill and had been an associate professor at McGill, and had written an undergraduate physical chemistry text with Otto Maass, left McGill around the beginning of the War and had gone up to Ottawa. He later converted the Research Council from a civil service effort to an absolutely first-class scientific institution, with, I understand, the support of Maass. Just a tremendous development! It made all the difference to Canadian science.

COHEN: Now how about all the stuff going on at Chalk River?

MARCUS: The atomic energy work was going on at Chalk River, and was also going on at the University of Montreal. I wasn't involved with any of that. But a number of people were. Sam was, and so was Charlie Barnes (the Caltech physicist). And a number of distinguished people would come there from the US, or abroad no doubt, from time to time. But for those of us who didn't get into that type of research, it was almost as though it didn't exist.

So this chap from the National Research Council came down to McGill to find out who might be interested in going up there to do some postdoctoral work, and several of us did. That was my first contact with cutting-edge research. There, Steacie was studying photochemical reactions in the gas phase; he was a leading contributor to the field nationally and internationally. So it was a totally different ball game. Winkler loved people; I'm sure Steacie liked people, too, but he wasn't as warm a person. Winkler was interested in so many research areas that perhaps he never really focused on a sufficiently narrow area in which he could do cutting-edge research. Eventually he did spend a lot of time in a particular area—"active nitrogen"—this was long after I'd left—but the systems were probably rather messy. Winkler became a Fellow of the Royal Society of Canada and a Vice-Principal of McGill. Steacie was a major international figure. For example, he was a Fellow of the Royal Society of London, and later became a Foreign Associate of the National Academy of Sciences and of other academies, and the President of the NRC. He'd go to international meetings. That's where I really saw first-class research. The other work was published, but it was not really that demanding. Actually, at first, when I went up to Ottawa, I worked jointly for Steacie and Darwent. I hadn't realized when Darwent came down that perhaps it was, in part, to find people to work jointly with him and Steacie. I wasn't pleased about doing that, because I thought of Darwent as a fellow graduate student, and I'd gone there assuming that I was going to work only with Steacie. Rightly or wrongly, I didn't really respect Darwent's abilities. He meant well, I'm sure, but he would come around and suggest things for the research that, it seemed to me, were obvious. [Laughter] Anyway, I made some sort of a fuss to Darwent, and Steacie came around and reproved me. I wrote two papers when I was there. The first one was written with them jointly, and thereafter it was just with Steacie. The Research Council was a fine place to work. There was also some continuity with life at McGill because some of those westerners and chaps from the Atlantic provinces were there, too. So there was some continuity of drinking, playing touch football, bowling, and all that. But at Ottawa I started to play more tennis. And as an example of remnants of segregated communities, there was a Jewish tennis club and a non-Jewish one. So I joined the former. The better players, of which I was not one, were occasionally invited to come over to the non-Jewish tennis club to play. [Laughter]

COHEN: Were you still living at home?

MARCUS: No, this was in Ottawa. That was a big change, although it didn't seem like that to me at the time. In each case, working at the lab often late at night, or going out with the boys late at night, it was similar.

COHEN: And it sounds like your parents were not demanding.

MARCUS: No, they weren't. I'm not sure I spent that much time at home. On arriving in Ottawa I tried but couldn't get an apartment; there weren't any apartments at that time, because the War had just finished and had created a shortage. Instead I obtained a room nearby.

While I was at the Research Council I enjoyed the fact that you had to construct a glass apparatus. I always liked working with my hands and enjoyed building things. Here at the NRC there was a real chance to build things. But I tended to be a little bit impetuous in the research—quickly doing something, and the glass apparatus might break. The research equipment was a high-vacuum apparatus, so there's mercury. Mercury would start bouncing around and cause more breakages. As a result, I'm sure that I didn't spend my time at the NRC the most efficiently.

COHEN: But you did get to have respect for experiments.

MARCUS: Oh, yes, absolutely. I'm sure that the time I spent with experiments, both as a graduate student and as a postdoctoral, flavored my subsequent research in theoretical chemistry. As an experimentalist, I'm sure I had limitations. For example, I remember just after the second year of college, I was able to get a summer job in Boyer's lab there, a good organic chemistry lab. The task I was assigned was to synthesize a particular compound. So I would go through the various steps, and then, right near the very end, I wanted to hurry it up. So instead of removing the solvent by putting the vessel on a steam bath, I used an electric burner. But then I would tend to be forgetful, and a number of times in that final step I'd end up unintentionally destroying the compound. [Laughter]

I'd been up in Ottawa for about a year or so when Walter Trost—the chap I had played chess with at McGill, who had also gone up there—and I decided that we would learn something about theory. Even though I had had some courses in theory at McGill, they were not taught by a theoretician. In fact, there were no theoretical chemists in Canada at that time. So we probably didn't get the real flavor of the beauty of it. But now we were at the National Research Council, and in contact with front-line research, and we began to realize that there was something called theory—even though we had had some theory in our classes, but somehow it hadn't gelled. The two of us formed a two-man seminar. We would Xerox certain theoretical articles, some of which I still have. They weren't "Xeroxes" at the time, of course; they were photostats. Some of them were related to our research, some of them were just for background. We'd go over some of these papers. This was my first real exposure to theory—seeing what theorists did and then trying to apply it to some particular data I had obtained in Ottawa. It wasn't very creative work, but it was a way of getting familiar with theory.

I couldn't stay at the Research Council indefinitely, since the postdoc appointments were only for two or so years. For the very first time, I had to make a conscious career decision: What to do? Because before that, just going from step to step, there was really no thinking involved on my part.

COHEN: How old were you then?

MARCUS: When I received my PhD I was twenty-two, and turned twenty-three a month later. In order to expedite the education at McGill because of the War, undergraduates who were in chemistry took the fourth year in the summer. And three years was the standard period for receiving a PhD there. So I was reasonably young when I received my PhD. In 1948 when I was thinking about leaving the Research Council to go elsewhere, I was twenty-five.

COHEN: So for the first time you had to think about what you wanted to do.

MARCUS: Absolutely. There I made what could be considered to be a radical break. Normally, if you're working in the lab doing experiments, you continue doing experiments in your subsequent research. But I didn't feel really good about the experiments. For one thing there was the breakage of the apparatus that I mentioned earlier. For another, and no doubt much more importantly, I really liked mathematics, and I hadn't had any demanding chance to apply it. I remember, for example, when I was at McGill, the course in complex variables. The students majoring in math/physics were taking it at the same time. But they'd also be taking a course in physics—potential theory—where they could apply it. I couldn't take all those other courses, because of the requirement to take specified chemistry courses. I had a sense of frustration, not being able to apply it. I was very conscious of not applying something that I really enjoyed. As a result of reading these theory articles, and actually making some simple calculations using one of the theories, I decided that I'd try to do theory. I'd apply to various people. I selected what I thought were six of the best theoretical chemists in the United States.

COHEN: At this time, it was clear that if you wanted to do this, you had to go to the United States?

MARCUS: Absolutely, because there were no theoretical chemists in Canada at that time. Either around that time, or maybe before I went up to the Research Council, there were some fellowships available for study in France. And indeed, one of my friends, who was one year ahead of me at McGill, ended up taking one of those. I could have applied for one, instead of applying to NRC, but somehow I felt that going over there wouldn't be serious study; it would be just having a good time, and in graduate school I had had much of that. So I think I was wise not to try for a postdoctoral fellowship in France, although I'm sure that culturally it would have been great. It sounded glamorous, too, going to Europe.

I applied to six of the top theoreticians in chemistry, and only one of them, Oscar Rice, held out a real possibility. A very good chap at Harvard, E. Bright Wilson, wanted somebody who would direct his graduate students while he was, I guess, taking a sabbatical. Steacie didn't think that that was an appropriate responsibility for me, and in fact, it undoubtedly wouldn't have been appropriate. Another of the people I had contacted, Joe Hirschfelder, suggested that I apply instead to the Institute for Advanced Study at Princeton, to learn theory. He had originally been at Princeton, but I didn't think that possibility was realistic. The three others that I had applied to were Henry Eyring and probably Jack Kirkwood and Robert Mulliken or Lars Onsager.

I'm not sure that every one of the six replied, but Rice, who did so, said that if he could get an Office of Naval Research grant he would take me on. In a way, it was perhaps too much to expect that I would receive a postdoctoral fellowship in theoretical work with no background in theory, with at the time only one publication—publication of the war work had been withheld, not that it would have made any difference anyway. [Laughter] It wasn't that high-quality research. It was probably too much to expect that somebody in theory would take on somebody with that background.

COHEN: Well, you probably had a very good recommendation from Steacie.

MARCUS: Well, I don't know. Steacie would probably have seen me as having broken the apparatus many times, and not writing as many papers as some of the other people there. I remember there was one chap who wrote six papers while he was there. His subsequent research career was not necessarily the most distinguished, though he became a professor at one of the British universities. I think probably Steacie thought I was reasonably bright. I remember one experience: There was a certain standard method of treating the experimental data in the area (free radical reactions) that Steacie was a specialist in. The method involved various assumptions, and resulted in a cumbersome expression. It was used also by H. S. Taylor of Princeton University, another leader in the photochemistry/free radical reaction field. I showed Steacie how by very simple algebra you could extract something from the experimental data that was more what you wanted, and that it wasn't cumbersome at all; it was very simple. I remember he was really enthusiastic about that, probably more so than about anything else I did in the two and a half years that I was there. [Laughter] I was so surprised. It was something that was really trivial, but it treated the data a different way. I wrote about this particular event in a symposium—in a Faraday Discussion in Nottingham that I spoke at—some thirty-five or so years later.

Anyway, Oscar Rice from the University of North Carolina said that he would take me on if he could get a grant. He applied for the Office of Naval Research grant, and he received it! I went down to North Carolina at the end of January, 1949.

COHEN: That must have been quite a difference, going down to North Carolina from Ottawa.

MARCUS: Oh, it was a tremendous cultural shock, and a very pleasant one, actually. That was the South. When I went there, I was in a train sleeper; I opened up the window, looked out in the morning, and saw all this red clay. I'd never seen red clay before. This train went as far as Raleigh; and from Raleigh, I was supposed to take a bus to go to Chapel Hill about 30 miles away. I went over to a small bus station, and I still remember the music that they were playing, "Sweet Georgia Brown," with a few local people hanging around. It was a type of store/bus station.

COHEN: You must have seen black people in quantity.

MARCUS: Yes. There were very few blacks in Montreal. It was such a different feeling, being in a relatively small southern community. The community of Chapel Hill itself was largely white. Chapel Hill was like a different world, and it *was* a different world.

Then I went and took the bus to Chapel Hill, met Rice, and first had to arrange for a place to stay. Once again, apartments were in such short supply, that I couldn't get one. So I rented a room instead. The room wasn't going to be ready for two weeks, because of remodeling. In the interim I stayed with somebody who happened to be a secretary in the sociology department. She knew Laura—Laura was a graduate student there in sociology and anthropology. It happened that Laura, while I was there, had Sunday dinner with her.

COHEN: So it was in the first two weeks you were there that you met your Laura.

MARCUS: In the first two weeks. [Laughter] Shortly after we met, I invited her to come over to this house where I was temporarily staying. I had brought some Drambuie with me from Montreal. [Laughter] I had also brought some records, and I invited her to come one evening and have some Drambuie and listen to the records. Anyway, in came my temporary landlady, a perky little white-haired lady, Miss Mabel Mallet. It had never occurred to me to mention this invitation to Miss Mabel. I remember she looked shocked; she didn't say anything, but she was sort of shocked. [Laughter] Here I'd invited somebody she knew well, and we were sitting cozily in her living room.

Laura's background was very different from mine. Her family has lived in eastern North Carolina for over two hundred years. Nevertheless, there was an immediate and mutual attraction.

Carolina turned out to be just a great place for what I wanted to do.

COHEN: How long were you actually there?

MARCUS: I was there from the beginning of February '49 until August of '51, when I went to my first teaching job. It was just wonderful. Oscar Rice was absolutely the right person to work with, for what my needs were, as it turned out.

COHEN: You were just doing theory?

MARCUS: Just doing theory. No more glass. In the first three months there, I had to learn something about theory. I sat in on a theoretical physics course that Nathan Rosen taught—he later emigrated to Israel. He was a very well-known theoretical physicist at the time. I interacted with another theoretical physicist there, Wayne Bowers. And I interacted a tremendous amount with Rice. In order to help me learn about theory, he set up a weekly get-together—the two of us would meet in his office, he sat in his favorite lounge chair, and we would talk. I would describe to him some theoretical paper that I had read, and he would point out certain assumptions that might be present. It was really very good for me. I just read and read and read. A lot of the theoretical papers were in German. Although I'd had a year of German in twelfth grade, I had to learn a lot more German—and may, in fact, have learned as much German as theoretical chemistry during that time! I still have some notes of some of the articles I translated, because without a real theoretical background it was difficult for me to understand some of the theoretical material in English, not merely in German.

After three months of that reading, Rice very gently suggested that it might be good to work on a specific research problem.

COHEN: Do something? [Laughter]

MARCUS: Yes, that's right. After all, he was paying my salary. But he never raised that issue. He was very gentle.

COHEN: Was he a Southerner?

MARCUS: No. He actually received his degrees from Berkeley. In fact, for a short while he was a National Research Council Fellow at Caltech in the late 1920s. Part of the time he was a National Research Fellow in Europe. He was, in fact, an early theoretical American chemist. There weren't many, and he was a pioneer in what you might call theories of chemical dynamics. Some of that theoretical work was related to the theory of the experimental studies I'd done, particularly in Ottawa—rates of chemical reactions. It was really a natural.

COHEN: So it probably wasn't at all an accident that he asked you to come.

MARCUS: No. In fact, you see, I had had this experimental background at what surely was one of the best laboratories in North America, Steacie's lab.

So he suggested my working on some concrete problem. He said he'd made some calculations on unimolecular reactions. He was famous for a number of pieces of research, one of which was the theory of unimolecular reactions, which he developed in the 1920s; it was known as RRK theory (Rice, Ramsperger and Kassel). He suggested I work on that. He had had a few ideas. I had studied his papers—those of the 1920s and those of later on. Then I started to look into the theory, and looked into another type of theory that was for more general reactions. It was developed in the 1930s and was called transition-state theory. I realized that the two hadn't been brought together, and that some ideas expressed in some of the formulations of the transition state theory weren't quite appropriate. What I ended up doing, without being very conscious of it, was putting little bits and pieces of the various ideas and pieces of work together.

Really, that's all it was. After three months on this research—some six months after arriving in Carolina, I had done the essential part of what later became known as RRKM theory. It's still the theory used by most people in that field today, and books have been written about it. Of course, during the next year or two, I refined what I did, but the essence was there.

We had to send in reports every three months to the ONR [Office of Naval Research]. The sixth-month report was due. Meanwhile, by that time, Laura and I had decided we would get married. The night before the wedding, I was still working on the report. And the report was really this RRKM theory. Rice needed the report to send to ONR before we left on our honeymoon. So Laura pitched in and in fact, into the wee hours of the morning, she was typing this report. [Laughter] That was the wee hours of our wedding day! Things went well, from that point of view, at least for me!

I felt, also, that it was a breath of fresh air, that whole new experience of coming to Carolina. In Montreal, I had been so conscious of these two societies—or really three societies, French, English, and Jewish. But, in Carolina, I guess there were so few Jewish people that I wasn't conscious of that at all. For me it was like a breath of fresh air. Also I was enjoying the work so much. This type of work was really what I wanted to do. I was just so happy doing that type of research and study. Now, more than forty years later, I still feel the same way about it. I mean, I'd really found what I wanted to do. You know I had been sort of internally dissatisfied, I'm sure, doing those experiments.

COHEN: So here you were in this new climate, everything was new.

MARCUS: Yes. I should add one thing. From time to time, Rice did both theory <u>and</u> experiment. It was my impression that the equipment for the experiments in gas phase reactions was down in quality from what we had had at the Research Council. He had some students who were doing experiments. From time to time, during my two and a half years' stay there, he would ask me about doing some experiments. Here I'd come from a top-quality lab, and he had these experiments he wanted to do with this other equipment. He never pushed me for it, but he asked me about doing it. I never did. I guess I knew what I wanted to do and knew that I hadn't come down there to work on experiments.

And then the time came to look for a job.

COHEN: Meanwhile, you published papers?

MARCUS: Well, actually shortly after that first six months, a well-known physical chemist specializing in chemical reaction rates, Milton Burton, came by. He was at the University of Notre Dame and was a good friend of Oscar Rice. He came to Chapel Hill, and I guess Oscar told him what I was doing. Burton was organizing a symposium at the University of Minnesota for 1950—it was called "Anomalies in Reaction Kinetics." He suggested to Rice that Rice send me up to give a paper on this work. Rice did. That was the very first symposium that I attended—my first scientific meeting. The whole time I was at the Research Council in Ottawa, I hadn't gone to any. At that meeting—this was a paper that Rice and I published one year later, as part of the symposium issue—it was the first time I presented this RRKM theory. (At my second scientific meeting, if I recall correctly, in December 1956, I presented the electron transfer theory.)

It's hard to remember details of everything as you look back, but certain things do stand out. I guess that at the time, in the 1940s and 1950s, I was probably quite brash, and no doubt I didn't know my place, because I remember two well-known leading researchers, Milton Burton and Joseph Hirschfelder, were having a discussion or mild argument at this 1950 symposium. I thought I saw where each of them was not seeing the other's point of view. So I got up to try to explain to them what they meant. [Laughter] Later both of them became good friends of mine—quite a bit later. But at the time one of them stated they could settle it between themselves. [Laughter] I think this probably was a characteristic, even going way back. Like that time I mentioned earlier at McGill, when they wouldn't let us take a physics lab: I went in with a group of students to complain. The chap who was head of the chemistry department, Hatcher, let us know in no uncertain terms that if we didn't like it. [Laughter] I'm sure I wasn't a shrinking violet, though no doubt I was intrinsically shy.

RULDOPH MARCUS

SESSION 2

December 7, 1993

Begin Tape 2, Side 1

COHEN: You said you wanted to say something more about athletics.

MARCUS: Oh, yes, that's right. This was in high school. And one thing I forgot, in the high school I went to, there was no tennis. But there was lots of badminton and lots of Ping-Pong. So we played those two things incessantly—during school recess, and after school. So that was my athletics, really.

COHEN: I think we had gotten as far as the end of your stay in North Carolina.

MARCUS: Yes. Then the question came to look for a job. At North Carolina, I was just a postdoc, although I had taught at least one of Rice's courses when he was ill from colitis—it may have been in my second year there that I taught. That was later to serve me in good stead.

COHEN: What course was this?

MARCUS: It may have been a course based on Rice's book. He had a very forward-looking book at the time, essentially on theory—on various theoretical aspects of the interaction of molecules and ions. I've forgotten exactly whether it was that subject, but it probably was.

Anyway, now I had to look for a job. I wrote 35 letters.

COHEN: What year was this?

MARCUS: That was probably early 1951 or late 1950. As a precaution, I did talk with a couple of people in case I couldn't get a faculty position. I spoke with two famous people, one of which was Fritz London, who was a highly esteemed scientist, a professor at Duke University, which was nearby. That was one possibility.

COHEN: As part of the faculty there?

MARCUS: No. If I didn't get an academic job, at the least I would go on to another postdoc with somebody, though I didn't want to do that. Then somehow I went up to the University of Chicago—I don't remember just how, probably when I went to that 1950 symposium—and spoke with Robert Mulliken, who later won a Nobel Prize in chemistry. I spoke with him, and there may have been a possibility for a postdoc with him. But I didn't pursue either of these possibilities, because ultimately a job did come.

COHEN: How did you pick the 35?

MARCUS: I looked through lists of universities and picked 35 places I'd be willing to go to. Some of them were very good. I didn't apply to places like Harvard or Caltech—those were too good, you see, from my point of view. [Laughter] But I applied to places like Cornell, the University of Kansas, and Michigan State.

COHEN: You didn't care about location.

MARCUS: No. I was interested in getting a job. The quality of the department would have meant more to me than the location. No, location wasn't a factor at all. I didn't get any positive response. As I've often said in my talks to general groups nowadays, I didn't get 35 "no" answers because not everybody replied. [Laughter]

One of the people that did mention something—Frank Long at Cornell—said that they had an instructorship available, but that it was obviously too low for someone with my qualifications, so they wouldn't offer it to me. And here I was just dying! If they'd offered it to me, I would have, of course, accepted it. But they didn't, and so I didn't tell them that I would have taken it. [Laughter] So I didn't have a job lined up.

I then went with a friend, Hans Jaffé, to an ACS [American Chemical Society] meeting in Cleveland. We drove there. He loved to drive. He was actually a graduate student of Oscar Rice's. We arrived at the ACS meeting, and I registered for interviews. And I obtained one interview, I think—at really a third-rate place. I spoke with a couple of their people, but I didn't want to go there. I think if I'd wanted, I probably could have received an offer. I would have taken a postdoc, instead. [Laughter]

Then, a student who had been in the class that I taught at North Carolina because Rice was ill, happened at the meeting to run into the dean of the school he went to before he came to Carolina. The school was the Polytechnic Institute of Brooklyn. The dean told this student (Seymour Yolles) that he was there to look for faculty members; they were going to do some hiring. In fact, that year they hired three people. The dean asked him if he knew anybody. Yolles mentioned my name. An interview was set up. The dean asked me very few questions. The only one I remember was "Do you like young people?" Well, I was still in my twenties at the time, and I was surprised at the question, but I assured him I did. [Laughter] Anyway, he invited me to visit the Institute in Brooklyn.

So I went up for an interview. I remember that at the start of the interview, the dean—an awfully nice person, Raymond Kirk—in fact, we named our youngest son after him—mentioned that the salary would be somewhere between $4800 and $5200 a year. I went around and talked with the people. When he again mentioned the salary it was just $4800, and I didn't try to negotiate. [Laughter] This was a nine-month salary. I didn't balk at that at all. I really wanted the job. So I don't remember if they offered me the job then, or if I went back and waited until I heard from them. In any event, I got a job at Brooklyn Poly.

COHEN: This would be as an assistant professor?

MARCUS: As an assistant professor. And that was just great. Poly was not one of the 35 schools. I'd never heard of Poly.

COHEN: But Poly Tech must have been a terribly interesting place in those years.

MARCUS: It was; it really was. It turned out that—for me, anyway, and probably for a lot of others—it was a great place to be. It was very exciting. They had many excellent people, some of whom had come from Europe. For example, Herman Mark was there. He was a world-famous polymer chemist, and he had developed a very strong polymer program. Everybody who was active in polymers used to come through there. Some who were there in one capacity or another went on to excellent places. For example, Paul Doty went on to Harvard, and Bruno Zimm went to General Electric, and then eventually to UC San Diego. So there were all sorts of capable scientists there. It was an exciting place to be—in part because of the polymer group, but in other fields, too. Electrical engineering was very strong there, one of the strongest in the nation. In physics, they had Paul Ewald, who had been a world-renowned crystallographer and was an awfully nice person. So they had some good people in various fields.

In chemistry, we were a bunch of eager, hard workers. Poly didn't have a campus, and it wasn't a prestigious place. My office was in an old—I'm not sure whether it was World War II or World War I—Quonset hut. My lab and office, for a good part of the time I was there, was in that Quonset hut, which was attached to the main building.

However, the principal fact is that there was considerable enthusiasm on the part of the faculty and the students, and it had a very congenial atmosphere. Many of the students, especially in graduate school, worked in some job, the chemists probably as chemists, during the day and attended their courses or did their research at night. Their companies frequently sponsored them. Many of them received their PhDs that way. There were quite a few day students also. Poly filled a very important role.

I don't recall feeling any strong pressure or hype at Poly. In fact, for the first few years I wondered what kind of research to do in theoretical chemistry. Of course, my main training had been in experiment. Although I had had only two years or so of theory, it was theory I loved the most. But I really didn't know what to do in theory. I'd done this work on the theory of unimolecular reactions, which later became well known. But I didn't want to continue in that direction, because there were no experimental data available at that time, and somehow I guess I was mature enough at the time to realize that one shouldn't just play mathematical games; one should do theories in the real world.

But I didn't know what to do in theoretical work. In the case of experiments, there was no question—there were various experiments I was interested in doing. I wrote a research proposal, and about a year later I received a grant from the Office of Naval Research, which was really my mainstay until the NSF [National Science Foundation] began its support of my work a few years later. But what a thrill it was to receive the news of that first ONR grant!

COHEN: What sort of experiments were you doing?

MARCUS: They were largely related to the kinds of experiments I did when I was a postdoctoral at the National Research Council. That is, working with gases, high-vacuum equipment, rates of various chemical and photochemical reactions, and trying to study certain individual reactions that way. As I mentioned earlier, my training both as a graduate student, as a first postdoctoral, and then in theory, in the second postdoctoral, was all in chemical reaction rates. Much of my work since, but not all, has been in that general area.

Two students joined my group on my arrival, and I certainly had enough students while I was there. They were doing experimental work. So that part of the research was rolling along.

Then I happened to see some paper, and figured that I could obtain the result in a much easier way. So I wrote a theoretical paper. But it was on an isolated topic. During the first summer that I was at Poly—that would be the summer of ’52—I hadn’t yet received the contract, so I did some research for one of the other professors there, a chap by the name of Harry Gregor, trying to interpret some of his experimental results on ion exchange resins. We wrote a couple of papers based on that research. I wouldn’t call that high-quality work, but at least it covered the summer employment. But I said to myself, “Never again.” [Laughter]

COHEN: And you were looking to live in Brooklyn at that time?

MARCUS: We were living in the center of Brooklyn, known as Flatbush. Later we found an apartment in Brooklyn Heights, within walking distance of Poly and close to the East River. There was a wonderful promenade there, overlooking the river.

After a couple of years, I found another problem to work on and did some work on that. But these were little things.

COHEN: Did you have a small teaching load?

MARCUS: Oh, no, it wasn’t a small teaching load. I taught two lecture courses, and I probably had responsibility for a lab. In fact, during the first year that I was there, which was 1951-52, one of my lecture classes was from 8:00 to 10:00 on Friday night. But that was the last year that they had 8:00 to 10:00 Friday night classes. Frequently I would teach on Friday from 6:00 to 8:00, and often 8:00 to 10:00 on other nights. Graduate courses were always at night. Towards the end of my stay, they started having some of the graduate classes from 4:00 to 6:00. But the courses were interesting to teach. They were largely graduate courses and I was learning something. There were some good students there. Poly has had some very good students who later did well in science.

One of the classes I taught was a class in statistical mechanics. It must have been around 1953 that one of the students in that class, Abe Kotliar, asked me a question about how a certain topic that we were treating might be applied to a particular problem that he was doing experiments on. His research was in polymers with Herbert Morawetz, who was hired at Poly the same year I was. Actually, the research was in what's known as polyelectrolytes. They have ionic charges attached to a long polymer chain, interacting with each other and with ions in solution. I thought about his question and saw how it could be related to what we were doing in class. Then I got pretty interested in polyelectrolytes. I saw, too, as a result of subsequently reading about them, that there was another problem in that field: There were three ways of calculating a particular quantity (the free energy) in the literature, and I wondered, how are these ways related to each other? Three seemingly independent ways! So I looked into that topic, and it forced me to look into electrostatics. I had had a course in undergraduate physics in electrostatics. But now I really had to look into the subject in more detail, because I was trying to apply it to a specific problem. I figured out how these three methods of calculation were related to each other, on the basis of this reading. I wrote another paper on that topic, and on some other electrostatic properties of poly-electrolytes, which was later well received by researchers in that field. So that experience gave me a very extensive background in electrostatics and its applications.

That training was going to be the key to what came later, because I believe it was the following year that I happened to read a symposium issue of the *Journal of Physical Chemistry* on what was called "electron-transfer reactions." These reactions were studied at that time by a small group of people—people who typically worked or had worked in national atomic energy labs, because they used radioactive isotopes to follow the rate of these reactions, and they found out how fast some particular electron transfer reactions were. Actually these isotopic exchange reactions turned out to be the simplest of all electron-transfer reactions—so simple that you could focus directly on fewer factors that influenced the chemical reaction rates—and indeed they turned out to form the simplest class of all chemical reactions.

One of those papers in the symposium issue contained a novel explanation of some experimental results, and explained why certain isotopic exchange electron transfer reactions were faster than others. In the explanation the author invoked an unusual idea, unusual in this context, the so-called Franck-Condon principle. James Franck had received the Nobel Prize in chemistry—not for this work, but for some other. E. U. Condon was the chap who later was the head of the US Bureau of Standards. (Remember, he had some problem with Congress because of his ruling on a bogus battery additive. Later he was vindicated.)

In any event, the author invoked this well known Franck-Condon principle. It was his application to these chemical reactions that was novel—an electron is so light a particle that when it jumps from one reactant to another (an electron transfer), the heavy nuclei don't have time to move; being heavy they move sluggishly. That idea permitted an explanation of the experimental facts. The author of the paper using that principle was Bill Libby, who later received the Nobel Prize for a totally different subject, radiocarbon dating. He also had a back-of-the-envelope calculation in the article. The calculation involved some electrostatics, but somehow it didn't seem correct. During the next month I figured out what was wrong. You see, I'd had enough electrostatics background, and I was able to do it right. What was missing was that Libby just had the electron jumping and then afterwards the nuclei adjusted themselves to its new state. Actually, one could show—and I guess this was the point that was bugging me—I could show that that idea violated the law of conservation of energy. What really had to occur was that there had to be fluctuations in the positions of the nuclei beforehand, such that the electron could jump and energy would still be conserved. I found a way of treating that problem, with all the solvent molecules, in an approximate way. That was really the heart of the key paper, which came out in 1956, in electron transfers. When I got the result it was the most exciting moment that I'd ever had in science in my life. There was just such exhilaration—after working hard for a month it had come out. After massaging it—you know, you get it in various forms, and you may substitute one thing or another, and so after massaging it, it came out in such a simple form. It really was a thing of beauty—to me, anyway.

So that's how I got into electron transfer.

COHEN: Did you ever share this with Libby afterwards?

MARCUS: I discussed it much, much later—not until more than twenty years later. But I never discussed it with him at the time. I just published it. I don't think it ever occurred to me to discuss it with him. Many years later, in 1979 I think, at a meeting in Philadelphia, he mentioned to me that he showed my 1956 paper to Condon, and that Condon said it was right.

That paper was received very well, because during the next year I believe I received about a dozen invitations to give a talk about it, whereas before I think I'd only given maybe one talk or so on the unimolecular work; essentially nobody at that time knew about the 1951-52 unimolecular theory—recognition of it was to come some years later. But here, for the electron transfer work, there was instant recognition that something was new and interesting. It wasn't particularly applied by experimentalists at that time; rather, that people saw that here was something different. It looked different, and they wanted to hear about it. Then, for the next few years, I worked on extending it to electron transfers at metal electrodes (electrochemistry) and extending it to another problem, charge transfer spectra.

Then, around 1960, I decided it was time to take a sabbatical. I've taken very few, maybe two away from school in my entire life.

COHEN: And you'd been promoted.

MARCUS: Yes, I obtained the position in '51; in 1954, I was made associate professor, and in 1958, professor. The department chairman apologized for not being able to get me the latter promotion in '57. But that was fairly common to get promoted at Brooklyn Poly reasonably early. I don't think I was ever worried about tenure.

COHEN: Let me ask you something else at this point. What about your citizenship?

MARCUS: Around 1958, it was clear that I was going to remain in the United States. So I applied for citizenship. Of course, that made other things possible, too. You're more a part of the country; you're also eligible for more things, such as fellowships. But it was clear that I was not going to return to Canada. I had no interest in returning. For me the US was a scientific beehive.

Just before 1960, I applied for and was awarded a Senior Postdoctoral Fellowship from the National Science Foundation. In 1960 I also received an Alfred P. Sloan Fellowship. So I decided I would take a sabbatical.

COHEN: Was it common at Brooklyn Poly to do this?

MARCUS: Well, Poly didn't pay for sabbaticals. But, I had the funds available from these other sources. In fact, the NSF Fellowship in itself provided enough funds. I think I probably used the Sloan fellowship for the research, there was much flexibility in its use, and so, like many others no doubt, I husbanded its resources.

But the question was: What to do? And I decided that what I wanted to do was spend some time learning more mathematics, which as I mentioned earlier I had loved. I felt that there was a lot more in math, various techniques that I could learn. I spoke with some of the faculty that I knew over at the Courant Institute of Mathematical Sciences at New York University in lower Manhattan. One of the chaps there, who later became very well known in mathematics, the same ex-McGill student I mentioned earlier, was Louis Nirenberg, who won the Bôcher Prize and later was the first winner—co-winner, really—of the Crafoord Prize. I was a year ahead of him at McGill, but we had shared two classes in mathematics. He had moved to New York; in fact he received his PhD with Courant at the Courant Institute. So I spoke with him about courses, and I spoke with Cathleen Morawetz, a mathematician at the Institute and wife of Herbert Morawetz, one of my fellow professors of chemistry at Poly. She and Nirenberg said, "Fine. Sit in on the courses." So I decided to sit in on many of those courses. They said, "Well, you know this, and you know that." I ended up sitting in on courses that were much too advanced and abstract for me—I didn't really have the prerequisites. I really got to know what it was like, for the first time in my life, to be a dummy in a classroom, because I was spending so much time trying to catch up, to acquire the missing background so as to understand what was in those courses. It was hard work. I never really, I think, got as much out of the courses as I might have if I'd taken more elementary courses or more applied mathematics, instead. But if nothing else, I learned not to be afraid. When I see some mathematics that I don't know anything about, I'm not too dismayed; I can go and try to look it up, and in some cases learn something about it. I have a better overall picture of mathematics as a field.

COHEN: You didn't move from Brooklyn?

MARCUS: No. There was one other attraction. We had moved, as I mentioned earlier, after a few years in Flatbush, to Brooklyn Heights, a very nice area that was very close to Poly, so I could walk to school. We subsequently purchased and were in the process of renovating a house in Brooklyn Heights. So there was a two-fold reason for not moving from Poly. Our first youngster was born in '58; the next one was born in '61.

COHEN: So in some sense, you were just a visitor at the Courant. You didn't have an official position.

MARCUS: They called it "temporary member." They gave me an office there, which I shared with some mathematician from Poland. I attended many lectures at the Courant Institute. While there I was especially impressed with the beauty or "art" that was evident in some presentations at these mathematical seminars. There was a certain amount of intuition and use of analogy. I had some lectures by Courant. He had a strong geometric flair, which he used in describing some theorems in a course on Hilbert's space theory. However, he gave only six lectures.

COHEN: Did you get to know him personally in any way?

MARCUS: Not really. I got to know Friedrichs more, who also was a famous mathematician. I got to know Joe Keller more (Herb Keller's brother). I attended one or two classes by Joe. He was teaching at the Courant at the time; this was before he went to Stanford. I got to know Lipman Bers, who's an algebraist who later went to Columbia, and Peter Lax and Jürgen Moser. I got to know a number of other people. But I was really just auditing the courses, rather than taking an active role. During that period (1960-62), I didn't try to get much done on my research papers. Of course, since I was living in Brooklyn, I could go over and talk with my graduate students at Poly, and was in charge of a laboratory course at Poly in 1961-1962. (Poly gave me this extra year for study at the Courant Institute.)

It was about that time that I decided to wind down my experimental work. It was clear that the work I was doing in theory was, above all, something I really enjoyed more. Secondly, I felt it was having a real impact. So I decided around that time to get out of experiment. And actually, a gentle nudge from a friend of mine, who later became a leading experimentalist in molecular beams, Dick Bernstein, helped propel me. He was at the University of Michigan at the time and some decades later was a Fairchild Scholar here; he died recently. But I would have stopped the experimental work anyway. He wondered why I was constructing a particular and rather demanding atomic beam apparatus when I could do theory that had an impact. In turn, he was describing some work he was doing experimentally, work related to unimolecular reactions of ions and isotopic effects, but it didn't seem to be all that interesting, and perhaps I had indicated as much. I think that had some effect on one aspect of his work—years later we discussed this mutually beneficial interaction that we had about 1959 when he was at Michigan.

In any event, it was around 1960 or so that I decided finally to wind down my experimental program. You see, I'd really become active in various areas in theory, probably most of them related to electron transfer.

During the time at Poly, there had been several approaches from a few universities. There was one by Brandeis University, for example. But I felt that was too much of a sideways move. Laurie was very, very sorry that I didn't follow the Brandeis one up, because she really wanted to leave the city, especially with the children. They didn't actually make me an offer, but I didn't try to follow it up, even though the surroundings would be much nicer. And there may have been one or two others.

Minnesota had offered me a visiting professorship for a year, but I felt that would have interrupted the research I was doing, and I had no interest in going there. Perhaps they were actually interested in looking me over for a year, I don't know. That's the way the department head at Poly interpreted it, because he asked me what they were offering in salary, and immediately jumped up my salary. [Laughter] But again, I felt it would be taking me away from my research group of students, so why go to another place?

Finally, an offer came from the University of Illinois. The University of Illinois—in terms of the quality of the chemistry department and of the university—might be called a step up. As a department, I think it was ranked No. 6 in the nation. There were quite a few of the departments at the U. of I. that ranked highly. It offered a much better place for the children to grow up. We were delighted to go there. We went to Urbana in 1964, and our children grew up there.

COHEN: And you had a third child.

MARCUS: The third child came in 1962. So we went with three young children. It was fine for them growing up, although the quality of the schools deteriorated when they were in junior high school and high school, with the introduction of open classroom instruction. In the hands of well-trained people, it might have worked well. But it was sort of chaos. After several years the parents actively objected. It wasn't working out well; there were all sorts of problems. Initially, in grade school the schooling was fine, and at one time, I think the Urbana public schools were very good.

COHEN: They seemed to be very strong in music, am I correct?

MARCUS: The University of Illinois is very strong in music. It has (or had) a large music department.

COHEN: I mean the public school.

MARCUS: That could be, I don't remember. Certainly, all our boys had some music.

So I did research and taught at the University of Illinois. I don't know that my work there was any better than it was at Brooklyn Poly. But now I was doing full-time theoretical research. At Brooklyn Poly, the whole time I was there, I had just one graduate student who worked in theory; the others did experiments. At Illinois, all my graduate students were in theory, and there were a fair number of them. Some were very good. So from that point of view, it was fine. We were absolutely delighted to be at Illinois.

COHEN: How big is Urbana?

MARCUS: Well, I don't know what it is now, but at that time its population was 30,000, and Champaign, which it adjoined, had 60,000. We really enjoyed being there, in part for the sake of the children. I remember when I first went there—aware that it was a "real" university, with a campus. I used to enjoy walking around the campus at lunch time.

COHEN: And you went as a full professor.

MARCUS: Yes. The first year I was there, I went to a number of lectures that I could never have gone to at Poly. One was on linguistics. I've always enjoyed comparison of languages. But when I went to that lecture, it was so technical that I got very little out of it. Another lecture I remember going to was on a relatively unknown period in Greek history, between 1200 BC and 800 BC. You know, 800 is when the more modern Greek culture begins, and 1200 was around the time of the destruction of Troy. This was a lecture on what they could learn about that period from examining the burial urns in a cemetery in, or near, Athens, and whether any changes in the period had been reflected in the burial urns. As far as I remember, they didn't learn anything. [Laughter] But that was a reflection of the diversity of lectures at the university. It was exciting.

COHEN: One thing we didn't talk about earlier in this interview was influential colleagues at Brooklyn Polytechnic. Let me just interrupt this and do that.

MARCUS: There were a number of people I really enjoyed interacting with at Brooklyn Poly. One was Ernest Loebl. He had received his primary and secondary education in Vienna, with four or so years of Greek and six or so of Latin. He really had a very good education. He had come to the US as a refugee, and received his PhD at Columbia. He came to Poly as an assistant professor a year or so after I came. It was just so great to talk things over with him. He was a chemist. I'd have these ideas on things. He was just a wonderful person with excellent logic to bounce ideas off of. That was one person that I really enjoyed talking with a lot. I've already mentioned that Herbert Morawetz, through the student Abe Kotliar, was influential. I talked with Herb about polyelectrolytes, because he was a world expert on that. I spoke a lot with an ex-New Zealander, Frank Collins, a theoretician who received his PhD degree, like Loebl, at Columbia. He really had a million ideas and was developing a dynamical theory of liquids. I remember that, every day it seemed, he'd come down to my office in the Quonset hut with a new idea about his work! That time was a period when I didn't have any ideas. It was good to talk with him. Eventually, he ended up being in competition in his field with a famous chemist, Jack Kirkwood, who was here briefly at Caltech, but was at Yale at the time. Kirkwood had a large group of students and there was no way that Frank could compete. Eventually Frank ended up going into another area. But, certainly, I talked with him a lot. The Dean, Raymond Kirk, was just a wonderful person. We met him socially, had him and his wife over for dinner, we were over at their place. Just a gentle person—gentle to us, anyway. I think he used to throw chalk at students who fell asleep in his general chemistry lectures. [Laughter] Anyway, there were a number of people, and there was a very good spirit at Poly. Ben Post, and Isadore Fankuchen. One faculty member who became a lifelong friend, a chap in electrical engineering who's now retired from Poly, is Athanasios Papoulis, the author of a number of very successful texts bridging mathematics and electrical engineering. (He received his BS in Athens in engineering and his PhD at the University of Pennsylvania in pure math.) I used to enjoy talking with him a lot, and in fact, still do. He occasionally comes out to UC Irvine. So it was great from that point of view.

At Illinois, I think, there were fewer people I talked with.

COHEN: Do you think it was the times?

MARCUS: Well, I think it makes a difference whether you come to a place, start off there and grow up with other people who've done the same, or come to a place at a senior level, where people have already developed their close relationships with each other. So I think there's a big difference in that respect. But probably more importantly, there weren't many people at Illinois who were working in something related to what I was doing. There were a few. [Tape ends. Presumably Cohen asked about interacting with Aron Kuppermann at Illinois.]

Begin Tape 2, Side 2

MARCUS: No, in fact, Aron Kuppermann had left Illinois the year before I came. My guess is that they asked him for suggestions for a replacement and that he suggested my name. Certainly, there were two people there who could have done that. Aron, who knew about my work, and Peter Yankwich, who was working isotope effects on unimolecular reaction rates. And actually, by that time, I was probably known more, or as much, for the RRKM work than for the electron transfer. The RRKM theory had begun to be popular. It began to be used in experimental work in 1959, which was seven or eight years after the papers came out. There was a big delay.

COHEN: There weren't so many meetings in those days.

MARCUS: There weren't meetings. Also, since I had left the unimolecular reaction rate field largely, I wasn't around to try to popularize the theory. It was only when somebody—in fact, a fellow who was a few years ahead of me at McGill—saw the theory and saw that it would help explain his experimental results—that its extensive use began. He was the one who made it popular. That was B. S. Rabinovitch. He retired recently from the University of Washington in Seattle. He is a highly respected reaction-rate person in the gas phase, and was the one, in fact, who called it RRKM—gave it that name, that acronym, and used it for interpreting many, many of his experimental studies. He found ways of studying experimentally the rates of reaction of molecules that contained a lot of vibrational energy, and he and his students made extensive use of the theory.

So at Illinois, I didn't really have—I think, in retrospect—all that many people to talk with, but it was a great institution, we were glad to be there. The net gain for us was positive, even though in terms of individual interactions, I had certainly been more at home with the people at Poly. Those were the people I'd "grown up" with.

Interestingly enough, at Illinois, they have a housing development where new faculty people could stay for two years. We stayed there. Some of the people we met there became our friends for long after. In fact, even now, today, our children will meet with their children if they happen to be in the same city. So there's an example of couples coming into a similar environment, all new, and making friendships. But we were never really as settled into the community as we had been at Brooklyn Poly—or indeed, as we had been at Carolina. At Carolina, there were a number of young people about our age and about a half a dozen we saw a lot of.

COHEN: There must have been a real ferment at Poly, though, in those days.

MARCUS: Oh, yes, there was. For example, there were polymer seminars once a week, perhaps even more often sometimes. They had Saturday morning seminars. They had cocktail parties galore when there was an outside polymer lecturer during an evening, and we had some for the chemistry department seminars. It was really stimulating.

COHEN: And this was New York City.

MARCUS: Yes. During our stay in New York City Laura and I enjoyed going to so many plays that later became famous. Laura would sometimes get these cheaper, two-for-one tickets; there was a place uptown where you could get them. We saw many outstanding plays. Until the children came, it really was wonderful.

While I was at Illinois, I had various opportunities to move, but a number of them involved administrative work, like being dean of this or that university. I had decided, above all, that I had no interest in doing anything involving administration; and secondly, that there's no point in making a move that is professionally sideways.

But after a while, we became a little restless. We did go on a sabbatical. We went to Oxford, where I was a visiting professor, in 1975. For six months—two terms, actually—we were at Oxford. Our boys went to excellent English schools. In the remainder of the sabbatical time, which was five or six months, I had a Humboldt Fellowship, and we spent the time in

Munich, Germany. Our Humboldt "sponsor" at the Technical University of Munich was Ed Schlag, who as a graduate student in Rabinovitch's laboratory at the University of Washington, first used, at Seymour's suggestion, RRKM theory to explain his experimental results.

In Munich the children went to German schools, *gymnasia*. In fact, the youngest had been learning Latin in the English school, and continued trying to learn Latin, but this time translating from German!

From a cultural point of view, that year proved to be great. At Oxford the person who had been professor of theoretical chemistry there, Charles Coulson, had recently died. They received money from IBM to have a Visiting Professor of Theoretical Chemistry. You didn't really have to teach, but I gave ten lectures, probably at their request. The professorship was attached to University College. So I became a Fellow at University College ("Univ"). That, too, was just great. I enjoyed going to lunches there. It's a little close to the round table lunches at Caltech, only it's not round. There are long tables, so you can't talk with as many people as you can at round tables, but I enjoyed it very much. Oxford is just wonderful. And on weekends we had fun exploring the English countryside. [Note: A year or two after this interview, Marcus received an honorary doctorate from the University of Oxford and also became an Honorary Fellow at "Univ".]

COHEN: Was this the first time you had gone to Europe?

MARCUS: No, the first time was when I was about a year old and my mother, who was born in Manchester, England, took me back to see her family.

COHEN: Did you have any family there?

MARCUS: I didn't know where they lived at that time. I had lost contact with them. I also went to England in 1960 to a meeting, and I had been there several times before the sabbatical. But that was a great cultural year. I don't think I did all that much scientifically, other than writing papers long distance with my graduate students at Illinois.

Then we went to Germany where I had this Humboldt Fellowship. I gave a series of lectures there, the same lectures that I had given at Oxford. It was great, exploring the countryside on the weekends.

COHEN: You didn't mind being in Germany.

MARCUS: Well, actually, I felt somewhat uneasy, because I couldn't help but remember what had happened there in the Hitler period. The people that I met were very nice. One of them had been, I believe, when very young, in one of those Nazi youth movements. Many of the people, I think, were. But he was really nice. These people, three of them, including Ed Schlag, had all been professors at Northwestern for a number of years, and then were enticed back to Germany. They were Germans, but had spent a lot of time here—and they were the people in Munich I knew best. From a cultural point of view, that was special. I certainly did have some unease and discomfort, though the overall experience was just wonderful.

It clearly had a lifelong effect on the members of my family. Two of them ended up going to Cambridge for their doctorates. One of those took his year abroad from Berkeley at Göttingen. All three have been back to Europe a number of times, and one of them lives there. We had gone to Europe as a family many times, taking off four or five weeks. We'd go there during the summer.

Then we returned to Illinois, and after we'd been back a few months, Oxford offered me a professorship—this same professorship but now on a permanent basis. That was a really tough decision for me. Of course, the salary was relatively low, but I was assured that you could live on it. I don't think in the long run that the salary was the determining factor in our decision not to go. It would have meant that the children—the three boys, who had received their training in American schools—would have had to compete with the others, and at a big disadvantage, I think, because the others had received much more advanced training. I thought that would be just too much.

COHEN: What year was this?

MARCUS: That was 1976 or early '77. We returned in August of '76, so it was some time after that. So I suggested to the people at Oxford that perhaps I could spend part-time at Illinois and part-time there. I understand that some of the committee were willing to do that, but the committee as a whole didn't. In fact, it really wasn't a good idea. I didn't think it was a good idea at the time, but the attraction of being a professor at Oxford… [Laughter] But I didn't want to go there full-time because of the boys, really. My going there part-time was all right with

Illinois; they were willing. But the Oxford committee balked. So we ended up not going there.

I figured then that that was probably it, as far as my moving from Illinois. At that point, I guess I was interested in moving from Illinois, and this going away for a year was a bit unsettling. I didn't have real strong roots in Illinois, in terms of research interactions with a variety of people there.

Then came the invitation to come out here and discuss things, which I did. Eventually Caltech made an offer. So we came out in 1978.

COHEN: Were there any people at Illinois that were influential to you?

MARCUS: I don't know if there are any that were really influential. But I spoke a lot with people such as Doug McDonald, who's a very good researcher in the unimolecular reaction-molecular beam field in chemistry. We co-authored a paper. I spoke quite a bit with Bill Flygare, who was an outstanding scientist—died young from Lou Gehrig's disease. I spoke a lot with Harry Drickamer, who is an excellent scientist. I spoke with some of the biochemists, like I. C. Gunsalus and Gregorio Weber, and with several other chemists. So there were a number of people that I did speak with. Some of the discussions were related to my research, but mostly it wasn't very directly related. In fact, over the years—actually both at Poly and at Illinois—the person I interacted most with was at neither place. This was the chemist Norman Sutin, who incidentally is the nephew of the late well-known artist Chaim Soutine and is a leading inorganic chemist.

COHEN: Where was he?

MARCUS: He was at Brookhaven National Laboratory, doing isotopic-exchange work in the rates of electron transfer reactions. When I was at Brooklyn Poly, going to Brookhaven was probably my main inspiration, on thinking about it. I went about once a month to Brookhaven and talked with Norman Sutin and Dick Dodson. Dick was chairman of the chemistry department at Brookhaven, and he also had an adjunct appointment at Columbia University. (Dick had received his PhD at Caltech, as a graduate student of Don Yost, if I recall correctly, but that didn't mean anything to me at the time.) That really was the biggest single influence on my research.

COHEN: What took you out? What originally made you start?

MARCUS: Well, there was a lot of electron transfer work at Brookhaven. They probably invited me out around ’57 to give a seminar. And then they had consulting appointments. So I became a consultant. But I would have gone whether they had paid me or not. [Laughter]

COHEN: Did you ever think of going out there to work?

MARCUS: No, they never made an offer, and I always wanted to stay in academic work anyway. But that certainly was a big influence—the biggest influence.

COHEN: So they were doing experiments, and you’d go out and discuss the theory.

MARCUS: That’s right, yes. In fact, it was by interacting a lot—mostly with Norman, after a while—that he and I tested certain relationships that I had derived in the electron transfer theory, and he tested the relations with additional experiments. It really helped develop the field. Norman later became a leading researcher in that field and later a member of the National Academy of Sciences.

COHEN: Did he stay at Brookhaven?

MARCUS: Yes. He’s at present chairman of the department. He’s been chairman for quite a while. I’m sure he’d like to stop being chairman, but I think they want him to stay on for a while. [Note: In 1995 he retired from the chairmanship.]

COHEN: Did you go to Brookhaven when you were at Illinois, too?

MARCUS: Probably very little. But Norman and I would talk a lot by phone. For a while I was also on the Visiting Committee at Brookhaven. That may well have been when I was at Illinois. So I would go there during that period. But that would only be once a year, for a couple of days. The telephone was our major way of communicating.

COHEN: So you didn’t do a lot of travel.

MARCUS: Not a lot, I guess. There were quite a few talks to give here and there. But not to spend any extended time.

COHEN: So you really were ready for that offer from Caltech?

MARCUS: Yes. Once again, that was also a real step forward professionally, I felt.

At Caltech, it proved to be great, as far as interaction, much more so than at Illinois. In particular, Harry Gray was chairman of the department, and Harry was working on electron transfer. Harry is a wonderfully interactive person. He and I have never collaborated on a paper, but we've talked a lot together, and I've done some theoretical work on some of his experimental results. He certainly made a tremendous difference.

COHEN: Did you know Harry in New York?

MARCUS: Well, you never know how to interpret things that Harry says. But Harry said that when he was at Columbia and I was at Brooklyn Poly, he used to come over to hear me talk about things. He may have come over, maybe for some of those polymer seminars—because the polymer seminars were famous—and I may have spoken up at some of them. So my guess is that there are some elements of truth in what he said. [Laughter] But I'm not quite sure. But you know, Harry is always very, very nice, and makes you feel good. So it was just great being here at the Institute with him. Harry, in turn, brought other people in, such as Nate Lewis, in more recent times. And of course, there's Fred Anson, who works on electrochemistry, which concerns, in part, electron transfers. Jackie Barton works on electron transfers. So does Sunney Chan. John Hopfield did at first, too, but has gone over to neural networks, a totally different field. But there certainly is extensive interaction, especially with Harry, and all the visitors that come through as a result of his being here; and now with these other people who are here, there certainly has been a real ferment. So that was interaction on one side of my research.

Then, on the unimolecular side, there's Ahmed Zewail. We interact a tremendous amount. We've written only two papers together, but we certainly have talked a lot. Each of us has undoubtedly had a major influence on the other's work. I speak with Aron Kuppermann and with Jack Beauchamp about their work from time to time. So in terms of interaction, there's been for me much more than at Illinois, for several reasons. One, Caltech is a very interactive place. Some of these people are very interactive. [Laughter] And, two, some of them are working on subjects close to what I'm interested in.

COHEN: And I think the place gives you time.

MARCUS: Oh, yes. And there are lots of good people coming through in seminars—maybe too many. So it's really a stimulating place. I enjoy so much, also, going to the round tables at the Athenaeum at lunch, and talking with people outside of the chemistry field. You know, all sorts of different subjects come up, in which one or the other at the table might be an expert. Maarten Schmidt was there yesterday; Jesse Greenstein came in and was there today.

COHEN: Have you done much teaching here?

MARCUS: I teach two courses a year. In other words, that's typically three hours a quarter, for two quarters.

COHEN: You haven't taken any sabbaticals since you've been here.

MARCUS: No. I've done something I did at Illinois once. I've taken a year off from classes and stayed here. I think I've done that only once. If there were a place I thought I would benefit a lot from going to, I'd be glad to go. People have made approaches of one sort or another, but I like it here, and I would probably lose more in research time than I would gain.

COHEN: We spoke a lot about sports early on. Did you do any sports, other than…?

MARCUS: Oh, yes. In fact, when we were on that sabbatical in '75 to '76 in England, we were required, as part of the Humboldt Fellowship, to spend six months in Germany. Well, there was a problem with the schooling. For the children to attend school in England for two terms, we had to spend seven months in England, because there was a month vacation between two of the terms there. So that left only five months. Then I realized, that if we went to Germany for one month during the wintertime—during the Christmas holidays—one plus five is six. So I asked them about that, and they said, "Sure, that's fine. Doesn't matter; there's nobody around there—

as long as you spend the month in Germany." So we made arrangements to go to Munich—actually, to go to one of the ski resorts. I had skied a bit when I was a boy, because we had a small mountain in Montreal, at least it was called Mount Royal, but I hadn't skied since. So we bought some skiing clothes, and we rented skis when we were in Munich, I imagine. And we went skiing. Laura didn't ski, but the three boys and I took skiing lessons. That was just great. It was hectic, too, because we made the arrangements at the last moment, and all the places in the Bavarian Alps were taken. We ended up in Bayrischzell, spending a few days here and a few days there. Every other morning or so, Laura would go out and see if she could find us a room for that night, or double rooms with the boys. [Laughter] Meanwhile the four of us went skiing, not knowing where we were going to spend the night. [Laughter]

COHEN: She's a good sport.

MARCUS: Oh, she's a tremendous sport. Anyway, we whiled away several weeks there. Then we ran out of places. The final place we stayed at was on the outskirts of Bayrischzell, in some farmhouse connected to a barn—somewhat primitive.

Then one of the German professors we knew, Sighart Fischer, who had been at Northwestern, invited us to come as a family to his house in the outskirts of Munich. That was a life saver. The day we left there was a deluge of rain. It rained out the resort. So we couldn't have done any more skiing—or hiking—anyway. [Laughter] Ever since then, we've gone skiing just about every year, until I had the hip operation two years ago. Laura would usually come with me and the three boys. So we've done a lot of skiing together. When we were in Illinois, we went once to northern Michigan; another year we went to Colorado—Aspen; and, one spring, to other Colorado resorts. Then when we came here, we typically would go to Mammoth, year after year.

COHEN: Didn't you play tennis also?

MARCUS: Yes. I started playing tennis that summer of 1942 at McGill, and then later when I was a postdoctoral in Ottawa. But I hadn't played after that for many years. Then, at Illinois, in the years shortly before we left, I thought it would be good for the boys to take some tennis lessons. And I would also play with them. We then moved here, and I began taking tennis lessons with one of the boys (Ken), who became a very good player. I then started to play with the Caltech people. So I played once a week, sometimes twice a week, until a year or so before the hip operation. I'll start playing again, but I haven't started yet.

COHEN: No more Ping-Pong.

MARCUS: No. That was a thing of the past. I mean, just as when I was in high school—checkers was very popular, and actually I became the checkers champ at the high school. My boys, many years later, wanted me to play checkers with them, but I was burned out, I guess.

RULDOLPH MARCUS

SESSION 3

December 14, 1993

Begin Tape 3, Side 1

COHEN: I think we can start by talking about your graduate and research fellows that you've had here at Caltech.

MARCUS: Most that I've had have gone into academic work. Very few have gone into industry. Of course, probably for theoreticians, unless they're mainly computer-type people, the best opportunities are in academic work. And that's the way it's ended up.

Certainly, on the average, they've been very good people. Here and there, some of them have been, I think, exceptional. I was just delighted this morning when I heard that a Russian postdoctoral of mine—who's been having difficulty getting a job, but who I think is brilliant—has just got a job offer from Washington State University and Pacific Northwest Laboratories—a joint appointment. [Note: This is Alexei Stuchebrukov, who is mentioned by name later in the interview. He subsequently received and accepted a faculty appointment at the University of California at Davis.] The students have certainly ranged in innovativeness, in capability, but on the whole, they've been very good. But at Illinois I think I had some of the best students there, too.

COHEN: How do you make a decision about who to take?

MARCUS: Well, in the case of postdoctorals—that's really where most of the decision comes. Because there aren't all that many graduate students—although, at the moment, I do have four. That's a lot for me; sometimes I haven't had any. But my real decision, as far as choice, comes in postdoctorals. But in the case of students, I first try to find out if they've had a reasonable amount of physics and math. If they haven't had that, I certainly don't try to encourage them to join my group, although I'm not sure that any have actually come and asked to work with me without having a reasonable background in math and physics. But normally I try to emphasize that when I talk with them—that we use a fair amount of math, even though it's not all that advanced. But still it's a certain amount.

In the case of postdoctorals, that's where there is a lot of decision, because there are so many applicants, mostly people in Europe or Asia.

COHEN: Is that in recent years, or has that always been the case?

MARCUS: I think it's more so now—at least in my case. And there's almost always a question of numbers and funds. I don't want to have a large research group, so almost automatically I'm going to say no. And, in fact, in recent times, postdocs have had a hard time finding an academic job and I've said no to essentially everybody, except one, who's coming with his own money. At the moment, I just simply have been saying no, no matter how good they are. Once in a while, there's been one that's just so good that it's hurt me to do that, because my funds were already committed. But normally the decision would be on the basis of what their background seems to be in fields like physics and math and on what concepts or techniques they've used in their research—and to try to make sure that they're not just somebody who's only been well trained in computer programming but doesn't have extensive training in these other areas. Of course, it's also based to some extent on the letters: Where they come from, whether I know the people who have written the letters—although that's not the overwhelming factor. But the numbers of my postdocs are so few—I have three at the moment, which is what I've had in recent years. So I haven't taken on many.

COHEN: And they stay for three years?

MARCUS: Well, it varies. Two years would be best. But it's been difficult to find jobs lately. So it's closer to three years. For example, a chap from mainland China [Hui Ou-Yang] has been here for about three years, and fortunately he now has a position at a university in Hong Kong, a new university, and that looks very good. And the chap from Russia has been here for approximately three years, maybe even four. And fortunately he now has an offer.

COHEN: So you keep your people until they have something to go to?

MARCUS: Well, that's what I've been doing. Although I encourage them in some cases to look elsewhere for a postdoctoral if I can't. It's not good for them to stay too long. But on the other hand, if the choices for them to go somewhere else aren't great, then there's something of an obligation to support them.

COHEN: Who's been funding your grants?

MARCUS: For years and years and years, since the middle 1950s, the National Science Foundation has, continuously I believe, funded my research.

My first grant, as I mentioned earlier, was from the Office of Naval Research. The navy was a pioneer in the support of basic research. Except for one period during my stay at Illinois, the ONR has funded my research for most of the time. Certainly the whole time I've been at Caltech, and probably almost the entire time I was at Brooklyn Poly, and much of the time I was at Illinois. So those have been my two main sources.

There have been other sources; there have been consortiums here at Caltech. The Beckman Institute provided a small amount. The Caltech consortium (now expired) also provided a small amount. Currently now I have a grant from a Japanese organization that sponsors research that about five or six of us—from Japan, the US and Germany—are involved in. We're doing related research, and we're supposed to get together once a year. That is just starting, actually. I'll use that to support graduate students, and postdocs. So that's where my support comes from. I also have some funds from the Noyes professorship for support of research. Salaries, when you add overhead and fringe benefits, amount to a lot. So it is expensive to do research here. There aren't any services that are free, or very few that are free.

One of the interesting aspects in working with students—and, indeed, with postdocs, too—is to try to get a particular point of view across about theoretical research. I didn't realize how much I had this point of view until relatively recently. But with some exceptions, it's something that I think I've had for the entire time I've been a faculty member.

In doing theoretical research, there are several types of work that go on. One type, which I believe is very much the way most physicists do their research, is to use experiments as a starting point and to try to explain the experimental results, develop the theory, and use it to predict more experimental results. In recent years, in chemistry, there's been another type of theoretical research, in which people can do what might be termed experiments with computers. Then the focus has almost become on thought experiments, or imaginary experiments, which you treat as the real system, and then you make calculations and draw conclusions. Now it's true that they draw some inspiration from experimental systems in many cases, but the focus appears to be on achieving the numerical results, with some focus on physically understanding them.

What I've tried to do more consciously in recent years (and what I did automatically in my years at Brooklyn Poly) is to really focus on the experimental phenomena. Given a phenomenon, the plan is to then try to design a theory that is related to it. Of course, we don't select every kind of phenomena; we just select those that are, in some sense, within our scope, our expertise. That's one of the things, since I've become conscious of it, that I've tried to convey to the students. I've been increasingly aware of this aspect, I guess, because some foreign postdoctorals don't have that same focus. So I've consciously tried to convert them to this "true cause," so to speak. [Laughter]

COHEN: They come in so computer-literate, they don't look beyond that.

MARCUS: Yes, in some cases. Or it's just that there's a lot of research that's been done—in Russia, for example—where, the focus was on refining models. One then has maybe an improved model, but it isn't necessarily that the model is required by the experiment. It was just "Let's get a better model." In other words, the research may become somewhat divorced from experiments, and the focus in that case is not so much on the experiment but either in the refinement of the theory or in the computer calculations themselves.

So in one way or another, I've certainly concentrated in recent years in trying to stress to my students that they draw upon experimental phenomena for this inspiration. First, there's a greater chance that the work will be relevant. Secondly, nature's been at it a long time, and springs many surprises and provides results to think about that we might not have thought about just on our own. There are some things we're working on that we really don't understand yet. You know, some things we have an idea about; we can roughly see a possible explanation down the line. We think we know we'll come out with an answer, right or wrong. In some cases, we're not even sure that we'll come even close, that we'll even capture the qualitative aspects. Of course, it's probably true that the better scientist you are, the more you can propose a correct approach in advance. But I know there are certainly some problems we're working on that are, to us, sufficiently complicated that I'm not quite sure which way they are going to come out. There's an excitement there. I've certainly tried to communicate that spirit to the students and to the postdocs.

COHEN: So you like it if they come in having seen something that they think would be good to work on?

MARCUS: Yes. But that doesn't happen often. They have certain expertise. If they have something in a totally different field from what I can possibly handle, then that would be difficult. It would be almost a waste of everybody's time—certainly of my time—to try to become literate in a million fields. So normally when they come, they work on some area that I'm working in or that I want to get involved in. In a number of cases, that goal is stretched. For example, in the past five years, with a couple of members of the group here, we've entered into research areas that I wouldn't have thought of working on some time ago.

COHEN: Could you give an example of one of those?

MARCUS: Yes. For example, in the electron-transfer case, almost all of my work since the middle 1950s was focused on the reorganization of the material around the reactants before and after electron transfer, in order to fit all the conditions necessary for the electron to be able to jump. That area became one of my main areas of expertise, and I formulated a theory about that. One thing that I essentially did nothing about was to investigate the detailed electronic coupling between the electron donor molecule and the electron acceptor. But it became clear that if one wanted to treat certain new experimental phenomena involving the electron transfer over long distances, then the amount of electronic coupling was extremely important. It is less important when the two reactants can come into contact, as they often can in solution. But if they're fixed—far away from each other, as they are in some biological systems and in some synthetic systems—then you need to focus also on the electronic aspects of the transfer. Well, I'd never worked on electronic aspects of chemistry. But one of the people who applied for a postdoc had experience in electronic structure calculations. So I suggested that she work on applying her expertise to this problem. That's had a major effect on my research, since for the moment most of our studies on electron transfers are concerned with the electronic aspects. Once we entered that area, a variety of other problems arose for us to treat, such as scanning tunneling microscopy (STM).

Another person I accepted as a postdoctoral and who is working here now also had expertise in electronic structure, and so I suggested he work on a problem related to STM.

Those are some examples where, because of the people entering the group, I've been able to enter more easily into certain areas of research. Of course, I probably had in mind blending our joint areas of expertise when I agreed to their joining the research group.

Again, one of the persons—this Russian chap—

COHEN: Maybe we should have their names, since you've mentioned them several times.

MARCUS: Yes. In the electronic-structure area, the main person involved was Prabha Siddarth. She received her training in India and at a good institution there. The other person—the one who's now going to Hong Kong and who has been a main factor in our entering the STM field—is Hui Ou-Yang.

In a different area, an area closer to unimolecular reactions, came this chap Alexei Stuchebrukhov. He came highly recommended by a leading Russian laser physicist, Vladilen Letokhov. Alexei came with fabulous training. You know there's this Lebedev Institute, where Lifshitz and Ginsburg were. Ginsburg was one of his teachers. So Alexei came with a very strong background in physics and mathematics. He had used some detailed intricate mathematical physics techniques in his work in the Soviet Union, after an intensive study of them. We ended up first writing a number of papers in which he made use of his expertise to treat with quantum mechanical methods the dynamics of vibrational energy transfer within molecules. That topic is related to the unimolecular reaction rate area that we were interested in. He adapted the techniques to that problem, and then innovated further, using it to treat the long range electron transfer in proteins, using the whole protein. In various ways he really stretched my own background. There are certain areas where he is so far beyond me it isn't funny. But I've benefited a lot from his being here. In turn, he has helped train two of my graduate students, so there have been some joint publications with them as well. In fact, in at least two papers I made such a little contribution to the work that I removed my name from them. I didn't do anything for it. It's not just a matter of altruism; it's self-protection, too, you know. [Laughter] I'd hate to have my name on a paper I didn't know much about.

One of the students who's come here—Xueyu Song—is also extremely able with mathematics and mathematical physics. He's written a number of papers—one with Alexei alone, and one with me—where he's really made a very definite contribution. He's clearly very, very clever, and I am learning from his work about these more sophisticated techniques. And then the other people who are working with me are, I think, very capable.

COHEN: And have they followed your feelings about dealing with real phenomena?

MARCUS: Yes, though not always. It depends on the individual. Usually I suggest, "Here are some experimental results related to so-and-so. Let's look into that." If ever one of them told me that they couldn't do anything with the problem, then I might suggest that we select something else. But in each case, we've usually been able to accomplish something with the problem.

In my own case, it wasn't always that way, an emphasis on inspiration from experiments. There was a period—perhaps five to ten years when I was at Illinois—when I began to stray away from treating experiments with theory. I became focused on what's called semi-classical theory and on molecular "chaos." In semi-classical theory, which bridges classical and quantum mechanics, I became more enamored of the theory than of its possible application to real molecular systems. (We encountered some real complication, because of the chaos.) In that case what I was doing became sufficiently removed from experiment that I felt uncomfortable—and especially when I came to Caltech and used to get remarks from my colleague Ahmed Zewail, who's very much experiment-oriented. And that was very good, from that point of view. I believe that his urging, hints, and statements were one factor in returning my focus to experiments. In turn, I made suggestions to him about his work, to make it more chemically relevant, which had a positive influence on the trend of his research. So it's been a good interaction—that really got me back on track and back to the real world. Also, having some of the people around here—like Harry Gray, with whom I've interacted—people doing electron transfer, or hearing many of the seminars by people coming through, I think these were factors also.

COHEN: I can see that you like this place a lot more than Illinois.

MARCUS: Well, I was delighted—and as a family, we were delighted—to go to Illinois. Illinois was certainly great for us. It's a terrific place to be. It's a leading institution in many fields. It also has an outstanding library which I made considerable use of. During the whole time there, one or the other of its libraries had any book or journal I wanted, no matter how obscure. I really enjoyed going there, but I have certainly interacted in research with more people here. Again, growing up in my early faculty experience at a relatively small institution, Brooklyn Poly, I guess I probably prefer the extra intimacy that you have in a small institution. Illinois, for us, was a fantastic place to go to, but to some extent one might feel a bit lost in a large university. Certainly, for me it's been very satisfying here at Caltech. On the other hand, there are a number of outstanding people at Illinois who remained there all their lives, and clearly they must have felt comfortable with it. It may make a difference, too, as I mentioned earlier, when one comes to a place, whether one has grown up there scientifically, i.e., began there as a junior faculty member. In my case, I didn't do that at Illinois. There were interactions—names I mentioned earlier—and that was very good. But at Caltech, first of all, everything is more concentrated. I certainly talk with the individuals here a lot more, as well as with the many visitors that come through, especially in the electron-transfer field. Of course, there were various people who have visited here whose work is related to unimolecular processes or related to energy-transfer molecules. Ahmed Zewail being here has undoubtedly helped attract them. For electron transfers, Caltech is a center, because of the number of people involved in that area and their quality. They include, besides Harry, Nate Lewis, Fred Anson, Jackie Barton, and Sunney Chan.

COHEN: You mentioned, once or twice before, that occasionally you were offered positions as an administrator—a dean, typically—and you refused that.

MARCUS: Yes, I didn't even go for a visit.

COHEN: How about on a lower level, like committee work and things like that?

MARCUS: Oh, there's been a certain amount of committee work. At Illinois, for a while, I was chairman of the Staffing Committee—for hiring and for reviewing promotions of faculty in chemistry. There were various other committees—I was on, for example, the university-wide fellowship committee. At Caltech I've done some committee work—nowhere near as much as people like Bob Grubbs or John Bercaw. And now Ahmed is doing a lot of work. Those people have been real Trojans.

COHEN: Is that department, or institute?

MARCUS: In Bob's case and in John Bercaw's case, I think it's been mainly departmental. In Ahmed's case, it's been departmental and institute-wide. I've been on the Institute Programs Committee. But I was on the Junior Admissions Committee for a while. I don't think I was ever on the Staffing Committee. I've been on the Awards Committee for years now. These are some committees that I've been on.

COHEN: Have you ever been on the faculty board, or anything like that?

MARCUS: Yes, I was on the faculty board, and also was on the Steering Committee of the Faculty Board. I was a member of the House Committee of the Athenaeum. One thing I remember about my assignment on that committee was that they asked new members what subcommittees they would like to join. I liked tennis; I put down tennis. There's also the wine committee, and I enjoy wine. So those were the subcommittees I put down. But, instead, for the three years I was there I was on the Decorations Committee. [Laughter]

COHEN: So you have done your share.

MARCUS: Well, I've done something—reasonable, but far less than some.

COHEN: Let's go on about all your honors. You said something about one involving an issue of the *Journal of Physical Chemistry*?

MARCUS: What happened there was that they decided to put out a special issue in the *Journal of Physical Chemistry* in honor of my work. There wasn't a symposium, but papers were solicited, and a number of people were in charge of the issue, different sections of it.

COHEN: What year was this?

MARCUS: That was 1986. Norman Sutin was involved, and also Jonathan Connor, a former postdoc, now a professor at the University of Manchester. These were people who knew a fair amount about me. Norman knows a lot about my work in electron transfer, so he suggested various people in electron transfer to contribute articles. Dick Bernstein and Ahmed asked people in the field of gas phase reactions, including unimolecular reactions. I don't remember the details of how it went. It was really centered, in a sense, around the two main theories—the electron-transfer theory, on one hand, and the unimolecular reaction theory, or RRKM, on the other. Those theories and the semiclassical work were brought out in a number of introductory articles that Zewail and Bernstein, Sutin, and Connor, wrote in this special issue. Zewail was the editor in charge of the issue.

COHEN: Then you went on, I don't even have them listed here, but there were lots of them. How about the Wolf Prize?

MARCUS: That was, of course, the first really big award I received. I think I heard about it in January 1985, and the ceremonies in Israel were in May of 1985.

COHEN: Was that your first visit to Israel?

MARCUS: No, because I had given a series of lectures there—the Raymond and Beverly Sachler Lectures—in 1980. So it was my second visit. The Wolf Prize is highly regarded. I was glad to receive it. [Laughter]

COHEN: Then after that, want to mention some of the others?

MARCUS: Well, there have been a number. There have been various honorary doctorates from universities, including some in Canada, and one in Sweden.

COHEN: The one in Canada, was that your own school?

MARCUS: Yes, McGill; then also the University of New Brunswick, and Queens University. In the States, it's been the University of Chicago and Brooklyn Polytechnic, where I taught. Abroad, it's been in Gothenburg, in Sweden. So those have been the honorary doctorates. There have been quite a few awards from the American Chemical Society, including medals from various sections, such as the Gibbs Medal from the Chicago section, the Richards Medal from the Boston area, and the Pauling Medal from the northwest section. In Britain there have been some medals from the Royal Society of Chemistry—which is different from the Royal Society of London.

COHEN: Have some of them given you more pleasure than others?

MARCUS: I'm sure there have been, but at the moment… [Laughter] I'm sure the Wolf was special. Probably also in '78, receiving the Langmuir Award in Chemical Physics of the American Chemical Society. I'm sure that must have brought me a lot of pleasure. That was the first fairly big one. There are awards such as the fellowships—the Alfred P. Sloan Fellowship in '60—and various other honors like that that no doubt brought much pleasure. Then some years later, in '88, I received the Peter Debye Award in Physical Chemistry and in '89 the National Medal of Science. It was a good feeling to get those prizes. Also I was delighted to receive the first honorary degree, which was from the University of Chicago.

COHEN: What year was that?

MARCUS: That was 1983. It was a novelty for me. Or the first commencement address that I gave—I have now given several—which was at McGill, in 1988. You know, when something is a first in one's experience, that's special. But I've enjoyed each of these occasions. There are various other honors that, of course, give one a certain amount of pleasure. For example, something that meant a lot to me was being elected a foreign member of the Royal Society of London. There aren't many foreign members, and perhaps because I grew up in Canada I've always had a somewhat different feeling for England. Being a professorial fellow at University College at Oxford brought me tremendous pleasure. Being elected to the American Philosophical Society was a pleasure. Of course, going back further, to 1970—being elected to membership in the National Academy of Sciences was something that I felt very good about. It's true that once one receives some of these honors, they sort of fall into a kind of perspective. One doesn't think about them. But at the time, and for a while after, one certainly does.

It's also true that to some extent the awards that one wins may depend upon the particular field that one's in. For example, some fields are more attractive, more known to people. Some individuals can do terrifically good work in a little-occupied field and not be recognized for it.

COHEN: And then, of course, last year was the big one.

MARCUS: Last year was the very big one [referring to the Nobel Prize]. In experience, it's an order of magnitude bigger than anything—maybe two orders of magnitude. [Laughter] The week in Stockholm was probably the most intensive week of our lives. There was something going on almost all the time. It involved the whole family; we took everybody, the two daughters-in-law and the three sons. And then we also invited some people who were very influential in one way or another in my previous scientific work and who came, Norman Sutin and his wife, for example.

COHEN: Were you able to invite whomever you wanted?

MARCUS: No, you're limited to a certain number. The committee there was kind enough to let me exceed that number by two or three. I offered to pay the travel and other expenses of the guests, because the Nobel Foundation would cover the expenses only of the honoree and spouse and of any children under 21. However, these non-family guests insisted on covering their own expenses. For example, one of the guests—the key person involved in experimental tests of the unimolecular theory, even though the award was not for the unimolecular work—was Seymour Rabinovitch, so I invited him and his wife. A person who was involved in the experiment that tested a key part of the electron transfer theory—the test of a prediction that took twenty-five years for confirmation by Closs, Miller and Calcaterra of the University of Chicago and Argonne National Laboratory—was one of my guests. Unfortunately, Gerhard Closs had already died, but John Miller came.

COHEN: Is that traditional, or typical?

MARCUS: I don't know what's typical. I suspect it isn't. But anyway, I wanted to do that.

COHEN: Has your life changed?

MARCUS: Yes, it's certainly changed. Now, it's a question of trying to get back to some normalcy.

COHEN: Do people bother you about this money?

MARCUS: No, very rarely now. I certainly have received a number of requests for funds. One wanted it for a dowry for his daughter. Another wanted it for the education of his sons. Another one was afraid the FBI was after him, and wanted $400 million. But there have been very few of those requests, and certainly none recently. For a while there were some, no more than half a dozen, and probably closer to four or five.

But there are a lot of requests for me to sign various petitions. These are petitions that many Nobel laureates may have signed.

COHEN: What do you do about that?

MARCUS: Well, in most cases I don't do anything. In some cases, I'll send them a note, saying why I'm not doing something. But I think that just because other Nobel laureates have signed is no reason why I should sign it.

COHEN: Would you think of yourself as being apolitical, and that's why you just don't want to do it?

MARCUS: Well, I have definite political feelings, but I want to keep the science separate from that. I've been asked to talk on all sorts of subjects, over the phone, or to write about them—topics I have no real expertise on. Typically I haven't done anything there. I have probably replied in most cases, but not with any information. Because I'm not really an expert in those subjects, I prefer not to just lend a name to some cause without wholeheartedly and knowledgeably supporting it. Somehow that course seems a little superficial to me. I'm sure it's very meritorious, but on the other hand, just because you get a Nobel Prize—it may impress some people, but I feel a little uncomfortable with using it for another purpose. I know there's one I believe I did sign, and another one I'm thinking of signing. However, there was one petition, if I recall correctly, that came in about teachers in India, and I don't know anything about teachers in India. [Laughter] And then there is quite a bit of miscellaneous correspondence, and a large number of autograph and photograph requests. At some international meetings there are frequently many autograph requests.

In terms of numbers of invitations, it's made a lot of difference. I certainly was receiving a large number of invitations before this; but the number really exploded, and in some cases they offered to pay Laura's way. [Tape ends]

Begin Tape 3, Side 2

COHEN: How about social things right here in town?

MARCUS: There, I've done a large number of things—many talks. I gave a talk to the Associates here and talked to the Associates in San Francisco and to alumni, to the students, a brief talk to the trustees at one of their meetings, and to other groups at Caltech. There have been quite a few different group talks of a type that I'd never given before. Normally, the lectures I'd given before had all been scientific ones, except for the occasional commencement address. I think that at that point there may have just been one. I've since given several. It turns out that most of the invitations I receive I turn down, such as those that are not that close to my research interest, or I wouldn't learn much from. But there are some that look very interesting—perhaps it's a particular meeting, or something else—that I've gone ahead. For instance, Laura and I enjoyed very much going to southern Portugal, and then on a different trip to southern Spain this year. [Laughter] Occasionally, there's some event—for example, there's a friend of ours in Berlin, Heinz Gerischer, who's going to be 75, and they want to have a special lecture, and they invited me to come. Since he's a friend, an electrochemist whom I've known for many years, I happily agreed. Normally, for some event like that I wouldn't go, but here I feel I really should. There have been all sorts of invitations that come up. In the past couple of weeks, for example, one is an invitation to be an honorary member of some particular international society of electrochemistry. Okay, I've been made an honorary member of some organizations before the prize. I'll accept that one. Another one is to be an honorary professor at one of the universities in Shanghai; and I'll write a letter accepting that. Another was an invitation from the Ministry of Education to go to Taiwan for a week with Laura; they would take care of everything. Also an invitation to visit several universities or institutes on the Chinese mainland. So there are some things that look very interesting, and those we do accept. There are certainly a large number of invitations, including now many named lectures. These are seminars that bear a name attached to it. I've been turning down even some of those. But I was turning down some before the prize anyway.

It's made a difference in terms of the nature and scope of the invitations, and the number. But I try to watch myself.

COHEN: Have you gotten sort of a freer feeling? I mean, there's more money. There are things you're doing that you didn't do before.

MARCUS: You mean money for personal travel?

COHEN: Well, no, for your enjoyment.

MARCUS: I would say that it hasn't made any difference there. I mean, we're not doing anything differently now, from that point of view, than we were doing before. You know, when you've been brought up in a certain way—especially in the Depression, you don't change just because of some big prize. I haven't gone out and bought some very expensive car, for example. I might have, had there not been so much crime around. [Laughter] I wouldn't want such a visible car. [Laughter] No, no. I probably wouldn't have anyway.

No, the main difference that it's made is in what seems to be the reception by various people: the types of invitations one gets, and the number, and then responding to all these invitations and lectures. Whether you go or not, you respond. So there's that correspondence, and there's other correspondence. And of course, rightly or wrongly, people recognize one more now, in one way or another, even if it's just walking on the campus. So there's maybe some sense of some difference. But mostly life goes on. Except, there's been so much distraction, and also answering these many letters, that it hasn't been easy to try to swing back yet into a largely research mode. In fact, the main work I've done recently is to go over various papers that the students or postdocs have given me, and make corrections, and do work like that. But in my own research, I have not put in as much of a concentrated effort as I would like to. I definitely will do that, one way or another. I've lost some of that. So I'm going to try to get back to it. It's just that there are commitments, responsibilities or requests, so many various requests for reviews, many of which I can't review. That there are simply too many things to do, and it's a question of deciding what can be done. But one has to make a lot of those decisions, and it takes time.

COHEN: Well, is there anything you haven't mentioned that you'd like to bring up?

MARCUS: There are certain things, and perhaps I've said them already. One is that I wish that there weren't all this hype that's associated with research nowadays. Perhaps it was present before and I didn't recognize it. But now there's so much of it—to get attention perhaps, or to get funds. It's not right, and it bothers me when I see it. I'll see somebody ascribing to their work more than is really warranted—work that really doesn't have the broad impact that is being claimed. Or somebody pumping up some work and saying what they could do. Better say what you do do. [Laughter] Or somebody's saying, "Well, this has implications for such-and-such." Perhaps it does, but not yet.

COHEN: Well, this is the competitiveness of grants and funds that has caused this.

MARCUS: Perhaps, yes, and also, maybe there are just so many people involved in the whole research world that maybe some of them may feel that in order to get attention they have to claim more than what's justified. In the early days, when I was starting, perhaps that happened, but I wasn't conscious of it. Now, of course I know more of what's going on. But maybe it's the crowded nature of the science profession now: we've been producing so many scientists—perhaps producing too many—that it may make some people do that. Some people are very quiet, restrained about everything. Of course, in a way, what is best is a balance. In meeting the scientific community and in talking to scientific groups, undoubtedly it helps to have a little charisma. One doesn't want to give a dull lecture. On the other hand, one shouldn't be making claims of things beyond what's justified, or inflating something out of all proportion. A certain happy mean would be more desirable. I don't know how one encourages that quality in individuals, but I wish it were the case.

Another point—what I believe should be a guiding light, the crux that makes this life in science interesting—is really trying to find interesting problems and work on them, instead of doing the research because it might have an impact on colleagues. Perhaps in some fields one may have to think about vast implications immediately, but it seems to me that that preoccupation with the effect on one's scientific colleagues can focus attention, in a sense, on the wrong goal. What can be so exciting about research is encountering problems that you don't quite understand, and then seeing whether you can come up with an answer. Focusing instead on "Oh, this may have applications to this, and make this place famous or make that individual famous"—I think that's wrong. I'm not sure how much of that attitude there is, but I think it isn't the best one. What I've enjoyed so much is coming across, in one way or another, some problem and trying to obtain an answer when I didn't know the answer to start with, and maybe wasn't even clear in what direction to go. I would like to see a lot more of that. But maybe, in the world we have now, that hope is too naive.

Another observation that has struck me is the randomness, the chance nature of so many different steps in one's career. I can probably think of at least a half a dozen steps that, if they had turned out slightly differently, would have affected my career considerably. There are just so many different chances: the direction one goes in, the problems one works on. So when people wonder about what to do some time from now in research, it's hard to predict that far ahead. I remember one incident in the early days, when I was a sophomore at McGill. I had a small bursary to cover some expenses. (To receive it I worked for some evenings each week in a dusty law library.) But I didn't know where the money was going to come from for the second semester. I happened to accidentally meet a chap I'd gone to high school with, Hyman Charles Felix Shatan (later a psychiatrist with an appointment at Columbia). He mentioned some new funds that had appeared, called the Dominion-Provincial Bursaries or Scholarships. The idea was to support students in the sciences, so as to get them through college so that they could get on with the war effort. I then applied for one of those. They were probably not too difficult to get. In any event, I received one. It covered my tuition for the remaining period of my undergraduate education. But, if I hadn't bumped into him, I'm not sure whether or where I would have acquired the funds.

Another factor in my now rather lengthy career is the tremendous intensity and amount of work that I've done—although I enjoy it most of the time, but there has been less time for other activities. Above all I would like to stress the role that Laura has played these forty-four years—her upbeat attitude, her sense of humor, and the wholehearted and enthusiastic support she has provided during these many years. She has been of considerable help with the writing of sensitive letters—she has a real writing talent, and has written many articles, often based on interviews, for various Caltech-related publications, for example. They have been well-received.

One thing I've enjoyed very much is outdoor activity. Before our children were born, Laura and I used to go camping and traveling around for roughly one month every summer. After they came, we went camping for a while. Then going skiing with the boys, or playing tennis with them—I've enjoyed that so much. And now coming to Caltech, and playing tennis. I mentioned how good my father was in sports. Well, I don't know that any of it was passed on, but some of his same love of sports is certainly present, and I've enjoyed it as a family activity. It's been an important part. Our three sons love sports.

COHEN: Did Hollywood come to find you after the Nobel Prize?

MARCUS: There was somebody from Hollywood, shortly after the prize, who wanted to get together. Normally, at that point I was handling everything involving media through Max [Benavidez] in Public Relations, because he would know how to deal with it. Max was wonderful, incidentally, when I heard about the prize at a meeting in Toronto. Let me just divert.

As soon as Caltech heard about the prize, the person in charge of Public Relations, Bob O'Rourke, suggested that Max Benavidez, who was the new director of media relations, go to Toronto to screen the media's calls for interviews. So Max did. That was a tremendous help to me, because for a good part of the first day I was trying to handle the press over the phone; a huge number of calls came in, in a short period of time. Then Max arrived and by that time I'd been moved up to the Queen's royal suite at the Royal York Hotel at the suggestion of Wayne Worrell, who was the president of the Electrochemical Society at that time. I occupied one of the bedrooms; Max used the Queen's dining room as a base for screening phone calls from the press and others.

Sometime after I returned to Pasadena, somebody from Hollywood who had made some well-known film or TV production contacted us, through Max, about talking with the new Nobel Prize winner. This director gets ideas from talking with all sorts of people. [Laughter] At first, without any thought about it, I said no. But then afterwards, I had second thoughts, because one of our sons had previously been in the television industry. He was what's called a producer/director for ABC, and he shot about 25 episodes there. He was involved with documentaries over in England—he was at this point a graduate student at Cambridge there. I knew that he probably wanted to return to it. So then I quickly afterwards told Max, "Change the answers. If they can make an appointment when Alan is here during the Christmas holidays, then we'll all get together." So an appointment was set up, but afterwards this chap had to cancel for that day. He wanted to set another date, but Alan would have left by then, so I said no.

COHEN: What meeting did you enjoy the most during the last year?

MARCUS: It's the most unusual meeting I've ever been to. The event or award is called the

American Academy of Achievement Golden Plate Award. It brings together some 450 high school students who are close to graduating, and quite a few—of the order of 125—"celebrities," and lets them mix together for a couple of days. It's a yearly event, but with different celebrities, although no doubt there are many repeaters. Some of them talk to the group about their experiences. That was a fabulous experience. It's held in different places, but when we went in June 1993, it was held at Glacier National Park. We stayed at its main lodge, in that magnificent park. There was such a variety of people. There were perhaps 20 Nobel laureates, most of whom had received their prize some years before. There were individuals from all walks of life. There were country singers, such as Johnny Cash; a general, Schwarzkopf; the director of the CIA, Woolsey; the outgoing head of the FBI, Webster; high achievers in industry; big name authors and artists; heads of studios, such as Sherry Lansing from, I believe, Paramount; top athletes in various fields, such as Doctor J. in basketball and Herschel Walker in football. The emcee on the night they introduced the new awardees—there were 25 of us—was Tom Selleck, who did, I thought, a very good job of describing the accomplishments of each. The students were high school kids, and we interacted with them. In fact, when I gave some talks here at Caltech—such as at the Frosh Camp at Catalina—there were at least some of these conference students who came later that year (1993) to study at Caltech, one or two anyway. Three common themes of the 25 honorees who spoke at the Academy of Achievement were the role of chance, hard work and good luck.

COHEN: Who sponsors this?

MARCUS: The American Academy of Achievement receives money from a wide variety of industries and other sources. I believe it takes $10,000 to sponsor one student. But they get money from those sources. In any event, it was such a fabulous affair! The person who arranged for much of the local support there was somebody from Montana. I think he owned part of the railroad there. But we went to a certain airport, collected as a group, then went to the railroad at Kalispell, and then went in his private train to the lodge.

What I've done much of since the announcement of the prize is to give many talks to groups that are not really scientific groups. It has certainly been an interesting and busy period of my life.